3 It must be observed that the instruments intended for the combinatorics are mostly traditional, static and trite; the instruments for analysis powerfully embody innovation.

4 And in the *Elementa nova matheseos universalis* (written between 1684 and 1687): "*Tradetur et Synthesis et Analysis, sive tam Combinatoria, quam Algebra.*" (Leibniz VE, 987).

5 In this way, if the specific knowledge that enters in a logical calculation is already set up, it will be easier to coordinate this particular specimen of the art to the general frame of the universal characteristic.

CRAIG G. FRASER

THE BACKGROUND TO AND EARLY EMERGENCE OF EULER'S ANALYSIS

I Introduction

In cultivating analysis Euler is sometimes seen as someone whose primary achievement was the development of tendencies in the Leibnizian school. Typical here is Bourbaki's statement (Bourbaki 1974, 246) that he carried "the Leibnizian formalism to an extreme" thereby "completing the work of Leibniz". A somewhat different view is expressed by Boyer (Boyer 1939, 243) who calls attention to Euler's originality: "Most of his predecessors had considered the differential calculus as bound up with geometry, but Euler made the subject a formal theory of functions which had no need to revert to diagrams or geometrical conceptions"[1].

The present paper is devoted to a study of the role of analysis in the background to and early development of Euler's mathematical research. Euler's *Methodus inveniendi lineas curvas* of 1744 (Euler 1744), the first systematic treatise on what would later become known as the calculus of variations, is here identified as the locus classicus for the initial emergence of a fully analytical conception of the calculus. The work contained many of the technical and notational innovations that would be elaborated in his mid-century textbooks on infinitesimal analysis. In addition, in chapter four of the treatise Euler developed the subject in a way that exhibited its analytical character at a deeper theoretical level.

To understand the origins of Euler's programme we first provide a survey of analytical conceptions in earlier mathematics. We then turn to a consideration of the relevant parts of the *Methodus inveniendi*, ending with a discussion of the mathematical and philosophical character of his approach to analysis.

II Analytical Methods in Early Modern Mathematics

It is possible to trace a continuous development in European mathematics that begins in the thirteenth century and leads by 1700 to the extensive employment of symbolic methods. Techniques of analysis came to play an important role in such distinct areas as the theory of determinate equations, arithmetic, coordinate geometry and the calculus. Our survey will focus on the emergence of the concepts

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 47–78.

of equation and variable, and on the question of the degree to which symbolic methods formulated essential mathematical features of the subject under study.

II.1 Analytic Art

The concept of analysis and the name itself became part of early modern mathematics largely as a result of the work of François Viète. His essay of 1591, *In artem analyticen isagoge* (1591*a*), initiated a series of researches by himself and such contemporaries of his as Marino Ghetaldi and Thomas Harriot that together contributed to the widespread employment in the seventeenth century of symbolic mathematical methods.

A substantial historical literature, deriving from the work of Jakob Klein (Klein 1934-1936), emphasizes Viète's modernity as a mathematician. It is suggested that his notion of specious logistic involved a theoretical widening of the concept of magnitude to include both arithmetic and geometric quantity. In adapting ideas from Diophantus's arithmetic to the realm of geometric analysis he was led to generalize Diophantus's concept of species. According to Klein (*ibid.*, 166-167), "the *eidos* concept, the concept of the 'species', *undergoes a universalizing extension while preserving its tie to the realm of numbers. In the light of this general procedure, the species*, or as Viète also says, the 'forms of things'[...] *represent 'general' magnitudes simply*"[2].

Associated with this general concept of number, it is suggested, there emerged in his analytic art, with its use of symbols to represent unknowns and parameters, a structural, syntactic approach to mathematics[3]. Because the terms of his system could be given different interpretations in arithmetic and geometry the purely combinatorial properties of operations performed on analytical expressions were exhibited as an object of interest.

Klein's essay and the historical writings it has inspired have resulted in a renewed interest in Viète's algebra and have led to a better appreciation of his role in early modern mathematics. We will however argue in what follows that suggestive and informative as Klein's essay has been, his whole thesis must be qualified at certain fundamental points.

The widening of the concept of magnitude that is attributed to Viète had already taken place and was well assimilated within algebraic practice at least a century before he wrote. Algebra was known as "the art of the thing and the power" or "the great art" or "the greater part of arithmetic". The progress of symbolic methods consisted of the replacement of the largely rhetorical procedures inherited from Islamic mathematicians by ones that used a syncopated or partial formalism in the solution of problems involving the determination of an unknown quantity. Study of quadratic, cubic and quartic equations led to the introduction of expressions denoting the roots of non-square numbers; thus magnitudes traditionally regarded as geometrical entities were denoted as numbers within the confines of what was essentially an arithmetical algebra.

In emphasizing the radical character of the Viètan concept of magnitude, Klein has overlooked the full *mathematical* significance inherent in the assimilation (well established by 1590) of surd numbers into arithmetical algebra.He is to be sure aware of this earlier tradition, writing that "the new number concept [...] already controlled, although not explicitly, the algebraic expressions and investigations of Stifel, Cardano, Tartaglia, etc." (*ibid.*, 178). Nevertheless he concludes of the cossist school that "in its whole mode of operating with numbers and number signs, its self-understanding fails to keep pace with these technical advances. This algebraic school becomes conscious of its own 'scientific' character and of the novelty of its 'number' concept only at the moment of direct contact with the corresponding Greek science, *i.e.*, with the *Arithmetic* of Diophantus" (*ibid.*, 148). To this one may reply in two ways. Self-consciousness on the part of researchers, however significant, is not necessary in order for important conceptual advances to take place; the latter may be, as they were for the cossist algebraists, logical concomitants of technical developments within the subject itself. Second, if indeed an explicit awareness of conceptual advance is present it is necessary to show how this influenced and shaped the direction of mathematical research.

Another difficulty with Klein's thesis is that it understates the extent to which Viète situated his notion of species within a classical Euclidean theory of magnitude. He seems to have regarded the general magnitudes of his specious logistic as geometrical entities. He uses the words "ducere" and "adplicare", terms denoting geometric operations, in his definition of the multiplication and division of magnitudes (writing for example, "magnitudinem in magnitudinem ducere"), and retains dimensional homogeneity as a fundamental principle. His vision of a general theory of quantity applicable to either number or line segments was already realized in *Elements* V, a part of the Euclidean canon that he drew upon in chapter 2 of his *Analytic Art.* (Advocates of the notion of "symbolic magnitude" never explain how book V of the *Elements*—a general theory of magnitude without symbols in the Viètan sense—is possible.)

Certainly Viète showed a stronger interest in mathematical method than had earlier researchers. To attribute to him a radical new syntactic or structural conception of mathematics seems however doubtful. He viewed analysis not as an autonomous subject but as an "art", as a tool in solving problems, be they ones in geometry, the theory of equations or Diophantine arithmetic. The content of mathematics was for him not a system of relations but a set of concrete problems in these subjects. His notational innovations were developed within this historically particular programme of research. His technical vocabulary and fondness for formal categories indicate the continued influence on him of scholastic thought. Incongruous mathematical elements were contained in his attempt to adapt ideas

from Diophantine arithmetic, essentially a work of rational number theory, to the art of algebra as it was employed in the solution of geometrical problems.

Viète's conceptual advances, the introduction of distinct symbols for variables and parameters and the adoption of an operational formalism, represented a significant contribution to mathematical method. They provided an orderly and uniform notation for handling the material on algebraic identities and polynomial equations that had appeared in Cardano's *Ars Magna* (Cardano 1545), and permitted the emergence of "the first consciously articulated theory of equations" (Mahoney 1973, 36). Perhaps most important mathematically, his notational system allowed one to investigate the relationship between the coefficients of a polynomial and the structure of its roots; it must be said however that this last line of investigation developed slowly and only became established in the later eighteenth century.

Of considerable conceptual significance, particularly for the later development of the calculus, was the idea of a function. The notion of a general expression $f(A)$ defined in terms of the variable A was present in embryonic form in Viète's system, where the square of the magnitude denoted by the symbol A was denoted by an expression ("A quadratus") that itself contained A. Instead of the "res" and the "census" of traditional algebra, separate terms denoting distinct entities, one now had a notation that reflected the underlying operations performed on the magnitudes being represented. That the functional idea could only receive a somewhat limited development by Viète was a consequence of the fact that he viewed his symbol "A" not as a variable in the full sense but as an unknown, an object whose value was to be determined in the course of the solution of a problem (Boyer 1956, 60). His definition of an equation, "the coupling of an unknown magnitude with a known" reflected this particular perspective.

II.2 Theory of Numbers

The figures of Euclidean plane geometry are coherent unitary objects whose identity is defined in terms of certain universal attributes, such as being three-sided or being right-angled. Results in geometry become theorems by virtue of the inherent generality of figures as mathematical objects. As commentators from Leibniz to Frege have emphasized, whole numbers—the objects of arithmetic—are different sorts of things, possessing particular individual characteristics[4]. Propositions in Euclidean arithmetic (*Elements* VII, VIII and IX) are formulated in terms of classes of numbers, such as being prime, being perfect, or being a member of a geometric progression. These classes are delineated rhetorically, without the aid of symbolic notation.

It is ironic that Viète turned to Diophantus's *Arithmetic*, a work of rational number theory, as a source of inspiration for developing methods in algebra and geometry, the sciences (for him) of continuous magnitude. An opposite sort of irony characterized Pierre Fermat's extensive researches in theoretical arithmetic[5]. In his study of geometry he adopted Viète's system of notation, using it to formulate mathematically the idea of coordinate geometry. He also studied the *Arithmetic* carefully and greatly extended the results contained there, in the process laying the foundation of modern number theory. Throughout these latter researches he employed a predominately rhetorical mode of presentation. Although he used hindu-arabic numerals and some signs for arithmetic operations, his statement and demonstration of theorems were presented in words without the aid of symbolic notation.

The style of Fermat's writings is illustrated by a comparison with Euclid, whose mode of expression in number theory was also rhetorical. Consider Euclid's assertion (*Elements* IX, 36) that a number of the form $2^{p-1}(2^p-1)$ is perfect if 2^p-1 is prime[6]: "If as many numbers as we please beginning from an unit be set out continuously in double proportion, until the sum of all becomes prime, and if the sum multiplied into the last make some number, the product will be perfect".

Consider now Fermat's original statement of what is known as Fermat's little theorem, the assertion (in modern mathematical language) that p divides $a^{p-1}-1$, where a and p are relatively prime numbers[7]: "Without exception, every prime number measures one of the powers -1 of any progression whatever, and the exponent of the said power is a submultiple of the given prime number -1" (Fermat, TH, V. 1, 209).

In his rhetorical expression as well as in his interest in integral rather than rational solutions Fermat seemed to be looking past Diophantus to the arithmetic books of Euclid's *Elements* as a source of inspiration. In 1657 he explicitly criticized the use of geometrical considerations in arithmetic (presumably because they entailed conceptions of continuous magnitude) and, appealing to Euclid, urged that "arithmetic redeem the doctrine of whole numbers as a patrimony of its own"[8]. Although many problems of rational arithmetic reduced to ones of whole-number arithmetic it was also the case that certain interesting questions in the latter subject became trivial when the class of permissible solutions was extended to rational numbers. It is very possible that his disinclination to use literal notation derived from a desire to emphasize the autonomy of whole-number arithmetic.

There is it must be noted some evidence that Fermat privately employed algebraic methods in his arithmetic researches, and some of his correspondents suspected him of having done so. His contemporary Descartes made use of formulas to express arithmetical results. Nevertheless, in all of his extant writings, in all of the different phases of his research, Fermat did not employ symbolic algebraic notation.

The awkwardness of rhetorical formulations and the need for more and more detailed statements of results eventually imposed restrictions on the sort of theory

that could be developed. Fermat's decision not to give a fuller account of his researches may have derived in part from the demands that such a mode of exposition entailed. The concept of an arithmetic variable—an entity that could assume any of a given set of whole-number values—was central to the progress of number theory as it was to develop after him. It enabled one to reify in formulas expressions and relations that could then be studied or manipulated at will in the course of the investigation.

It should nevertheless be remembered that at the most fundamental level it was numbers and their properties, and not any system of relations embodying these properties, which constituted the fundamental subject of the theory of numbers. The role of the variable was not an essential one; each symbolic statement could always be re-expressed in terms of a proposition about classes of numbers.

II.3 Coordinate Geometry[9]

Euclid and Apollonius had derived results about curves that express relations of equality between magnitudes associated with these figures, relations that are valid for an arbitrary point taken on the perimeter of the curve. In *Elements* III, 36 one is given a point D outside of a circle and asked to draw from it two lines; the first DB is tangent to the circle and the second DCA cuts the circle at the points C and A (fig. 1). Euclid showed that the square on DB is equal to the rectangle on DC and DA. In book I of the *Conics* Apollonius introduced the ellipse as the section obtained by intersecting a plane with an oblique circular cone (fig. 2). Such a cone is formed by the lines joining the perimeter of a circle to a point not in the plane of the circle. Let PP' be a given axis through the centre of the ellipse and let Q be a point on the perimeter of the ellipse. Consider the line VQ of intersection of the plane of the ellipse and the plane of that circle through V which is parallel to the base; Q is the point where the line meets the ellipse. The line VQ is called an "ordinate". In I, 15 Apollonius showed that the rectangle on PV and VP' is in a given constant ratio to the square on VQ.

In these propositions the curve is introduced and the given relation is then exhibited as a property satisfied by it. The relation represents one of several properties and is not regarded as defining or definitively expressing the curve. The primary purpose of the results is found in the solution of other problems. In *Elements* IV Euclid used III, 36 in his construction of the regular pentagon. In *Conics* III Apollonius employed the theory of the earlier books in his investigation of the problem of the locus to three and four lines.

This last problem is of great historical significance for the later development of coordinate and projective geometry and possesses in its own right certain points of conceptual interest. Consider four lines given in position in the plane. It is necessary to determine the locus of points P such that the rectangle formed by the

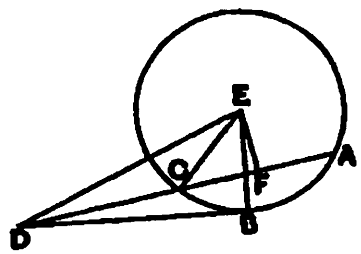


Figure 1

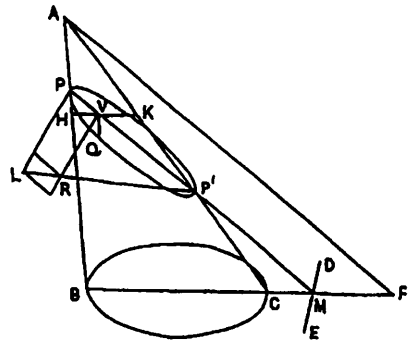


Figure 2

distances from P measured in given directions to the first two lines is in a specified ratio to the rectangle formed by the distances measured in given directions to the other two lines. (In the case of three lines one of the rectangles becomes a square.) It turns out that the locus is in every instance a conic section. In the same book Apollonius provided a detailed discussion of the problem, developing results that would (at least in principle) form the basis for a complete solution[10].

In book VII of his *Collection* Pappus called attention to the three and four line problem and discussed the work of earlier geometers[11]. He also raised the question of the nature of the locus when the number of lines exceeds four. The distances that appear in this problem are magnitudes that are assumed to vary while the relation expressed by the locus condition itself continues to hold. (This relation was expressed in two forms by Pappus, in terms of the ratio of figures or solids, and for the more general case in terms of compound proportions.) What logically distinguishes these magnitudes within the problem is that they vary, and that the locus is produced in consequence of their variation. The concept of a variable would therefore seem to be implicitly present in Pappus's formulation.

The *Collection* became available in Western Europe in 1588 in Commandino's Latin translation (Commandino 1588). When Descartes began to study the locus problem in 1632 he did so having already had some grounding in Viètan algebra and the theory of equations. His *Géométrie* (1637) may be seen as a fairly natural development arising from the application of algebraic methods to a problem of current interest. His approach to the investigation of the locus was very simple. Let AB be one of the lines that are given in position, C be a point on the locus and CB the line segment that is to be drawn from C to AB. Descartes took AB and CB as his given reference lines and let $x=AB$ and $y=CB$ (fig. 3). (Notice that the problem is especially suited to coordinate methods, because the line segment CB from C to AB is always drawn at the same angle to AB.) He calculated

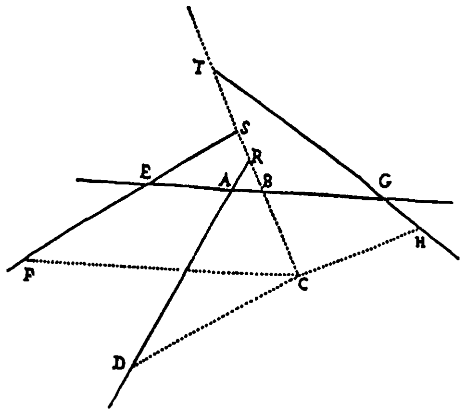


Figure 3

the various distances of the problem in terms of x and y and proceeded to express the locus condition as an indeterminate equation in these variables.

In the original locus problem there were as many variable magnitudes as there were lines given in position. In Descartes' geometry by contrast the problem was reduced to the consideration of two variables connected by means of an equation. His theory opened up the possibility—at least in principle—that continuous variation could be studied by examining how one variable changes with respect to the other within such a relation.

The last question however was one that Descartes never pursued. His investigation remained firmly centred on the classical problem of constructing solutions to geometrical problems. His interest in equations was based primarily on the role they played in such solutions. Within this programme it was necessary to determine points on a curve by means of acceptable instruments of construction (Bos 1981).The curve enjoyed a dual status, as something that was a solution to a geometrical problem and as something that could itself be used as a tool in the construction of a solution. The study of indeterminate equations yielded information about the associated curves, while determinate equations could be solved to obtain particular points on the curve.

Fermat's writings from the same period demonstrate a better appreciation of the general methodological character of coordinate geometry. In his *Ad locos et solidos isagoge* of 1637 (TH, I, 4, 91-110) he enunciated the principle that to any equation in two variables there corresponds a curve in the plane, one given by means of the graphical method of his coordinate system[12]. He was however primarily interested in geometrical loci problems, in which the final equation is always

an algebraic or polynomial relation. His continued interest in restoring Greek mathematical works indicated the strong classical character of his investigation.

Throughout the early history of coordinate geometry there seems to have been little interest in the mathematical investigation by means of graphical techniques of arbitrary relations among magnitudes, abstractly considered. The familiar modern use of graphs to represent the behaviour of virtually any two related quantities that are found anywhere was notably absent during the period.

II.4 The Calculus

II.4.1 *Equations*

While established research in coordinate geometry remained centred on geometrical construction a whole new line of investigation was opened up with the growing interest in quadrature and tangent problems. Early work on what later became the calculus was connected with the programme of study set forth in Van Schooten's Latin edition of Descartes's *Géométrie* (Descartes, 1659-1661). Out of these developments came a new part of mathematics, one that soon achieved considerable prominence as an area of research[13].The relevant history has been well documented in the literature. Our discussion will be confined to two examples which illustrate some of the conceptual and technical issues associated with the role of the equation in the early calculus.

The first example involves a comparison of Wallis's *Arithmetical infinitorum* (1656) and Newton's researches on infinite series from the 1660s. Wallis was a proponent of the new analysis and employed symbolic notation freely in his book. His primary goal was to investigate quadratures and cubatures by means of arithmetic methods involving infinite numerical series. In Proposition XIX he considered the series

$$\frac{0+1=1}{1+1=2}=\frac{1}{2}=\frac{1}{3}+\frac{1}{6}, \qquad \frac{0+1+4=5}{4+4+4=12}=\frac{1}{3}+\frac{1}{12}$$
$$\frac{0+1+4+9=14}{9+9+9+9=34}=\frac{7}{18}=\frac{1}{3}+\frac{1}{18}, etc.$$

It is clear that when the number of terms become infinite the value of the series is 1/3. (Wallis wrote down the general formula for the numerator as $\frac{l+1}{3}l^2+\frac{l+1}{6l}l^2$.

He showed how this result may be used to obtain the ratio of the area under a parabola to the circumscribed rectangle, and the ratio of the volume of a cone to

the circumscribed cylinder. He proceeded in the treatise to extend the result, and through the skilful and extensive use of interpolation went very far in obtaining numerical series expressions for various quadratures[14].

In the winter of 1664-1665 Newton began to study the *Arithmetica infinitorum*, research which he carried out at the same time he was reading Van Schooten's edition of the *Géométrie*. He recorded his progress in notebooks which have survived[15]. His fundamental innovation was to reformulate Wallis' investigation in terms of equations between Cartesian coordinate variables. By setting the problem in this way he made relations between continuously changing magnitudes the central object of study. An equation implies the existence of a relation that remains valid as the variables change continuously in value. It is this fundamental fact—the continuous and permanent character of the relation, its persistence differentially in the neighbourhood of each real number—that was exploited by Newton in expressing the connection between the equation of the curve and the formula for its quadrature. This fact would also be the basis for his subsequent investigation, set forth in the 1669 paper *De analysi*, relating the quadrature of a curve to its equation by means of differentiation[16].

Although Wallis was an advocate of the new analysis he did not make essential use of relations among variable magnitudes in his investigation. His approach was not "analytical" in the deeper sense discernible in Newton's early work on infinite series and quadratures.

Our second example concerns some later work of Newton and the French mathematician Pierre Varignon. The motion of a freely moving particle acted upon by a central force was the subject of book one of Newton's *Principia mathematica* (1687) as well as of a memoir by Varignon published by the Paris Academy in 1703 (Varignon 1701). Both men established that motion in an ellipse with the force centre at one focus implies an inverse-square force law. In a break with his early mathematical work of the 1660s Newton abandoned Cartesian analytical methods, turning instead to a kind of infinitesimal-geometrical theory of limits. Varignon by contrast used techniques of the recently established Leibnizian calculus in his solution.

In Proposition VI and its corollaries Newton had derived a measure for the force in terms of geometrical quantities associated with the curve. In the next few propositions he calculated the force law when the trajectory was assumed to have a given form. In Proposition XI he considered the case of the ellipse. In fig. 4 the point P is the position of the particle on the ellipse at a given instant, C is the centre of ellipse, S is one of the foci and the centre of the force, and CA and CB are the semi-major and semi-minor axes. Through P draw the tangent RP. The line DCK is drawn through C parallel to the tangent intersecting the ellipse at the points D and K. The lines CP and CD are then conjugate axes of the ellipse corresponding to the point P. Let E be the intersection of the SP and DC. Draw the

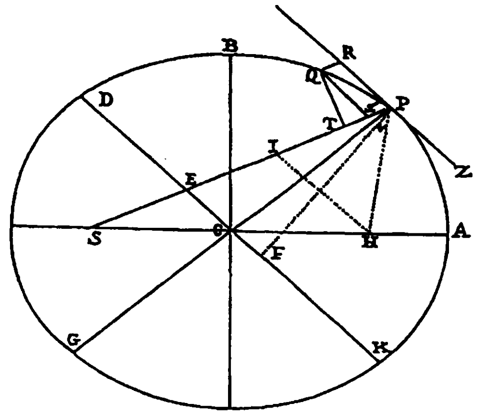


Figure 4

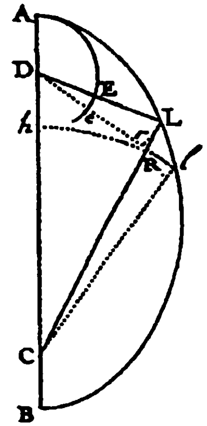


Figure 5

perpendicular PF from P to Dk. Let Q be a point on the ellipse near P. Draw the line Qv parallel to the tangent intersecting the conjugate diameter PCG at v. In the course of his derivation Newton made use of the following two equations:

$$Gv \cdot Pv : Qv^2 = PC^2 : CD^2$$
$$CA : PF = CD : CB$$

These he presented as known properties of the ellipse; of the second relation he noted that it had been "demonstrated by the writers on the conic sections."(Note that the first of these relations is the one from Apollonius's *Conics* I, discussed earlier.) He also proved that the quantity EP is a constant equal to the semi-major axis CA. Using this fact and the above relations he was able to show that the force is inversely proportional to the distance SP.

Varignon began by expressing the trajectory relative to a coordinate system in which the variables are the distance r from the force centre and the quantity z, where dz is defined as the projection of the element of path-length ds on the perpendicular to the radius. The tangential component of the force is equated to the expression dds/ddt, where s is the path length and t is the time. The derivation of the inverse-square law for the case of the ellipse is a model of simplicity. Consider the ellipse with major axis AB, foci D and C and force centre at C (fig. 5). Set $AB=a$, $DC=c$ and $b^2=a^2-c^2$. Let L be a point on the ellipse, $CL=r$. If l is a point close to L and the perpendicular lR is drawn to CL then the differential $dz=Rl$. Varignon gave the equation of the ellipse in the form[17]

$$bdr = dz\sqrt{4ar - 4rr - bb}$$

Using the relation $ds^2=dr^2+dz^2$ and the area law $rdz=dt$ he reexpressed this equation in the form

$$\frac{4a-4r}{r} = \frac{bbds^2}{dt^2}$$

Differentiation of this equation with respect to t led to the expression $\frac{2a}{b^2r^2}$ for the force, which yielded the desired result.

Both Newton and Varignon employed equations that express relations between continuously varying magnitudes and in this sense both of their derivations may be said to be analytical. There were however important differences of approach. In Newton's solution the ellipse with its various properties acts as a synthetic geometrical object, controlling the form of the derivation. In Varignon's memoir by contrast the ellipse is specified by a single equation between two variables relative to a fixed coordinate system. The entire mathematical content of the problem is reduced to the study of this equation; all of the properties of the ellipse needed for the solution are contained in it. The solution therefore evolves through a mechanical application of the differential algorithm.

II.4.2 *Graphical Techniques*

The curve was an object of considerable mathematical and physical interest throughout the seventeenth and eighteenth centuries. A few examples from the period 1680-1740 illustrate this point. The study of the relations that subsist between the lengths of curves gave rise to a theory of elliptic integrals. In work in the calculus of variations classes of curves constituted the primary object of study. In analytical dynamics attention was concentrated on determining the relation between trajectories and force laws. In the theory of elasticity researchers studied the shape of static equilibrium assumed by an elastic lamina under various loadings, as well as the configurations of a vibrating string.

The curve also played a fundamental and very different role in the conceptual foundation of the calculus. The situation is illustrated by work in problems of maxima and minima, an important part of the subject. In the very first published paper in the calculus Leibniz (1684) used his differential algorithm to derive the optical law of refraction from the principle that light follows the path of least time. He considered the points E and C on opposite sides of a line SS separating two optical media (fig. 6). It is necessary to find the point F on SS such that a ray of light travelling the path EFC does so in the least time. The time of transit from

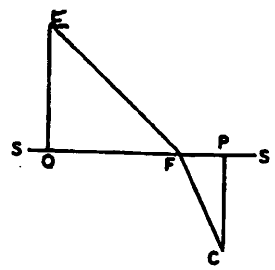


Figure 6

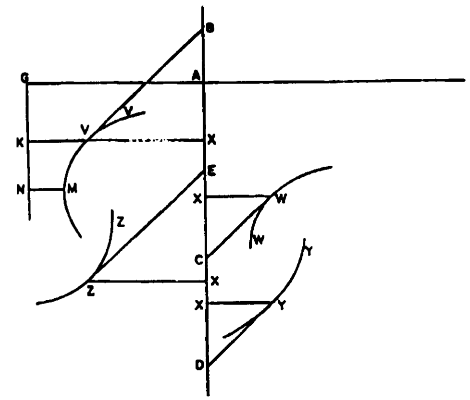


Figure 7

E to F is equal to the product of the distance EF and a constant equal to the reciprocal of the velocity in the first medium; this product Leibniz regarded as a rectangle of sides EF and a given constant line r. The time from F to C was likewise regarded as a rectangle of sides FC and a line h. The total time of transit along EFC is therefore equal to the sum of these rectangles. Leibniz (*ibid.* 1684) wrote: "Let us assume that all such possible sums of rectangles, or all possible paths, are represented by the ordinates KV of curve VV perpendicular to the line GK" (fig. 7)[18]. Letting $x=QF=GK$ be the abscissa and $w=KV$ be the ordinate he had in fig. 7 a curve VVM representing the time of transit as a function of the distance x from Q to F. He calculated this time as an expression in x and applied the differential theory he had previously introduced for curves to obtain the path given by the known law of refraction.

In this problem the primary object of interest is the relation between two magnitudes, the distance QF and the time of transit that corresponds to this distance. Although there is nothing in the nature of this relation that logically entails a geometric interpretation Leibniz nevertheless chose to represent it graphically by means of a curve. He could then apply his differential algorithm which had been introduced earlier for the analysis of curves.

Graphical procedures had been employed by Galileo in his *Discorsi* (1638) to relate the speed of a falling body to the time of its descent. They had become common in mathematical treatises by the late seventeenth century. Barrow in his *Lectiones geometricae* (1670) represented quadrature relationships in this way. In his *Principia mathematica* (1687) Newton investigated the inverse problem of central-force particle motion. In Propositions XXXIX and XLI of book one he graphed the force as a function of the projection of position on the orbital axis and

analyzed the resulting curve to arrive at expressions for the particle's trajectory. Jakob Bernoulli employed graphical methods throughout his researches of the 1690s. In his study of the elastica the relation between the restoring force and the distance along the lamina was superimposed in graphical form on the diagram of the actual physical system.

The first textbook on the differential calculus, l'Hôpital's *Analyse des infiniment petits* (1696), was a systematic attempt to ground the calculus in a theory of curves. The way in which this was done by him and other researchers of the period has been documented in the historical literature (Bos 1974). Of particular interest for the present discussion is his treatment of problems of maxima and minima. These problems were explicitly formulated as ones of finding the maximum or minimum ordinate of a curve. The equation of condition $dy = 0$ or $dy = \infty$ was deduced by considering successive values of dy and noting that about a maximum or minimum ordinate these values must change in sign. In several examples, each of which gave rise to a relation between two variables, he used graphical techniques to refer the problem of finding an extremum to the consideration of an associated curve.

In the ninth example l'Hôpital introduced a curve *AEB* (fig. 8) given in position and two fixed points *C* and *F*. Consider a variable point *P* on the curve and let $CP=u$ and $PF=z$. Consider a quantity (what would later be called a function) composed in some definite way from the variables u and z. It is necessary to find the point *P* so that this quantity is a maximum or a minimum. To solve this problem l'Hôpital joined the points *C* and *F* to form a base axis *CF*. The ordinates *QM* and *OD* give the values of the quantity corresponding to the points *P* and *E*. In contrast to the primary curve *AEB* the curve *MD* joining *M* and *D* is a purely

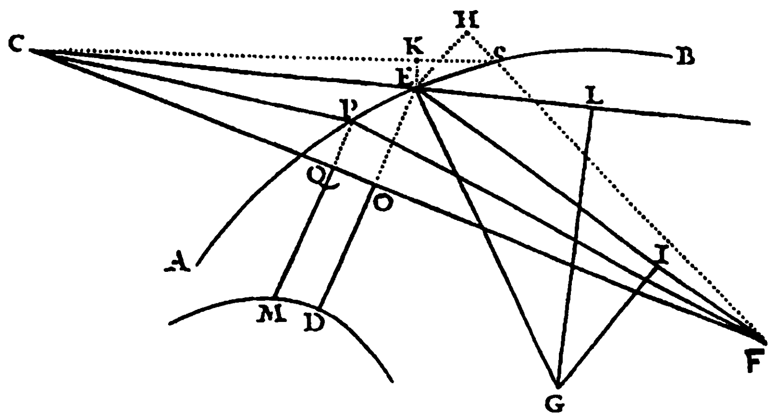


Figure 8

logical construction expressing the quantity as a function of position along *CF*. L'Hôpital observed that at *P* "the ordinate *QM* which becomes *OD* must be the greatest or least of all companion ordinates." He derived using the differential algorithm a solution in the particular case where the quantity is equal to $au+z^2$ (a constant), obtaining $adu+2zdz=0$ or $du : dz = 2z : a$ as the differential equation which defines *P*.

The grounding of basic calculus procedures in terms of the properties of the curve, and the common practice of representing relations between magnitudes graphically by means of curves, led to a tendency to see the early calculus as something that was essentially geometrical. The term "fine geometry" employed at the time conveys the contemporary understanding. At the most fundamental level the geometrical character of the early calculus conditioned how the subject was understood, allowing it to be experienced intellectually as an interpreted, meaningful body of mathematics.

II.4.3 *Coordinate Systems*

It is clear that graphical methods played a role in the early calculus that would later be filled by the function concept. An example of this is Varignon's 1706 memoir "Nouvelle formation des spirales" (1704). The paper is devoted to the investigation of curves given in terms of polar variables. Although Cartesian geometry was originally developed for oblique and orthogonal coordinates there had been an early interest in other reference systems. Study of Archimedes's *On spirals* led in the seventeenth century to the invention of transformations that correlated areas expressed in terms of polar quantities to ones defined in terms of Cartesian coordinates. In the writings of Cavalieri, Roberval, James Gregory, Barrow, Newton and Jakob Bernoulli there was an interest in applying calculus-related procedures to curves expressed in polar quantities. In Varignon's own earlier work in orbital dynamics (as we saw in § II.4.1) he considered expressions for the force that were functions of the distance from the particle to a given centre; it was therefore natural that polar quantities were employed to analyze the resulting motion.

In his 1706 memoir Varignon considered a fixed reference circle *ABYA* with centre *C* (fig. 9). A "courbe génératrice" *HHV* is given; a point *H* on this curve is specified by the perpendicular ordinate *GH*, where *G* is a point on the axis *xCX* of the circle. The line *CX* is conceived as a ruler that rotates with centre *C* in a clockwise direction tracing out a spiral *OEZAEK*. Consider a point *E* on the spiral. With centre *C* draw the arc *EG*. Let c = the circumference of the reference circle *ABYA*, x = arc *AMB*, $CA=a$, $CE=y$, $GH=z$ and $AD=b$ a constant line. The arc x is defined by the proportion $c{:}x=b{:}z$. Varignon wrote what he called the "équation générale de spirals à l'infini" as $cz=bx$. By substituting the value for z

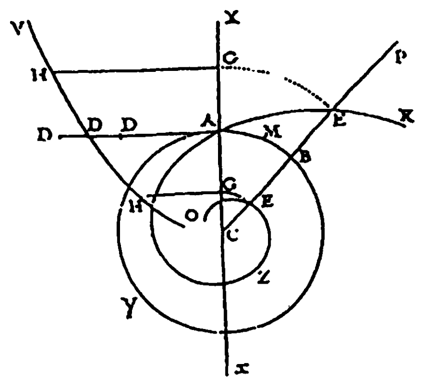


Figure 9

given by the nature of the generating curve into this equation the character of the spiral was revealed. Depending on whether the generating curve was a parabola, hyperbola, logarithm, circle, etc., the corresponding spiral was called parabolic, hyperbolic, logarithmic, circular etc.

That one could introduce curves in a polar reference system by considering arbitrary relations between the radius and the pole angle was presented by Varignon as a substantial advance. Earlier mathematical researches had concerned such special cases as the parabolic spiral. In Varignon's dynamical investigations the trajectory was something that was logically given as part of the physical problem. In the present paper by contrast the "equation" of the spiral is formulated *a priori* in terms of Cartesian coordinates in the associated "generating curve". The latter embodies in graphical form the functional relationship between the polar variables and acts as a standard model to which this relationship may be referred.

A prominent subject of Varignon's paper, the rectification of polar curves, is of interest from the viewpoint of the conceptual foundations of analysis. Newton and Jakob Bernoulli had independently studied the path-lengths of pairs of associated curves, one member given in Cartesian and the other in polar coordinates[19]. The Cartesian formula for the differential element of path length is $ds^2=dx^2+dy^2$, where x is the ordinate and y the abscissa; the polar expression of the same quantity is $ds^2=dx^2+x^2d\theta^2$, where x is now the radius and θ is the polar angle. If the element of length is assumed to be the same along both curves (so that there respective lengths for a given value of x are equal) we are led to the differential equation $dy=xd\theta$ relating the respective coordinate variables. It was clear for example that the integral $\int_a^b \sqrt{1+x^2}\,dx$ gives both the length along the parabola $y=\frac{1}{2}x^2$ as well as the length along the Archimedean spiral $x=\theta$. The rectification of the spiral, a mechanical curve, was reduced to that of the simpler and better known conic section, a result of considerable interest to mathematicians of the period. Varignon's study of rectification consisted in large part in the extension and further development of this result.

The common use of non-Cartesian coordinates in the early calculus was in the computation of geometric quantities associated with the curve. Thus polar coordinates were employed in certain problems because they provided a suitable measure of the radius of curvature of a curve. The geometrical object was given and the coordinate description was varied for the purposes of investigation. Varignon's paper pointed in the opposite direction. Contained in his study, if only implicitly, was the realization that the same formula could receive distinct geometric interpretations, depending on the meaning assigned to the coordinate variables of the problem. The interpretation of the formula $\int_a^b \sqrt{1+x^2}\,dx$ in the preceding paragraph will differ depending on whether x is regarded as an orthogonal or a polar variable. This conclusion suggested more generally the possible existence of a stable analytical core for the calculus. The work of Euler that we shall we consider in the next section was based in large part on the elevation of this insight to an explicit and systematic programme of research in infinitesimal analysis.

III Euler's Analysis

III.1 By the early eighteenth century symbolic methods were common in Continental mathematics. In the infinitesimal calculus especially there were strong analytical elements in the researches of the Bernoullis, Varignon, Taylor (English, but an important influence on the Continent), Hermann, Fagnano, Riccati, and others, elements that were combined however with pervasive geometric modes of representation.

Euler became established as a mathematician of note during the decade of the 1730s. He was a young man in his twenties, a member of the St. Petersburg Academy of Sciences and a colleague of Hermann, Daniel Bernoulli and Goldbach. His interest in analysis is evident in writings from this period, including his major treatise on particle dynamics, *Mechanica sive motus scientia analytice exposita* (1736). Although the theme of analysis was well established at the time there was in his work something new, the beginning of an explicit awareness of the distinction between analytical and geometrical methods and an emphasis on the desirability of the former in proving theorems of the calculus.

The direction of Euler's research in the later 1730s and early 1740s may be followed in his work in the calculus of variations, leading up to the publication in

1744 of his *Methodus inveniendi*. His investigation began from earlier results of Jakob Bernoulli, Brook Taylor and Johann Bernoulli. Jakob and Taylor's researches were linked by an appreciation at the level of technical approach for the analytical solution of isoperimetric problems. By contrast, Johann's major memoir of 1719, an extended exposition of his brother's ideas, emphasized a more geometric approach to the same subject. Although Euler had been Johann's student in Basel his own conception of variational calculus seems to have evolved under the influence of Jakob and Taylor (Fraser 1994).

III.2 The *Methodus inveniendi* contained many of the advances that would be systematically developed by Euler in his later treatises: the function concept; the notion of a trigonometric function and the associated notation; and a uniform procedure for introducing higher-order differentials. At a deeper level the work expressed an appreciation for the mathematical possibilities of a more abstract approach to analysis.

A typical problem of the early calculus involved the determination of a magnitude associated in a specified way with a curve. To find the tangent to a curve at a point it was necessary to determine the length of the subtangent there; to find the maximum or minimum of a curve one needed to calculate the value of the abscissa that corresponded to an infinite subtangent; to find the area under a curve it was necessary to calculate an integral; to determine the curvature at a point one had to calculate the radius of curvature.

The calculus of variations extended this paradigm to classes of curves. In the fundamental problem of the *Methodus inveniendi* it is required to select that curve from among a class of curves which makes a given magnitude expressing some property a maximum or minimum. More precisely, Euler considered curves that are represented analytically by means of relations between x and y in terms of an orthogonal coordinate system (fig. 10). The magnitude that is to be maximized or minimized is expressed as a definite integral

$$W = \int Z dx \qquad (\text{from } x = a \text{ to } x = b), \tag{1}$$

a formula that quantifies in analytical terms the given extremal property. Z is regarded by Euler as a "function" of x, y and the differential coefficients (*i.e.*, derivatives) $p, q, r, \ldots$ of y with respect to x. The latter are given by the relations $dy=pdx$, $dp=qdx$, $dq=rdx$, ..., a procedure for introducing higher-order derivatives that was Euler's own invention[20].

Near the beginning of his treatise Euler (Euler 1744, 13) noted that a purely analytical interpretation of the theory is possible. Instead of seeking the curve which renders W an extremum one seeks that "equation" between x and y which

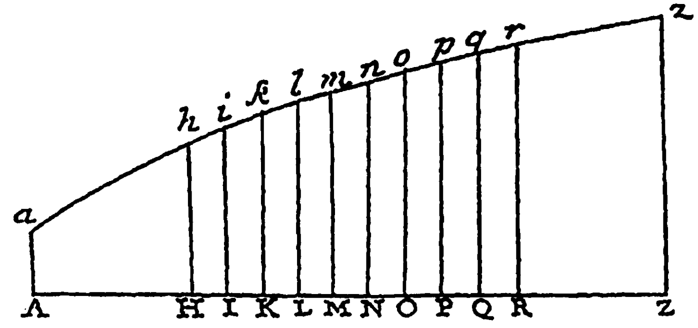


Figure 10

among all such equations when introduced into (1) renders the quantity W a maximum or minimum. He wrote:

> "Corollary 8. In this way questions in the doctrine of curved lines may be referred back to pure analysis. Conversely, if questions of this type in pure analysis be proposed, they may be referred to and solved by means of the doctrine of curved lines.
>
> *Scholium* 2. Although questions of this kind may be reduced to pure analysis, nevertheless it is useful to consider them as part of the doctrine of curved lines. For though indeed we may abstract from curved lines and consider absolute quantities alone, so these questions at once become abstruse and inelegant and appear to us less useful and worthwhile. For indeed methods of resolving these sorts of questions, if they are formulated in terms of abstract quantities alone, are very abstruse and troublesome, just as they become wonderfully practical and are made simple to the understanding by the inspection of figures and the linear representation of quantities.So although questions of this kind may be applied equally to abstract and concrete quantities it is most convenient to formulate and solve them by means of curved lines. Thus if a formula composed of x and y is given, and that equation between x and y is sought such that, the expression for y in terms of x given by the equation being substituted, there is a maximum or minimum; then we can always transform this question to the determination of the curved line, whose abscissa is x and ordinate is y, for which the formula W is a maximum or minimum, if the abscissa x is assumed to have a given magnitude."[21] (Euler 1744, 14)

Euler's view seems to have been that while it is possible in principle to approach the calculus of variations purely analytically it is more effective in practice to refer problems to the study of curves. This conclusion could hardly have seemed surprising. Each of the various examples and problems which historically made up the subject had as its explicit goal the determination of a curve; the selection of such objects was part of the defining character of this part of mathematics. What is perhaps noteworthy about Euler's discussion is that he should have considered the possibility at all of a purely analytical treatment.

III.3 The main body of variational results, presented in chapters two and three, is formulated throughout in terms of the properties of curves. Euler's approach is indicated by his derivation of the fundamental necessary condition known in the modern subject as the Euler (or Lagrange-Euler) differential equation. He developed his derivation with reference to fig. 11, in which the line *amnoz* is the hypothetical extremalizing curve. The letters M, N, O designate points of the x-axis AZ infinitely close together. The letters m, n, o designate corresponding points on the curve given by the ordinates Mm, Nn, Oo. Let $AM=x$, $AN=x'$, $AO=x''$ and $Mm=y$, $Nn=y'$, $Oo=y'$. The differential coefficient p is defined by the relation $dy=pdx$; hence $p=dy/dx$. We have the following relations

$$p=\frac{y'-y}{dx} \qquad p'=\frac{y''-y'}{dx} \tag{2}$$

Suppose now that we are given a determinate "function" Z containing x, y and $p=dy/dx$. The integral (1) was regarded by Euler as an infinite sum of the form $\ldots+Z, dx+Zdx+Z'dx+\ldots$, where $Z,$ is the value of Z at x-dx, Z its value at x and Z' its value at $x+dx$, and where the summation begins at $x=a$ and ends at $x=b$. Let us increase the ordinate y' by the infinitesimal "particle" $n\nu$, obtaining in this way a comparison curve *amvoz*. Consider the value of (1) along this curve. By hypothesis the difference between this value and the value of (1) along the actual curve will be zero. The only part of (1) that is affected by varying y' is $Zdx+Z'dx=(Z+Z')dx$. Euler wrote:

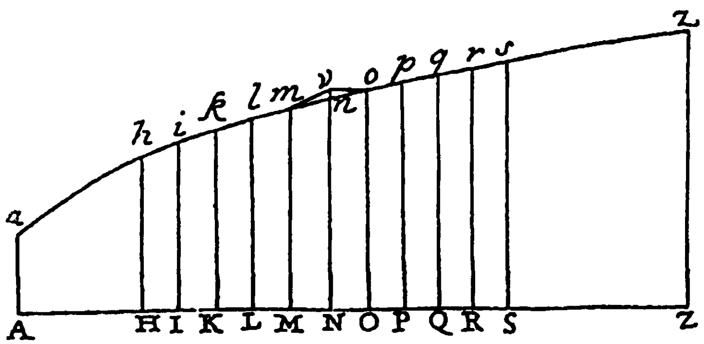


Figure 11

$$dZ = Mdx + Ndy + Pdp \qquad dZ' = M'dx + N'dy' + P'dp' \tag{3}$$

He proceeded to interpret the differentials in (3) as the infinitesimal changes in Z, Z', x, y, y', p, p' that result when y' is increased by nn. From (2) we see that dp and dp' equal nn/dx and $-nn/dx$. (These changes are presented in the form of a table, with the variables in the left column and their corresponding increments in the right column.) Hence (3) becomes

$$dZ = P\cdot\frac{n\nu}{dx} \qquad dZ' = N'\cdot n\nu - P'\cdot\frac{n\nu}{dx} \tag{4}$$

Thus the total change in $\int_a^b Zdx$ equals $(dZ+dZ')dx = n\nu\cdot(P+N'dx-P')$. This expression must be equated to zero. Euler set $P'-P=dP$ and replaced N' by N. He therefore obtained $0=Ndx-dP$ or

$$N-\frac{dP}{dx}=0 \tag{5}$$

as the final equation of the problem.

Equation (5) is the simplest instance of the Euler differential equation, yielding a necessary condition that must be satisfied by the extremalizing arc. In modern notation it is written $\frac{\partial f}{\partial y}-\frac{d}{dx}\left(\frac{\partial f}{\partial y'}\right)=0$. Its derivation by Euler was a major theoretical achievement, representing the synthesis in one equational form of the many special cases and examples that had appeared in the work of earlier researchers.

The remainder of chapter two consists of the presentation of a large number of examples as well as the extension of the variational theory to the case where higher-order derivatives of y with respect to x appear in the integrand Z of (1). In chapter three, mathematically the most advanced of the treatise, Euler considered problems where variables that satisfy certain auxiliary relations are introduced into the integrand Z of the variational integral (1). This investigation, which was motivated by examples involving the constrained gravitational motion of particles

in resisting media, led once again to an analytical solution in the form of differential equations.

III.4 The basic variational problem of maximizing or minimizing (1) involves the selection of a curve from among a class of curves. In the derivation of (5) the variables x and y are regarded as the orthogonal Cartesian coordinates of a curve. Each of the steps in this derivation involves reference to the geometrical diagram in Figure 11. In chapter four, however, Euler returned to the point of view that he had indicated at the beginning of the treatise. In the opening proposition the variational problem is formulated as one of determining that "equation" connecting two variables x and y for which a magnitude of the form (1) (given for the general case where higher-order derivatives and auxiliary quantities are contained in Z) is a maximum or minimum. In his solution he noted that such variables can always be regarded as orthogonal coordinates and so determine a curve. The solution then follows from the theory developed in the preceding chapters. In the first corollary he wrote:

> "Thus the method presented earlier may be applied widely to the determination of equations between the coordinates of a curve which render any given expression $\eth Zdx$ a maximum or a minimum. Indeed it may be extended to any two variables, whether they involve an arbitrary curve, or are considered purely in analytical abstraction."[22] (Euler 1744, 129)

Euler illustrated this claim by solving several examples using variables other than the usual rectangular Cartesian coordinates. In the first example he employed polar coordinates to find the curve of shortest length between two points. We are given (fig. 12) the points A and M and a centre C; it is necessary to find the shortest curve AM joining A and M. Let x be the pole angle ACM and y the radius

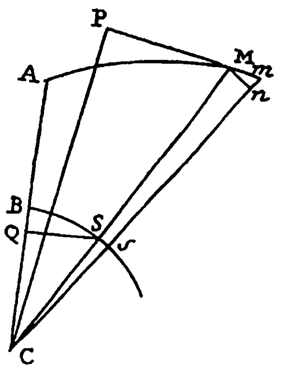


Figure 12

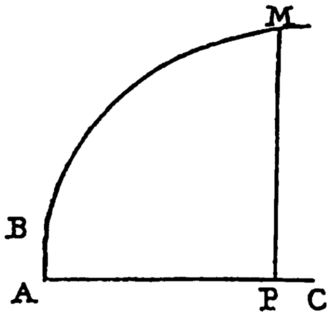


Figure 13

CM. Because the differential element of path-length is equal to $\sqrt{dy^2 + y^2dx^2}$ the formula for the total path-length is $\int dx\sqrt{yy+pp}$, where $pdx=dy$ and the integral is taken from $x=0$ to $x=\angle ACM$. Here x does not appear in the integrand Z of the variational integral, so that $dZ=Ndy+Pdp$. The equation (5) gives $N=dP/dx$ so that we have $dZ=dPp+Pdp$ and a first integral is $Z+C=Pp$, where C is a constant. Since $ZZ=\sqrt{yy+pp}$ we have

$$C+\sqrt{(yy+pp)}=\frac{pp}{\sqrt{(yy+pp)}} \quad i.\ e.: \quad \frac{yy}{\sqrt{(yy+pp)}}=Const.=b$$

Let PM be the tangent to the curve at M and CP the perpendicular from C to this tangent. By comparing similar triangles in fig. 12 we see that $Mm{:}Mn=MC{:}CP$. Since $Mm=dx\sqrt{y^2p^2}$, $Mn=ydx$ and $MC=y$ it follows that $CP=\dfrac{y^2}{\sqrt{y^2+p^2}}$. Hence CP is a constant. Euler concluded from this property that the given curve AM is a straight line.

Note that Euler was completely comfortable with polar coordinates; gone is the Cartesian "generating curve" Varignon had employed in his investigation of 1706 in order to introduce general curves using polar quantities. In the second example he displayed a further level of abstraction in his choice of variables. Here we are given the axis AC with the points A and P, the perpendicular line PM and a curve ABM joining A and M (fig. 13). Given that the area $ABMP$ is some given constant value we must find that curve ABM which is of the shortest length. Euler set the abscissa $AP=t$, the ordinate $PM=y$ and let x equal the area under the curve from A to P. We have $dx=ydt$ and the variational integral becomes $\int\sqrt{\dfrac{dy^2+dx^2}{yy}}$.

Because x does not appear in the integrand we obtain as before the first integral $Z=C+pP$. Substituting the expressions for Z and P into this integral we obtain

$$\frac{\sqrt{(1+yypp)}}{y}=C+\frac{ypp}{\sqrt{(1+yypp)}}$$

VOLUME 196

ANALYSIS AND SYNTHESIS IN MATHEMATICS

History and Philosophy

Edited by

MICHAEL OTTE
Institute for Didactics of Mathematics,
University of Bielefeld

and

MARCO PANZA
Centre F. Viète of the History and Philosophy of Sciences,
University of Nantes

KLUWER ACADEMIC PUBLISHERS
DORDRECHT / BOSTON / LONDON

A C.I.P. Catalogue record for this book is available from the Library of Congress.

ISBN 0-7923-4570-3

Published by Kluwer Academic Publishers,
P.O. Box 17, 3300 AA Dordrecht, The Netherlands.

Sold and distributed in the U.S.A. and Canada
by Kluwer Academic Publishers,
101 Philip Drive, Norwell, MA 02061, U.S.A.

In all other countries, sold and distributed
by Kluwer Academic Publishers Group,
P.O. Box 322, 3300 AH Dordrecht, The Netherlands.

Printed on acid-free paper

Printed in the Netherlands

Table of Contents

II. Philosophy

INTRODUCTION

Time and again, philosophy, in trying to untangle the issues surrounding the analytic-synthetic distinction, has doubted that such a distinction can significantly be drawn at all. We think, in face of the varied and age-old discussions on it, that such reflections amount only to one more documentation of the tenacity of the problems behind this distinction. We could even be justified in promoting the thesis that this distinction refers to the complex relationship between the universe of meanings and the universe of objects and thus concerns each domain of human thinking where a form of objectivity is pursued.

If one accepts such a thesis, one will find it very natural that this distinction has so frequently occurred in the history of mathematics and in philosophical discussions about mathematics. Since Plato, we may encounter quite a number of interpretations of the ideas of analysis and synthesis, which are related in one sense or other with mathematical thought. Mathematicians of all ages have appealed to them in order to distinguish different forms and styles in their argumentation and expositions. Philosophers have referred to them for clarification of the specific character of mathematics in its relations to knowledge in general.

In the present volume various instances of the analytic-synthetic distinction are discussed in relation to the history and philosophy of mathematics, and some new perspectives about possible interpretations and consequences are suggested.

Let us briefly recall a number of interpretations of the notions of analysis and synthesis which played a role in history with respect to mathematics.

- The "logical" interpretation. Analysis proceeds from the general to the particular; synthesis advances in the opposite direction.
- The "structuralist" interpretation. Analysis is conceived as the decomposition of a complex construction given as a whole, in order to reduce it to its elementary components. Synthesis is accomplishment of the complex construction, starting from its elements.
- The "methodological" interpretation. Analysis proceeds on the level of the general only; synthesis is concerned with the particular, considering the general in the particular or even individual.
- The "gnoseological" interpretation. A judgment, or more generally a proposition, is synthetic, if it provides new knowledge, otherwise it is analytic.
- The "mereologic" interpretation. A predication is analytic if it assigns a certain entity to the whole of which it is a part; it is synthetic if this entity is connected to a different and independent (even more general) entity.

– The "semantic" interpretation. A true statement is analytic if its truth just depends on the meaning of the terms occurring in it (and it is then always true as long as these meanings do not change). It is synthetic if its truth depends on the particular character of the model to which it refers (and it is then true in some models and false in others).
– The "syntactical" interpretations. A sentence is analytic if it is logically deduced (or can be logically deduced) from a certain class of axioms, satisfying certain conditions. If not, it is synthetic (except if its negation can be logically deduced from the same axioms). The different interpretations of such a class obviously differ according to the conditions which the axioms have to satisfy. You may require them, for example, to be sentences expressing true analytic statements according to the semantic interpretation of analyticity, or sentences expressing true statements which are true only because of the meaning of the logical constants occurring in them, or even, that they are "logical axioms", or finally that they are simply accepted as starting points of deductive reasoning.
– The "phenomenological" interpretation. By this, analysis and synthesis are understood to be different stages or moments or modalities of mental activity. Synthesis is just will, while analysis is deliberation, the complex research which prepares and justifies synthesis.
– The "genetic" interpretation. Analysis proceeds from ideas which are given as such in a certain stage of the evolution of reason to the original ones from which these ideas originate; synthesis composes or connects the original ideas in order to realize the evolutionary process: it is just a figure of the evolution of reason.
– The "representationalist" interpretation. Analysis presents something through its specific details; synthesis expresses some essential features or characteristics of it.
– The "pragmaticist" interpretation. According to this interpretation analytical reasoning depends upon associations of similarity, synthetical reasoning upon associations of contiguity.
– The "programmatic" interpretation. This is expressed in the ideal of the Enlightenment to organize all knowledge in terms of an "analytic" system. Analysis is then the aim of a program of classification of knowledge, according to a genetic, historical and logical order. It is not concerned with problems of existence, since this is rather the problem of synthesis. Synthesis exhibits contents or being, without caring for their concepts and it remains deaf when analysis does not follow.
– The "directional interpretation". In mathematical reasoning or proof, synthesis proceeds from the given or known to that which we have to deduce or construct in order to solve a certain problem or prove a certain theorem; analysis, in contrast, proceeds from the unknown as if it were known, to its possible antecedents until

arriving at premises we recognize to be true, proven or known. These premises then serve as the basis of synthesis.
– The "configurational" interpretation. Again in mathematical reasoning or proof, synthesis determines the consequences of certain premises, by producing a tree of successive and related deductions; analysis identifies the functional relations existing in a certain specified domain of known or unknown entities, by transforming them into a functional configuration.
– The "logico-theoretical" interpretation. A mathematical theory is synthetic if its objects are constructs, being introduced by recursive reasoning, or simply by successive descriptions of the repeatable conduct that lead to their exhibition. It is analytic, if its objects are characterized by specifying certain conditions or properties they have to satisfy or share either individually or together as a whole system or domain.
– The "historico-theoretical" interpretation. A mathematical theory is synthetic, if it refers to the classical geometrical objects or arguments or even to the classical theories of proportion, of numbers or magnitudes. It is analytic if it considers its objects as arguments of certain equations (rather than proportions) or operations, or even as functions.
– The "linguistic" interpretation. A mathematical arguments or the formulation of a mathematical problem or proof is synthetic if it uses the language of classical geometry and of the theory of proportions. It is analytic if it uses the language of equations, functions or operations.
– The "disciplinary" interpretations. A version of it is typical for eighteenth century mathematicians, according to whom analysis is a theory in terms of which all of mathematics can be formulated. A modern version of this interpretation states that analysis is a branch of mathematics, variously the mathematical theory including calculus, or the mathematical theory of the continuum, or the domain of all the theories where topological arguments, conditions or problems occur; etc.

Though it is not the intention of the following presentations to give a classification or even an account of the different ways in which mathematicians and philosophers have addressed analysis and synthesis or have discussed the analytic-synthetic distinction, the greater part of the previous interpretations are directly or indirectly discussed in the different articles of the present volume. Other interpretations, less customary or not as explicitly advanced in the history of mathematics and philosophy, are also presented or evoked. Finally, in certain cases, new interpretations are proposed.

So extended an inquiry is motivated by two convictions. First, behind such a wide variety of interpretations a deep unity in meaning and attitude seems to subsist, an invariant kernel, which justifies the use of the same terms to express different distinctions or views. Second, because of this unity and by searching for it, the discussion of the different interpretations of the analytic- synthetic distinction with regard to mathematics, becomes a *Königsweg* for tackling what we see as the essential problem mathematics presents for historical and philosophical considerations, the problem of objectivity as a form of knowledge.

It appears to us that the connections between this fundamental question and the analytic-synthetic distinction become particularly pertinent to the philosophical and methodological discussions about mathematics after 1800. All the different positions in their respective peculiarities, as characterized above, have since then been more or less overshadowed by the contrast between pure and applied mathematics. Expressed in philosophical terms: all kinds of foundationalism became obsolete and at the same time issues of objectivity of knowledge became ever more pressing. It seems as if the general spirit of the problems that was expressed by the terms "analysis" and "synthesis" can now be summarized by what may be called the question of philosophical realism (as opposed to nominalism as well as social individualism).

Towards the end of the eighteenth century a new understanding of cognition, of science and scientific development as well as of philosophy, emerged. More than ever before, the sciences were faced with the inevitability of the complexity of experience. Even though quantitative extensions of knowledge had always led to changes in scientific methods, techniques and theories, this increase in knowledge accelerated to such a degree that the capacity of the traditional information processing technologies, based on the spatial organization of knowledge seemed exhausted. This led to an estrangement of the natural sciences from the mathematically dominated spirit of the past and it also led to new developments in mathematics itself.

Since the turn to the nineteenth century a fundamental transition from thinking in substances (being the subjects of predication) towards relational thinking has occurred. Science no longer aimed at phenomena but at the form of things, and theories became realities *sui generis*. It became just as obvious, however, that every pertinent piece of theoretical knowledge, being part of some idea or model of the real world, will in some way or other take into account that the person having the knowledge is part of the system this knowledge represents. All knowledge presupposes a subject and an object and relations between these two, (which are established by the subject's activity). And as the multiplicity of subjective perspectives grew with the increasing division of labor, it could no longer be overlooked that the subject is not only the dynamical source of knowledge and change, but also its object or task. In as much as all knowledge is concerned with either of these aspects of the subject's role, it has a distinctly bipartite structure, which may be represented in various ways; for instance, in terms of the well-known complementarity of means and objects of human activity.

This complementarity of means, that is signs, and objects now seems to lie at the heart of the analytic-synthetic distinction.

The present volume offers various suggestions to substantiate such a thesis.

We wish to acknowledge our gratitude to the following persons, without whom this project could not have been completed: Lydia Bauer, Michael Detlefsen, Anita von Duhn, Michael Hoffmann, Michael Möse, Marianne Murphy, Gloria Origgi and Klaus Peters.

The financial assistance from the IDM / University of Bielefeld is greatly appreciated. In the process of editing this volume we have also received indispensable help from the Series Editor and from the Publisher's side. We feel particularly grateful to Evelien Bakker and Annie Kuipers.

Michael Otte
Marco Panza

I. History

GIORGIO ISRAEL

THE ANALYTICAL METHOD IN DESCARTES' *GÉOMÉTRIE**

To describe *La Géométrie* as an "*essai*" of the Cartesian method, or as the application of the rules given in *Discours de la méthode*, has paradoxically contributed to an undervaluation of existing connections between this brilliant and famous "*essai*" and Descartes' philosophical work. In a way this is a paradox, considering the fact that this description of *La Géométrie* underlines the dependency of Descartes' only complete mathematical treatment on the method to follow "pour bien conduire sa raison et chercher la vérité dans les sciences" and on the metaphysical principles on which it is based. Nevertheless the connection between *La Géométrie* and the Cartesian method thus established appears weak. Because of this unsatisfactory situation, the essays dedicated to the study of this text appear to be split into "philosophical-humanistic" analyses and "scientific" analyses.

Let us try to clarify the previous statement, beginning with the reasons why the connection between *La Géométrie* and the rules of *Discours de la méthode* appears weak. The fundamental reason lies in the vagueness of the methodological rules expressed in the *Discours* and summarized in the four famous rules governing scientific thought, even if Leibniz's severity seems excessive when he compares them with common recipes and sums them up in the almost obvious rule: "sume quod debes, operare ut debes et habebis quod optas" (Leibniz GP, IV, 329). Nevertheless it is difficult to deny that those who aim at establishing a tight connection between the rules of the *Discours* and the contents of *La Géométrie*, by trying to demonstrate in some way that the latter represent an application of the first, as if Descartes had endeavoured to obtain the results of *La Géométrie* as a direct application of his methodological rules, would be disappointed, and achieve little more than the impression of a vague link. The situation appears different, however, when the whole of Descartes' work is considered, and not only the *Discours*. Then, particularly when referring to the *Regulæ ad directionem ingenii*, it is possible to trace a much tighter connection between Descartes' method and the contents of *La Géométrie*, and at the same time to examine some historiographical questions on viewing Descartes' mathematical work from a different angle. The aim of this article is to attempt to highlight these connections and to briefly consider the historiographical questions mentioned above.

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 3–34.

As an introduction we will use some observations by E.J. Dijksterhuis, which, even though rather general, emphasize the existing link between *La Géométrie* and the *Regulæ*. Dijksterhuis observes that

> "[...] if you really want to get to know Descartes' method, you should not read the enchanting *Discours*, which is more a *causerie* than a treatise, but rather the *Regulæ ad directionem ingenii* [...]. As a matter of fact, the *Regulæ* contain an exposition of the so-called *Mathesis universalis*, which Descartes always considered one of his major methodological discoveries and which he hoped to see applied in all natural sciences." (Dijksterhuis 1961, 542)

Further on he continues:

> "The essay *La Géométrie*, in which Descartes presents his new discovery, fully deserves [...] to be described as a demonstration of the Cartesian method; yet it does not contain an application of the four rules of the *Discours*, to which this essay constitutes an appendix. In fact, the true *Discours de la méthode* is set up by the *Regulæ ad directionem ingenii*." (Dijksterhuis 1961, 543)

Dijksterhuis identifies this methodology in the ***Mathesis universalis*** and consequently the Cartesian ideal in the process of making science mathematical, which establishes a central role for *La Géométrie* as the first step of this process and as a model for its realization. Nevertheless, the way in which he characterizes the ***Mathesis universalis*** and the methodology he derives from it is not only vague but also misleading, in a way typical of many ambiguities in historiography dealing with these topics.

First of all Dijksterhuis completely identifies the ***Mathesis universalis*** with the "algebra speciosa" of Viète: consequently, Descartes' ideal would be nothing but the systematic "application of algebraic methods" to all science. In this way *La Géométrie* is nothing but the application of algebraic methods to geometry[1], which, in part, is true, but in our opinion insufficient to describe the characteristic features of Cartesian geometry. Secondly, Dijksterhuis identifies the deductive Cartesian method with the logical deductive method of modern mathematics, explicitly referring to the axiomatic method, which constitutes its complete codification[2].

In reality, these two comparisons are strictly correlated so that the discussion of one leads directly to the discussion of the other. We will start by commenting on the second comparison, recognizing that it is misleading, which a brief reading of *La Géométrie* demonstrates. As will be clarified later, the Cartesian deductivism clearly has "constructivistic" character: the only kind of reasoning allowed is that which will give an explicit construction of the entity under investigation or the result being demonstrated. Consequently any form of reasoning *ab absurdo* is excluded in Cartesian mathematics; moreover the entities all have to be constructible, which makes it impossible to define them in a conventional or axiomatic way. Furthermore, the admissible deductive chains must be finite; consequently, also the rudimentary forms of inductive reasoning in Descartes' work differentiate from modern mathematical inductive reasoning which, by means of a finite number of steps, makes it possible to pass from the finite to the infinite. Thus Cartesian deductivism is "constructivistic" and "finitistic", *i.e.* far from, if not the opposite of, the "logical-formal" deductivism of modern mathematics.

Descartes seems conscious of the particular nature of his method and its position in comparison with past traditions in mathematics. When Descartes criticizes the "vulgar mathematics" (Descartes AT, X, 376 and LR, 34)[3] of his time, he does not only refer to a sort of intuitive-experimental knowledge, in which the validity of the discoveries is particularly uncertain because of the frailty of the method used to obtain them[4]; he also criticizes the deductivism of classical mathematics, in particular that of the "ancients" and the synthetic method on which it is based (Descartes AT, X, 376 and LG, 34)[5]. Therefore the "analytical" method he proposes is neither an intuitive procedure, which relies on the senses, nor an abstract formal deductive procedure, which is unable to account for the way in which the discovery was reached—similar to the one characterizing the forms of reasoning of ancient mathematics[6].

The difference between the analytical and synthetic methods and Descartes' evaluations of them are shown in an extremely clear manner in a passage of the "answers" to the "second objections" to the *Meditationes*[7]. Here Descartes points out that in the works of the geometer the methods of demonstration are twofold: "l'une se fait par l'analyse ou résolution, et l'autre par la synthèse ou composition" (Descartes 1647, 387 and AT, VII, 155) and he continues:

> "L'analyse montre la vraie voie par laquelle une chose a été méthodiquement inventée, et fait voir comment les effets dépendent des causes; en sorte que, si le lecteur la veut suivre, et jeter les yeux soigneusement sur tout ce qu'elle contient, il n'entendra pas moins parfaitement la chose ainsi démontrée, et ne la rendra pas moins sienne, que si lui-même l'avait inventée.
>
> Mais cette sorte de démonstration n'est pas propre à convaincre les lecteurs opiniâtres ou peu attentifs: car si on laisse échapper, sans y prendre garde, la moindre des choses qu'elle propose, la nécessité de ses conclusions ne paraîtra point; et on n'a pas coutume d'y exprimer fort amplement les choses qui sont assez claires de soi-même, bien que ce soit ordinairement celles auxquelles il faut le plus prendre garde." (Descartes 1647, 387-388 and AT, VII, 155-156)

Therefore the value of the analytical procedure lies in the connection with the "true way", which has made the invention possible, and in the fact that it shows the links of causal dependence: this means that it derives from the "constructive" nature of this method, even if this advantage can be easily lost, if the chain linking the causes and the effects is interrupted, however slightly. The synthetic method proceeds in a different way:

> "La synthèse, au contraire, par une voie tout autre, et comme en examinant les causes par leurs effets (bien que la preuve qu'elle contient soit aussi des effets par les causes), démontre à la vérité clairement ce qui est contenu en ses conclusions, et se sert d'une lungue suite de définitions, de demandes, d'axiomes, de théorèmes et de problèmes, afin que, si on lui nie quelques consequences, elle fasse voir comment elles sont contenues dans les antécédents, et qu'elle arrache le consentement

du lecteur, tant obstiné et opiniâtre qu'il puisse être; mais elle ne donne pas, comme l'autre, une entière satisfaction aux esprits de ceux qui désirent d'apprendre, parce qu'elle n'enseigne pas la méthode par laquelle la chose a été inventée." (Descartes 1647, 388 and AT, VII, 156)

Descartes' description of the procedure of the synthetic method clearly refers to the geometry of the ancients and, in particular, to the model of Euclid. Differing from the analytical method, this procedure gains the reader's consent, using procedures of "coercion" typical of formal logic[8]. Nevertheless, Descartes criticizes the absence of constructivism in it: it "does not teach the method by which the thing has been invented"[9]. The analytical method, on the contrary, has this great advantage, which was also recognized but kept "secret"(Descartes 1647, 388 and AT, VII, 156)[10] by the ancients, and which Descartes, brought to light and exposed as a method.

The difference between the analytical and the synthetic method is discussed by Descartes as an answer to a concluding remark of the *Seconde obiezioni* to the *Meditationes* "collected by Mersenne on the basis of remarks from various theologicians and philosophers" (Descartes 1647, 359), which invites Descartes to procede "more geometrico" in his exposition:

"[...] ce serait une chose fort utile, si, à la fin de vos solutions, après avoir prémièrement avancé quelques définitions, demandes et axiomes, vous concluiez le tout selon la méthode des géomètres, en laquelle vous êtes si bien versé, afin que tout d'un coup, et comme d'une seule illade, vos lecteurs y puissent voir de quoi se satisfaire, et que vous remplissiez leur esprit de la connaissance de la divinité." (Descartes 1647, 365 and AT, VII, 128)

On the one hand Descartes' answer makes clear in which sense he believes to have to accept the invitation to procede "more geometrico"—*i.e.* according to the analytical and not the synthetic method; on the other hand, as he deals with metaphysical matters, he endeavours to show the particular inadequacy of synthesis in these kinds of questions, recognizing that synthesis appears more acceptable in geometrical problems. In specifying this aspect he touches on an issue that is particularly interesting for our topic: he asks himself why synthesis can "be useful when put after analysis" (Descartes 1647, 388 and AT, VII, 156). This derives from the nature of the basic notions of geometry, which, since they are not in contradiction with the senses, are accepted unanimously:

"Car il y a cette différence, que les prémières notions qui sont supposées pour demontrer les propositions géométriques, ayant de la convenance avec les sens, sont reçues facilement d'un chacun; c'est pourquoi il n'y a point là de difficulté, sinon à bien tirer les conséquences, ce qui se peut faire par toutes sortes de personnes, même par les moins attentives, pourvu seulement qu'elles se ressouviennent des choses précedentes; et on les oblige aisement à s'en souvenir, en distinguant autant de diverses propositions qu'il y a de choses à remarquer dans la difficulté proposée, afin qu'elles s'arrêtent séparément sur chacune, et qu'on les leur puisse citer par après, pour les avertir de celles auxquelles elles doivent penser."[11] (Descartes 1647, 388-389 and AT, VII, 156-157)

Therefore it is obvious that the axioms of geometry are not only far from being conventional but also only acceptable as far as their contents of truth are "clear" and "distinct": only for this reason the synthetic method can be useful when introduced in geometry, naturally "après l'analyse". Thus, once again the superiority and priority of the analytical-constructive method over the synthetic-formal one is emphasized. This has led to two errors: the first one to believe that the use of axiomatic procedures is at the centre of the Cartesian "revolution" in mathematics—with Descartes actually dissociating himself from these procedures, even if it is in a "form of contents" typical of the geometry of the ancients; the second one to speak generally of the central position of the "deductive method" (evoking improper associations with the deductive logics of modern mathematics) without specifying and clearly underlining the "constructive" character of this method in Descartes' vision. It is necessary, however, to give an exact definition of this "constructivism". For this purpose it will be useful to re-examine the *Regulæ* in order to show how it can be directly translated into the concept of "geometric construction" and into a precise definition of the forms of such a construction. This leads Descartes to a critical re-examination of the concept of "constructibility" of a geometric figure as it was defined by previous geometry and to the introduction of a new interpretation of such a concept. The Cartesian classification of the curves—which can be considered Descartes' most important contribution to mathematics—is the consequence of such a re-examination and re-definition. In the end the Cartesian classification of the curves is a direct consequence of the general principles of the Cartesian analytical method, which are unfolded in the *Regulæ*.

Before concentrating on this more specific analysis, we have to make some general observations.

We have tried to show that an accurate explanation of the meaning which Descartes attributes to the terms "analytical" and "synthetic" is necessary to fully understand the method he follows in his mathematical arguments. Therefore it is also necessary to give an exact explanation of these terms with respect to the context of Cartesian thought and to their prevalent use at that time, avoiding any reference to a non-specific and thus debatable "general meaning" of these terms in the history of mathematics. This kind of use, uncritical and unrelated to time, is not infrequent in historiography—particularly the one manifesting itself as a kind of by-product of research—and has been the source of quite a few misunderstandings. A typical manifestation of the cumulative historiographic analysis is the incomprehension—or at least the negligence—of the changes in meaning in scientific terminology, subtle transformations of meaning which occur silently in the course of history, below the unchanged surface of its formal appearance. Marc Bloch, having observed how much the term "history" has changed its meaning in the course of 2000 years, has given a sharp comment which should be read, reread and remembered by the historian as a precious *memento*: "Si les sciences devaient, à chacune de leurs conquêtes, se chercher une appellation nouvelle—au royaume des académies que de baptêmes, et de pertes de temps!" (Bloch 1964, 1). Yet historians of science often forget this rule and venture to analyse a context of scientific concepts by taking a meaning for granted that is determined by recurrent terms which have nothing to do with that very context and that is almost always related to a more recent context. In this way the historic specificity of the term, *i.e.* its meaning in relation to the context in which it is used, is changed, with rather negative consequences for a correct understanding of the subject. The use (and abuse) of the term "analytical geometry" in historiography is an evident example of this: the use of the term recurrent in the handbooks of contemporary mathematics or at least of the end of the 19th century has been widely accepted without any closer examination. In our opinion, this point of view is completely inadequate for the specific meaning of "analytical" geometry in Descartes' work.

Both, the terms "synthetic" and "analytic" have a completely different meaning in Descartes' work than in modern and contemporary mathematics. Since the times of Descartes, the modern meaning of the term "synthetic" (*i.e.* the meaning implied from the second half of the 19th century onwards) has undergone radical changes: what was essential in the ancient interpretation (*i.e.* the very demonstrative procedures effectively described by Descartes in the *Meditationes*) was put last and the aspect of the intuitive meaning of the discovery first[12]. Yet the changes undergone by the term "analytic" are even more complex. No doubt we have to speak about a sequence of slight alterations of meaning during a long period of historical development. The history of these changes should be seen within the framework of the history of changes in meaning of the concept of analysis. Neither of these ambitious projects will be carried out here and we will limit ourselves to pointing out some of the historical layers that cover the Cartesian conception of "analytical" geometry. The marked constructive nature of analysis in Cartesian geometry—something nonexisting in the modern meaning of the term—should be an indication of the occurrence of possible historical sedimentation.

Let us now look at historiography (particularly but not solely at the sector of historiography that is linked with research, referred to above). We may even be fortunate enough to witness an ongoing attempt of "concealment"! Actually, J. Dieudonné, after having listed "analytical geometry" among those "pseudo-sciences" which "it remains to hope we can forget the existence and even the name of" (Dieudonné 1968, 6), continues like this: "Furthermore it is urgent to free the term 'analytical geometry', which, no doubt, is best to indicate one of the most vivid and profound theories of modern mathematics, *i.e.* the one of analytical varieties, compared to 'algebraic geometry', which is the study of 'algebraic varieties'" (*ibid.*, 6). In another piece of writing (where the "liberation" has already taken place) Dieudonné clearly explains the contents which he wants to free the term "algebraic geometry" from:

> "It is absolutely intolerable to use 'analytical geometry' for linear algebra with coordinates, still called 'analytical geometry' in the elementary books. Analytical geometry in this sense has never existed. There are only people who do linear algebra badly, by taking coordinates and this they call analytical geometry. Out with them! Everyone knows that analytical geometry is the theory of analytical spaces, one of the deepest and most difficult theories of all mathematics."(Dieudonné 1970, 140)

It is not our aim to discuss this kind of historical destructions (which are perpetrated by one of the most authoritative voices not only in mathematics but also in the history of mathematics of our time).

Certainly analytical geometry in the sense of "coordinate geometry" has existed. It is important to remember that the term "analytical geometry" did not first appear in Descartes but in the *Introduction* of the first volume of Lacroix' *Traité du calcul différentiel et du Calcul intégral* in the 1797 edition (Lacroix 1797-1798). Lacroix explains that his point of view differs completely from the traditional constructive one:

> "En écartant avec soin toutes les constructions géométriques j'ai voulu faire sentir au Lecteur qu'il existoit une manière d'envisager la géométrie, qu'on pourrait appeler 'Géométrie analytique', et qui consisteroit à déduire les propriétés de l'étendue du plus petit nombre de principes, par des méthodes purement analytiques, comme Lagrange l'a fait dans sa Méchanique à l'égard des propriétés de l'équilibre et du mouvement." (*ibid.*, I, xxv-xxvi)

In spite of recalling Lagrange, Lacroix admits that it was Monge who first presented "sous cette forme l'application de l'Algèbre à la Géométrie" (*ibid.*, I, xxv-xxvi). Actually, his homonymous treatise (Monge and Hachette 1802) still uses this terminology—"application of algebra to geometry"—which, on one hand, conveys the idea of an "ancillary" use of algebra in geometry, on the other hand suggests a one-sided relationship between the two disciplines in one direction only: the use of algebra in geometry as an instrument leads to the need to justify algebraic techniques in terms of the main subject—geometry—and consequently the translation of algebraic operations into geometrical constructions (*i.e.* from geometry to algebra), while the opposite (from algebra to geometry) does not exist. This is exactly Descartes' point of view—which justifies the definition of his approach as "application of algebra to geometry"—but it is not Monge's point of view, as revealed by Lacroix:

> "Qu'on ne croie pas qu'en insistant ainsi sur les avantages de l'Analyse algébrique, je veuille faire le procès à la Synthèse et à l'Analyse géométrique. Je pense au contraire qu'on néglige trop aujourd'hui l'étude des Anciens mais je ne voudrais pas qu'on mêlât, comme on le fait dans presque tous les ouvrages, les considérations géométriques avec les calculs algébriques; il séroit mieux, ce me semble, que chacun de ces moyens fût porté dans des traités séparés, aussi loin qu'il peut aller et que les résultats de l'un et de l'autre s'éclairassent mutuellement en se correspondant pour ainsi dire, comme le texte d'un livre et sa traduction." (Lacroix 1797-1798, I, xxv-xxvi)

Therefore Lacroix' merit is to have given a new name (which also contains an element of continuity in the use of the term "analytical") to a turning-point in geometrical thinking carried out by Monge in the first place. This turning-point consists in having given autonomy to the two disciplines—algebra and geometry—transforming their relationship into a form of specular correspondence. Even more clearly Monge pointed out in his lectures on descriptive geometry held at the *Ecole Normale* of the year III that the student had to

> "[...] se mettre en état d'une part de pouvoir écrire en Analyse tous les mouvements qu'il peut concevoir dans l'espace, et de l'autre de se répresenter perpétuellement dans l'espace le spectacle mouvant dont chacune des opérations analytiques est l'écriture."[13] (Monge LEN, 367 and 1799, 62)

Algebra is no longer a mere instrument to obtain geometrical constructions in an easy way: it offers a translation of the "book" of geometry which one can work with; and, *vice versa*, from the translation it is possible to return to the original text. Therefore every geometrical problem is susceptible of an algebraic treatment that permits reasoning in a somewhat stenographic abbreviated form, which, in the long run, is more powerful than the classic synthetic reasoning; however, a geometrical translation exists of every algebraic formulation. So, it is possible to obtain from every geometrical locus the algebraic equation representing it, which can be manipulated with the autonomous methods of algebra, and vice versa a geometrical locus can be obtained from a given equation.

This specularity has been the essence of modern analytical geometry from Monge and Lacroix onwards. In this concept coordinate geometry no longer plays an accessory or technical but a central role: the role of mediator between algebra and geometry, a kind of dictionary to translate from one text to another, indicating the correspondence between geometrical locus and equation and vice versa. Therefore it is understandable how, in the modern meaning, the notion of analytical geometry has been confounded with the one of "coordinate geometry", exactly because of the central position of this method in guaranteeing the bi-univocal relationship between the two disciplines.

Referring to this interpretation of analytical geometry as the study of the properties of extension based on the recognition of the specularity between algebraic and geometrical operations and on the consequent central position of coordinate geometry we are led back to Fermat and not Descartes. On this point C. B. Boyer is completely right, when he observes that it is in Fermat's work—precisely in his short treatise entitled *Ad locos planos et solidos isagoge* (Fermat TH, I, 4, 91-110)—that "the fundamental principle of analytical geometry is to be found in a precise and clear language" (Boyer 1956, 218). Boyer is also right when he observes that Fermat's phrase stating that a locus exists whenever there are two unknown quantities in a final equation, since the extreme of one of them describes

a straight line or curve, "represents one of the most significant statements in the history of mathematics" (Boyer 1956, 190). This is certainly most important as regards the notion of analytical geometry of Monge and Lacroix mentioned above: actually, Fermat puts forward the principle of bi-univocal correspondence between algebra and geometry in a rather explicit way, when he admits that beginning with an algebraic equation there can be a geometrical locus—a really revolutionary idea for his time. The geometric constructions have lost their central position at a single blow: it is no longer necessary that a curve can be constructed in order to be admissible—which has been fundamental for the priority of geometry over algebra—the curve exists only because the equation is given, it is not defined by a construction but as "the locus of the points that satisfy the equation". The central position of coordinate geometry follows as an obvious necessity. The fact that Fermat's approach is more "modern" than Descartes' has been correctly observed for some time (Taton 1951, 102). Descartes does not admit this vision of geometrical loci at all, nor does he accept the specularity between algebra and geometry or renounces the central position of the concept of construction. Finally, coordinate geometry has a purely technical and accessory role in his work.

At this point some historiographical difficulties arise. Fermat's point of view, though apparently more modern, was certainly not the more influential one: it is well-known that the 17th and part of the 18th century was dominated by the Cartesian geometrical conception; and even when the mathematics of the Enlightenment period and the time of the French Revolution—Lagrange, Monge and Lacroix in particular—distanced itself from the Cartesian tradition this was done silently, underlining in a clear but implicit way the breaking with this tradition. Monge's use of the expression "application of algebra to geometry" reminds us of the continuity with the Cartesian tradition. On the contrary, Lacroix' naming (the introduction of the term "analytical geometry") equals a more explicit separation, but because of the apparent character of continuity, due to the common use of the term "analytical", may not have sufficiently drawn the historians' attention. This different meaning attributed to the concept of analysis, however, is the very basis of the big difference between the Cartesian vision and the "modern" use of "analytical geometry".

It has to be pointed out that the choice of examining the problem of the birth of analytical geometry according to a view typical of a "cumulative" historiography—and thus starting from the modern notion of analytical geometry[14]—has led to serious difficulties and has caused contradictions among numerous historians. So, Gino Loria does not conceal the sense of confusion that overcomes the historian when he tries to determine the birth of analytical geometry:

> "All those who long for knowing the work which is the starting point of literature on coordinate geometry experience a great disappointment since Descartes' *La Géométrie* differs from a modern

treaty of analytical geometry infinitely more than do two expositions, one ancient, one modern of any other mathematical discipline." (Loria 1924, 777)

He continues like this, providing a perfect model of cumulative historiography:

"[...] Descartes (and this also holds true for Fermat) considered the new discipline a simple metamorphosis in the geometry of the ancients from the influence of algebra [...]; so the comparison of the author of the *Discours de la méthode* with Christopher Columbus, who took the conviction to have discovered a new world to his grave, is evident; this state of being blind was transmitted from the Supreme to his immediate disciples [...]." (Loria 1924, 777)

Taton reveals the differences between Fermat and Descartes more skillfully, characterizing the technical aspects of Cartesian geometry quite well:

"[Descartes] avait conçu cette science comme 'une application de l'algèbre à la géométrie', nom qu'elle conservera d'ailleurs jusqu'au prémières décadaes du XIX siècle et que Monge lui-même adoptera, c'est-à-dire comme une technique de structure algébrique, adaptée à la résolution des problèmes d'essence géométrique et spécialement des problèmes des lieux à la manière d'Apollonius. Ainsi, apparaît-elle, non pas comme une branche autonome de la science, mais plûtot comme un outil permettant de résoudre de nombreux problèmes géométriques qui n'entrent pas dans le champs normal d'application directe des propriétés classiques tirées des *Eléments* d'Euclide. Les courbes ne s'y trouvent pas étudiées pour elles-mêmes d'après leurs équations, mais l'intérêt se porte *quasi* exclusivement sur celles qui apparaissent comme solutions de problèmes à résoudre." (Taton 1951, 101)

Most important in the historiography of analytical geometry remains the work of C. B. Boyer (Boyer, 1956), whose merit was to clarify the difference between Fermat's and Descartes' point of view. As Taton, he recognizes that Descartes' geometry is more an application of algebra to geometry than analytical geometry in the sense we understand it today and calls Chasles' definition of analytical geometry as a "*proles sine matre creata*" (Chasles 1875, 94) "unfortunate". And, after having observed that Cartesian geometry has now become a synonym of analytical geometry, but that Descartes' fundamental goal is quite different from the one of modern handbooks, he offers the following characterization of Cartesian geometry:

"Descartes was not interested in the curves as such. He derived equations of curves with one purpose in mind—to use them in the construction of determinate geometrical problems which had been expressed by polynomial equations in a single variable .[...] The method of Descartes is that of coordinate geometry, but his aim is now found in the theory of equations rather in analytic geometry. [...] where Descartes had begun with a locus problem and from this derived an equation of the locus, Fermat conversely was inclined to begin with an equation from which he derived the properties of the curve. Descartes repeteadly refers to the generation of curve 'by a continuous and regular motion'; in Fermat one finds more frequently the phrase, 'Let a curve be given having the equation [...]' The one admitted curves into geometry if it was possible to find their equations, the other studied curves defined by equations."[15] (Boyer 1956, 216-217)

What exactly are the characteristics of Cartesian geometry? They are described by Boyer, when he refers to the differences between Descartes' and Fermat's approach. These differences could be summed up as follows: both Descartes and Fermat were influenced by Viète; Fermat applied Viète's method to the problems of geometrical loci, whereas Descartes renewed the method by introducing the algebraic symbolism, without changing the object of Viète's researches, *i.e.* the geometrical construction of the roots of an equation. This is certainly correct and yet it means that Descartes is nothing but a descendant of Viète: his analytical geometry is the continuation of Viète's *ars analytica* with the introduction of the powerful instrument of algebraic symbolism—without doubt a considerable step forward but not doing justice to Fermat's innovating contribution. This does not mean that it is scandalous to reconsider the significance of the Cartesian work. But as to the connection between Descartes' and Viète's work, it seems that the above-named interpretation is based on a merely technical vision of the question.

There are also other reasons for not being satisfied. The problem of the origin of analytical geometry cannot be solved by simply stating that Descartes' geometry is not the same as modern analytical geometry, and by concluding with renaming it "application of algebra to geometry". Moreover this term goes back to a later date, so that the question why Cartesian geometry (*i.e.* the application of algebra to geometry) originated as "analytical" geometry or at least as the application of "analysis" to geometry remains. This is not a simple question of terminology but a basic problem which must not be disregarded and reduced to a question of names. Once again the answer could be that Descartes is a descendant of Viète[16]. At this point, however, we really are dissatisfied. We have already seen how the notion of "analytical" in Descartes can neither be reduced to the notion of "analytical" of modern analytical geometry nor to the *ars analytica* of Viète. The characteristics of this notion are to be found in the philosophical sense of the term and not in the strictly mathematical sense. It is obvious that the study of Descartes' philosophical work does not provide the key to the understanding of the importance of his mathematical works, it is true, but it is equally evident that, in order to understand the work of a scientist-philosopher like Descartes, an analysis which is restricted to the study of his contribution viewed solely from the angle of the history of geometrical methods is not sufficient.

One of the most important contributions to Descartes' *La Géométrie*, apart from the writings of Boyer[17], is an article by H. J. M. Bos (Bos 1981). Bos's point of view is different from the one prevailing in literature, which he considers unsatisfactory as it aims at solving "the sterile question whether Descartes invented analytical geometry or not" (Bos 1981, 297). Also Bos's approach is strictly internal and in no way detached from a "cumulative" point of view. In this way the "sterile" question is taken up, possibly in the paradoxical form according to which the very "programmatic" intentions of Descartes had restrained the establishment of analytical geometry.

> "The later synthesis of algebraic and geometrical methods into what is now called analytic geometry was possible only because later mathematicians were not aware of (or forgot) the programmatic problems with which Descartes had struggled." (Bos 1981, 298)

Special emphasis must be put on the fact that Bos's analysis is clearly directed towards the topics and problems we have dealt with so far. First of all, Bos demonstrates that the central theme of *La Géométrie* and "the key to understand its underlying structure [...] and programme" (Bos 1981, 332) is the representation of curves. In fact, it is the basis of the relationship between geometry and algebra in Descartes' work, which is connected to the topic of his constructivistic conception and the relation between analysis and synthesis. After a profound analysis of the text, Bos indicates what he considers contradictory: the co-existence of a classical programme (already clearly expressed in 1619) which regards geometry as a science that "constructs" or solves geometrical problems, which changes the ancient classification of the curves only slightly (basing it on the use of machines which are nothing but the generalization of ruler and compasses) and where algebra has no place, and a programme which attributes an important role to algebra[18] and abolishes the ancient classification of curves, and, in doing so, opens the way to the modern distinction between algebraic and transcendental curves. In fact, there are two co-existing programmes, since Descartes never abandons the vision of geometry as science of "constructions" and remains prisoner of some essential difficulties. The main difficulty revealed by Bos is the contradiction which can be found in the criteria of geometrical acceptability of curves in the programme of *La Géométrie*:

> "On the one hand Descartes claimed that he accepted curves as geometrical only if they could be traced by certain continuous motions. This requirement was to ensure that intersections with other curves could be found, and it was induced by the use of the curve as means of construction in geometry. On the other hand Descartes stated that, under certain conditions, curves represented by pointwise constructions were truly geometrical. Pointwise constructions were related to curve equations in the sense that an equation for a curve directly implied its pointwise constructions. Pointwise construction was used primarily for curves that occurred as solutions to locus problems.
> The link between the two criteria is Descartes' argument that pointwise constructible curves can be traced by continuous motions. We have seen that that argument, and hence also the link, is very weak." (Bos 1981, 326)

Looking once again at Cartesian geometry through the lens of modern mathematics, *i.e.* of "analytical geometry", Bos asks himself:

> "Why then did Descartes not cut this Gordian knot in the most obvious way, namely by defining geometrical curves as those which admit algebraic equations? Why did he not simply state that all such curves are acceptable means of construction and that the degrees of their equations determine their order of simplicity? That principle would have removed the contradictions mentioned above." (Bos 1981, 326)

After an accurate analysis Bos comes to the conclusion that the contradiction is based on the co-existence of the two programmes mentioned above, the second of which being the result of a paradigmatical change that occurred between 1619 and 1637. This change, even though it emphasizes the role of algebra, does not modify the nucleus of the first programme and consequently the idea that "geometry is the science that solves geometrical problems by constructing points by means of the intersection of curves" (Bos 1981, 331). The programme of 1619 "may have been impracticable but coherent" (Bos 1981, 331), whereas the programme of 1637 is innovative but incoherent: it introduces algebra without renouncing the link with the old geometrical programme and consequently brings about a series of difficulties.

This explanation, though accurate, is only descriptive: it does not say anything about the motives that led Descartes to take this new position and remain obliged to the old one at the same time. Was it a question of mere attachment to the past? There is one possible answer, on condition of leaving the link with the "sterile" question of Descartes' relationship with analytical geometry definitely behind. In the time between 1619 and 1637 something crucial happened: Descartes' enunciation of the principles of the method. The influence of such an enunciation on the programme of *La Géométrie* is revealed by Bos[19]. However, by restricting the connection to the *Discours de la Méthode*, it is impossible to see the amplitude and complexity of the profound link between geometry and method. The crucial event between 1619 and 1637 is the publication of the *Regulæ*. In this text we find Descartes' so-called attachment to the classical constructive vision of geometry and, at the same time, the importance he attributes to the procedures of algebra.

Attempting to draw a parallel between the changes in Descartes' approach to geometrical problems—which certainly exist and consist in passing from a nearly orthodoxically classical vision to one that attributes an important role to algebraic procedures—Bos refers to Schuster's theses. Schuster maintains that after 1628 Descartes abandoned the programme of *Mathesis universalis* formulated in the *Regulæ* because he had encountered some difficulties in constructing a geometrical theory of equations (Schuster 1980); consequently he turned to algebra in order to solve his technical difficulties. This explanation, however, is not very convincing, not only because Descartes was not easily influenced by technical difficulties or details[20]. In the first place the changes in Descartes' approach to geometry are reduced to merely technical reasons. Secondly, it is taken for granted that after 1628 Descartes abandoned his programme of *Mathesis universalis*: this means that all connections between the *Regulæ* and *La Géométrie* are disregarded, which really is something impossible to do. Moreover it appears arbitrary to talk about a programme of *Mathesis universalis* which Descartes was to have developed in detail, since he considered the enunciation of the methodical rules of reasoning more important than anything else. Last but not least the traditional conception of geometry which Descartes adhered to in 1619 is claimed to be the exact opposite of the method expressed in the *Regulæ* and does not leave any space for the algebraic approach. We shall see that this does not hold true: in the method enunciated in the *Regulæ* the algebraic procedures, though under a constructive framework, have a fundamental role. It is not true that in 1628 Descartes formulated the *Regulæ* as a specular translation of his geometry of 1619; nor is it true that after 1628 a programme that did not exist came to a crisis for technical reasons. The contrary holds true: exactly in 1628 the determination of the principles of a new method by Descartes induced a radical change in his consideration of geometrical problems. On the one hand, this method is "analytical" and consequently chooses methods used in algebra and it is a "constructive" analytical method and therefore uses the constructive procedures of classical geometry as its point of reference. On the other hand, this constructive analytical procedure, which will be described shortly, completely changes the picture of geometry, in particular the criteria of representation and admission of curves, where progress and difficulties analysed by Bos become evident. It remains doubtful, however, whether Descartes ever worried about those difficulties or even perceived them.

Therefore the crucial knot is to be found in Descartes' method, which is both, analytical and constructive. Such a method needs algebra as a universal language, which reflects the generality of the method, but at the same time it is constructive and does not admit leaps or lacerations in its procedures. Descartes strives to unite these two requirements: therefore the contradictions in his text do not arise from the co-existence between two different visions of geometry but represent the difficulties of one coherent vision[21], which is based on a philosophical programme and not on one of mathematical nature. The Cartesian "incoherences" only exist when they are viewed under the point of view of modern analytical geometry, which requires a balanced co-existence between geometry and algebra: Descartes, however, was not at all interested in cutting the "Gordian knot". For him this "Gordian knot" did not exist, nor could he have solved it—not because he was attached to an ancient vision of geometry but because it would have been in contradiction with his methodological approach. Let us rather have a look at the subordination of algebra to geometry, which is no residue of the past but a necessary consequence of the Cartesian methodical principles.

A purely internalist historiographic vision can make it even more difficult to value the significance of Descartes' contribution and its position in the history of science. Our opinion, which is also based on the big influence that *La Géométrie* had for more than a century, is that Descartes' contribution meant an enormous methodological revolution: this is why his success went beyond the importance of

the results. In order to understand the reasons, there is no need to go back to the traditional connection between *La Géométrie* and the *Discours de la méthode*: in the *Regulæ ad directionem ingenii* it is possible to trace a stronger connection which permits re-reading *La Géométrie*, bringing to light aspects of great importance which, up to now, may have been valued in a one-sided way. The re-reading of the work combined with the sound contributions of historiography, in particular those of Boyer and Bos, provides a satisfactory picture of the basic arguments of the Cartesian text.

Within the limits of this article it is impossible to embark on a detailed and exhaustive analysis of the *Regulæ*, let alone *La Géométrie*, the contents of which will be taken for granted. Here we shall limit ourselves to undo the most important conceptual knots of the Cartesian analytical method emerging from the *Regulæ*. They can be described as follows.

The first point is the affirmation that knowledge is achieved in a two-fold way: by "intuition" (an elementary act the basis of which is not any unreliable information provided by the senses or by imagination but the conception of a "pure attentive spirit" which does not leave any doubts as to what has been understood[22] and is the matrix of the formation of clear and distinct ideas) and by "deduction" (with deduction being a chain of intuitions). It follows that reasoning, being invariably based on the use of "concatenations of elementary acts of intuition", is deductive—which is the second fundamental point.

The third aspect centers on the "constructive" character of the deductive procedure: the chain of deductions on which it is based must not be interrupted, the result has to be reached without leaps. In the process of reasoning one term must not approach another pre-existing term but all links and relations between them have to be indicated so that a chain of intuitions connecting them is constructed. Its validity is proved by the fact that the deductive chain can be run through time and again in an "ordered" and "continuous" movement, which makes it possible to verify whether the construction which conducts to the final truth is valid.

The fourth aspect deals with the possibility of reducing any differences between objects to "differences between geometrical figures": this is the first form of the Cartesian notion of reducing differences to differences of extension in the *Regulæ*, which is basic to the Cartesian quantitative conception of the Universe. In the *Regulæ* this idea does not immediately appear as a metaphysical principle (the possibility of reducing any object to extension), but as an intuitive aid to represent the relations which are difficult to conceive in a form more accessible to intuition. But this first presentation is followed by an interpretation which outlines very clearly the explicit metaphysical valence which the concept of extension will assume in subsequent works. In fact, Descartes proposes a quantitative interpretation of the Universe, centering on mathematics, *i.e. Mathesis universalis*, completely different from the "vulgar" mathematics of his time, and a universal knowledge that permits reducing the analysis of any phenomenon to problems of "order" and "relations". In the deductive chain of reasoning every intuition can be compared with the subsequent one, as in the comparison of two quantities. So, deductive reasoning is transformed, or rather reveals its true nature as a sequence of concatenated relations: in it there occurs what happens in mathematical progression, where every term is determined by the relationship with the preceding term. By means of the language of algebra, deductive reasoning is translated into a sequence of proportions—hence the fundamental role of the theory of proportions. Another important consequence is the following: it has been maintained that, due to the constructive character of the deductive procedure, no ring of the chain can be left out, nor can there be any data without having defined the procedure which, starting from a further well-known truth, permits obtaining it (*i.e.* by way of construction). Therefore the translation of the deductive procedure into algebraic language (*i.e.* by means of equations via the theory of proportions) is "one-directional"; it is possible to move from the deductive procedure to algebra, but not the other way round, since there are no constructive procedures expressed by algebra. In the specific field of the relations between algebra and geometry this implies that their relations are one-directional: it is possible to pass from the geometrical problem to its algebraic translation (provided that the algebraic operations applied are geometrically and thus constructively justified), but not to do the opposite, since no "algebraic problems" as such exist. The *Mathesis universalis*, which reflects the constructive form of deductive reasoning and the universality of the well-defined relations that exist among the objects of the Universe, only contemplates problems of geometrical construction.

The fifth and last aspect is the following: in all the *Regulæ* there is a parallel between "arts" and science, between the procedures of the "mechanical arts" and the constructive procedure of deductive reasoning. This is one of the great number of aspects of the Cartesian mechanistic conception. This parallelism is translated into a parallelism between the procedures of the mechanical "arts" and geometrical constructions and is the basis of the definition of the new criterion of demarcation between admissible and inadmissible curves, introduced by Descartes, which made it possible to go beyond the ancient classification of the curves and reclassify them: an important result, since, apart from some significant but not decisive differences, it coincides with the modern classification of algebraic and transcendental curves.

Let us now examine more closely the significance and implications of the five aspects mentioned above, looking at the form in which they are presented and taken up in the *Regulæ*.

Before doing so, however, we want to discuss to two general topics which represent a sort of "*leit-motiv*" of the *Regulæ*. The first is the refusal of specialistic knowledge in favour of the unity of learning. This is the central theme of

Regula I[23], but also recurs in many other passages and has important consequences for mathematics. For Descartes the study of specific mathematical problems is of no interest at all:

> "neque enim magni facerem has regulas, si non sufficerent nisi ad inania problemata resolvenda, quibus Logistae vel Geometrae otiosi ludere consueverunt; sic enim me nihil aliud praestitisse crederem, quam quod fortasse subtilius nugarer quam caeteri. Et quamvis multa de figuris et numeris hic sum dicturus, quoniam ex nullis disciplinis tam evidentia nec tam certa peti possunt exempla, quicumque tamen attente respexerit ad meum sensum, facile percipiet me nihil minus quam de vulgari Mathematica hic cogitare, sed quamdam alima me exponere disciplinam, cujus integumentum sint potius quam partes." (Descartes AT, X, 373-374 and LR, 30-32)

In a certain sense he explicitly declares that he neither is nor wants to be a mathematician; he uses mathematics (*i.e.* certain mathematics, different from the "vulgar" mathematics of his time) to determine the principles of a universal method of reasoning[24].

The second theme is the refusal of a historical approach to science: *Regula III* establishes a distinct opposition between historical and scientific learning. According to Descartes, we would not be able to "express a firm judgement on a given question", even if we read all the works of the ancients. In the following he enunciates most clearly the opposition mentioned above: "in fact, we seem to have learned from history and not from science" (AT, X, 367 and LR, 16-18). It is evident that this opposition is a result of Descartes' need to proclaim the necessity of leaving previous learning aside so as to promote the development of a science free from the prejudices of bookish learning. But in doing so he accomplishes more than a tactical move: he actually establishes the basis of one of the cornerstones of modern sciences, which has greatly influenced research and its view of the role of history: it is a matter of affirming the uselessness of historical learning in the determination of trends in scientific research and its opposition to the acquisition of scientific learning. As will be shown, this point of view also helps Descartes to deal with traditional principles without being prejudiced, re-examining them independent of any reference to historical tradition and only because of their conceptual value: an impartiality which is particularly important at the moment he abolishes the classification of the curves, consolidated by a time-honoured tradition.

Let us now return to the analysis of the five fundamental topics of the *Regulæ*. The first two of them are already clearly enunciated in *Regula III*: the only two acts of intellect suitable to obtain knowledge without errors are "intuition" and "deduction". It has to be emphasized that, by defining intuition as the "firm conception of a pure and attentive spirit that is born by the light of reasoning alone", Descartes underlines that this act is purely intellective and differentiates it from the "unreliable testimony of the senses" or the "deceptive judgement of imagination" (AT, X, 368 and LR, 20). In order to explain the changes in the use of the term, he refers to its Latin meaning "*intuitus*"[25]. "Deduction", on the other hand, is the means to get to know other things (most things, actually) which are not evident by themselves, provided that they are deduced from true and known principles by way of a chain of elementary acts of intuition and consequently controllable at each step. The difference between the first and second act mainly consists in the fact that the latter needs a "movement" or a "succession". This "movement" is the key to the deductive process: in fact, it is a "continuous and uninterrupted movement of thought with a clear intuition of everything"[26] (AT, X, 369 and LR, 22). Here the influence of two fundamental principles of the Cartesian conception is to be found: the principle of "continuity" and the principle of "completeness". The consequence is a conception of the Universe as a "continuum" free from lacerations and interruptions: it is well-known that Descartes completely refused the existence of a vacuum. As to processes of reasoning (which are of the same nature as material processes), these principles are reflected in the concept of continuity of the deductive chain and in the absence of ruptures and interruptions. It has to be emphasized that the two terms are not synonymous and their meanings do not even partially overlap. This is made clear in ***Regula VII***, from which emerges that "continuous" means "which does not stop", "without pauses", "arriving at the very end of the course" and follows all necessary concatenations so as to make up for the weaknesses of memory, which is unable to seize the whole course of reasoning at once. Moreover, it is evident that the deductive chain moves in one direction—from the introduction to the conclusion: by going through the chain time and again in a continuous movement (faster and faster), it is possible to leave the role of memory aside and obtain a sort of global intuition of the whole[27]. Consequently continuity presents itself as a characteristic feature that leads to the comprehension of the whole. *Vice versa*, the fact, that in the act of deducing, the movement of thought is uninterrupted implies that it is not permitted to skip any ring of the chain; the conclusions are no longer certain[28].

Let us have a look at the consequences which these characteristics of deductive reasoning have on the "status" of geometry (as well as that of physics, since the possibility of a gap is negated): geometrical reasoning must be constructive, as it is based on chains of steps, each depending on the preceding one. The geometrical object is only imaginable as constructed by such a succession. Therefore the geometrical point cannot be seen "isolated": when the geometrical object (*e.g.* a curve) is constructed, it has to be explained how to pass from one point to the next one in a "continuous" and uninterrupted procedure. As in physical space, also in geometrical space there can be no gap. From this results Descartes' inconceivability of the notion of geometrical locus assigned in an abstract way by means of an equation and not defined by a construction. The above-mentioned refusal of reasoning *ab absurdo* equally depends on this vision (it is not constructive: skipping

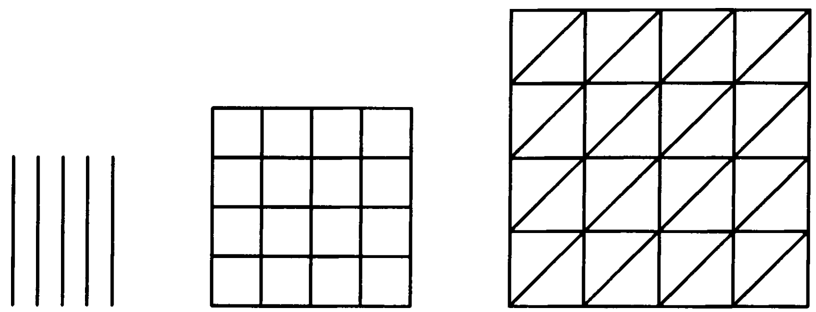
all rings of the chain, it directly compares the last one with the first one and is not one-directional).

It has been considered useful to look at the implications of the Cartesian notion of geometry, so as not to keep apart two important and intimately interrelated aspects. In doing so, however, we have not yet specified the role of geometry in Descartes' notion. It is explained in ***Regula IV***, which contains some famous passages on the meaning of the ***Mathesis universalis***. We are not going to spend much time on such a well-known topic, but want to emphasize the parallels between Descartes' criticism of the particular sciences and the claim that a form of universal knowledge is necessary, as well as his criticism of the way to do mathematics (geometry and arithmetics). The latter emerges from tradition and the claim to "true" mathematics, certainly well-known to the ancients as the easiest and most necessary science of all to form and prepare the mind to understand other more elevated sciences[29]. In order to understand what this is all about, however, it is not sufficient to refer to etymology, according to which "Mathematics" simply means "science", because in this case also Music, Optics and Mechanics would have the same right as Geometry to be called Mathematics[30]. The substance of Mathematics (which makes it a universal science or *Mathesis universalis*) is the study of everything that is connected with order and measure, "no matter whether these measures are to be found in numbers, figures, stars, sounds or in any other objects"[31] (AT, X, 377-378 and LR, 38).

The link between *Mathesis universalis* and the deductive procedure is evident: just as *Mathesis universalis* searches order in things, according to *Regula V* "the whole method consists in order and the arrangement of the things towards which the mind has to be directed in order to discover some truths"[32] (AT, X, 379 and LR, 42). It follows that the classification of things must no longer be attained by means of categories, as in scholastic philosophy, but "according to deductive order"[33]. Finally, "in order to attain science, all things leading to our goal, and every singular one in particular, have to be run through in a continuous and uninterrupted movement of thought and have to be understood in a sufficient and methodical enumeration"[34].

The concept of "sufficient enumeration" or "induction" could be considered a rudimentary version of the principle of mathematical induction known in modern mathematics. As the only certain procedure Descartes puts induction next to intuition, since it defines inference in each point of the chain. Nevertheless it is quite a rudimentary concept of induction: it is true that the link between subsequent steps is decisive, but all steps have to be examined: moreover, in Descartes' work this notion is not subjected to a clear concept of numeration of the steps (as is shown in Descartes' example on circles in ***Regula V***)[35]. The distance separating the Cartesian induction from the modern one still refers to the theme of constructivism, which is well-explained by the contents of *Regula XI*. It has been observed that each step has to be controlled, constructed, and nothing can be left out or taken for granted, while two verifications are sufficient in modern induction and from these infinite cases are deduced. However, Descartes' opinion on the infinite is well-known: his finitism is the second fundamental aspect, mentioned above. In the *Principia*[36], for instance, he distinguishes between infinite and indefinite, reserving the first attribute to God. In this text Descartes declares that he will never deal with discussions of the infinite, since he considers it ridiculous that "finite" beings pretend to say something about the infinite. This also explains the boundaries of the relationship between the Cartesian notion of "continuum" and the modern one: the idea of completeness and lack of interruptions has nothing to do with continuity in the modern sense of the word: therefore Descartes' completeness is a sort of "discrete continuity".

As has been pointed out, *Mathesis universalis* also deals with the study of measure. The answer to the question whether there exist any parallels between mathematics and the deductive method also at this level is positive; in order to establish this connection, however, another crucial knot of Cartesian thinking has to be considered: the concept of extension and the notion that every object can be reduced to characteristics of extension.This is another aspect of Cartesian philosophy too well-known to deal with it here: we shall limit ourselves to say something on how it is presented in the *Regulæ*. It is in *Regula XII* that a representation of differences between objects (and related representations) as differences between figures is introduced. Descartes observes that there is nothing wrong with conceiving the differences that exist between colours like white, blue and red as differences between figures, as the following or similar ones:



In this way the introduction of useless entities is avoided and an extremely natural representation is recurred to. Moreover, the infinite number of figures is sufficient to describe all differences between sensitive objects. In this form the notion of the quantitative description of differences between sensitive objects is

introduced in the *Regulæ*: it will be expressed in the different shape of two geometrical figures and consequently in their relationship. This is a crucial passage: by establishing the central position of the concept of extension, the central role of geometry as science of extension in the learning process is also established.

Nevertheless, the form which the principle of reduction to extension assumes in the *Regulæ* somewhat differs not only from the metaphysical version of the *Principia*[37], but apparently also from the meaning implicit in the text of *La Géométrie*. In fact, in the *Regulæ* differences are described by means of references to illustrated extensions applying to imagination and, in a certain sense, juxtaposed to quantity. In *La Géométrie* this juxtaposition disappears, actually it turns to a hierarchical relationship: in fact, the role of imagination disappears and extension is reduced to quantity by means of algebra. More precisely, the quantitative description of differences by means of extension is realized in a purely intellectual way and the instrument of such a realization is the algebraic description. We have spoken of apparent differences between *Regulæ* and *Géométrie*; the following propositions represent a decisive step forward: generic references to an illustrated representation of differences are abandoned and are described in terms of the theory of proportions—at this point the decisive step towards the introduction of algebra has been taken. Furthermore, as we shall see, the concept of "problem with unknown quantities" and thus the concept of equations is introduced. Last but not least the final part of the first book (incomplete as well) makes evident that Descartes had a clear notion of the central role of algebra.

Before continuing with these topics, we will point out an important consequence of what has been mentioned before: the relationship between extension and quantity thus established shows the subordinate position of algebra compared to the central one of geometry. Geometry comes first: as science of extension it is the instrument to describe and analyse the substance of things: algebra has the essential but subordinate role to make it possible to treat extension as quantitative description and not as a complex of figurations perceptible by imagination alone. This hierarchy is clearly reflected in *La Géométrie* (from the first pages onwards), when Descartes tries to justify the introduction of algebraic operations by means of geometry (demonstrating "how arithmetical calculations refer to geometrical operations"[38] (AT, VI, 369)).

In *Regula XII* we find the distinction between simple propositions and "*quæstiones*": the first only need the distinct intuition of the object and the methods of reasoning expounded in the first twelve rules (which are the ones we have summarized up to this point), whereas the second (object of the remaining twelve rules[39]) regard the problems which are "perfectly understood"[40], even though the solution is unknown. These abstract problems in algebra and geometry lead to three kinds of questions: *a*) Which are the signs to recognize the object that is being searched? *b*) What can it be deduced from? *c*) How does the close dependency of these things manifest itself? In order to solve these problems, the deductive procedure is no longer sufficient, an "art" (*i.e.* Descartes' *ars analytica*, which he sets about to expound) has to be introduced: it consists in the "development of something that depends on many other" *simul implicatis*[41] (AT, X, 429 and LR, 140). This "art" is nothing else than the method of solving problems where "unknown quantities" appear (as is explained by *Regula XIII*, introducing the concept of designating something unknown by something that is known) and thus the "art" of solving equations. Even though this procedure is different from the deductive one (developing what is unknown from what is known), there is a tight link with the constructive procedures of deduction, particularly because the questions dealt with are perfectly determined[42].

Regula XIV contains the last important step towards the translation of "perfectly understood" problems into algebraic form. In fact, Descartes observes that any knowledge that is not gained by simple intuition is gained by comparison. In distinct objects common characteristics are to be found in relations and proportions which have to be reduced to equalities. But in virtue of former considerations, only quantity is susceptible to this reduction and it is extension that has to be chosen from the quantities. So the formulation of a perfectly determined problem is nothing else than the reduction of proportions to equalities. In this rule we observe a transition from the definition of these differences between things by means of figures to a definition of these differences by means of relations or proportions of quantities of extension. The intervention of algebra as an instrument, however, has not yet occurred: this happens in *Regula XVI*, where algebra is explicitly introduced as a more compact instrument of symbolical representation[43] than the geometrical-spatial signs which Descartes referred to in the preceeding rules; he did so in order to give an example of the translation of relations (or differences) between things into relations of extension. This rule is also important as a significant step towards algebra: it eliminates the distinction between root, square root, third root etc., all reduced to the language of the theory of proportions.

It is in *Regula XVII*, however, that the procedure which Descartes suggests in order to solve a perfectly determined problem is expound more clearly, having been translated into equations or a chain of proportions. In our opinion, a direct parallel can be established between this procedure and the one expound in *La Géométrie.*

Descartes observes that, while there exists an easy and direct way to solve a problem in a deductive manner which permits to pass easily from one term to another—it is the one of the direct concatenation exposed in *Regula XI*—the situation is different in the case of perfectly understood problems. Let us continue with Descartes' words:

> "Nunc igitur si dependentiam singularum ad invicem, nullibi interrupto ordinem, intueamur, ut inde inferamus quomodo ultima a prima dependeat, difficultatem directe percurremus; sed contra, si ex eo quod primam et ultimam certo modo inter se connexas esse cognoscemus, vellemus deducere quales sint mediae quae illas conjungunt, hunc omnino ordinem indirectum et praeposterum sequeremur. Quia vero hic versamur tantum circa quaestiones involutas, in quibus scilicet ab extremis cognitis quaedam intermedia turbato ordine sunt cognoscenda, totum hujus loci artificium consistet in eo quod, ignota pro cognitis supponendo, possimus facilem et directam quaerendi viam nobis proponere, etiam in difficultatibus quantumcunque intricatis; neque quicquam impedit quominus id semper fiat, cum supposuerimus ab initio hujus partis, nos agnoscere eorum, quae in quaestione sunt ignota, talem esse dependentiam a cognitis, ut plane ab illis sint determinata, adeo ut si reflectamus ad illa ipsa, quae primum occurrunt, dum illam determinationem agnoscimus, et eadem licet ignota inter cognita numeremus, ut ex illis gradatim et per veros discursus caetera omnia etiam cognita, quasi essent ignota, deducamus, totum id quod haec regula praecepit, exequeremur [...]" (Descartes AT, X, 460-461 and LR, 200)

Descartes declares that he wants to reserve the examples of this method for the subsequent *Regula XXIV*, which is missing: in our view, however, these examples can be found in *La Géométrie.*

The following *Regula XVII* teaches us that only four operations (addition, subtraction, multiplication and division) are sufficient to establish these mutual dependencies, which consents to reduce the definition of mutual dependencies to a sequence of proportions. The following step (*Regula XIX*) is to search as many quantities expressed in different ways as there are unknown variables. When the equations have been found and all remaining operations have been completed (*Regula XX*), all equations of this kind have to be reduced to a single one, "*i.e.* to the one whose terms occupy the minimum degrees on the scale of quantities, in continuous proportion according to which they must be arranged" (*Regula XXI*; Descartes AT, X, 469 and LR, 216).

Let us now turn to the first pages of *La Géométrie.* Here we find the translation of the procedure just expound with analogue terms and the same methodical sequence. It will suffice to read the following passage to verify this statement:

> "[...] voulant resoudre quelque problesme, on doit d'abord le considerer comme desia fait, & donner des noms a toutes les lignes qui semblent necessaires pour le construire, aussy bien a celles qui sont inconnuès qu'aux autres. Puis, sans considerer aucune difference entre ces lignes connues & inconnues, on doit parcourir la difficulté selon l'ordre qui monstre, le plus naturellement de tous, en quelle sorte elles dependent mutuellement les unes des autres, iusques a ce qu'on ait trouvé moyen d'exprimer une mesme quantité en deux façons: ce qui se nomme une Equation, car les termes de l'une de ces deux façons sont esgaux a ceux de l'autre. Et on doit trouver autant de telles Equations qu'on a supposé de lignes qui estoient inconnuès. Ou bien, s'il ne se trouve pas tant, & que, nonobstant, on n'omette rien de ce qui est desiré en la question, cela tesmoigne qu'elle n'est pas entierement determinée; et lors, on peut prendre a discretion des lignes connues, pour toutes les inconnues ausquelles ne correspond aucune Equation. Après cela, s'il en reste ancore plusieurs, il se faut servir par ordre de chascune des Equations qui restent aussy, soit en la considerant toute seule, soit en la comparant avec les autres, pour expliquer chascune de ces lignes inconnuès, & de faire ainsi en les desmelant, qu'il n'en demeure qu'une seule, esgale a quelque autre qui soit connuè, ou bien dont le quarré, ou le cube, ou le quarré de quarré, ou le sursolide, ou le quarré de

cube, &c., soit esgal a ce qui se produist par l'addition, ou soustraction, de deux ou plusieurs autres quantités, dont l'une soit connuè, & les autres coient composées de quelques moyennes proportionnelles entre l'unité & ce quarré, ou cube, ou quarré de quarré, & c., multipliées par d'autres connuès. Ce que i'escris en cete sorte:

$$z \propto b,$$
ou $$z^2 \propto -az + bb,$$
ou $$z^3 \propto +az^2 + bbz - c^3,$$
ou $$z^4 \propto +az^3 + cz^3 + d^4,$$
&c.

C'est a dire: z, que ie prens pour la quantité inconnuè, est esgale ab; ou le quarré de z est esgale au quarré de b, moins a multiplié par z ; ou le cube de z est esgal a a multiplié par le quarré de z, plus le quarré de b multiplié par z, moins le cube de c; & ainsi des autres.
Et on peut tousiours reduire ainsi toutes les quantités inconnuès a une seule, lorsque le Problesme se peut construire par des cercles & des lignes droites, ou aussy par des sections coniques, ou mesme par quelque autre ligne qui ne soit que d'un ou deux degrés plus composée. [...] ie me contenteray icy de vous avertir que, pourvû qu'en demeslant ces Equations on ne manque point a se servir de toutes les divisions qui seront possibles, on aura infalliblement les plus simples termes ausquels la question puisse estre reduite." (Descartes AT, VI, 372-374)

The close link between the general methodical principles enunciated in the *Regulæ* and their application in *La Géométrie* is more than evident. Actually, we could say that almost the whole procedure to "develop" the unknown quantity into equations described in *La Géométrie* is already contained in the *Regulæ*.

Let us now examine the last of the five fundamental themes which have been considered the nucleus of the *Regulæ*: the question of the relationship between mechanical arts and geometry. The first reference to this question appears in *Regula VIII*, where Descartes, after having given various examples on the use of the method, continues like this:

"Haec methodus siquidem illas ex mechanicis artibus imitatur, quae non aliarum ope indigent, sed tradunt ipsaemet quomodo sua instrumenta facienda sint. Si quis enim unam ex illis, ex.gr., fabrilem vellet exercere, omnibusque instrumentis esset destitutus, initio quidem uti cogeretur duro lapide, vel rudi aliqua ferri massa pro include, saxum mallei loco sumere, ligna in forcipes aptare, aliaque ejusmodi pro necessitate colligere: quibus deinde paratis, non statim enses aut cassides, neque quidquam eoruim quae fiunt ex ferro, in usus aliorum cudere conaretur; sed ante amnia malleos, incudem, forcipes, et reliqua sibi ipsi utilia fabricaret. Quo exemplo docemur, cum in his initiis nonnisi incondita quaedam praecepta, et quae videntur potius mentibus nostris ingenita, quam arte parata, poterimus invenire, non statim Philosophorum lites dirimere, vel solvere Mathematicorum nodos, illorum ope esse tentandum: sed iisdem prius utendum ad alia, quaecumque ad veritatis examen magis necessaria sunt, summo studio perquirenda; cum praecipue nulla ratio sit, quare difficilius videatur haec eadem invenire, quam ullas questiones ex iis quae in Geometria vel Physica aliisque disciplinis solent proponi". (Descartes AT, X, 397 and LR, 76-78)

Descartes' interest in machines and mechanical arts as a natural consequence of his mechanistic conception is well-known. Nevertheless, as Paolo Rossi points

out, "for Descartes the effective progress of science depends on the work of theorists. Technology as such does not contribute to the progress of scientific learning at all" (Rossi 1962, 111). Rossi remembers Baillet's description of Descartes' project to build some halls at the College de France: the craftsmen involved were taught the scientific principles of making machines work by professors of mathematics and physics. Technology remains subordinate to science, it has to follow its principles, in particular its methodological principles, and not the product but its principle of realization is of interest. In this way Descartes reveals a conception which, in a certain sense, is closer to a technological approach than to a technical one; the main difference, however, is that the relationship between science and technology is somewhat sterile, as technology is considered subordinate to science. In any case Descartes is interested in technology as deriving from methodical principles, since for him this is the practical verification of the world's mechanical nature. His description of the mechanical arts, where nobody proceeds at random but first prepares the necessary tools following methodical principles, shows that he sees a concrete connection between some historical forms of the "arts" and his method. This connection becomes less vague and boils down to a concrete reference in *Regula X*, where the importance of the simpler arts, which are "are ruled by order", is dealt with, those of craftsmen making cloths or carpets or embroideries "similar to number combinations and arithmetical operations"[44]. Embroidery is particularly interesting, as it links the characteristics of these arts, i.e. being simple and methodical, to a specification of their procedures: it is close to the theory of proportions. Therefore these arts appear as a concrete representation of the concatenated, continuous and uninterrupted movement which is the nucleus of the method. We know that one of the outstanding characteristics of technical development in France at the time of Descartes was the diffusion of the textile industry based on the use of the power loom (Dockès 1969). So Descartes' reference appears in no way fortuitous: in a new innovative technique like the one of power-loom weaving, Descartes saw the expression of a conception of the mechanical arts based on method in a double sense: in a general sense, since the methodical principle is put before the specific realization (the way of weaving is more important than the product itself) and in a specific sense, because—as is evident in the case of the power-looms—the functioning of the instrument is based on a concatenation of coordinate movements following one another according to a well-defined rule. This concatenation is determined by precise number relations and consequently based on the theory of proportions. All crucial conceptional knots of the Cartesian method (continuous and uninterrupted movement, theory of proportions) can be found in these examples of mechanical arts.

Several times it has been pointed out that Descartes' famous instrument of movable squares, or rather the instrument to multiply proportions, which appears in *La Géométrie* and has a fundamental role in the classification of curves, had been invented by him long before the publication of *La Géométrie*. This kind of power-loom seems to be a further manifestation of a predilection for the procedures of the mechanical arts based on the theory of proportions, clearly expressed in the ***Regulæ***. In any case, Descartes' instrument of movable squares permits the geometrical representation of a sequence of proportions and so it is nothing but the concrete translation of a continuous and uninterrupted movement, the subsequent steps of which are all concatenated according to precise and perfectly determined relations. Although this does not fully meet the necessary qualification of constructibility of the Cartesian conception, it stands for the prototype of a class of instruments that conform to these qualifications (Bos 1981). Descartes referred to this instrument, when he proposed a new classification of "admissible" curves which was to substitute the classical subdivision into "geometrical" curves (*i.e.* curves that can be drawn with ruler and compasses or planar loci), curves obtained by cutting a section (*i.e.* conics or linear loci) and "mechanical" curves (resulting from the "chaotic" motion of a point). This classification was based on the preference of ruler and compasses and could only be changed after their priviledged position had been abolished and different criteria introduced. Descartes, however, did not have the slightest reason to insist on recognizing the ancient classification—neither on the methodological nor the technical level ruler and compasses were to be preferred, since they only represented a partial and episodic working method as to the methodological principles. An instrument like the one of movable squares instead constituted their complete and faithful translation.

It is most interesting to read Descartes' comment on the problem of the classification of curves, which is characterized by the "anti-historic" spirit mentioned above.

> "Les anciens ont fort bien remarqué qu'entre les Problesmes de Geometrie, les uns sont plans, les autres solides, & les autres lineaires: c'est a dire que les uns peuvent estre construits en ne traçcant que des lignes droites & des cercles; au lieu que les autres ne le peuvent estre, qu'on n'y employe pour le moins quelque section conique; ni enfin les autres, qu'on n'y employe quelque autre ligne plus composée. Mais je m'estonne de ce qu'il n'ont point outre cela, distingué divers degrés entre ces lignes plus composées, & ie je sçaurois comprendre pourquoy il les ont nomées Mechaniques, plustot que Geometriques." (Descartes AT, VI, 388)

Descartes' astonishment could seem somewhat strange, if it were not regarded in the above-mentioned anti-historic context. Further on he emphasizes that mechanical curves do not derive their names from the fact that they are drawn by machines, because otherwise also the curves drawn with ruler and compasses, which actually are machines, too, would have to be rejected. We know, however, that in the ancient classification 'mechanical' has a different meaning and, at least in Greek tradition, ruler and compasses have an intellectual value—they represent ideal perfection (exactly like the machine of movable squares in Descartes' intentions). But Descartes continues as if he was not aware of this:

> "Ce n'est pas non plus a cause que les instrumens qui servent a les tracer, estant plus composés que la reigle & le compas, ne peuvent estre si iustes [...]." (Descartes AT, VI, 388-389)

Otherwise they would have to be rejected also from the mechanical curves

> "[...] ou c'est seulement la iustesse du raisonnement qu'on recherche, & qui peut sans doute estre aussy parfaite, touchant ces lignes, que touchant les autres autres." (Descartes AT, VI, 389)

So 'mechanical' does not mean inexact—on the contrary, mechanical procedures are based on exactness. On the other hand, Descartes does not even consider the possible meaning of 'mechanical', *i.e.* "generated by motion". In this way he discards all possible hypotheses of interpretation, only to demonstrate the incoherence of the ancients, and concludes, declaring that he does not want to change any names that have already been accepted by use. In doing so, however, he has deprived them from their original meaning: henceforth—though only conventionally—'geometrical' refers to what is precise and exact and 'mechanical' to what is not. 'Mechanical' alone does not mean anything any longer: it neither means "generated by motion" nor "obtained by means of a machine". Both of these meanings would only obstruct Descartes' new classification, which accepts many of the curves that, according to the old classification, were considered "mechanical" as admissible curves. Now the term 'mechanical' only serves to denote the opposite of something that is perfectly determined—the contrary of 'geometrical', which is well-determined. The names remain unchanged, the line of demarcation of the meanings changes.

Geometry is the science whose object is the measure of bodies. Therefore there is no reason to exclude composite lines in favour of simple lines,

> "pourvû qu'on les puisse imaginer descrites par un mouvement continu, ou par plusieurs qui s'entresuivent & dont les derniers soient entierement reglés par ceux qui les precedent: car, par ce moyen, on peut tousiours avoir une connoissance exacte de leur mesure." (Descartes AT, VI, 390)

Here the usual criterion, already familiar to us, re-emerges: constructibility by means of a continuous, uninterrupted and coordinate movement. This criterion (of which we want to emphasize its "constructive element") is the true conceptual core of Cartesian geometry. This makes the reference to coordinate geometry appear secondary, if not marginal, whereas the classification of curves obtained by Descartes by the conceptual use of the instrument of movable squares is most important. It is well-known that the classification is not complete—due to its constructive character which does not assume the order of the curve as an element of classification, as would happen in the case of a point of view based on the concept of geometrical locus, *i.e.* starting from the algebraic equation: it skips several steps and therefore does not obtain all algebraic curves, as one would expect on the basis of permitted operations, which are the algebraic ones. This topic has been widely discussed and studied in the literature[45]. There is no doubt, however, that the Cartesian classification is almost a complete step towards the modern distinction of the curves between algebraic and transcendental curves, which will be explicitly codified by Leibniz.

Here we conclude our analysis, which is not aimed at going through these specific themes already widely analysed by other exhaustive studies but rather at showing that certain specific themes (like the one of the classification of the curves or the position of Cartesian geometry in the history of analytical geometry) are brought into a new light by the analysis of the relationship between the method expound in the *Regulæ* and *La Géométrie*. Cartesian Geometry no longer appears as a step in the formation of analytical geometry in the modern sense of the word. It is the result of a very particular view of mathematics, in which the concept of geometrical extension has a central role. Descartes' geometry is "analytical", not because it highlights coordinate method, but because it recalls a methodological principle (indeed "analytical") centered upon the "deductive" and "constructive" procedures of reasoning which are the heart of Cartesian philosophy.

University of Rome "La Sapienza"
Department of Mathematics

Notes

* This essay is a revised English version of the paper "Dalle *Regulæ* alla *Géométrie*" published in Italian in the book *Descartes: il Metodo e i Saggi*, Atti del Convegno per il 350° anniversario della pubblicazione del *Discours de la Méthode* e degli *Essais* (G. Belgioioso, G. Cimino, P. Costabel, G. Papuli, eds.), *Acta Enciclopedica* no. 18 (2 voll.), Istituto della Enciclopedia Italiana, Roma, 1990: vol. 18**, pp. 441-474. We thank the *Istituto della Enciclopedia Italiana* for the authorization to publish a new English version of the essay.

1 This is the meaning that Dijksterhuis attributes to the term "analytical geometry"; Descartes is considered its creator: "With the introduction of the new symbolic algebra in geometry he actually became the creator of analytical geometry and consequently the author of one of the most fundamental reforms in mathematics." (Dijksterhuis 1961, 543)

2 The "possibility of setting out propositions in deductive chains" which Cartesius speaks of is identified with the possibility of "expressing the acquired knowledge in axioms". Dijksterhuis continues: "The intention of the cartesian method is [...] to make scientific thought occur [...] through deduction, starting out with axioms, and through algebra." (*ibid.*, 542)

3 The original edition of the *Regulæ* is Descartes (ROP).

4 Descartes talks about these demonstrations "quae casu saepius quam arte inveniuntur, et magis ad oculos et imaginationem pertinent quam ad intellectum." (Descartes AT, X, 376 and LR, 34-36)

5 Descartes remembers to have read nearly everything from the beginning that is usually taught in Arithmetics and Geometry and comments: "Sed in neutra Scriptores, qui mihi abunde satisfecerint, tunc forte incidebant in manus: nam plurima quidem in iisdem legebam circa numeros, quae subductis rationibus vera esse experiebar; circa figuras vero, multa ispismet oculis quondammodo exhibebant et ex quibusdam consequentibus concludebant; sed quare haec ita se habeant, et quomodo invenirent, menti ipsi non satis videbantur ostendere." (*ibidem*, AT, X, 375 and LR, 32)

6 Modern axiomatic mathematicians consider it the basis of their method. See J. Dieudonné's numerous references to the work of Euclid as a point of reference for the logical-deductive axiomatic method (he repeats that in order to find a valid reference modern axiomatics have to go back in history to Euclid). Cf. *e.g.* Dieudonné (1939).

7 The original edition is Descartes (1641). There exists a French translation of this text published in Descartes' lifetime: Descartes (1647). The quotations are taken from this translation (which Baillet maintains to be preferable to the Latin one), while references will be given both to it and to (Descartes AT, VII).

8 Note, in particular, the clear reference to the method of proving *ab absurdo* ("afin que, si on lui nie quelques consequences, elle fasse voir comment elles sont contenues dans les antécédents "), which Descartes implicitly declares not to wish to include in his method (as a consequence of his refusal of synthesis).

9 The fact that this formulation and the one of the *Regulæ* quoted in note 5 are nearly identical is rather important.

10 Also on that point the *Regulæ* and the *Meditationes* agree. The previous passage actually continues like this: "Les anciens géomètres avaient coutume de se servir seulement de cette synthèse dans leurs écrits, non qu'ils ignorassent entièrement l'analyse, mais, à mon avis, parce qu'ils en faisaient tant d'état, qu'ils la reservaient pour eux seuls, comme un secret d'importance" (Descartes 1647, 388). In the *Regulæ*: "Cum vero postea cogitarem, unde ergo fieret, ut primi olim Philosophiae inventores neminem Matheseos imperitum ad studium sapientiae vellent admittere, tanquam haec disciplina omnium facillima et maxime necessaria videretur ad ingenia capessendi aliis majoribus scientiis erudienda et praeparanda, plane suspicatus sum, quamdam eos Mathesim agnovisse valde diversam a vulgari nostrae aetatis [...]." (Descartes AT, X, 376 and LR, 34)

11 The contrary happens in metaphysics, where "la principale difficulté est de concevoir clairement et distinctement les prémières notions." (Descartes 1647, 389 and AT, VII, 157)

12 In modern mathematics the term 'synthetic' has taken a different meaning. It is true that the reaction to the "subordination" of geometry to algebra appeared as a return to the ancient world: exactly to "synthetic" geometry, which was seen as a way of doing geometry in an autonomous manner and not subordinated to the use of analytical procedures. This tendency was called "purism" (with the Italian mathematician Luigi Cremona as one of the most important exponents), since it suggested to restore the use of "pure" methods in geometry, which were free from any reference to algebra. In the "purist" movement, however, the return to intuition took a predominant, if not nearly obsessive, role. Reasoning in a "synthetic" way did not only mean to proceed with a sequence of logical operations which were to correlate the geometrical characteristics of the entities studied without recurring to algebraic instruments, but to "see" the result, to know it by intuition, make it evident for imagination. The prevailing trend of synthetic geometry of the 19th century expressed a reconquest of the "intuitive geometrical spirit" over the "abstract analytical spirit". Despite refusing the excesses of Cremonian "purism" later on, the Italian geometrical school defended "synthetic" geometry right to the bitter end: not so much as a refusal of the algebraic instrument but so as to support a vision of the "synthetic" method based on the use of intuition or, more precisely, on the psychological acquisition of geometrical concepts. For further details see Israel (1990).

13 For further detail cf. Taton (1951), 79-92.

14 Not from the one by Dieudonné, but the one by Monge-Lacroix, the influence of which reaches up to recent times.

15 Cf. also Itard (1984, 277): "Descartes affirme plusieurs fois que les courbes organiques conduisent à une équation algébrique. Il n'affirme ni nie jamais la proposition réciproque."

16 Cf. *ibid.*, 277-278: "A se placer au niveau élémentaire, la *Géométrie* de Descartes est un ouvrage parmi bien d'autres, et les accusations de plagiat [...] pleuvent de toutes part. [...] On pourra toujours trouver chez tel ou tel auteur contemporain ou plus ancien telle ou telle des idées émises par Descartes dans sa Géométrie."

17 It is impossible to give even an approximate report on the vast secondary bibliography. So we will only remind of some texts which are among the closest to the subject we have been dealing with (and consequently close to the general lines we have been following), apart from the ones already quoted. First of all the important work by J. Vuillemin, to which we owe an essential contribution to bring the philosophic theme closer to the mathematical theme in Descartes' work: Vuillemin (1960). In this respect also Lenoir (1979). It specifies the connection between *Regulæ* and *Géométrie*, presenting, however, only a general analysis of the relations between the two. See also: Molland (1976), Coolidge (1940), Milhaud (1921), Granger (1968), Dhombres (1978), Scriba (1960-1962) and Schuster (1980).

18 Despite the fact, as Bos observes, that "nowhere in the *Géométrie* did Descartes use an equation to introduce or to represent a curve." (Bos 1981, 322)

19 Bos observes: "[...] the use of the key words, clear and distinct [...] show that Descartes saw a parallel between the series of interdependent motions in [a] machine, all regulated by the first motions, and the "long chains of reasoning" in mathematics, discussed in the *Discours de la Méthode*, which provided each step in the argument is clear, yield results as clear and certain as their starting point." (*ibidem*, 310)

20 There is much evidence of that. In his texts numerous sentences like the following famous one can be found: "Mais ie ne m'areste point a expliquer cecy plus en detail, a cause que ie vous osterois le plaisir de l'apprendre de vous mesme, & l'utilité de cultiuer vostre esprit en vous y exerçant, qui est, a mon avis, la principale qu'on puisse tirer de cete science. Aussy que ie n'y remarque rien de si difficile, que ceux qui seront un peu versés en la Geometrie commune & en l'Algebre, & qui prendront garde a tout ce qui est en ce traité, ne puissent trover." (Descartes AT, VI, 374). The original edition of *La Géométrie* is Descartes (1637).

21 On the other hand even Bos observes: "Although there were contradictions in the structure and the programme, there was an underlying unity of vision." (Bos 1981, 332)

22 "Per intuitum intelligo, non fluctuantem sensuum fidem, vel male componentis imaginationis judicium fallax; sed mentis purae et attentae tam facilem distinctumque conceptum, ut de eo, quod intellegimus, nulla prorsus dubitatio relinquatur; seu, quod idem est, mentis purae et attentae non dubium conceptum, qui a sola rationis luce nascitur [...]." (Descartes AT, X, 368 and LR, 20)

23 "Si quis igitur serio rerum veritatem investigare vult, non singularem aliquam debet optare scientiam: sunt enim omnes inter se conjunctae et a se invicem dependentes; sed cogitet tantum de naturali rationis lumine augendo [...]." (Descartes AT, X, 361 and LR. 6)

24 It is interesting to note that Descartes refuses to establish any parallels between science and arts at the beginning of the Regulæ (cf. *Regula I*), due to the different nature of arts, where the employment of one speciality interferes with the employment of another, whereas, according to Descartes, the opposite holds true for the sciences, which are linked in such a way that they are more easily assimilated as a whole than separately.

25 The verb "intueor" is understood above all in the sense of "consider attentively", "ponder over".

26 "Sed hoc ita faciendum fuit, quia plurimae res certo sciuntur, quamvis non ipsae sint evidentes, modo tantum a veris cognitisque principiis deducantur per continuum et nullibi interruptum cogitationis motum singula perspicue intuentis."

27 "Quamobrem illas continuo quodam imaginationis motu singula intuentis simul et ad alia transeuntis aliquoties percurram, donec a prima ad ultimam tam celeriter transire didicerim, ut fere nullas memoriae partes relinquendo, rem totam simul videar intueri." (Descartes AT, X, 388 and LR, 58-60)

28 To be more precise, we should point out that Descartes' meaning of "uninterrupted" is the one closest to the modern concept of "continuous", in particular the one suggested by mathematical terminology. However, they only conform in part. We could say that the principles of completeness and continuity correspond to the Cartesian principles of continuity and absence of interruption respectively, with some translation of the meanings; but on the whole they convey an idea that is rather close to the one suggested by the concept of continuum used in modern mathematics.

29 "[...] tanquam haec disciplina omnium facillima et maxime necessaria videretur ad ingenia capessendis aliis majoribus scientiis erudienda et præparanda." (Descartes AT, X, 375 and LR, 34)

30 "[...] nam cum Matheseos nomen idem tantum sonet quod disciplina, non minori jure, quam Geometria ipsa, Mathematicae vocarentur." (Descartes AT, X, 377 and LR, 36)

31 "Quod attentius consideranti tandem innotuit, illa omnia tantum, in quibus ordo vel mensura examinatur, ad Mathesim referri, nec interesse utrum in numeris, vel figuris, vel astris, vel sonis, aliove quovis objecto, talis mensura quaerenda sit."

32 "Tota methodus consistit in ordine et dispositione eorum ad quae mentis acies est convertendo, ut aliquam veritatem inveniamus."

33 This consequence is discussed in *Regula VI*.

34 "Ad scientiae complementum oportet omnia et singula, quae ad institutum nostrum pertinent, continuo et nullibi interrupto cogitationis motu perlustrare, atque illa sufficienti et ordinata enumeratione complecti." (Descartes AT, X, 387 and LR, 58)

35 A cumulative historian could say that Descartes did not have the concept of natural number.

36 See First Part, Sections 24, 25, 26, 27.

37 In the *Principia* extension is defined as the main attribute of a body (Part I, Sect. 53) and it is confirmed that the nature of a body only consists in being a substance with extension (Part II, Sect. 4). Moreover it is affirmed that size does not differ from what is big nor does number differ from what is numbered but through thought. Despite a substantial coherence of the two texts, the ways to establish identity between matter and extension are different. In the *Regulæ* it is methodical, whereas in *Principia* it is metaphysical.

38 "Comment le calcul d'Arithmétique se rapporte aux operations de Géométric.Dijksterhuis "

39 We only possess the ones from XIII to XXI (the last three are without comments).

40 These differ from the "imperfectly understood" problems, which are part of physics and should have been dealt with by Descartes in his last twelve rules.

41 "[...] sed unum quid ex multis simul implicatis dependens tam artificiose evolvendo, ut nullibi major ingenii capacitas requiratur, quam ad simplicissimam illationem faciendam."

42 "Sed insuper ut quaestio sit perfecta, volumus illam omnino determinari, adeo ut nihil amplius quaeratur, quam id quod deduci potest ex datis [...]." (Descartes AT, X, 431 and LR, 142)

43 Algebra consists in abstracting the terms of difficulty from numbers in order to examine their nature. Cf. *Regula XVI*. (Descartes AT, X, 457 and LR 194)

44 "[...] non statim in difficilioribus et arduis nos occupari oportet, sed levissimas quasque artes et simplicissimas prius esse discutiendas, illasque maxime, in quibus magis ordo regnat, ut sunt artificum

qui telas et tapetia texunt, aut mulierum quae acu pingunt, vel fila intermiscent texturae infinitis modis variatae; item omnes lusus numerorum et quaecumque ad Arithmeticam pertinent, et similia [...]." (Descartes AT, X, 404 and LR, 92)

45 See, in particular, Boyer (1956), Vuillemin (1960) and Bos (1981).

ENRICO PASINI

ARCANUM ARTIS INVENIENDI: LEIBNIZ AND ANALYSIS

"Mathematics is an experimental science. The formulation and testing of hypotheses play in mathematics a part not other than in chemistry, physics, astronomy, or botany" (Wiener 1923, 271).

I Introduction

Leibniz was undoubtedly a many-sided man, and a polymathic mind, if ever there was one. The concept of analysis is notoriously, for its part, a polycephalous monster, and nearly all its meanings are spread through Leibniz's multifarious works, where the philosophical, epistemological, logical, and mathematical receptions of the term seem to be inextricably interwoven. Much the same is true of its counterterm, synthesis, and thus their mutual relation itself presents various aspects.

A thorough survey of these varieties lies far beyond the scope of the present study, and they have already supplied the subject-matter of some very good accounts (in particular Duchesneau 1993, 55-104). Here we shall just try to find some traces of what Goethe would have called a "red thread"—like the one he saw metaphorically twisted throughout the literary cordage of Ottilie's diary in the *Wahlverwandtschaften*. Analysis is introduced by Leibniz in juridical, scientifical, mathematical, or philosophical contexts, under different conditions and with different purposes; but even for such manifold uses should exist some common ground and univocal meaning. The analysis of thoughts and that of truths, the analysis of problems and that of things, all imply slightly or consistently different proceedings, and nevertheless they must perform somehow one and the same operation.

In a very general sense, analysis is for Leibniz, like for anyone else, the resolution of something complex into simpler elements. A procedure of this kind is applied, for instance, to physical objects by natural scientists. As Leibniz writes to des Billettes in 1697, they make use of "a certain analysis of sensible bodies, [protracted only to an extent] useful for the practice of their discipline" (Leibniz A, I, 13, 656). Depending on their object, such practices can in principle proceed in perfectly symmetrical manners, either from individual entities to universal features, or from universal concepts to particular instances. Thus Martial Gueroult distinguished two aspects of analysis with respect to Leibniz, one that "goes from

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 35–46.

the concrete to the abstract; this is the one which tends to ascend indefinitely towards the simple notions"; and another one "which, on the contrary, goes from the abstract to the concrete and, in principle, from the less to the more real" (Gueroult 1946, 251). There are Leibnitian texts on the analysis of physical bodies confirming this interpretation[1], but it is anyway somewhat too vague to be useful outside the immediate terrain of application.

II Truth Conditions

A first preciser specification of analysis, and a distinguishing one as for Leibniz's thought, is its application to truths, that is, as it may also be called, "conceptual" analysis:

> "According to Leibniz, truths of reason in general, and logical truths in particular, are necessary and eternal, true in all possible worlds, provable (*i.e.* reducible to identical propositions) in a finite number of steps, and hence 'analytic' in the strong sense (namely, the conceptual analysis that shows that the concept of the predicate is contained in that of the subject can be actually performed)" (Dascal 1988, 27).

Here a "truth" is the description of a state of fact expressed by one or several propositions in the form "subject-predicate" (substance-state), *i.e.* each proposition specifying a property of a determinated substance at a determinated instant of time—a property as such or a property acting as a non-relational "*requisitum*" to a relational state of things (Mugnai 1992). Leibniz writes in the § 33 of the *Monadology*:

> "When a truth is necessary, its reason can be found by analysis, resolving it into more simple ideas and truths, until we come to those which are primitive." (Leibniz GP, VI, 612)

In every propositional truth, the predicate is someway contained in the subject, connected by conditions that can be shown by analysis—just like mathematical theorems, Leibniz adds notably, "are reduced by analysis to Definitions, Axioms and Postulates" (*ibid.*).

So there must also be a reason, or a chain of reasons, for all truths of fact, that is to say, for contingent truths. They concern the sequences of events that constitute the universe of created beings, in which "the analysis into particular reasons might go on into endless detail" (*ibid.*, 613), because of the immense variety of things in nature and the infinite division of bodies.

> "There is an infinity of present and past forms and motions which join to make up the efficient cause of my present writing; and there is an infinity of minute tendencies and dispositions of my soul, which contribute to make its final cause." (*ibid.*)

And all this minuteness involves infinite other contingent objects and events, "each of which still requires a similar analysis" (*ibid.*). As Leibniz once briefly

condensed his theory of contingency, the root of contingency lies in the infinite (*radix contingentiae est in infinitum*): truths of fact are contingent, because no analysis can exhaust the infinite complexity of their truth conditions.

We are confronted here with the most general sense of the term, in which the concept of analysis is restricted to its fundamental elements. In so far as this is meant, it is true what Rescher maintains: that for Leibniz "'analysis' is a logical process of a very rudimentary sort, based on the inferential procedures of *definitional replacement* and *determination of predicational containment* through explicit use of logical processes of inference" (Rescher 1967, 23). But it's easy to find quite different epistemological conceptions of analysis in Leibniz's writings, in particular when questions concerning the scientific method are dealt with.

III There is Method in't

Leibniz felt a lively interest in the advancement of medical knowledge and of its methods. In a *De scribendis novis medicinae elementis*, written in 1680-82, we find the following remarks on the difference between analysis and synthesis in the study of pathology:

> "The method is truly analytical when, for every function, we investigate its media, or organs, and their modes of operating; thus we acquire knowledge of the body from [the knowledge of] its parts. After having completed this, we'll return to the synthesis, coordinating everything to the one, and we'll describe the prime motor, the instruments of motion (both the liquid and the solid ones), their connections, and the whole economy of the animal." (Pasini 1996, 214)

The synthesis is then drawn from theoretical principles, namely the Galenic distinction of vessels, humours and spirits, out of which Leibniz's favourite definition of the animal body as an "hydraulo-pneumatical-pyrobolical engine" can easily be deduced.

Synthesis is here an *a priori* proceeding, while analysis is a method to acquire empirical knowledge. Both contribute to the investigation of physiology, but analysis seems to act as first, being the chief means to systematically gather information, whereas synthesis represents the correct foundation by which it is possible to gain systematicity for the information collected. This conception, of course, is not in any way peculiar of Leibniz[2].

If we read further in the *De scribendis novis medicinae elementis*, towards the end we encounter again the opposition of analysis and synthesis; this time the matter is not the method of investigation, but the communication of knowledge. Both analysis and synthesis again play a defined role: this is quite relevant, since the idea that analysis pertains mainly to discovery and synthesis to explaining and teaching is at Leibniz's time very close to a commonplace.

"Duplex Methodus tractandi Morbos", he declares, *"una Analytica per symptomata, altera Synthetica per causas"* (*ibid.*, 217). Disease can be considered analytically, based upon symptoms, or synthetically, based upon causes. It is important to teach first the true analysis of illness, writes Leibniz further, namely "the art both to inquire into the signs, and to identify an illness by means of signs" (*ibid.*). Synthesis will be taught only after giving a specimen of analysis, *i.e.* "a general healing method, which is to the pathological synthesis what algebra is to the elements of geometry" (*ibid.*). Here again we see Leibniz draw a parallel with mathematics, and in particular between the method of analysis in general, and algebra—that is, for a mathematician of his time, analysis in the most proper sense.

IV The Anatomy of Wit

Leibniz maintains, more in general, that inventive people who make discoveries and enlarge knowledge usually proceed in two ways: "per Synthesin sive Combinationem et per analysin" (Leibniz VE, 1362), as we read in a *De arte characteristica et inventoria*. Combination, or synthesis, is a conjunction of thoughts, maybe even arbitrary, so devised as to let some new knowledge arise. Analysis requires dwelling upon the proposed subject, and to resolve its concept into other simpler concepts, or to determinate its requisite elements or components.

Leibniz observes that all inventive spirits are either more combinatorics or more analytical in disposition. A combinatorics wit can recall things past and connect them to present needs and experiences. Analytics thoroughly examine present things, but remain so immersed in their object as to limit their power of observation. Combinatorics spirits are superior, because their ability is a rare gift: *"Combinare vero remota promte, non est cujusvis"* (*ibid.*, 1363).

In the second version of a programmatic sketch *De arte combinatoria scribenda*, Leibniz remarks analogously:

> "I must premise a chapter concerning the difference between the analytical and the combinatory method, and the difference between analytical and combinatory wits." (Leibniz VE, 1098)

Analytical wits, according to him, are more short-sighted, so to speak, while combinatorics ones are rather long-sighted (*"Analytici magis Myopes; Combinatorii magis similes presbites"*, *ibid.*, 1099): in fact, in analysis it is suitable to pay attention to fewer things, but with more precision, whereas combinatorics considers many things together, and much more perspectively; thus analysis has more in common with miniature painting, and combinatorics with large-scale sculpture.

Analysis is much easier to apply, since it consists of definable procedures:

> "Once a procedure of analysis is detected, it requires only attention, or firmness of mind [...] and indeed there are such people, whose wit is not vagabond, and who are able to reckon in their imagination, even without paper and pencil." (*ibid.*)

Combinatorics, on the contrary, requires to quickly and promptly browse a manifold of subjects, and to treat them in unexpected ways. Their practical instruments also differ: people with a weaker imagination make use of figures and symbols to better focus questions, while those with a weaker memory and unable to represent many things together, are helped by the use of tables. *"Characteristica vera et tabulis et analysi auxiliatur"* (*ibid.*).

In the art of discovery, that is in the course of knowledge, both analytic and combinatorics spirits, as we read in the *De arte characteristica et inventoria*, will particularly profit of a method. The method is described in general: *"Methodus inveniendi consistit in quodam cogitandi filo id est regula transeundi de cogitatione in cogitationem"* (*ibid.*). Method means something that provides the thinking processes with a leading thread, *i.e.* with a rule regulating the movement from one thought to the other. The rule must consist in a palpable instrument: as the compass rules the hand in correctly tracing a circle, for correct thinking *"instrumentis quibusdam sensibilibus indigemus"* (*ibid.*). These palpable instruments of thought are again tables for the combinatorics and characters—symbols—for the analysis[3].

"Characterem voco quicquid rem aliam cogitanti repraesentat" (*ibid.*)—a character is anythings that represents another thing to a thinking person. If we could keep the things themselves before us, we would have less need for such characters. The representation is based on some relation or rule of correspondence between them: so the ellipse represents a circle by being its projection. Models and figures of things can be considered as characters: they too are crafted so as to express the essence of the thing. Characters do not need to be similar to the objects they represent: numerical symbols express correctly the properties of number, but they do not resemble them.

V Thought Instruments

This conception of the method as an instrument, or a collection of instruments and techniques, rather than a set of precepts, marks one of the most important differences between Leibniz and the greater part of his contemporaries, notably Descartes. For Leibniz a method "is" an instrument, and an instrument, in the method of analysis, is an algorithm based on characters. Hence, on non-mathematical ground too, analysis is in principle a symbolic operation for Leibniz. Moreover, systems of symbolic operations, *i.e.* algorithms, can legitimately be used, both for the comprehension and organization of existing knowledge and for the creation of new knowledge, also outside their traditional grounds.

The construction of general methods for the acquisition, sharing and transmission of knowledge, in the form of complex algorithmic instruments for logical and conceptual calculus, is an idea that dates back to the young Leibniz. Adolescent, he devised an "alphabet of human thoughts": it will grow into one of Leibniz's greatest projects, that of an art of discovery based on a "characteristic" (art of characters or symbols) of general use for combinatorics and analysis at the same time.

An analysis of our thoughts (*analyse de nos pensées*), states Leibniz in 1684, is "of the greatest importance both for judging and for inventing" (Leibniz A, I, 4, 342). This analysis of thought, he specifies elsewhere, "*respondet analysi characterum*", corresponds to a symbolic analysis, in that characters can express our thoughts and their relations, thus providing our reasonings with a "mechanical thread" (Leibniz VE, 811). This idea is explained more clearly in many programmatic essays, one of which received the not particularly original title of *Initia et specimina scientiae novae generalis* ("First steps and examples of a new general science"). Leibniz distinguishes between dialectics, or analysis of opinions, and analysis of truths; the latter, he affirms, is the secret for the development of the art of invention and discovery:

> "I shall also add the vulgar analysis of human judgements, *i.e.* the principles on which human opinions are based, that are dialectic and ought not to be despised. It wouldn't be necessary to bring them into surer principles, only with the purpose to confirm something we already know. But since the whole secret of the art of discovery [*totum arcanum artis inveniendi*], by virtue of which human science could make an immense progress, depends on the analysis of truths (that is the emendation of our thoughts), it is convenient to proceed to the highest levels of analysis." (Leibniz VE, 702)

This art will comprehend a method to perform rigorous demonstrations in any field, "equal or even superior to mathematical ones, which suppose many elements that here could be demonstrated" (*ibid.*). It is a wholly new calculus that, according to Leibniz, is at work, in every human reasoning and is nevertheless as accurate as arithmetical or algebraic calculations are.

The same concepts are repeated ever and again in Leibniz's countless manifestoes for this new discipline:

> "Since when I had the pleasure to considerably improve the art of discovery, or analysis, of the mathematicians, I began to have certain new views, that is, to reduce all human reasoning to a sort of calculus, which would be of use in discovering a truth in so far as it is possible *ex datis, i.e.* from what is given or known." (Leibniz GP, VII, 25)

A universal writing would also result from it, that "would be like a sort of general algebra, and would provide the means to perform reasoning by calculation" (*ibid.*, 26): such a calculus would not only be an instrument for learning and research, but it would be an infallible judge of controversies as well, offering a way to solve disputes by simple reckoning.

Leibniz explains this extended meaning of calculus in a letter he wrote to Tschirnhaus in 1678: "*Nihil enim aliud est Calculus, quam operatio per characteres, quae non solum in quantitate, sed et in omni alia ratiocinatione locum habet*" (Leibniz GM, IV, 462). A calculus is nothing else than an operation performed by means of characters—that is, an algorithm of symbolic analysis—that takes place not only with quantities, but in any kind of reasoning as well.

VI The Place of Analysis

The place of analysis in this more general frame is, as one may expect, quite variable. In a short and schematic note, Leibniz lists the chapters for a work to be entitled *Guilielmi Pacidii Plus Ultra sive Initia et specimina scientiae generalis*. There we find among others the following arrangement of analysis and synthesis, combinatorics and discovery, *mathesis* and art of invention:

> " 10. De arte inveniendi
> 11. De synthesi seu arte combinatoria
> 12. De Analysi.
> 13. De combinatoria speciali, seu scientia formarum, sive qualitatum in genere sive de simili et dissimili
> 14. De Analysi speciali seu scientia quantitatum in genere seu de magno et parvo
> 15. De mathesi generali ex duabus praecedentibus composita." (Leibniz GP, VII, 49-50)

Analysis and combinatoric in general seem to be tied to the art of invention; two more specific versions, that concern quantity and form, are presented as the two branches that compose universal mathesis[4].

Another, more detailed program is rubricated *Initia et specimina scientiae generalis*. It describes at length the structure of a complex work, dedicated to the "*instauratione et augmentis scientiarum*" (Leibniz GP VII, 57). After a first book dedicated to the logical form of arguments and to the ways to determine the eternal truths, the second book should treat *de arte inveniendi*, the "art of discovery, namely that of the tangible thread by which investigation is ruled", and of its divisions, "*ejusque artis speciebus*", namely combinatorics and analytics (*ibid.*).

In the *Fundamenta calculi ratiocinatoris* (1688-1689) Leibniz defines the calculus used in the universal art of characters as follows: "A calculus or operation consists in the exhibition of relations, performed by the transmutation of formulas according to some prescribed rule" (Leibniz VE, 1205); again, it might well be an exemplary definition of the analytical proceedings. And anyway, for Leibniz, any analytical calculation is a formal argument: as we read in a letter to the palatine countess Elisabeth of 1678:

> "un calcul d'analyse est un argument *in forma*, puisqu'il n'y a rien qui y manque, et puisque la forme ou la disposition de tout ce raisonnement est cause de l'evidence." (Leibniz A, II, 1, 437)

When Leibniz defines combinatorics in his *De artis combinatoriae usu in scientia generali* (of 1683-84), he states that "*Combinatoria agit de calculo in universum*", the combinatorics art deals with every aspect of the calculus, "that is to say, with universal marks or characters [...] and their rules, dispositions and processes, or with formulas universally. Of this general calculus, the algebraic calculus is a species, *i.e.* the one based on the laws of multiplication" (Leibniz VE, 1354).

If even combinatorics reveals itself blatantly to be framed just like analysis, on the other hand mathematical analysis is clearly, as Leibniz himself often affirms, a specimen of the *ars characteristica*. In 1691 Leibniz writes to Huygens that:

> "The best and most convenient feature in my new calculus is this: that it exhibit truths by means of a sort of analysis, without any of those efforts of the imagination, that often succeed only by chance, and thus gives us the same advantage over Archimedes that Vieta and Descartes let us gain over Apollonius." (Leibniz GM, II, 104)

The infinitesimal calculus, he means, frees the geometer from the need to concentrate on the geometrical situation of the problem in order to devise a helpful construction, such as the insertion of a suitable ad-hoc linear segment.

Three months later Leibniz hammers again the qualities of his calculus in Huygens' mind, and he supports his argument with an example:

> "I remember that, as I once studied the cycloid, my calculus presented to me the greater part of the discoveries that have been made on the subject, nearly without any need for meditation. Indeed, what I like best in this calculus, is that it gives us the same advantage in the field of Archimedean geometry that Vieta and Descartes have given us in Euclidean and Apollonian geometry, since it exempts us from working with the imagination." (*ibid.*, 123)

In fact, from the study of the function it is possible to exhibit numerous geometrical properties of the curve, by way of analysis: "*Caeteraque omnia circa cycloidem inventa, pluraque alia similiter ex tali calculo analytice derivantur.*" (Leibniz GM, II, 118)

VII Calculus on My Mind

Leibniz often intends by "analysis" a particular analytical method or a set of analytical techniques, developed by other mathematicians, and from some writings of his, one might imagine that "*quot sunt capita, tot sunt analyses*". Leibniz is clearly conscious of the novelty and peculiarity of his mathematical discoveries. He writes in 1692:

> "I have developed a new analysis concerning the infinite; it is quite different from Cavalieri's geometry of indivisibles and from Wallis' arithmetic of infinite series, since it doesn't depend on lines as the former, nor on numerical series, as the latter, but it is general, and thus symbolic or Specious. But instead of the vulgar analytical calculus applying to powers and roots, it performs the calculus of differences and summations." (Leibniz GM, V, 263-264)

"Vulgar" analysis (*i.e.* the algebra of Descartes, his mathematical and philosophical *tête de turque*) is often reprehended by Leibniz, since it doesn't comprehend some of the most fascinating concepts of seventeenth century mathematics (infinitesimals, imaginary numbers), nor some of the most important objects of Leibniz's analytic research (transcendent relations, the theory of determinants).

A very important methodological distinction is drawn in a famous letter addressed to Antonio Magliabecchi. There are two forms of analysis, states Leibniz here; first comes the analysis of Vieta and Descartes, that is considered by the moderns to be the only analysis, and "that solves every problem, studying the relation of the unknown to the known quantities" (Leibniz GM, VII, 312). The other one has its scope in reducing the problem "to a different problem, easier than the first one" (*ibid.*). The latter was known also to the ancients, as it appears, for instance, from the *Data*. In writing to Huygens, Leibniz explains this distinction as that between analysis "*per saltum*" and "*per gradus, cum problema propositum reducimus ad aliud facilius*" (Leibniz GM, II, 116-117). The first one is more absolute, but the second often works better.

In *De methodis synthetica et anagogica applicandis in algebra*, the synthetic method is defined analogously: "*cum problema difficile soluturi incipimus a facilioribus*" (Leibniz VE, 1095). Leibniz also observes that algebra performs a fake synthesis, in treating the unknown quantities as if they were known. The anagogic method is that of pure analysis, "*quae nihil syntheseos habet*" (*ibid.*); and the "*Data veterum*" are of pertinence to the anagogic method, that hence appears to be the heir of the method described to Magliabecchi. Here we proceed backwards, "always reducing the problem to another, easier problem. And this is my method" (*ibid.*), adds Leibniz, used for ordinary equations, but also for the resolution of the ordinates of a curve, viz. in transcendent problems.

Another front is to be opened soon. As Leibniz writes to Mélchisedec Thévenot in 1691:

> "Since I believe that geometry and mechanics have now become fully analytical, I have devised to extend the calculus to other subjects, even to subjects that until now nobody thought would have supported it." (Leibniz A, I, 7, 356)

And he adds, as usual: "Here I mean by 'calculus' every notation representing a reasoning, even without any relationship to numbers" (*ibid.*).

In 1679, four years after the completion of his work on the fundamentals of the infinitesimal calculus, Leibniz writes to Huygens: "Mais apres tous les pro-gres que j'ay faits en ces matieres, je ne suis pas encor content de l'algebre" (Leibniz GM, II, 18-19), after all the work I did with algebra I think we need something different and more powerful in treating with geometrical entities. It is "une autre analyse proprement geometrique ou lineaire, qui nous exprime directement *situm*" (*ibid.*, 19): an analysis specific to loci, *i.e.* an analytic topology. Algebra represents quantities by appropriate symbols and operations: other symbols and operations can calculate forms, angles, orientations, movements, in their qualitative aspects too.

The most important use of this analysis, anyway, is to help in geometrical reasoning: "on trouve ainsi par une espece de calcul", the same words used to describe the advantages of infinitesimal analysis, "tous ce que la geometrie enseigne jusqu'aux elemens d'une maniere analytique et determinée" (*ibid.*, 26). By this calculus it is possible to determine analytically everything that belongs to geometry, up to its most fundamental elements.

As an obvious example of immaterial cognitive technology, this new *analysis situs* is, of course, an art of characters, and an art of invention: "Cette caracteristique", adds Leibniz, will even express in symbols all mechanical structures and will help us to find new geometrical constructions, "à trouver de belles constructions", since it contains at one time both the procedures of calculus and of construction (Leibniz GM II, 30-31).

VIII An Engine for Your Thoughts

"*Quod omnium maxime quaero est Machina, quae pro nobis faciat operationes analyticas, quemadmodum Arithmetica a me reperta facit numericas*" (Leibniz A, VI, 3, 412). What I most desire, Leibniz writes already in 1674, is a machine that performs analytical operations, just as the calculating machine he invented carries out the arithmetic ones. This idea of an analytical engine is hindered, one may say, by the inadequacy of its programming language, since "the universal analysis depends on the development of a universal character" (Leibniz A, VI, 3, 413). Meanwhile, for the use of complex reasonings, it is acceptable to surrogate the required special-purpose characters with generic characters, such as the letters used in geometry[5]. But in general the signs we presently use to compose analytical formulas, adds Leibniz, can't suitably express the mental operations involved in their treatment by means of simple analytical procedures as transpositions or linear transformations. Anyway, it is not an impossible task, since "*omnes cogitationes non sunt nisi simplices complicationes idearum*" (*ibid.*): thoughts derive in the ultimate analysis from simple components, simply combined, as words are composed by simple letters, and the complex apparatus of thoughts needs only to be brought back to such simplicity.

But in reality our thoughts are not so transparent: even if we were able to perform thorough analyses of the concepts we use, we would not *ipso facto* be aware of its results at any moment of our thinking processes: "when a notion is very composite, we can't think of all its ingredients together, as with an intuitive notion" (Leibniz GM, IV, 610). Leibniz discusses such issues in his *Meditationes de cognitione, veritate et ideis*, a short essay published in 1684 and dealing mainly with the classification of ideas into clear, distinct, obscure, adequate etc. We have a distinct notion of something, Leibniz affirms, if our knowledge contains enough marks to discern it from all similar objects. But "in most cases, in particular when a very complex Analysis is required, we can't represent intuitively the whole nature of the object, and we use signs instead" (*ibid.*).

This sort of reasoning, says Leibniz, can be called "blind reasoning, or also symbolic reasoning, as we make use of in Algebra and Arithmetic, and indeed at every moment" (*ibid.*). Symbols, like those of analysis, are the true instruments of thought: in particular, they are for human thought a sort of indispensable blind-flying instruments—under conditions where normal thought is "blind-thought". "*Et huius generis cogitationes*", in Leibniz's words, "*soleo vocare caecas, quibus nihil apud homines frequentius aut necessarium magis*" (Leibniz A, VI, 2, 481). This is the most intimate kernel and the real operational mode of human thought: that it operates mostly by means of symbols, that is to say it operates in the same way as algebraic algorithms, or analytical algorithms do—those of the "literal" or "specious" analysis. That's why this last one is so successful, and useful, and sure in matters so difficult and general as reasoning and problem solving: "*Hinc Symbolica illa recentiorum analysis [...] tanti est ad celeriter et secure ratiocinandum usus*" (*ibid.*).

The *cogitatio caeca* or *symbolica* finally is, according to Leibniz, in itself the best human instrument for problem solving, that is to say for the augmentation of "both knowledge, and happyness" (*ibid.*)—and mathematical analysis mirrors it. Not bad, in the end.

Notes

1 In the *De modo perveniendi as veram corporum analysin* of 1677: "Duplex est resolutio: una corporum in varias qualitates per phenomena seu experimenta, altera, qualitatem sensibilium in causas sive rationes per ratiocinationem" (Leibniz GP, VII, 268). If we combine such analyses with experiments, adds Leibniz, we'll easily determine the causes of any quality found in any physical subject.

2 For instance, a quite conformable statement can be read in Newton's *Optics*: "The Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phænomena proceeding from them" (Newton 1704, 405).

3 It must be observed that the instruments intended for the combinatorics are mostly traditional, static and trite; the instruments for analysis powerfully embody innovation.

4 And in the *Elementa nova matheseos universalis* (written between 1684 and 1687): "*Tradetur et Synthesis et Analysis, sive tam Combinatoria, quam Algebra.*" (Leibniz VE, 987).

5 In this way, if the specific knowledge that enters in a logical calculation is already set up, it will be easier to coordinate this particular specimen of the art to the general frame of the universal characteristic.

CRAIG G. FRASER

THE BACKGROUND TO AND EARLY EMERGENCE OF EULER'S ANALYSIS

I Introduction

In cultivating analysis Euler is sometimes seen as someone whose primary achievement was the development of tendencies in the Leibnizian school. Typical here is Bourbaki's statement (Bourbaki 1974, 246) that he carried "the Leibnizian formalism to an extreme" thereby "completing the work of Leibniz". A somewhat different view is expressed by Boyer (Boyer 1939, 243) who calls attention to Euler's originality: "Most of his predecessors had considered the differential calculus as bound up with geometry, but Euler made the subject a formal theory of functions which had no need to revert to diagrams or geometrical conceptions"[1].

The present paper is devoted to a study of the role of analysis in the background to and early development of Euler's mathematical research. Euler's *Methodus inveniendi lineas curvas* of 1744 (Euler 1744), the first systematic treatise on what would later become known as the calculus of variations, is here identified as the locus classicus for the initial emergence of a fully analytical conception of the calculus. The work contained many of the technical and notational innovations that would be elaborated in his mid-century textbooks on infinitesimal analysis. In addition, in chapter four of the treatise Euler developed the subject in a way that exhibited its analytical character at a deeper theoretical level.

To understand the origins of Euler's programme we first provide a survey of analytical conceptions in earlier mathematics. We then turn to a consideration of the relevant parts of the *Methodus inveniendi*, ending with a discussion of the mathematical and philosophical character of his approach to analysis.

II Analytical Methods in Early Modern Mathematics

It is possible to trace a continuous development in European mathematics that begins in the thirteenth century and leads by 1700 to the extensive employment of symbolic methods. Techniques of analysis came to play an important role in such distinct areas as the theory of determinate equations, arithmetic, coordinate geometry and the calculus. Our survey will focus on the emergence of the concepts

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 47–78.

of equation and variable, and on the question of the degree to which symbolic methods formulated essential mathematical features of the subject under study.

II.1 Analytic Art

The concept of analysis and the name itself became part of early modern mathematics largely as a result of the work of François Viète. His essay of 1591, *In artem analyticen isagoge* (1591*a*), initiated a series of researches by himself and such contemporaries of his as Marino Ghetaldi and Thomas Harriot that together contributed to the widespread employment in the seventeenth century of symbolic mathematical methods.

A substantial historical literature, deriving from the work of Jakob Klein (Klein 1934-1936), emphasizes Viète's modernity as a mathematician. It is suggested that his notion of specious logistic involved a theoretical widening of the concept of magnitude to include both arithmetic and geometric quantity. In adapting ideas from Diophantus's arithmetic to the realm of geometric analysis he was led to generalize Diophantus's concept of species. According to Klein (*ibid.*, 166-167), "the *eidos* concept, the concept of the 'species', ***undergoes a universalizing extension while preserving its tie to the realm of numbers. In the light of this general procedure, the species***, or as Viète also says, the 'forms of things'[...] ***represent 'general' magnitudes simply***"[2].

Associated with this general concept of number, it is suggested, there emerged in his analytic art, with its use of symbols to represent unknowns and parameters, a structural, syntactic approach to mathematics[3]. Because the terms of his system could be given different interpretations in arithmetic and geometry the purely combinatorial properties of operations performed on analytical expressions were exhibited as an object of interest.

Klein's essay and the historical writings it has inspired have resulted in a renewed interest in Viète's algebra and have led to a better appreciation of his role in early modern mathematics. We will however argue in what follows that suggestive and informative as Klein's essay has been, his whole thesis must be qualified at certain fundamental points.

The widening of the concept of magnitude that is attributed to Viète had already taken place and was well assimilated within algebraic practice at least a century before he wrote. Algebra was known as "the art of the thing and the power" or "the great art" or "the greater part of arithmetic". The progress of symbolic methods consisted of the replacement of the largely rhetorical procedures inherited from Islamic mathematicians by ones that used a syncopated or partial formalism in the solution of problems involving the determination of an unknown quantity. Study of quadratic, cubic and quartic equations led to the introduction of expressions denoting the roots of non-square numbers; thus magnitudes traditionally regarded as geometrical entities were denoted as numbers within the confines of what was essentially an arithmetical algebra.

In emphasizing the radical character of the Viètan concept of magnitude, Klein has overlooked the full *mathematical* significance inherent in the assimilation (well established by 1590) of surd numbers into arithmetical algebra.He is to be sure aware of this earlier tradition, writing that "the new number concept [...] already controlled, although not explicitly, the algebraic expressions and investigations of Stifel, Cardano, Tartaglia, etc." (*ibid.*, 178). Nevertheless he concludes of the cossist school that "in its whole mode of operating with numbers and number signs, its self-understanding fails to keep pace with these technical advances. This algebraic school becomes conscious of its own 'scientific' character and of the novelty of its 'number' concept only at the moment of direct contact with the corresponding Greek science, *i.e.*, with the *Arithmetic* of Diophantus" (*ibid.*, 148). To this one may reply in two ways. Self-consciousness on the part of researchers, however significant, is not necessary in order for important conceptual advances to take place; the latter may be, as they were for the cossist algebraists, logical concomitants of technical developments within the subject itself. Second, if indeed an explicit awareness of conceptual advance is present it is necessary to show how this influenced and shaped the direction of mathematical research.

Another difficulty with Klein's thesis is that it understates the extent to which Viète situated his notion of species within a classical Euclidean theory of magnitude. He seems to have regarded the general magnitudes of his specious logistic as geometrical entities. He uses the words "ducere" and "adplicare", terms denoting geometric operations, in his definition of the multiplication and division of magnitudes (writing for example, "magnitudinem in magnitudinem ducere"), and retains dimensional homogeneity as a fundamental principle. His vision of a general theory of quantity applicable to either number or line segments was already realized in *Elements* V, a part of the Euclidean canon that he drew upon in chapter 2 of his *Analytic Art.* (Advocates of the notion of "symbolic magnitude" never explain how book V of the *Elements*—a general theory of magnitude without symbols in the Viètan sense—is possible.)

Certainly Viète showed a stronger interest in mathematical method than had earlier researchers. To attribute to him a radical new syntactic or structural conception of mathematics seems however doubtful. He viewed analysis not as an autonomous subject but as an "art", as a tool in solving problems, be they ones in geometry, the theory of equations or Diophantine arithmetic. The content of mathematics was for him not a system of relations but a set of concrete problems in these subjects. His notational innovations were developed within this historically particular programme of research. His technical vocabulary and fondness for formal categories indicate the continued influence on him of scholastic thought. Incongruous mathematical elements were contained in his attempt to adapt ideas

from Diophantine arithmetic, essentially a work of rational number theory, to the art of algebra as it was employed in the solution of geometrical problems.

Viète's conceptual advances, the introduction of distinct symbols for variables and parameters and the adoption of an operational formalism, represented a significant contribution to mathematical method. They provided an orderly and uniform notation for handling the material on algebraic identities and polynomial equations that had appeared in Cardano's *Ars Magna* (Cardano 1545), and permitted the emergence of "the first consciously articulated theory of equations" (Mahoney 1973, 36). Perhaps most important mathematically, his notational system allowed one to investigate the relationship between the coefficients of a polynomial and the structure of its roots; it must be said however that this last line of investigation developed slowly and only became established in the later eighteenth century.

Of considerable conceptual significance, particularly for the later development of the calculus, was the idea of a function. The notion of a general expression $f(A)$ defined in terms of the variable A was present in embryonic form in Viète's system, where the square of the magnitude denoted by the symbol A was denoted by an expression ("A quadratus") that itself contained A. Instead of the "res" and the "census" of traditional algebra, separate terms denoting distinct entities, one now had a notation that reflected the underlying operations performed on the magnitudes being represented. That the functional idea could only receive a somewhat limited development by Viète was a consequence of the fact that he viewed his symbol "A" not as a variable in the full sense but as an unknown, an object whose value was to be determined in the course of the solution of a problem (Boyer 1956, 60). His definition of an equation, "the coupling of an unknown magnitude with a known" reflected this particular perspective.

II.2 Theory of Numbers

The figures of Euclidean plane geometry are coherent unitary objects whose identity is defined in terms of certain universal attributes, such as being three-sided or being right-angled. Results in geometry become theorems by virtue of the inherent generality of figures as mathematical objects. As commentators from Leibniz to Frege have emphasized, whole numbers—the objects of arithmetic—are different sorts of things, possessing particular individual characteristics[4]. Propositions in Euclidean arithmetic (*Elements* VII, VIII and IX) are formulated in terms of classes of numbers, such as being prime, being perfect, or being a member of a geometric progression. These classes are delineated rhetorically, without the aid of symbolic notation.

It is ironic that Viète turned to Diophantus's *Arithmetic*, a work of rational number theory, as a source of inspiration for developing methods in algebra and geometry, the sciences (for him) of continuous magnitude. An opposite sort of irony characterized Pierre Fermat's extensive researches in theoretical arithmetic[5]. In his study of geometry he adopted Viète's system of notation, using it to formulate mathematically the idea of coordinate geometry. He also studied the *Arithmetic* carefully and greatly extended the results contained there, in the process laying the foundation of modern number theory. Throughout these latter researches he employed a predominately rhetorical mode of presentation. Although he used hindu-arabic numerals and some signs for arithmetic operations, his statement and demonstration of theorems were presented in words without the aid of symbolic notation.

The style of Fermat's writings is illustrated by a comparison with Euclid, whose mode of expression in number theory was also rhetorical. Consider Euclid's assertion (*Elements* IX, 36) that a number of the form $2^{p-1}(2^p-1)$ is perfect if 2^p-1 is prime[6]: "If as many numbers as we please beginning from an unit be set out continuously in double proportion, until the sum of all becomes prime, and if the sum multiplied into the last make some number, the product will be perfect".

Consider now Fermat's original statement of what is known as Fermat's little theorem, the assertion (in modern mathematical language) that p divides $a^{p-1}-1$, where a and p are relatively prime numbers[7]: "Without exception, every prime number measures one of the powers -1 of any progression whatever, and the exponent of the said power is a submultiple of the given prime number -1" (Fermat, TH, V. 1, 209).

In his rhetorical expression as well as in his interest in integral rather than rational solutions Fermat seemed to be looking past Diophantus to the arithmetic books of Euclid's *Elements* as a source of inspiration. In 1657 he explicitly criticized the use of geometrical considerations in arithmetic (presumably because they entailed conceptions of continuous magnitude) and, appealing to Euclid, urged that "arithmetic redeem the doctrine of whole numbers as a patrimony of its own"[8]. Although many problems of rational arithmetic reduced to ones of whole-number arithmetic it was also the case that certain interesting questions in the latter subject became trivial when the class of permissible solutions was extended to rational numbers. It is very possible that his disinclination to use literal notation derived from a desire to emphasize the autonomy of whole-number arithmetic.

There is it must be noted some evidence that Fermat privately employed algebraic methods in his arithmetic researches, and some of his correspondents suspected him of having done so. His contemporary Descartes made use of formulas to express arithmetical results. Nevertheless, in all of his extant writings, in all of the different phases of his research, Fermat did not employ symbolic algebraic notation.

The awkwardness of rhetorical formulations and the need for more and more detailed statements of results eventually imposed restrictions on the sort of theory

that could be developed. Fermat's decision not to give a fuller account of his researches may have derived in part from the demands that such a mode of exposition entailed. The concept of an arithmetic variable—an entity that could assume any of a given set of whole-number values—was central to the progress of number theory as it was to develop after him. It enabled one to reify in formulas expressions and relations that could then be studied or manipulated at will in the course of the investigation.

It should nevertheless be remembered that at the most fundamental level it was numbers and their properties, and not any system of relations embodying these properties, which constituted the fundamental subject of the theory of numbers. The role of the variable was not an essential one; each symbolic statement could always be re-expressed in terms of a proposition about classes of numbers.

II.3 Coordinate Geometry[9]

Euclid and Apollonius had derived results about curves that express relations of equality between magnitudes associated with these figures, relations that are valid for an arbitrary point taken on the perimeter of the curve. In *Elements* III, 36 one is given a point *D* outside of a circle and asked to draw from it two lines; the first *DB* is tangent to the circle and the second *DCA* cuts the circle at the points *C* and *A* (fig. 1). Euclid showed that the square on *DB* is equal to the rectangle on *DC* and *DA*. In book I of the *Conics* Apollonius introduced the ellipse as the section obtained by intersecting a plane with an oblique circular cone (fig. 2). Such a cone is formed by the lines joining the perimeter of a circle to a point not in the plane of the circle. Let *PP'* be a given axis through the centre of the ellipse and let *Q* be a point on the perimeter of the ellipse. Consider the line *VQ* of intersection of the plane of the ellipse and the plane of that circle through *V* which is parallel to the base; *Q* is the point where the line meets the ellipse. The line *VQ* is called an "ordinate". In I, 15 Apollonius showed that the rectangle on *PV* and *VP'* is in a given constant ratio to the square on *VQ*.

In these propositions the curve is introduced and the given relation is then exhibited as a property satisfied by it. The relation represents one of several properties and is not regarded as defining or definitively expressing the curve. The primary purpose of the results is found in the solution of other problems. In *Elements* IV Euclid used III, 36 in his construction of the regular pentagon. In *Conics* III Apollonius employed the theory of the earlier books in his investigation of the problem of the locus to three and four lines.

This last problem is of great historical significance for the later development of coordinate and projective geometry and possesses in its own right certain points of conceptual interest. Consider four lines given in position in the plane. It is necessary to determine the locus of points *P* such that the rectangle formed by the distances from *P* measured in given directions to the first two lines is in a specified ratio to the rectangle formed by the distances measured in given directions to the other two lines. (In the case of three lines one of the rectangles becomes a square.) It turns out that the locus is in every instance a conic section. In the same book Apollonius provided a detailed discussion of the problem, developing results that would (at least in principle) form the basis for a complete solution[10].

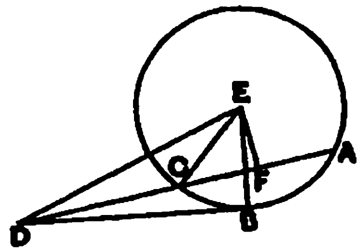


Figure 1

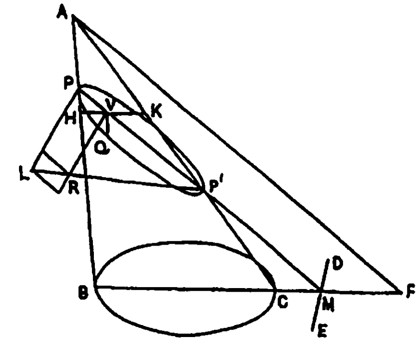


Figure 2

In book VII of his *Collection* Pappus called attention to the three and four line problem and discussed the work of earlier geometers[11]. He also raised the question of the nature of the locus when the number of lines exceeds four. The distances that appear in this problem are magnitudes that are assumed to vary while the relation expressed by the locus condition itself continues to hold. (This relation was expressed in two forms by Pappus, in terms of the ratio of figures or solids, and for the more general case in terms of compound proportions.) What logically distinguishes these magnitudes within the problem is that they vary, and that the locus is produced in consequence of their variation. The concept of a variable would therefore seem to be implicitly present in Pappus's formulation.

The *Collection* became available in Western Europe in 1588 in Commandino's Latin translation (Commandino 1588). When Descartes began to study the locus problem in 1632 he did so having already had some grounding in Viètan algebra and the theory of equations. His *Géométrie* (1637) may be seen as a fairly natural development arising from the application of algebraic methods to a problem of current interest. His approach to the investigation of the locus was very simple. Let *AB* be one of the lines that are given in position, *C* be a point on the locus and *CB* the line segment that is to be drawn from *C* to *AB*. Descartes took *AB* and *CB* as his given reference lines and let $x=AB$ and $y=CB$ (fig. 3). (Notice that the problem is especially suited to coordinate methods, because the line segment *CB* from *C* to *AB* is always drawn at the same angle to *AB*.) He calculated

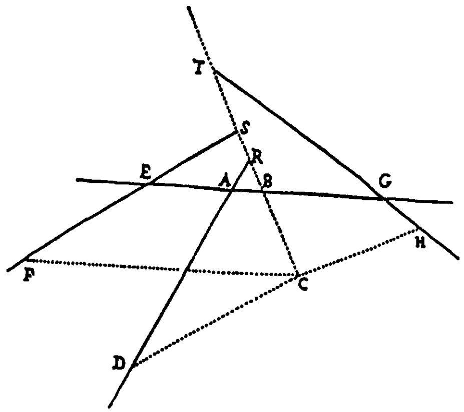


Figure 3

the various distances of the problem in terms of x and y and proceeded to express the locus condition as an indeterminate equation in these variables.

In the original locus problem there were as many variable magnitudes as there were lines given in position. In Descartes' geometry by contrast the problem was reduced to the consideration of two variables connected by means of an equation. His theory opened up the possibility—at least in principle—that continuous variation could be studied by examining how one variable changes with respect to the other within such a relation.

The last question however was one that Descartes never pursued. His investigation remained firmly centred on the classical problem of constructing solutions to geometrical problems. His interest in equations was based primarily on the role they played in such solutions. Within this programme it was necessary to determine points on a curve by means of acceptable instruments of construction (Bos 1981).The curve enjoyed a dual status, as something that was a solution to a geometrical problem and as something that could itself be used as a tool in the construction of a solution. The study of indeterminate equations yielded information about the associated curves, while determinate equations could be solved to obtain particular points on the curve.

Fermat's writings from the same period demonstrate a better appreciation of the general methodological character of coordinate geometry. In his *Ad locos et solidos isagoge* of 1637 (TH, I, 4, 91-110) he enunciated the principle that to any equation in two variables there corresponds a curve in the plane, one given by means of the graphical method of his coordinate system[12]. He was however primarily interested in geometrical loci problems, in which the final equation is always

an algebraic or polynomial relation. His continued interest in restoring Greek mathematical works indicated the strong classical character of his investigation.

Throughout the early history of coordinate geometry there seems to have been little interest in the mathematical investigation by means of graphical techniques of arbitrary relations among magnitudes, abstractly considered. The familiar modern use of graphs to represent the behaviour of virtually any two related quantities that are found anywhere was notably absent during the period.

II.4 THE CALCULUS

II.4.1 *EQUATIONS*

While established research in coordinate geometry remained centred on geometrical construction a whole new line of investigation was opened up with the growing interest in quadrature and tangent problems. Early work on what later became the calculus was connected with the programme of study set forth in Van Schooten's Latin edition of Descartes's *Géométrie* (Descartes, 1659-1661). Out of these developments came a new part of mathematics, one that soon achieved considerable prominence as an area of research[13].The relevant history has been well documented in the literature. Our discussion will be confined to two examples which illustrate some of the conceptual and technical issues associated with the role of the equation in the early calculus.

The first example involves a comparison of Wallis's *Arithmetical infinitorum* (1656) and Newton's researches on infinite series from the 1660s. Wallis was a proponent of the new analysis and employed symbolic notation freely in his book. His primary goal was to investigate quadratures and cubatures by means of arithmetic methods involving infinite numerical series. In Proposition XIX he considered the series

$$\frac{0+1=1}{1+1=2}=\frac{1}{2}=\frac{1}{3}+\frac{1}{6}, \qquad \frac{0+1+4=5}{4+4+4=12}=\frac{1}{3}+\frac{1}{12}$$
$$\frac{0+1+4+9=14}{9+9+9+9=34}=\frac{7}{18}=\frac{1}{3}+\frac{1}{18}, etc.$$

It is clear that when the number of terms become infinite the value of the series is 1/3. (Wallis wrote down the general formula for the numerator as $\frac{l+1}{3}l^2+\frac{l+1}{6l}l^2$.

He showed how this result may be used to obtain the ratio of the area under a parabola to the circumscribed rectangle, and the ratio of the volume of a cone to

the circumscribed cylinder. He proceeded in the treatise to extend the result, and through the skilful and extensive use of interpolation went very far in obtaining numerical series expressions for various quadratures[14].

In the winter of 1664-1665 Newton began to study the *Arithmetica infinitorum*, research which he carried out at the same time he was reading Van Schooten's edition of the *Géométrie*. He recorded his progress in notebooks which have survived[15]. His fundamental innovation was to reformulate Wallis' investigation in terms of equations between Cartesian coordinate variables. By setting the problem in this way he made relations between continuously changing magnitudes the central object of study. An equation implies the existence of a relation that remains valid as the variables change continuously in value. It is this fundamental fact—the continuous and permanent character of the relation, its persistence differentially in the neighbourhood of each real number—that was exploited by Newton in expressing the connection between the equation of the curve and the formula for its quadrature. This fact would also be the basis for his subsequent investigation, set forth in the 1669 paper *De analysi*, relating the quadrature of a curve to its equation by means of differentiation[16].

Although Wallis was an advocate of the new analysis he did not make essential use of relations among variable magnitudes in his investigation. His approach was not "analytical" in the deeper sense discernible in Newton's early work on infinite series and quadratures.

Our second example concerns some later work of Newton and the French mathematician Pierre Varignon. The motion of a freely moving particle acted upon by a central force was the subject of book one of Newton's *Principia mathematica* (1687) as well as of a memoir by Varignon published by the Paris Academy in 1703 (Varignon 1701). Both men established that motion in an ellipse with the force centre at one focus implies an inverse-square force law. In a break with his early mathematical work of the 1660s Newton abandoned Cartesian analytical methods, turning instead to a kind of infinitesimal-geometrical theory of limits. Varignon by contrast used techniques of the recently established Leibnizian calculus in his solution.

In Proposition VI and its corollaries Newton had derived a measure for the force in terms of geometrical quantities associated with the curve. In the next few propositions he calculated the force law when the trajectory was assumed to have a given form. In Proposition XI he considered the case of the ellipse. In fig. 4 the point P is the position of the particle on the ellipse at a given instant, C is the centre of ellipse, S is one of the foci and the centre of the force, and CA and CB are the semi-major and semi-minor axes. Through P draw the tangent RP. The line DCK is drawn through C parallel to the tangent intersecting the ellipse at the points D and K. The lines CP and CD are then conjugate axes of the ellipse corresponding to the point P. Let E be the intersection of the SP and DC. Draw the

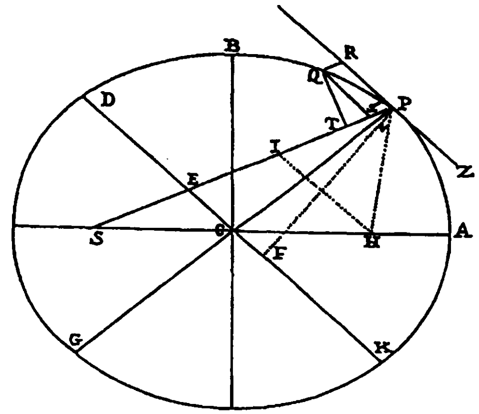


Figure 4

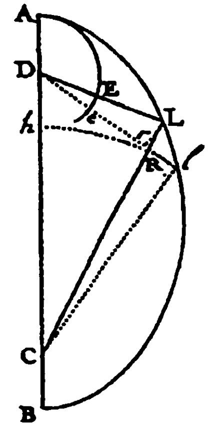


Figure 5

perpendicular PF from P to Dk. Let Q be a point on the ellipse near P. Draw the line Qv parallel to the tangent intersecting the conjugate diameter PCG at v. In the course of his derivation Newton made use of the following two equations:

$$Gv \cdot Pv : Qv^2 = PC^2 : CD^2$$
$$CA : PF = CD : CB$$

These he presented as known properties of the ellipse; of the second relation he noted that it had been "demonstrated by the writers on the conic sections."(Note that the first of these relations is the one from Apollonius's *Conics* I, discussed earlier.) He also proved that the quantity EP is a constant equal to the semi-major axis CA. Using this fact and the above relations he was able to show that the force is inversely proportional to the distance SP.

Varignon began by expressing the trajectory relative to a coordinate system in which the variables are the distance r from the force centre and the quantity z, where dz is defined as the projection of the element of path-length ds on the perpendicular to the radius. The tangential component of the force is equated to the expression dds/ddt, where s is the path length and t is the time. The derivation of the inverse-square law for the case of the ellipse is a model of simplicity. Consider the ellipse with major axis AB, foci D and C and force centre at C (fig. 5). Set $AB=a$, $DC=c$ and $b^2=a^2-c^2$. Let L be a point on the ellipse, $CL=r$. If l is a point close to L and the perpendicular lR is drawn to CL then the differential $dz=Rl$. Varignon gave the equation of the ellipse in the form[17]

$$bdr = dz\sqrt{4ar - 4rr - bb}$$

Using the relation $ds^2 = dr^2 + dz^2$ and the area law $rdz = dt$ he reexpressed this equation in the form

$$\frac{4a - 4r}{r} = \frac{bbds^2}{dt^2}$$

Differentiation of this equation with respect to t led to the expression $\frac{2a}{b^2r^2}$ for the force, which yielded the desired result.

Both Newton and Varignon employed equations that express relations between continuously varying magnitudes and in this sense both of their derivations may be said to be analytical. There were however important differences of approach. In Newton's solution the ellipse with its various properties acts as a synthetic geometrical object, controlling the form of the derivation. In Varignon's memoir by contrast the ellipse is specified by a single equation between two variables relative to a fixed coordinate system. The entire mathematical content of the problem is reduced to the study of this equation; all of the properties of the ellipse needed for the solution are contained in it. The solution therefore evolves through a mechanical application of the differential algorithm.

II.4.2 *Graphical Techniques*

The curve was an object of considerable mathematical and physical interest throughout the seventeenth and eighteenth centuries. A few examples from the period 1680-1740 illustrate this point. The study of the relations that subsist between the lengths of curves gave rise to a theory of elliptic integrals. In work in the calculus of variations classes of curves constituted the primary object of study. In analytical dynamics attention was concentrated on determining the relation between trajectories and force laws. In the theory of elasticity researchers studied the shape of static equilibrium assumed by an elastic lamina under various loadings, as well as the configurations of a vibrating string.

The curve also played a fundamental and very different role in the conceptual foundation of the calculus. The situation is illustrated by work in problems of maxima and minima, an important part of the subject. In the very first published paper in the calculus Leibniz (1684) used his differential algorithm to derive the optical law of refraction from the principle that light follows the path of least time. He considered the points E and C on opposite sides of a line SS separating two optical media (fig. 6). It is necessary to find the point F on SS such that a ray of light travelling the path EFC does so in the least time. The time of transit from

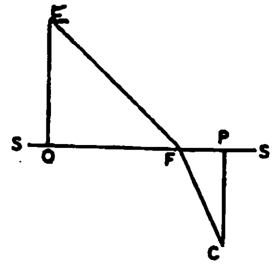


Figure 6

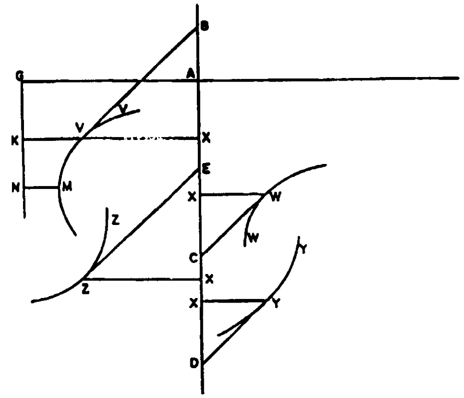


Figure 7

E to F is equal to the product of the distance EF and a constant equal to the reciprocal of the velocity in the first medium; this product Leibniz regarded as a rectangle of sides EF and a given constant line r. The time from F to C was likewise regarded as a rectangle of sides FC and a line h. The total time of transit along EFC is therefore equal to the sum of these rectangles. Leibniz (*ibid.* 1684) wrote: "Let us assume that all such possible sums of rectangles, or all possible paths, are represented by the ordinates KV of curve VV perpendicular to the line GK" (fig. 7)[18]. Letting $x=QF=GK$ be the abscissa and $w=KV$ be the ordinate he had in fig. 7 a curve VVM representing the time of transit as a function of the distance x from Q to F. He calculated this time as an expression in x and applied the differential theory he had previously introduced for curves to obtain the path given by the known law of refraction.

In this problem the primary object of interest is the relation between two magnitudes, the distance QF and the time of transit that corresponds to this distance. Although there is nothing in the nature of this relation that logically entails a geometric interpretation Leibniz nevertheless chose to represent it graphically by means of a curve. He could then apply his differential algorithm which had been introduced earlier for the analysis of curves.

Graphical procedures had been employed by Galileo in his *Discorsi* (1638) to relate the speed of a falling body to the time of its descent. They had become common in mathematical treatises by the late seventeenth century. Barrow in his *Lectiones geometricae* (1670) represented quadrature relationships in this way. In his *Principia mathematica* (1687) Newton investigated the inverse problem of central-force particle motion. In Propositions XXXIX and XLI of book one he graphed the force as a function of the projection of position on the orbital axis and

analyzed the resulting curve to arrive at expressions for the particle's trajectory. Jakob Bernoulli employed graphical methods throughout his researches of the 1690s. In his study of the elastica the relation between the restoring force and the distance along the lamina was superimposed in graphical form on the diagram of the actual physical system.

The first textbook on the differential calculus, l'Hôpital's *Analyse des infiniment petits* (1696), was a systematic attempt to ground the calculus in a theory of curves. The way in which this was done by him and other researchers of the period has been documented in the historical literature (Bos 1974). Of particular interest for the present discussion is his treatment of problems of maxima and minima. These problems were explicitly formulated as ones of finding the maximum or minimum ordinate of a curve. The equation of condition $dy = 0$ or $dy = \infty$ was deduced by considering successive values of dy and noting that about a maximum or minimum ordinate these values must change in sign. In several examples, each of which gave rise to a relation between two variables, he used graphical techniques to refer the problem of finding an extremum to the consideration of an associated curve.

In the ninth example l'Hôpital introduced a curve *AEB* (fig. 8) given in position and two fixed points *C* and *F*. Consider a variable point *P* on the curve and let $CP=u$ and $PF=z$. Consider a quantity (what would later be called a function) composed in some definite way from the variables u and z. It is necessary to find the point *P* so that this quantity is a maximum or a minimum. To solve this problem l'Hôpital joined the points *C* and *F* to form a base axis *CF*. The ordinates *QM* and *OD* give the values of the quantity corresponding to the points *P* and *E*. In contrast to the primary curve *AEB* the curve *MD* joining *M* and *D* is a purely

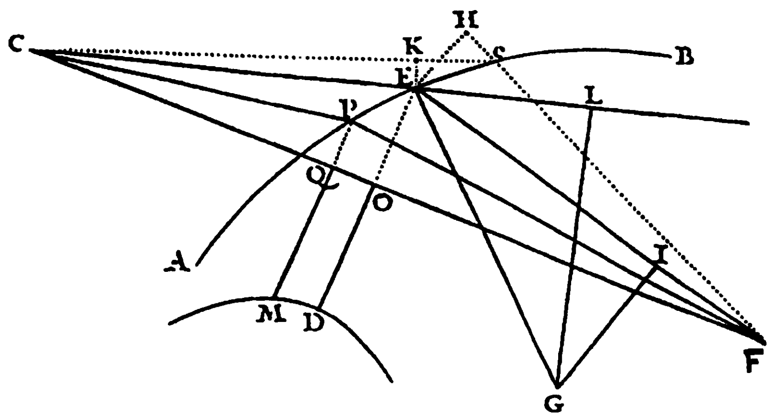


Figure 8

logical construction expressing the quantity as a function of position along *CF*. L'Hôpital observed that at *P* "the ordinate *QM* which becomes *OD* must be the greatest or least of all companion ordinates." He derived using the differential algorithm a solution in the particular case where the quantity is equal to $au+z^2$ (a constant), obtaining $adu+2zdz=0$ or $du : dz = 2z : a$ as the differential equation which defines *P*.

The grounding of basic calculus procedures in terms of the properties of the curve, and the common practice of representing relations between magnitudes graphically by means of curves, led to a tendency to see the early calculus as something that was essentially geometrical. The term "fine geometry" employed at the time conveys the contemporary understanding. At the most fundamental level the geometrical character of the early calculus conditioned how the subject was understood, allowing it to be experienced intellectually as an interpreted, meaningful body of mathematics.

II.4.3 *Coordinate Systems*

It is clear that graphical methods played a role in the early calculus that would later be filled by the function concept. An example of this is Varignon's 1706 memoir "Nouvelle formation des spirales" (1704). The paper is devoted to the investigation of curves given in terms of polar variables. Although Cartesian geometry was originally developed for oblique and orthogonal coordinates there had been an early interest in other reference systems. Study of Archimedes's *On spirals* led in the seventeenth century to the invention of transformations that correlated areas expressed in terms of polar quantities to ones defined in terms of Cartesian coordinates. In the writings of Cavalieri, Roberval, James Gregory, Barrow, Newton and Jakob Bernoulli there was an interest in applying calculus-related procedures to curves expressed in polar quantities. In Varignon's own earlier work in orbital dynamics (as we saw in § II.4.1) he considered expressions for the force that were functions of the distance from the particle to a given centre; it was therefore natural that polar quantities were employed to analyze the resulting motion.

In his 1706 memoir Varignon considered a fixed reference circle *ABYA* with centre *C* (fig. 9). A "courbe génératrice" *HHV* is given; a point *H* on this curve is specified by the perpendicular ordinate *GH*, where *G* is a point on the axis *xCX* of the circle. The line *CX* is conceived as a ruler that rotates with centre *C* in a clockwise direction tracing out a spiral *OEZAEK*. Consider a point *E* on the spiral. With centre *C* draw the arc *EG*. Let c = the circumference of the reference circle *ABYA*, x = arc *AMB*, $CA=a$, $CE=y$, $GH=z$ and $AD=b$ a constant line. The arc x is defined by the proportion $c{:}x=b{:}z$. Varignon wrote what he called the "équation générale de spirals à l'infini" as $cz=bx$. By substituting the value for z

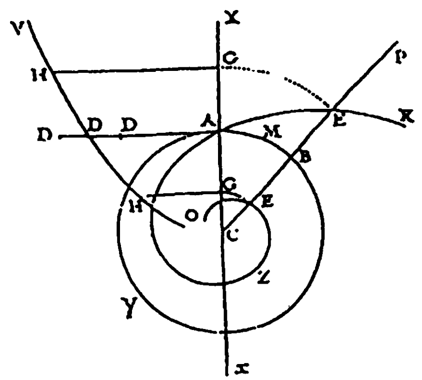


Figure 9

given by the nature of the generating curve into this equation the character of the spiral was revealed. Depending on whether the generating curve was a parabola, hyperbola, logarithm, circle, etc., the corresponding spiral was called parabolic, hyperbolic, logarithmic, circular etc.

That one could introduce curves in a polar reference system by considering arbitrary relations between the radius and the pole angle was presented by Varignon as a substantial advance. Earlier mathematical researches had concerned such special cases as the parabolic spiral. In Varignon's dynamical investigations the trajectory was something that was logically given as part of the physical problem. In the present paper by contrast the "equation" of the spiral is formulated *a priori* in terms of Cartesian coordinates in the associated "generating curve". The latter embodies in graphical form the functional relationship between the polar variables and acts as a standard model to which this relationship may be referred.

A prominent subject of Varignon's paper, the rectification of polar curves, is of interest from the viewpoint of the conceptual foundations of analysis. Newton and Jakob Bernoulli had independently studied the path-lengths of pairs of associated curves, one member given in Cartesian and the other in polar coordinates[19]. The Cartesian formula for the differential element of path length is $ds^2=dx^2+dy^2$, where x is the ordinate and y the abscissa; the polar expression of the same quantity is $ds^2=dx^2+x^2d\theta^2$, where x is now the radius and θ is the polar angle. If the element of length is assumed to be the same along both curves (so that there respective lengths for a given value of x are equal) we are led to the differential equation $dy=xd\theta$ relating the respective coordinate variables. It was clear for example that the integral $\int_a^b \sqrt{1+x^2}\,dx$ gives both the length along the parabola $y=\frac{1}{2}x^2$ as well

as the length along the Archimedean spiral $x=\theta$. The rectification of the spiral, a mechanical curve, was reduced to that of the simpler and better known conic section, a result of considerable interest to mathematicians of the period. Varignon's study of rectification consisted in large part in the extension and further development of this result.

The common use of non-Cartesian coordinates in the early calculus was in the computation of geometric quantities associated with the curve. Thus polar coordinates were employed in certain problems because they provided a suitable measure of the radius of curvature of a curve. The geometrical object was given and the coordinate description was varied for the purposes of investigation. Varignon's paper pointed in the opposite direction. Contained in his study, if only implicitly, was the realization that the same formula could receive distinct geometric interpretations, depending on the meaning assigned to the coordinate variables of the problem. The interpretation of the formula $\int_a^b \sqrt{1+x^2}\,dx$ in the preceding paragraph will differ depending on whether x is regarded as an orthogonal or a polar variable. This conclusion suggested more generally the possible existence of a stable analytical core for the calculus. The work of Euler that we shall we consider in the next section was based in large part on the elevation of this insight to an explicit and systematic programme of research in infinitesimal analysis.

III Euler's Analysis

III.1 By the early eighteenth century symbolic methods were common in Continental mathematics. In the infinitesimal calculus especially there were strong analytical elements in the researches of the Bernoullis, Varignon, Taylor (English, but an important influence on the Continent), Hermann, Fagnano, Riccati, and others, elements that were combined however with pervasive geometric modes of representation.

Euler became established as a mathematician of note during the decade of the 1730s. He was a young man in his twenties, a member of the St. Petersburg Academy of Sciences and a colleague of Hermann, Daniel Bernoulli and Goldbach. His interest in analysis is evident in writings from this period, including his major treatise on particle dynamics, *Mechanica sive motus scientia analytice exposita* (1736). Although the theme of analysis was well established at the time there was in his work something new, the beginning of an explicit awareness of the distinction between analytical and geometrical methods and an emphasis on the desirability of the former in proving theorems of the calculus.

The direction of Euler's research in the later 1730s and early 1740s may be followed in his work in the calculus of variations, leading up to the publication in

1744 of his *Methodus inveniendi*. His investigation began from earlier results of Jakob Bernoulli, Brook Taylor and Johann Bernoulli. Jakob and Taylor's researches were linked by an appreciation at the level of technical approach for the analytical solution of isoperimetric problems. By contrast, Johann's major memoir of 1719, an extended exposition of his brother's ideas, emphasized a more geometric approach to the same subject. Although Euler had been Johann's student in Basel his own conception of variational calculus seems to have evolved under the influence of Jakob and Taylor (Fraser 1994).

III.2 The *Methodus inveniendi* contained many of the advances that would be systematically developed by Euler in his later treatises: the function concept; the notion of a trigonometric function and the associated notation; and a uniform procedure for introducing higher-order differentials. At a deeper level the work expressed an appreciation for the mathematical possibilities of a more abstract approach to analysis.

A typical problem of the early calculus involved the determination of a magnitude associated in a specified way with a curve. To find the tangent to a curve at a point it was necessary to determine the length of the subtangent there; to find the maximum or minimum of a curve one needed to calculate the value of the abscissa that corresponded to an infinite subtangent; to find the area under a curve it was necessary to calculate an integral; to determine the curvature at a point one had to calculate the radius of curvature.

The calculus of variations extended this paradigm to classes of curves. In the fundamental problem of the *Methodus inveniendi* it is required to select that curve from among a class of curves which makes a given magnitude expressing some property a maximum or minimum. More precisely, Euler considered curves that are represented analytically by means of relations between x and y in terms of an orthogonal coordinate system (fig. 10). The magnitude that is to be maximized or minimized is expressed as a definite integral

$$W = \int Z dx \qquad (\text{from } x = a \text{ to } x = b), \tag{1}$$

a formula that quantifies in analytical terms the given extremal property. Z is regarded by Euler as a "function" of x, y and the differential coefficients (*i.e.*, derivatives) $p, q, r, \ldots$ of y with respect to x. The latter are given by the relations $dy=pdx$, $dp=qdx$, $dq=rdx$, ..., a procedure for introducing higher-order derivatives that was Euler's own invention[20].

Near the beginning of his treatise Euler (Euler 1744, 13) noted that a purely analytical interpretation of the theory is possible. Instead of seeking the curve which renders W an extremum one seeks that "equation" between x and y which

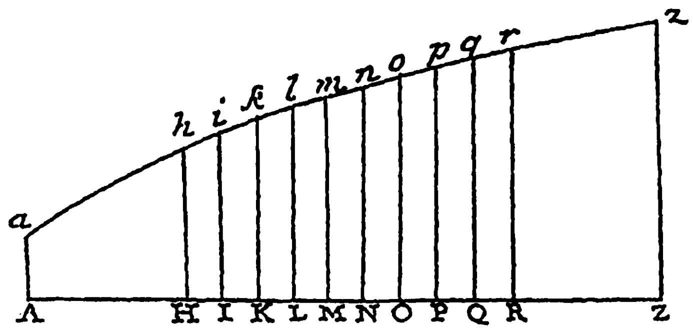


Figure 10

among all such equations when introduced into (1) renders the quantity W a maximum or minimum. He wrote:

> "Corollary 8. In this way questions in the doctrine of curved lines may be referred back to pure analysis. Conversely, if questions of this type in pure analysis be proposed, they may be referred to and solved by means of the doctrine of curved lines.
>
> *Scholium* 2. Although questions of this kind may be reduced to pure analysis, nevertheless it is useful to consider them as part of the doctrine of curved lines. For though indeed we may abstract from curved lines and consider absolute quantities alone, so these questions at once become abstruse and inelegant and appear to us less useful and worthwhile. For indeed methods of resolving these sorts of questions, if they are formulated in terms of abstract quantities alone, are very abstruse and troublesome, just as they become wonderfully practical and are made simple to the understanding by the inspection of figures and the linear representation of quantities.So although questions of this kind may be applied equally to abstract and concrete quantities it is most convenient to formulate and solve them by means of curved lines. Thus if a formula composed of x and y is given, and that equation between x and y is sought such that, the expression for y in terms of x given by the equation being substituted, there is a maximum or minimum; then we can always transform this question to the determination of the curved line, whose abscissa is x and ordinate is y, for which the formula W is a maximum or minimum, if the abscissa x is assumed to have a given magnitude."[21] (Euler 1744, 14)

Euler's view seems to have been that while it is possible in principle to approach the calculus of variations purely analytically it is more effective in practice to refer problems to the study of curves. This conclusion could hardly have seemed surprising. Each of the various examples and problems which historically made up the subject had as its explicit goal the determination of a curve; the selection of such objects was part of the defining character of this part of mathematics. What is perhaps noteworthy about Euler's discussion is that he should have considered the possibility at all of a purely analytical treatment.

III.3 The main body of variational results, presented in chapters two and three, is formulated throughout in terms of the properties of curves. Euler's approach is indicated by his derivation of the fundamental necessary condition known in the modern subject as the Euler (or Lagrange-Euler) differential equation. He developed his derivation with reference to fig. 11, in which the line *amnoz* is the hypothetical extremalizing curve. The letters M, N, O designate points of the x-axis AZ infinitely close together. The letters m, n, o designate corresponding points on the curve given by the ordinates Mm, Nn, Oo. Let $AM=x$, $AN=x'$, $AO=x''$ and $Mm=y$, $Nn=y'$, $Oo=y'$. The differential coefficient p is defined by the relation $dy=pdx$; hence $p=dy/dx$. We have the following relations

$$p=\frac{y'-y}{dx} \qquad p'=\frac{y''-y'}{dx} \tag{2}$$

Suppose now that we are given a determinate "function" Z containing x, y and $p=dy/dx$.The integral (1) was regarded by Euler as an infinite sum of the form $\ldots+Z, dx+Zdx+Z'dx+\ldots$, where Z, is the value of Z at x-dx, Z its value at x and Z' its value at $x+dx$, and where the summation begins at $x=a$ and ends at $x=b$. Let us increase the ordinate y' by the infinitesimal "particle" $n\nu$, obtaining in this way a comparison curve *amvoz*. Consider the value of (1) along this curve. By hypothesis the difference between this value and the value of (1) along the actual curve will be zero. The only part of (1) that is affected by varying y' is $Zdx+Z'dx=(Z+Z')dx$. Euler wrote:

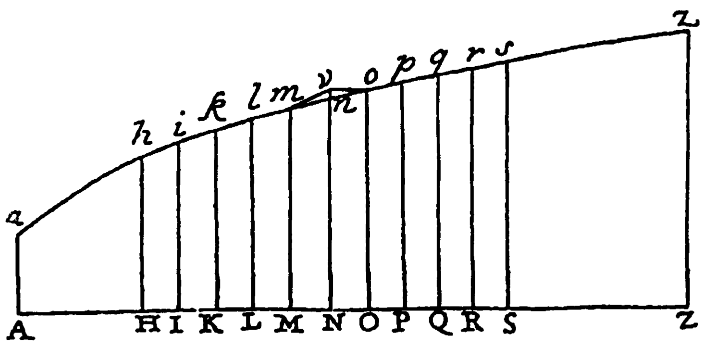


Figure 11

$$\begin{aligned} dZ &= Mdx+Ndy+Pdp \\ dZ' &= M'dx+N'dy'+P'dp' \end{aligned} \tag{3}$$

He proceeded to interpret the differentials in (3) as the infinitesimal changes in Z, Z', x, y, y', p, p' that result when y' is increased by nn. From (2) we see that dp and dp' equal nn/dx and $-nn/dx$. (These changes are presented in the form of a table, with the variables in the left column and their corresponding increments in the right column.) Hence (3) becomes

$$\begin{aligned} dZ &= P\cdot\frac{n\nu}{dx} \\ dZ' &= N'\cdot n\nu - P'\cdot\frac{n\nu}{dx} \end{aligned} \tag{4}$$

Thus the total change in $\int_a^b Zdx$ equals $(dZ+dZ')dx = n\nu\cdot(P+N'dx-P')$. This expression must be equated to zero. Euler set $P'-P=dP$ and replaced N' by N. He therefore obtained $0=Ndx-dP$ or

$$N-\frac{dP}{dx}=0 \tag{5}$$

as the final equation of the problem.

Equation (5) is the simplest instance of the Euler differential equation, yielding a necessary condition that must be satisfied by the extremalizing arc. In modern notation it is written $\frac{\partial f}{\partial y}-\frac{d}{dx}\left(\frac{\partial f}{\partial y'}\right)=0$. Its derivation by Euler was a major theoretical achievement, representing the synthesis in one equational form of the many special cases and examples that had appeared in the work of earlier researchers.

The remainder of chapter two consists of the presentation of a large number of examples as well as the extension of the variational theory to the case where higher-order derivatives of y with respect to x appear in the integrand Z of (1). In chapter three, mathematically the most advanced of the treatise, Euler considered problems where variables that satisfy certain auxiliary relations are introduced into the integrand Z of the variational integral (1). This investigation, which was motivated by examples involving the constrained gravitational motion of particles

in resisting media, led once again to an analytical solution in the form of differential equations.

III.4 The basic variational problem of maximizing or minimizing (1) involves the selection of a curve from among a class of curves. In the derivation of (5) the variables x and y are regarded as the orthogonal Cartesian coordinates of a curve. Each of the steps in this derivation involves reference to the geometrical diagram in Figure 11. In chapter four, however, Euler returned to the point of view that he had indicated at the beginning of the treatise. In the opening proposition the variational problem is formulated as one of determining that "equation" connecting two variables x and y for which a magnitude of the form (1) (given for the general case where higher-order derivatives and auxiliary quantities are contained in Z) is a maximum or minimum. In his solution he noted that such variables can always be regarded as orthogonal coordinates and so determine a curve. The solution then follows from the theory developed in the preceding chapters. In the first corollary he wrote:

> "Thus the method presented earlier may be applied widely to the determination of equations between the coordinates of a curve which render any given expression δZdx a maximum or a minimum. Indeed it may be extended to any two variables, whether they involve an arbitrary curve, or are considered purely in analytical abstraction."[22] (Euler 1744, 129)

Euler illustrated this claim by solving several examples using variables other than the usual rectangular Cartesian coordinates. In the first example he employed polar coordinates to find the curve of shortest length between two points. We are given (fig. 12) the points A and M and a centre C; it is necessary to find the shortest curve AM joining A and M. Let x be the pole angle ACM and y the radius

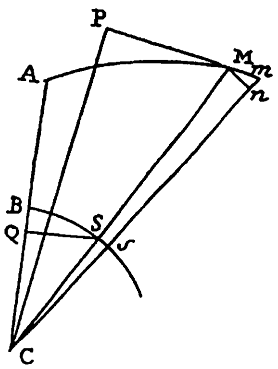


Figure 12

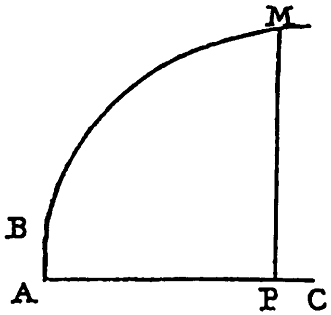


Figure 13

CM. Because the differential element of path-length is equal to $\sqrt{dy^2 + y^2dx^2}$ the formula for the total path-length is $\int dx\sqrt{yy+pp}$, where $pdx=dy$ and the integral is taken from $x=0$ to $x=\angle ACM$. Here x does not appear in the integrand Z of the variational integral, so that $dZ=Ndy+Pdp$. The equation (5) gives $N=dP/dx$ so that we have $dZ=dPp+Pdp$ and a first integral is $Z+C=Pp$, where C is a constant. Since $ZZ = \sqrt{yy+pp}$ we have

$$C+\sqrt{(yy+pp)} = \frac{pp}{\sqrt{(yy+pp)}} \quad i.\ e.: \frac{yy}{\sqrt{(yy+pp)}} = Const. = b$$

Let PM be the tangent to the curve at M and CP the perpendicular from C to this tangent. By comparing similar triangles in fig. 12 we see that $Mm{:}Mn=MC{:}CP$. Since $Mm = dx\sqrt{y^2p^2}$, $Mn=ydx$ and $MC=y$ it follows that $CP = \frac{y^2}{\sqrt{y^2+p^2}}$. Hence CP is a constant. Euler concluded from this property that the given curve AM is a straight line.

Note that Euler was completely comfortable with polar coordinates; gone is the Cartesian "generating curve" Varignon had employed in his investigation of 1706 in order to introduce general curves using polar quantities. In the second example he displayed a further level of abstraction in his choice of variables. Here we are given the axis AC with the points A and P, the perpendicular line PM and a curve ABM joining A and M (fig. 13). Given that the area $ABMP$ is some given constant value we must find that curve ABM which is of the shortest length. Euler set the abscissa $AP=t$, the ordinate $PM=y$ and let x equal the area under the curve from A to P. We have $dx=ydt$ and the variational integral becomes $\int\sqrt{\frac{dy^2+dx^2}{yy}}$.

Because x does not appear in the integrand we obtain as before the first integral $Z=C+pP$. Substituting the expressions for Z and P into this integral we obtain

$$\frac{\sqrt{(1+yypp)}}{y} = C + \frac{ypp}{\sqrt{(1+yypp)}}$$

Letting $dx=ydt$ we obtain after some further reductions the final equation $t = c \pm \sqrt{(bb - yy)}$. Hence the desired curve is the arc of a circle with its centre on the axis AP.

A range of non-Cartesian coordinate systems had been employed in earlier mathematics but never with the same theoretical import as in Euler's variational analysis. Here one had a fully developed mathematical process, centred on the consideration of a given analytically-expressed magnitude, in which a general equational form was seen to be valid independent of the geometric interpretation conferred upon the variables of the problem. Thus it is not at all essential in the reasoning employed in the derivation of (5) that the line *AZ* be perpendicular to *Mm* (fig. 11); indeed it is clear that the variable x need not be a length nor even a coordinate variable in the usual sense. As Euler observed in the first corollary, the variables of the problem are abstract quantities, and fig. 11 is simply a convenient geometrical visualization of an underlying analytical process[23].

Euler's statement at the beginning of the treatise that it was possible to consider the subject as one of "pure analysis" seemed somewhat speculative. By showing in chapter four how the basic variational problem and its solution could be interpreted abstractly he had supplied this view with a considerable degree of mathematical credibility.

III.5 Refinement

Although Euler in 1744 clearly recognized the essential analytical character of the variational calculus his insight was not fully developed in his treatise. Its title "Method of finding curves..." indicated that the primary object of study continued to be the curve. In his later variational writings, in part in response to Lagrange's research, he developed and refined further the conception outlined in chapter four. More generally there was an increasing emphasis on analysis throughout his mathematical work. Conceptually, the most significant change was the explicit replacement of the geometric curve by the analytical relation (conceived as a functional equation between two variables) as the fundamental concept of the variational theory; instead of selecting a curve from among a class of curves it was now required to select a relation from among a class of relations.

The function concept played a dual role in Euler's emerging programme. The functional equation $y=f(x)$ enabled one to conceive analytically of arbitrary relations between the variables x and y. In addition, the notion of an expression composed of variables and constants (denoted for example by Z in the formulation of (1)) allowed the formal statement of general propositions and made it possible to express the content of the subject in purely analytical terms.

A relation between variables is regarded by Euler as a primitive of the theory; it is not further conceptualized, as it would be in later real-variable calculus, in terms of the numerical structure of the continuum of values assumed by each variable. This notion of a primitive abstract relation in large part defined the distinctive character of his approach to analysis. The point in question is illustrated by his demonstration of theorems of the calculus. We will consider one example in detail. At the same time he was composing the *Methodus inveniendi* he published a memoir (Euler 1734-1735) containing an analytical proof of the theorem on the equality of mixed partial differentials. He was motivated in doing so by a belief that a geometrical demonstration would be "drawn from an alien source". He considered a quantity z that is a function of the variables x and a. If dx and da are the differentials of x and a, let $e, f,$ and g denote the values of z at $(x+dx, a)$, $(x, a+da)$ and $(x+dx, a+da)$. Euler differentiated z holding a constant to obtain

$$Pdx = e-z \tag{6}$$

Here P denotes the differential coefficient, in later mathematics the partial derivative of z with respect to x. He differentiated Pdx holding x constant

$$Bdxda = g-f-e+z \tag{7}$$

He then differentiated z holding a constant to obtain

$$Qda = f-z \tag{8}$$

Finally he differentiated Qda holding x constant:

$$Cdadx = g-e-f+z \tag{9}$$

By rearrangement of terms the right sides of (7) and (9) are seen to be equal. Equating the left sides Euler obtained

$$B = C \tag{10}$$

which is the desired result.

In later real analysis this argument would be reformulated using the law of the mean and a limit argument. Suppose $z=z(x,a)$ and its first and second partial derivatives are defined and continuous on a rectangular region in the x–a plane. For x and a in this region we have by the law of the mean for small h and k the four equations

$$\frac{\partial z}{\partial x}(x+\varepsilon_1 h,a)h = z(x+h,a) - z(x,a) \qquad 0 \le \varepsilon_1 \le 1 \tag{6'}$$

$$\frac{\partial^2 z}{\partial a dx}(x+\varepsilon_1 h, a+\varepsilon_2 k)hk = z(x+h,a+k) - z(x,a+k) - z(x+h,a) + z(x,a) \tag{7'}$$

$$0 \le \varepsilon_1 \le 1,\ 0 \le \varepsilon_2 \le 1$$

$$\frac{\partial z}{\partial a}(x,a+\eta_1 k)k = z(x,a+k) - z(x,a) \qquad 0 \le \eta_1 \le 1 \tag{8'}$$

$$\frac{\partial^2 z}{\partial x \partial a}(x+\eta_1 h, a+\eta_2 k)kh = z(x+h,a+k) - z(x,a+k) + z(x,a) \tag{9'}$$

$$0 \le \eta_1 \le 1,\ 0 \le \eta_2 \le 1$$

By rearrangement the right sides of (7') and (9') are equal. The left sides may therefore be equated:

$$\frac{\partial^2 z}{\partial a \partial x}(x+\varepsilon_1 h, a+\varepsilon_2 k) = \frac{\partial^2 z}{\partial x \partial a}(x+\eta_1 h, a+\eta_2 k)$$

Letting h and k tend to zero we obtain from the continuity of the second partial derivatives the desired result

$$\frac{\partial^2 z}{\partial a \partial x} = \frac{\partial^2 z}{\partial x \partial a} \tag{10'}$$

This example is rather typical of eighteenth-century calculus theorems and their counterparts in modern analysis[24]. The law of the mean introduces a distinguished value, localizing at a particular number the analytical relation or property in question. The result is then deduced using conditions of continuity and differentiability by means of a limit argument. In Euler's formulation by contrast there was no consideration of distinguished or individual values as such. Euler believed that the essential element in the demonstration was its generality, guaranteed by a formal analytical or algebraic identity. Thus the key step in his proof, the equality of the right sides of equations (7) and (9), was an algebraic identity that ensured the validity of the result.

IV Discussion

Euler perceived that the calculus is concerned ultimately with equations expressing relations of continuous change between variable magnitudes. His thesis concerning the primacy of pure analysis derived from a logical appreciation that geometrical methods and reasonings are extrinsic to the subject. In formulating this view he established the general framework within which analysis would be understood by subsequent researchers of the period, most notably Lagrange.

The distinctive character of Euler's doctrine is apparent when one considers it at a general epistemological level. There is a certain formal quality to his analysis; it arises ultimately from his conception of the subject as the study of primitive abstract relations. In this respect his viewpoint was very different from that of the early pioneers, who conceived of the foundation of the calculus in terms of geometric conceptions, or that of the nineteenth-century researchers, for whom the numerical continuum provided a fundamental structure of interpretation.

The notion of a primitive abstract relation among variables allowed for a direct and general approach to the subject, evident in Euler's derivation of (5) and (10) above. This generality was however of a particular sort, accompanied by a certain inflexibility of outlook. This became apparent during his debate with d'Alembert in the 1750s over the question of the general solution of the wave equation. Faced with some of the restrictions imposed by the precepts of his own theory (and insisted upon by d'Alembert) Euler advocated a rejection of the concept of a functional equation as a strict relation of equality between analytical expressions. As is well known his defence of this viewpoint reduced to *ad hoc* arguments and "visionary" presentiments of a more general mathematics, presented in a few papers; his systematic treatises of the 1750s remained firmly grounded in the established conception of analysis (Lützen 1983) and (Fraser 1989).

It should be emphasized that the rejection of geometric conceptions by Euler and other eighteenth-century researchers was not accompanied by the realization that the calculus could be developed in full logical isolation as part of pure analysis. In Euler's writings the relationship between foundation, theoretical development and problem generation is not worked out. The entire project of the *Methodus inveniendi* consisted of the derivation of differential equations for general problems, each of which embodied characteristics found in a given set of examples from geometry or mechanics. In his subsequent research the separation of analysis from geometry was made more explicit at a theoretical level. His variational investigations however remained centred on the derivation of general differential equational forms. He provided no account of how the problems in question might originate or be generated within this or any other branch of pure analysis.

He sometimes wrote as if problems are things that are external to analysis that guarantee its meaning and validity. In a memoir published in 1758 he investigat-

ed singular solutions to ordinary differential equations, that is, solutions which are not included in the general integral containing arbitrary constants. He took a differential equation and exhibited a particular function $y=f(x)$ that satisfied the equation but was not in the general solution. He wrote: "Concerning the example that I have just set forth, as it is drawn from fantasy, one could doubt whether this case is ever encountered in a real problem. But the same examples that I adduced in order to clarify the first paradox, will serve also to clarify this one" (Euler 1756; OO, ser. 1, XXII, 231)[25]. (The examples in question concerned curves in the plane that satisfied certain tangent conditions.)

The point here is connected to a larger difference of outlook between eighteenth-century and modern mathematics. That the problems of geometry and mechanics should conform to treatment by pure analysis was something that Euler implicitly accepted as a point of philosophical principle. The term "philosophy" (or "metaphysics") is here being used in the sense identified by Daston:

> "The presuppositions (often unexamined) that inform a scientist's work, which may be of either epistemological or ontological import [...] metaphysics is what is left over once the mathematical and empirical content have been subtracted [...]." (Daston 1991, 522)

In the writings of such post-positivist intellectual historians as E. A. Burtt the term 'metaphysics' in this sense referred to very broad assumptions, such as a general Platonic belief among early modern thinkers in the mathematical character of physical reality[26]. We suggest that it is also useful at a more concrete level in explaining certain tacit but definite attitudes displayed by Euler in his research in geometry and analysis.

Demidov writing of the failure of Euler and d'Alembert to understand each other's point of view in the discussion of the wave equation observes:

> "A cause no less important of this incomprehension rests, in our opinion, on the understanding of the notion of a solution of a mathematical problem. For d'Alembert as for Euler the notion of such a solution does not depend on the way in which it is defined [...] rather the solution represents a certain reality endowed with properties that are independent of the method of defining the solution. To reveal these properties diverse methods are acceptable, including the physical reasonings employed by d'Alembert and Euler." (Demidov 1982, 37)

A biographer of d'Alembert (Grimsley 1963, 248) has noted his insistence on "the elementary truth that the scientist must always accept the essential 'giveness' of the situation in which he finds himself." The sense of logical freedom that is inherent in modern mathematics was notably absent in the eighteenth century.

University of Toronto
Institute for the History and Philosophy
of Science and Technology
Victoria College

Notes

1 In his history of analytic geometry Boyer (1956, 190) observes that for Euler "analysis was not the application of algebra to geometry; it was a subject in its own right—the study of variables and functions—and graphs were but visual aids in this connection [...] it now dealt with continuous variability based on the function concept [...] only with Euler did it [this meaning of analysis] take on the status of conscious program."

2 Emphasis in the original.

3 This view is most clearly presented by Mahoney (1973, 36 and 39):

> "In the *Introduction to the Analytic Art*, as in the whole of the *Analytic Art* itself, algebra was transformed from a sophisticated sort of arithmetical problem-solving into the art of mathematical reasoning itself, insofar as that reasoning was based on combinatory operations [...] the analytic art rose to a position subsuming all combinatory mathematics, whether arithmetic, geometry, or trigonometry".
>
> "The elevation of algebra from a subdiscipline of arithmetic to the art of analysis deprived it of its content at the same time that it extended its applicability. Viète's *specious logistic*, the system of symbolic expressions set forth in the *Introduction*, is, to use modern terms, a language of uninterpreted symbols. As a formal language, specious logistic can itself generate problems of syntax alone."

4 In his *Die Grundlagen der Arithmetik* (1884, §10) Frege rejected the use of induction (as it was understood in the physical sciences) as a valid principle of arithmetic. He wrote:

> "For here there is none of that uniformity, which in other fields can give the method a high degree of reliability. Leibniz recognized this already: for to his Philathèthe, who had asserted that 'the several modes of number are not capable of any other difference but more or less; which is why they are simple modes, like those of space' ".

He returns the answer:

> "That can be said of time and of the straight line, but certainly not for the figures and still less of the numbers, which are not merely different in magnitude, but also dissimilar. An even number can be divided into two equal parts, an odd number cannot; three or six are triangular numbers, four and nine are squares, eight is a cube, and so on. And this is even more case with the numbers than with the figures; for two unequal figures can be perfectly similar to each other, but never two numbers."

Later in this section Frege continues:

> "In ordinary induction we often make good use of the proposition that every position in space and every moment in time is as good in itself as every other. Our results must hold good for any other place and any other time, provided only that the conditions are the same. But in the case of the numbers this does not apply, since they are not in space or time. Position in the number series is not a matter of indifference like position in space."

5 Our account of Fermat's number theory is based on Ore (1948), Hoffmann (1960-1962) and especially Mahoney (1973, Chapter VI).

6 I quote from Heath translation Euclid (EH).

7 "Tout nombre premier mesure infailliblement une des puissances −1 de quelque progression que ce soit, et l'exposant de la dite puissance est sous-multiple du nombre premier donné −1 [...]."

8 Quoted in translation in Mahoney (1973, 329).

9 We use the term "coordinate geometry" to designate the subject known since around 1800 as "analytic geometry". The first work to contain the latter term in its title was J.B. Biot's *Essai de géométrie analytique* (1803). Loria (1923, 142-143) identifies analytic geometry with the "method of coordinates" and states that it "has as its goal the investigation, with the aid of coordinates, of all figures that are conceivable in the plane or in space." The employment of coordinate methods to investigate the elementary plane and solid geometry of Euclid, the use of transformations to study conic sections and higher-order polynomial curves, more broadly the study by means of coordinate methods of any class of geometric curves, all lie within the province of analytic geometry.

10 Coolidge (1945, 20-21) writes:

> "This dreary problem, whose algebraic solution gives a conic immediately, seems to have haunted the Greek mind. We noted at the beginning of the present chapter Apollonius' statement that others had unsuccessfully tried to solve it. But Apollonius himself does not appear able to carry it through. Certain modern mathematicians have put not a little time and strength into the attempt to complete such proofs by what we might call strictly Greek methods".

He mentions Zeuthen (1886, 126-63) and Heath for his edition of Apollonius (Apollonius CH, cxxxviii-cl).

11 Pappus's discussion is in Pappus (CI, part I). On pp. 587-591 of part two Jones (following Zeuthen (1886)) provides an account of how a synthesis of the four-line locus might have been achieved by earlier Greek mathematicians, especially Aristaeus.

12 Mahoney (1973, ch. 3) provides an account of Fermat's researches in coordinate geometry.

13 With the invention and increasing development of the calculus analytic geometry weakened as an area of research. Boyer (1956, 153-154) writes:

> "In general, l'Hospital (like Descartes) was more interested in analytic geometry as a means of expressing loci algebraically than as a method of deriving the properties of a curve from its equation. This latter aspect he seems to have felt belonged more properly to work in the calculus."

In reference to the eighteenth century he (1956, 193) observes "there was a natural tendency for material on curves to be merged with that on the calculus, and hence analytic geometry sometimes lost its identity."

14 Scott (1938, ch. 4) gives a good account of Wallis' treatise.

15 Westfall (1980, ch. 4) provides an account of Newton's early mathematical researches. Newton's papers from this period are published in Newton (MP, I.).

16 Both Westfall and Whiteside comment on this difference of approach, although neither identify the fundamental character of Newton's innovation as consisting precisely in his decision to use equations between Cartesian variables. Whiteside (1960-1962, 245) writes:

> "The advance Newton has made on Wallis' inductive approach to integrals—taking the upper bound of the integral variable—is that, in allowing a free variable (and its powers) into the pattern, he has been able to use the ordering of coefficients given by powers of the variable to point a more general aspect of the pattern lost in Wallis tabulated numerical instances."

Westfall (1980, 114-115) writes:

> "[...] Newton realized that Wallis's method was more flexible than Wallis himself had realized. It is not necessary always to compare the area under a curve with the area of the same fixed square. In the case of the simple power functions ($y = x, x^2, x^3, \ldots$), for example, any value of x provides a base line that can be divided into an infinite number of segments, and with the corresponding value of y it implicitly defines a rectangle with which the area under the curve can be compared."

17 Varignon does not give the derivation of this equation. It may be obtained from the polar equation

$$\frac{b^2}{2ar} = 1 + \frac{c}{a}\cos\theta \qquad (\theta = \angle\, BCL)$$

by differentiating with respect to θ, eliminating $\sin\theta$ and setting $dz = rd\theta$. Since notation for the trigonometric functions has not yet been invented, Varignon would have worked from an equation of the form

$$\frac{b^2}{2ar} = 1 - \frac{c}{a}\frac{(CM)}{r}$$

where CM is the projection of CL on the axis AB.

18 "Ponamus omnia ista rectangulorum aggregata possibilia, vel omnes viarum possibilium difficultates, repraesentari per ipsas KV, curvae VV odinatas ad rectam GK normales [...]." English translation from Struik (1969, 278).

19 Cf. Jakob Bernoulli (1691), Newton (MF, 176-178) and Newton (MP, III, 312-313) (for the draft from the early 1670s). The seventeenth-century history of this problem is described by Whiteside Newton (MP, III, 308-311) who writes (*ibid.*, 308):

> "The development of this length-preserving transformation in the three decades preceding 1670 is a fascinating case-history in human insight and preconception which has never been systematically explored in the monograph needed to do it full justice."

20 In his treatise on the differential calculus Euler provided a detailed account of this procedure for introducing higher-order differential coefficients. A discussion of this subject is provided by Bos (1974).

21 "Corollarium 8: Hoc ergo pacto quaestiones ad doctrinam linearum curvarum pertinentes ad Analysin puram revocari possunt. Atque vicissim, si huis generis quaestio in Analysi pura sit proposita, ea ad doctrinam de lineis curvis poterit referri ac resolvi".

Scholion 2: "Quanquam huius generis quaestiones ad puram Analysin reduci possunt, tamen expedit eas cum doctrina linearum curvarum coniungere. Quodsi enim animum a lineis curvis abducere atque ad solas quantitates absolutas firmare velimus, quaestiones primum ipsae admodum fierent abstrusae et inelegantes ususque earum ac dignitas minus conspiceretur. Deinde etiam methodus resolvendi huismodi quaestiones, si in solis quantitatibus abstractis proponeretur, nimium foret abstrusa et molesta; cum tamen eadem, per inspectionem figurarum et quantitatum repraesentationem linearem, mirifice adiuvetur atque intellectu facilis reddatur. Hanc ob causam, etsi huius generis quaestiones cum ad quantitates abstractas tum concretas applicari possunt, tamen eas ad lineas curvas commodissime traducemus et resolvemus. Scilicet, quoties aequation eiusmodi inter x et y quaeritur, ut formula quaedam proposita et composta ex x ety y, si ex illa aequatione quaesita valor ipsius y subrogetur et ipsi x determinatus valor tribuatur, maxima fiat vel minima, tum semper quaestionem transferemus ad inventionem lineae curvae, cuius abscissa sit x et applicata y, pro qua illa formula W fiat maxima vel minima, si abscissa x datae magnitudinis capiatur."

22 "Methodus ergo ante tradita multo latius patet, quam ad aequationes inter coordinatas curvarum inveniendas, ut quaepiam expressio $\int Zdx$ fiat maximum mimimumve. Extenditur scilicet ad binas quascunque variabiles, sive eas ad curvam aliquam pertineant quomodocunque, sive in sola analytica abstractione versentur."

[23] Carathéodory (1952, xxii) offers a different account of this part of the *Methodus inveniendi*; he writes:

> "[...] die Beispiele, die im ersten Teil desselben Kapitels (Nr. 1 bis 14) behandelt werden, können als Probme für die Kovarianz der Eulerschen Gleichungen bei beliebigen Koordinaten-Transformationen bewertet werden. Somit finden wir im Eulerschen Buche die ersten Ansätzen zu einer Theorie, die erst in unseren Tagen systematisch entwickelt worden ist."

In his index (*ibid.*, lix) of Euler's variational calculus he places these examples under the heading "Kovariante transformation von variationsproblemen." Goldstine (1980, 84) also observes:

> "It is remarkable that as early as 1744 Euler was already concerned with the problem of the invariance of his fundamental equation or necessary condition. In the first part of his Chapter IV he indicates that this fundamental condition remains invariant under 'general' transformations of the coordinate axes [...] he considers a number of examples where x, y are not related by being cartesian, rectangular coordinates, and shows the utility of his ideas on covariance [...]. It is truly in keeping with Euler's genius that he should have worked at ideas that were only to be satisfactorily and completely discussed in modern times."

In our view one should not speak of transformations, invariance or covariance in reference to Chapter Four. Although coordinate transformations had appeared in a memoir published by Hermann (1729) and were employed by Euler in his *Introductio* (1748, II, ch. II; for further references cf. Boyer 1956, ch. 7) they appear nowhere in the *Methodus inveniendi*. Euler does not have to show anything when he writes down the fundamental equation (5) in polar coordinates; its validity is a logical consequence of the generality of the variables in the original derivation. It is unnecessary to invoke concepts of modern differential geometry in order to reach a full appreciation of his theory.

[24] Other examples are the fundamental theorem of the calculus, the theorem on the change of variables in multiple integrals and the fundamental lemma of the calculus of variations.

[25] "Pour l'example que je viens d'alléguer ici, comme il est formé à fantaisie, on pourrait aussi douter, si ce cas se recontre jamais dans la solution d'un problème réel. Mais les mêmes exemples, que j'ai rapportés pour éclaircir le premier paradoxe, serviront aussi à éclaircir celui-ci."

[26] Daston is identifying the sense in which the term metaphysics is used by Burtt and others. She is somewhat critical of this usage because it does not take into account the various actual historical systems of metaphysics which prevailed in the early modern period. To the extent however that the term serves to designate certain extra-scientific or extra-mathematical attitudes in past research it remains a useful concept of historical analysis.

EDITH DUDLEY SYLLA

JACOB BERNOULLI ON ANALYSIS, SYNTHESIS, AND THE LAW OF LARGE NUMBERS

I Introduction

Jacob Bernoulli was the earliest mathematician to prove a law of large numbers. Following in the directions opened by Christiaan Huygens's *On calculations in games of chance* (1657), he knew how expectations could be calculated for games in which the possible outcomes result from the design of game pieces such as dice or cards. He was interested, however, in developing an "art of conjecturing" that would apply mathematics to make prudent decisions in civil, moral, and economic matters. By his proof of the law of large numbers, he believed he had shown that observed relative frequencies could be reliably used in such calculations. Bernoulli's law of large numbers showed that if, for example, one has a die with a one-sixth chance of falling with any given side up, then as the die is repeatedly thrown, it becomes more and more probable that the observed relative frequency of that side being up will fall within some small interval around one-sixth. In the proof of this law, Bernoulli assumed that there are *a priori* equally likely possible cases in a given ratio and demonstrated that, if so, then the observed relative frequencies will tend to converge toward the *a priori* ratio of cases over a large number of trials. He also implied, however, that the truth of this proposition meant that it would be possible to find, within narrow limits, otherwise unknown ratios of cases *a posteriori*, from the outcomes of frequently repeated trials:

> "[...] another way is open to us by which we may obtain what is sought. What cannot be ascertained *a priori* may at least be found out *a posteriori*, that is from the results many times observed in similar situations, since it should be presumed that something can happen or not happen in the future in as many cases as it was observed to happen or not to happen in the past in a similar state of things."[1] (Bernoulli 1713, 224)

Although Jacob Bernoulli was a pioneer in the development of the mathematical theory of probability, his *The Art of Conjecturing* had less immediate influence than it might have had because he left it unfinished at his death. While large parts of the work were completed in the 1680s, well before Bernoulli's death in 1705, the book was not published until 1713, by which time Pierre Rémond de Montmort, Abraham De Moivre, and Nicholas Bernoulli were all active in the

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 79–101.

field of mathematical probability and in direct communication with each other, so that they tended to be more influenced by each other than by Jacob Bernoulli's work directly[2]. Because of this publication history, it may be difficult to discern Jacob Bernoulli's personal understanding of the foundations of mathematical probability and hence difficult to understand what he intended to accomplish through his proof of the law of large numbers. Ian Hacking, in particular, has raised problems about the correct understanding of Bernoulli's intended interpretation of the law of large numbers (Hacking 1975, ch. 17, 154-165)[3]. These problems are compounded by the fact that Bernoulli's work breaks off immediately after his proof. In one sense it does not matter what Bernoulli intended, since the proof of the theorem holds mathematically no matter how Bernoulli himself understood it. Nevertheless, we may more easily place Jacob Bernoulli within in the history of probability theory if his own interpretation of his work is understood. If I seem to belabor my criticism of Hacking's discussion of Bernoulli's work, it is because it has been influential in shaping subsequent research concerning the early history of probability theory.

Why, then, did Bernoulli believe that his proof of the law of large numbers implied that, if one makes a sufficient number of observations, it is possible to discover the ratio of cases, within narrow limits, *a posteriori* in a trustworthy way? Why did he believe that his proof was such a significant achievement, more significant than if he had discovered a way to square the circle—a discovery which, even if it would have been great, would have been of little use?[4] Is there evidence elsewhere in his work in general and in *The Art of Conjecturing* in particular that would help to answer this question?

In this paper I attempt to discern Jacob Bernoulli's understanding of the significance and use of his law of large numbers by first examining what Bernoulli had to say on mathematical methodology, and in particular on the uses of mathematical analysis and synthesis. For Bernoulli, a mathematical synthesis moves from what is prior and better known to what is posterior, but a mathematical analysis lacks this sense of direction. When Bernoulli contrasts an analytic method to a synthetic one, by an analytic method he almost always means an algebraic one. The central lemmas of Bernoulli's proof of the law of large numbers are algebraic and so analytic in his sense.

After examining what Bernoulli had to say about analysis and synthesis and how he went about proving the law of large numbers, I then describe how the law of large numbers and its proof fit into Bernoulli's more general world view. Jacob Bernoulli developed his art of conjecturing or doctrine of chances with the understanding that God has designed the universe to follow natural laws or regularities and that we only use ideas of chance where we lack knowledge of the underlying causes—not that these underlying causes do not in fact exist. To God everything is known and certain. In Bernoulli's view, the law of large numbers shows that over

the long run the underlying regularities of nature will manifest themselves. Finally, Bernoulli's particular use of algebra and of the properties of binomial expansions to prove the lemmas that form the core of his demonstration of the law of large numbers fit with this "God's eye" view of the universe, in which everything is immediate and there is no scope for ordering into what is mathematically prior or posterior. Thus Jacob Bernoulli's ideas about God and the world combine with his reliance on algebra in proving the law of large numbers to explain what has seemed so problematic to critics like Hacking about Bernoulli's intended interpretation of his law of large numbers: why he "assumed" the existence of a ratio of cases in his proof of the law of large numbers and nevertheless believed that the proof justified the use of observed frequencies to discover such ratios to a close approximation. Thus an understanding of Bernoulli's ideas of analysis and synthesis helps to clear up modern philosophical perplexities about his intended interpretation of the law of large numbers.

II Jacob Bernoulli on Analysis and Synthesis

Part I of *The Art of Conjecturing* is a reprinting with notes of Christiaan Huygens's *On Calculations in Games of Chance*. In it Huygens, and Bernoulli following him, frequently derive expectations in games of chance iteratively, by building up from the simplest cases (for instance to find players' relative expectations when one more round will determine the winner) to more complex cases (for instance to find the players' relative expectations when the game is broken off considerably before the end). In games in which each player's chances depend on those of other previous players and *vice versa*, however, Huygens and Bernoulli sometimes use simultaneous equations to determine the expectations. About this resort to algebra, Bernoulli says in his note on the first problem of Huygens's Appendix:

> "Now since all these chances are different and unknown and since any preceding chance depends on the following chance and the following chance in turn on the preceding [...] it follows that this Problem cannot be solved, at least by the Author's method [...] otherwise than by means of algebraic analysis."[5] (1713, 50)

But Bernoulli seems to think a synthetic approach is preferable. Thus, earlier, in his note on Huygens's Proposition XIV, Bernoulli writes:

> "The Author in this Problem is compelled for the first time to employ algebraic analysis, while in the preceding only synthesis was used. The difference between these two is that in all the former propositions the expectation sought was derived from other expectations that were either totally known and given, or, indeed, not known, but naturally prior and simpler, and not dependent in turn upon that sought. For this reason, it was possible, by beginning with the aid of the simplest of all of them, to proceed step by step to unravel other more complex cases without any analysis. Here, however, the matter is different [...]. It is worthwhile to have observed this, so that by a clear

example it may appear what the difference is between the two methods and when one or the other is to be turned to."[6] (*ibid.*, 47-48)

Bernoulli follows this by suggesting his own alternative method that can be used both when synthesis is normally used and when algebraic analysis had been resorted to:

"I have said that it cannot be done following in the author's footsteps. There is, however, still another special way by which I may pursue what is sought short of any analysis. This additional way may also be usefully employed in what follows. Let us, in place of the two alternate players, hypothesize infinitely many players, to each of whom in order, one after the other, only one throw is conceded [...]. (*ibid.*, 60-61)
Further, the method familiar to us may also be used with regard to this hypothesis, nor is this method less compatible with questions that are commonly solved by synthesis alone than with those that require analysis."[7] (*ibid.*, 48)

Since Bernoulli's terminology alternates between "algebraic analysis [*analysis algebraica*]" and simply "analysis [*analysis*]", it is clear that by "analysis" he often means, in our terms, simply algebra. Elsewhere, following Huygens, he calls "analysis" the working out of the solution to a problem (*ibid.*, 2-3)[8]. On the other hand, as is clear from his definitions of analysis and synthesis, he does sometimes have a directional differentiation between analysis and synthesis in mind. In Bernoulli's terminology a "synthesis" is mathematical reasoning that goes step by step from what is prior and already known to what is at first unknown, while "analysis" is a line of mathematical reasoning that may involve recursion and/or solution of simultaneous equations. Discussing a problem in which three players in turn draw stones without replacing them from an urn originally containing 12 stones, Bernoulli states that in the end one comes down to known chances, so that the problem can then be reversed to build up a synthesis from the simplest cases:

"If, again, the sense of the problem were that the stones taken from a common supply of 12 were not replaced after being taken from the urn, then the first player indeed would, after playing, take third place and the third player second place and the second player first place, but, on that account, the players would not exchange among themselves chances equal to those that existed at the start, as happened under the preceding hypothesis. Rather, they would continually acquire new chances, different from the earlier chances, because of the changed number of stones. These chances would be simpler to the extent that more black stones were withdrawn and such that finally they end in chances that are altogether known. On account of this, we can begin, using the Author's accustomed method, from the simplest cases, and proceed backwards through all the intermediate cases, arriving finally at the case proposed in the question, having used the method of synthesis."[9] (*ibid.*, 59)

In sum, Bernoulli uses the word "synthesis" in the sense standard from the time of the Greeks to mean a demonstration beginning from axioms, postulates, or what is prior and better known and moving to what was previously not known or not proved. "Analysis," on the other hand, for him as for the Greeks, is a method that does not begin from what is better known, but from something not

known, or not yet proved. Bernoulli is unlike the Greeks, however, because he has a method of analysis in mind, namely algebra, or the solution of simultaneous equations with unknowns.[10] While there is a perennial question about Greek geometrical analysis, because it seems to assume unjustifiably that the deductions of the analysis will always be reversible to construct the desired synthesis (Mahoney 1968), there is no such problem with algebraic analysis, which is, in this sense, directionless. Thus Bernoulli understands Huygens's *On Calculations in Games of Chance* to exhibit or demonstrate a small number of approaches or methods, both synthetic and analytic, by which problems concerning games of chance may be solved. While a synthetic method may be more natural, building up from the prior and better known to what is sought, an algebraic method also achieves the desired results, and that without the necessity of being supplemented by a synthesis.

III *A Priori*, *A Posteriori*, and the Law of Large Numbers

When in Part IV of *The Art of Conjecturing* Bernoulli introduces his law of large numbers, he does not use concepts of analysis and synthesis to indicate directions of reasoning, but rather the concepts of *a priori* and *a posteriori*. After the lines quoted above (at note 1), Bernoulli goes on:

"If, for example, there once existed three hundred people with the same age and body type as Titius now has, and you observed that two hundred of them died before the end of a decade, while the rest lived longer, you could safely enough conclude that there are twice as many cases in which Titius also may die within a decade as there are cases in which he may live beyond a decade. Likewise if someone for several years past should have observed the weather and noted how many times it was clear or rainy or if someone should have very frequently watched two players at a game and should have seen how many times this or that player won, just by doing so one would have discovered the ratio that probably exists between the numbers of cases in which the same outcomes can happen or not happen in the future in circumstances similar to the previous ones."[11] (1713, 224-225)

The method of arguing *a posteriori*, or empirically, in this period could also be called "analysis," as Isaac Newton does in his famous Query 31 of the *Opticks*:

"As in Mathematicks, so in Natural Philosophy, the Investigation of difficult Things by the Method of Analysis, ought ever to precede the Method of Composition. This Analysis consists in making Experiments, and Observations, and in drawing general Conclusions from them by Induction [...]. By this way of Analysis we may proceed from Compounds to Ingredients, and from Motions to the Forces producing them; and in general, from Effects to their Causes, and from particular Causes to more general ones, till the Argument end in the most general. This is the Method of Analysis: And the Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phaenomena proceeding from them and proving the Explanations." (Newton 1704, 404-405)

Here Newton links the methods of analysis and synthesis in mathematics and in physics, explaining physical analysis as induction from experimental data. This might lead us to believe that Jacob Bernoulli also intended his *a posteriori* method to be based on induction. But in turning to the proof of his law of large numbers, Bernoulli shows no concern about problems of induction. Rather, in order to justify the use of observed frequencies as the basis for decisions or predictions, Bernoulli thinks he needs to show two things. First, he wants to demonstrate that as the number of observations increases, the probability that the *a posteriori* observed ratio of outcomes corresponds closely to an *a priori* ratio also increases—this is something even ordinary people commonly assume, but they do not know how to prove it:

> "This empirical way of determining the number of cases by experiment is neither new nor uncommon. The author of *The Art of Thinking* [*i.e.*, Antoine Arnauld], a man of great acuteness and talent, made a similar recommendation in Chapter 12 and following of the last part [*i.e.*, Part IV], and everyone consistently does the same thing in daily practice. Neither should it escape anyone that to judge in this way concerning some future event it would not suffice to take one or another experiment, but a great abundance of experiments would be required, given that even the most foolish person, by some instinct of nature, alone and with no previous instruction (which is truly astonishing), has discovered that the more observations of this sort are made, the less danger there will be of error. But although this is naturally known to everyone, the demonstration by which it can be inferred from the principles of the art is hardly known at all, and, accordingly, it is incumbent upon us to expound it here."[12] (1713, 225)

But, second, beyond demonstrating the effect of increasing numbers of observations, Bernoulli also wants to prove that the process does not reach a limit of certainty or probability beyond which greater probability is impossible:

> "But I would consider that I had not achieved enough if I limited myself to demonstrating this one thing, of which no one is ignorant. Something else remains to consider, which perhaps no one has thought about up to this point. It remains, namely, to ask whether as the number of observations increases, so the probability increases of obtaining the true ratio between the numbers of cases in which some event can happen and not happen, such that this probability may eventually exceed any given degree of certainty. Or whether, instead, the problem has an asymptote, so to speak; whether, that is, there is some degree of certainty that may never be exceeded no matter how far the number of observations is multiplied, so that, for example, we may never be certain that we have discovered the true ratio of cases with more than a half or two-thirds or three-fourths parts of certainty."[13] (*ibid.*)

With this introduction, Bernoulli then goes on to his proof, which assumes *a priori* ratios exist, although they may or may not be known. What then is the relationship of analysis and synthesis, or the relationship of the *a priori* and the *a posteriori*, in this proof? Given Bernoulli's earlier discussions of mathematical analysis and synthesis, we should expect him to take a consistent position on these matters[14].

IV Bernoulli's Proof of the Law of Large Numbers

In order to investigate this question further, it will be worthwhile to examine Bernoulli's proof of his law of large numbers. Bernoulli achieves his proof by first demonstrating five lemmas concerning the terms of a binomial expansion. He then is able to prove his law essentially by showing how the various terms of the binomial expansion correspond to possible outcomes of nt trials of a situation in which there are r cases for a positive outcome and s cases for a negative one, $t = r + s$, and n is some large integer. Todhunter states the essentials of the proof quite clearly and succinctly:

> "We will now state the purely algebraical part of the theorem. Suppose that $(r+s)^{nt}$ is expanded by the Binomial Theorem, the letters all denoting integral numbers and t being equal to $r + s$. Let u denote the sum of the greatest term and the n preceding terms and the n following terms. Then by taking n large enough the ratio of u to the sum of all the remaining terms of the expansion may be made as great as we please. If we wish that this ratio should not be less than c it will be sufficient to take n equal to the greater of the two following expressions:
>
> $$\frac{\log c + \log(s+1)}{\log(r+1) - \log r}\left(1 + \frac{s}{r+1}\right) - \frac{s}{r+1},$$
>
> and
>
> $$\frac{\log c + \log(r-1)}{\log(s+1) - \log s}\left(1 + \frac{r}{s+1}\right) - \frac{1}{s+1}.$$
>
> [...] Let us now take the application of the algebraical result to the Theory of Probability. The greatest term of $(r+s)^{nt}$, where $t = r+s$ is the term involving $r^{nr}s^{ns}$. Let r and s be proportional to the probability of the happening and failing of an event in a single trial. Then the sum of the $2n+1$ terms of $(r+s)^{nt}$ which have the greatest term for their middle term corresponds to the probability that in nt trials the number of times the event happens will lie between $n(r-1)$ and $n(r+1)$, both inclusive; so that the ratio of the number of times the event happens to the whole number of trials lies between $\frac{r+1}{t}$ and $\frac{r-1}{t}$. Then, by taking for n the greater of the two expressions in the preceding [...], we have the odds of c to 1 that the ratio of the number of times the event happens to the whole number of trials lies between $\frac{r+1}{t}$ and $\frac{r-1}{t}$." (Todhunter 1949, 71-72)

Now, because the central work of the proof is done by means of lemmas concerning any binomial expansion, it is not immediately clear whether Bernoulli would consider the reasoning in his proof of the main theorem to have been analytic or synthetic. But Todhunter's labelling of the lemmas as "the purely algebraical part of the theorem," provides a needed clue: the five lemmas are in a sense Bernoulli's analysis of the problem, while the synthesis is what Todhunter calls "the application of the algebraical result to the Theory of Probability" and what Bernoulli himself calls the demonstration of the principal proposition[15]. In the proofs of his lemmas, Bernoulli takes it for granted that mathematicians know the series expansions of binomials to various powers, and he treats them as pure mathematics, abstracted from any particular application[16]. He raises the possibility in a scholium that someone may object to the way he has made use of infinites in his proof of Lemmas 4 and 5, but provides an alternative interpretation for such objectors that requires only finite numbers and not infinites[17]. The proof of the second lemma is an informal induction[18].

Based on the algebraic analysis of the lemmas, Bernoulli's proof of the law of large numbers is synthetic, starting from what is known through the lemmas and moving to prove the desired conclusion. How he gets from the pure mathematics of the lemmas to the proof of his law of large numbers is, in his terms, simply "by the application of the foregoing lemmas to the present purpose", that is, by interpreting the terms of the binomial expansion as expressing the numbers of ways in which various possible outcomes of a series of observations can occur. Bernoulli writes:

> "*Demonstration.* Let *nt* be the number of observations to be taken, and let us ask how great is the expectation or how great is the probability, that they will all be fecund except for, first, none, then 1, 2, 3, 4, etc. sterile. But since in any observation there are, by hypothesis, *t* cases at hand, and of them *r* are fecund and *s* sterile, and the individual cases of one observation can be combined with the individual cases of the other, and those combined can be joined again with the individual cases of the third, fourth, etc., it is easy to see that this situation fits the Rule in the Notes appended to the end of Proposition XIII. [*sic*, should be XII] in Part I, and its Corollary 2, which contains a general formula, with the help of which it is seen that the expectation of no sterile observations is $r^{nt}:t^{nt}$, of one $\frac{nt}{1}r^{nt-1}s:t^{nt}$, of two $\frac{nt(nt-1)}{1\cdot 2}r^{nt-2}ss:t^{nt}$ of three $\frac{nt(nt-1)(nt-2)}{1\cdot 2\cdot 3}r^{nt-3}s^3:t^{nt}$ and so forth. Consequently, omitting the common denominator t^{nt} the degrees 'of probability or the numbers' of cases in which it can happen that all the experiences are fecund, or all except one sterile one, or all except 2, 3, 4, etc. are expressed in order by $r^{nt}, \frac{nt}{1}r^{nt-1}s, \frac{nt(nt-1)}{1\cdot 2}r^{nt-2}ss, \frac{nt(nt-1)(nt-2)}{1\cdot 2\cdot 3}r^{nt-3}s^3$ etc. Now these, in fact, are the terms of the power *nt* of the binomial $r+s$, investigated just now in our lemmas. Then all the rest is completely evident. Indeed, it is clear from the nature of the progression that the number of cases that combine *ns* sterile experiences with *nr* fecund ones is the maximum term M, or the term that *ns* terms preceed and *nr* follow, by Lemma 3."[19] (Bernoulli 1713, 236-237)

Thus Bernoulli bases his demonstration upon the algebraic lemmas, interpreting the terms of the binomial expansion in terms of the probabilities of various outcomes of a series of observations. The largest term of the binomial expansion represents the numbers of ways in which the ratio of fertile to sterile observations may equal the underlying ratio of cases.

V Cases (*casus*) and Bernoulli's Conceptions of God and the World

What in Jacob Bernoulli's larger world view justified his belief that cases of death, or of weather, or of winning at tennis, and so forth could be represented by the terms of a binomial expansion as he represented them in his proof of the law of large numbers? Or why would a prudent physician or insurance agent be wise or justified in using observed ages at death of people in various situations to calculate the life expectancies of living people of given age and circumstances? Not only did Bernoulli not know *what* the fundamental *a priori* ratios of cases were for diseases or the weather or any other of the political, moral, or economic situations to which he hoped to apply the art of conjecturing, but also, from our point of view, he did not know *that* there were fundamental *a priori* ratios of cases.

Leibniz raised this objection in correspondence with Bernoulli near the end of the latter's life, arguing that the risks of various diseases are not known and, in fact, may not be stable. In response, Bernoulli admitted that the situation quite likely changes over time. Modern life expectancy, Bernoulli reasoned, was doubtless different from the life expectancy in Biblical times. Nevertheless Bernoulli was optimistic that there was enough stability in the real world for his *a posteriori* method to be useful. A central reason for this optimism was that even if we do not know anything about the ratios in real world cases, God knows. Things are uncertain to us, but not to God:

> "All things under the sun, which are, were, or will be, in themselves and objectively always have the highest certainty. This is evident concerning past and present things, since by the very fact that they are or were, these things cannot not exist or not have existed. Nor should there be any doubt about future things, which in like manner, even if not by the necessity of some inevitable fate, nevertheless by divine foreknowledge and predetermination, cannot not be in the future. Unless, indeed, whatever will be will occur with certainty, it is not apparent how the praise of the highest Creator's omniscience and omnipotence can prevail."[20] (*ibid.*, 210-211)

Responding directly to Leibniz's argument, Bernoulli said:

> "Let me remove a few objections which certain learned men have raised against these views. [...] They object first that the ratio of stones is different from the ratio of diseases or changes in the air: the former have a determinate number, the latter an indeterminate and varying one. I reply to this that both are considered to be equally uncertain and indeterminate with respect to our knowledge. On the other hand, that either is indeterminate in itself and with respect to its nature can no more be conceived by us than it can be conceived that the same thing at the same time is both created and not created by the Author of nature: for whatever God has made, he has, by that very act, also determined at the same time."[21] (*ibid.*, 227)

Thus Jacob Bernoulli did not believe that nature or even human life is inherently statistical or probabilistic[22] (Daston 1992). Although he was not sure how human freedom could be reconciled with the fact that God determines and foresees everything that will happen, Bernoulli nevertheless believed that everything is determined by God[23]. Humans use probabilistic reasoning, he said, not because the world is inherently governed by chance, but because we do not know its hidden mechanisms. The laws of physics and the initial conditions determine which side of a die will fall facing up. We say that each face has a one-sixth chance of facing upwards because we do not know the exact initial conditions or perhaps all the laws of physics, but the fall of the die is nonetheless physically determined.

Bernoulli's understanding *that* there are *a priori* ratios of cases in real world situations helps to elucidate the cryptic statement with which *The Art of Conjecturing* ends:

> "Whence at last this remarkable result is seen to follow, that if the observations of all events were continued for the whole of eternity (with the probability finally ending in perfect certainty) then everything in the world would be observed to happen in fixed ratios and with a constant law of alternation. Thus in even the most accidental and fortuitous we would be bound to acknowledge a certain quasi necessity and, so to speak, fatality. I do not know whether Plato already wished to assert this result in his dogma of the universal return of things to their former positions, in which he predicted that after the unrolling of innumerable centuries everything would return to its original state."[24] (*ibid.*, 239)

To a modern eye this passage seems to mean only that if all events of all eternity are taken into account, then they will have some ratio to each other, whatever that may be. Bernoulli, however, when he says, "fixed ratios and with a constant law of alternation," implies that there will be some lawlike ratios of integers, small or large, but not unrecognizable as such.

Up to this point, I have been translating "casus" when it appears in Bernoulli's Latin as "cases," as when he says in introducing his *a posteriori* method of determining the ratios of cases, "it ought to be anticipated that something can happen or not happen in the future in as many cases as it was observed to happen or not to happen in the past in a similar state of things."[25] (*ibid.*, 224) When, in the eighteenth century, other authors writing about games of chance translated "casus" in this sense into English, they almost always translated it as "chances." If I translated Bernoulli as saying, "it ought to be anticipated that something can happen or not happen in the future with as many chances as it was observed to happen or not to happen in the past in a similar state of things," then Bernoulli might seem to believe that chance was intrinsic to physical reality and not only to our thinking about it.

What, then, is a "casus" for Bernoulli? His models or metaphors for *casus* come first of all from games. *Casus* sometimes correspond to stones to be drawn out of an urn[26]. Dice and cards also provide common models. With stones in an urn there may be many stones of the same color any of which is equally likely to be drawn. Cards provide a more complicated set of possibilities of a similar type. With normal dice, on the other hand, each die might be thought to have an equal proclivity for falling with any of its faces up. Hence, with stones in an urn or cards

or dice, the "casus" correspond to separable aspects of physical reality, but their ease in occurring may depend on various factors configuring the situation, as well as on the items themselves. It is not essential to these models that the game pieces themselves have some inherent "proclivity" to exhibit one or another case, that is, that they have in themselves some inherent "probability" of appearing one way or another. Jacob Bernoulli in *The Art of Conjecturing* never uses "probability [*probabilitas*]" to refer to inherent properties or proclivities of stones or cards or dice, but always to refer to degrees of certainty about the truth of propositions.

While Abraham De Moivre in his *The Doctrine of Chances* consistently uses "chances" with regard to alternative possibilities, his statements about these chances show that he too did not believe that the underlying reality was governed by chance as we understand it. In a typical problem, he says:

> "To find the Probability of throwing a Chance assigned a given number of times without intermission, in any given number of Trials." (De Moivre 1718, 254)

Here the "chance assigned" could be anything, say to throw a 7 with two dice: the "chance assigned" is some specific outcome, one of several possible outcomes.

After De Moivre has discussed the law of large numbers he says:

> "*Chance*, as we understand it, supposes the *Existence* of things, and their general known *Properties*: that a number of Dice, for instance, being thrown, each of them shall settle upon one or other of its Bases. After which, the *Probability* of an assigned Chance, that is of some particular disposition of the Dice, becomes as proper a subject of Investigation as any other quantity or Ratio can be.
>
> But *Chance*, in atheistical writings or discourse, is a sound utterly insignificant: It imports no determination to any *mode of Existence*; nor indeed to *Existence* itself, more than to *non-existence*; it can neither be defined nor understood: nor can any Proposition concerning it be either affirmed or denied, excepting this one, 'That it is a mere word.'" (*ibid.*, 253)

Shortly before this passage, De Moivre wrote:

> "From what has been said, it follows, that Chance very little disturbs the Events which in their natural Institution were designed to happen or fail, according to some determinate Law; for if in order to help our conception, we imagine a round piece of Metal, with two polished opposite faces, differing in nothing but their colour, whereof one may be supposed to be white, and the other black; it is plain that we may say, that this piece may with equal facility exhibit a white or black face, and we may even suppose that it was framed with that particular view of shewing sometimes one face, sometimes the other, and that consequently if it be tossed up Chance shall decide the appearance [...] yet the appearances, either one way or the other, will perpetually tend to a proportion of Equality [...]. What we have said is also applicable to a Ratio of Inequality [...]. And thus in all Cases it will be found, that *altho' Chance produces Irregularities, still the Odds will be infinitely great, that in the process of Time, those Irregularities will bear no proportion to the recurrency of that Order which naturally results from* Original Design." (*ibid.*, 250-251)

Thus, for De Moivre, and I suggest also for Jacob Bernoulli, there are laws of nature which in the long run will appear, however "chance" may obscure them in the short run. Moreover, according to De Moivre, it is God who has determined and continues to determine these regularities, not some intrinsic propensities or proclivities of material bodies:

> "[...] such Laws, as well as the original Design and Purpose of their Establishment, must all be *from without*; the *Inertia* of matter, and the nature of created Beings, rendering it impossible that any thing should modify its own essence, or give to itself, or to any thing else, an original determination or propensity. And hence, if we blind not ourselves with metaphysical dust, we shall be led, by a short and obvious way, to the acknowledgment of the great *Maker* and *Governour* of all; *Himself all-wise, all-powerful* and *good.*" (*ibid.*, 252)

From this point of view, then, for De Moivre (and for Bernoulli as well) it is clear that consistently observed frequencies of events in the world reveal the laws of nature or structures built into the universe no less than faces built into a die:

> "As, upon the Supposition of a certain determinate Law according to which any Event is to happen, we demonstrate that the Ratio of Happenings will continually approach to that Law, as the Experiments or Observations are multiplied: so, *conversely*, if from numberless Observations we find the Ratio of the Events to converge to a determinate quantity, as to the Ratio of P to Q; then we conclude that this Ratio expresses the determinate Law according to which the Event is to happen.
> For let that Law be expressed not by the Ratio P:Q, but by some other, as R:S; then would the Ratio of the Events converge to this last, not to the former: which contradicts our *Hypothesis*. And the like, or greater, Absurdity follows, if we should suppose the Event not to happen according to any Law, but in a manner altogether desultory and uncertain; for then the Events would converge to no fixt Ratio at all." (*ibid.*, 251-252)

Thus De Moivre's "chances," no less than Bernoulli's "casus"/"cases", reflect the laws of nature built into the existence of things and not something "desultory." They come "from without," that is from God or the First Cause, who, in creating, gives determination to creation. If there is chance in creation, it is only because God, like a dice maker, has designed into creation certain features that will result in the appearance of events with certain frequencies, as the designer of a die designs the die to come up one-sixth of the time on each of its faces. It is these features of God's design that can be found out *a posteriori* by observing the ratios of outcomes in the world over sufficiently long periods of time.

That there will be ratios in events observed over long periods of time results from God's design, but ratios observed *a posteriori* will not always correspond to the most fundamental structures of reality. In his commentary on Huygens, Bernoulli at first assumed that the ratios of cases used in the calculations resulted from the nature of the game pieces, but as he went on he noticed that Huygens sometimes treated the numerator and denominator of a fraction representing an expectation as if they represented numbers of cases, even if they were derived in a different way:

> "It helps here to observe that the Author supposes that any expectation expressed as a fraction may also be considered as if it resulted from as many cases for obtaining the stake a as are indicated by the numerator of the fraction and as many cases for obtaining nothing as are signified by the difference between the denominator and the numerator, notwithstanding that perhaps that expectation was arrived at in another way. Thus although the person who undertakes to throw two sixes in two tries arrives at his expectation of $(71/1296)a$ by a case for obtaining a and 35 cases for $(1/36)a$, nevertheless one could judge him to obtain it by 71 cases for obtaining a and 1225 cases for 0." (Bernoulli 1713, 29)

Thus the ratios of cases observed in wins and losses of tennis players over time may not correspond directly to some basic features of the minds or bodies of the players or their equipment, but to complex interactions of many factors. In an early consideration of tennis published in 1686, Bernoulli stated that the underlying ratio of cases may be incommensurable[27].

Mathematically, "cases" enter Bernoulli's proof of the law of large numbers in two ways. First of all, there are the fundamental cases with which the proof begins, that is r cases for a fertile outcome and s cases for a sterile one. But after nt observations have been made, there are also more complex cases, first the case in which all outcomes are fertile or positive, then the case in which the first trial is sterile, but the rest fertile, and so forth. The largest term of the binomial expansion is shown to represent the numbers of cases in which the individual outcomes are in the ratio of the underlying cases (corresponding to the two terms of the binomial, r and s). The probability that the ratio of outcomes will fall within some small interval around the ratio corresponding to the ratio of the underlying cases is explained to be proportional to the sum of a certain number of terms of the binomial expansion on either side of the largest term. Then the ratio between this sum and the sum of all the terms outside the limits is shown to increase without limit as the number of trials, nt, increases. A very large number of trials is required if it is desired that there be a very high probability that the ratio fall within very narrow limits. In interpreting this result, Bernoulli assumes that he is looking for ratios of integers and he talks about finding, determining, or discovering the ratio[28]. He seems to take it for granted that it will be obvious what the "real ratio" of cases is, even if the observed ratio of frequencies after many trials should deviate from it very slightly[29].

Did Bernoulli think that there really were in the outside world "cases" corresponding to various diseases or other possible causes of death and that by examining statistics for death rates he could discover underlying causes? Given his remark in commenting on Huygens's treatment of the numerators and denominators of expressions for expectation as if they referred to numbers of cases, I conclude that Bernoulli thought that the ratios found by experience might not represent fundamental cases or causes in the external world, but that they would represent the result of complex interactions of such cases or causes. In the proof of the law of large numbers, at first the cases are simple successes and failures, but once one has observed nt trials, then the cases become not just the r cases for success and s

cases for failure in a single trial, but instead the case of *nt* successes, the cases of *nt*–1 successes combined with one failure, etc., up through the case of *nt* failures. When Bernoulli talks about diseases or the weather, he sometimes talks as if the diseases would be the cases, but twice (in a letter to Leibniz and in *The Art of Conjecturing*) he chooses the word "tinder" (*fomitem*), which seems to be a purposefully vague or multivalent word with some connotations like "seed" or "germ"[30].

The one work in which Bernoulli did apply his method to a concrete situation was his *Letter to a Friend* on the game of tennis, published together with the *Ars Conjectandi* in 1713. His idea was that it would be possible to take the ratios of points or games that players won when playing against each other and to use these ratios to predict, for instance, the likelihood of victory when such individuals played as parts of doubles teams. The sorts of factors that Bernoulli then considered were, for instance, whether the opponents would consistently try to hit to the weaker player, whether the player who has to hit more balls will become tired sooner, etc., such things meaning that the strength of a team could not be supposed to be simply equal to the strength of the better player, nor simply the average between the two players.

In the introduction to the *Letter to a Friend*, Bernoulli writes as if he were cognizant of our question about the physical meaning of the concept of "cases." Bernoulli writes that his friend has seen a thesis of his concerning the game of tennis and:

> "[...] you ask me if these propositions contain some reality that can be demonstrated or if they are only founded on pure conjectures made in the air and which have nothing solid about them. According to what you say, you cannot conceive that the forces of players can be measured by numbers, much less that one can draw the conclusions from them that I have drawn."[31] (*ibid.*, new numeration, 1)

After referring to games of chance in which the numbers of cases are known *a priori*, he discusses games of skill in which they are not:

> "[...] it is not the same with games that depend only or in part on the genius, the industry, or the application of the players, such as tennis, chess, and most card games. It is very clear that one could not know how to determine by their causes or *a priori*, as one says, how much one person is more knowledgeable than another, more skillful, or more able, unless one had a perfect knowledge of the nature of the soul and of the disposition of the organs of the human body, which the thousand hidden causes that interact make absolutely impossible. But this does not prevent one from knowing this almost as certainly *a posteriori*, by the observation of the outcome many times repeated, doing what can be done even in games of pure chance when one does not know the number of cases that can occur."[32] (*ibid.*, new numeration, 2)

Bernoulli then goes on to describe the drawing of tickets from an urn without knowing the number of tickets of each kind that it contains. If, he says, he drew out a black ticket a hundred times and a white ticket two hundred times, he would not hesitate to conclude that the number of white tickets was about double the

number of black tickets. Having referred to his proof of the law of large numbers, Bernoulli then says that the same reasoning can be applied to games of skill. If, he says, he observed two men playing tennis and one man won 200 or 300 points while the other won 100, then he would judge with sufficient certainty that the first man was a two or three times better player than the second. The first player would have, so to speak, two or three times as many cases or causes making him win as the other[33]. Thus in the one concrete application that we have, Bernoulli makes no claims of knowing what in the real world corresponds to his cases or causes, only that the observed ratio of outcomes can be used as a ratio of cases in making judgments or predictions. If he knew what percentage of the time player *A* had beaten player *B* over a long series of games in the past, this did not mean that Bernoulli knew what it was that made one player more or less likely to win, but only that he thought he could predict the future reliably or with probability.

VI Algebra and the Law of Large Numbers

With this discussion about the meaning of "casus" or "cases" in hand, let me return to an examination of Bernoulli's proof of the law of large numbers. Whatever else "cases" or, for that matter "chances" were, they were always countable, or represented by integers. One always has some number of cases or chances for some outcome, never a fractional amount. The fact that Bernoulli's intuitive understanding of the "cases" is, to use modern terminology, digital rather than analog, may explain why, even though the infinitesimal calculus was in development by this time, he did not think to try proving the law of large numbers using calculus or even geometry, but instead used algebra[34]. Once Bernoulli began to think of his law of large numbers in algebraic terms, the mathematics itself may have become for him a model of the processes he was dealing with. The fact is that in the algebraic part of Bernoulli's proof of the law of large numbers, that is in the lemmas which are "pure mathematics" and which, indeed, contain the whole proof aside from its "application" or interpretation in terms of possible outcomes or expectations, there is no "prior" or "posterior," but everything is, so to speak, at the same cognitive level. One considers, as if laid out together in an array, all the possible outcomes of *nt* trials. This is not like the analysis of a game in which one round of the game precedes the next and in which the ratios of cases or chances may change depending upon the outcomes of the various rounds. Time is not a factor (nor is "sampling" from a larger population). The mathematics takes a "God's eye" point of view, in which every possibility is present and on an equal footing. On the other hand, each "snapshot" of the situation is for some *nt* number of observations. One chooses the level of risk one is willing to take (or the probability of being correct that one requires) and then determines how many observations are necessary to keep the risk that low (or the probability of being correct that high). As the number of trials is never infinite, the risk is never zero (or the probability never one). One may always be wrong. As long as *nt* is not infinite, it is always possible to observe a ratio of frequencies that does not reflect the underlying law of nature. All the law of large numbers tells you is what the chances are that you are wrong or the probability that you are right or very nearly so. What the art of conjecturing then provides as a mathematical instrument for decision making is knowledge of how to maximize your expectations before the fact and how to measure the chances that you may be wrong. If, after acting on the basis of the art of conjecturing, you lose, you nevertheless have the consolation of knowing you followed prudent strategy[35].

VII Summary

In this paper, I have made the following points. Jacob Bernoulli had notions of mathematical analysis and synthesis that were not atypical of his times. For him, a mathematical synthesis moves from what is prior and better known, while a mathematical analysis may move in any direction, sometimes deducing what is mathematically prior or better known from what is mathematically posterior. Like many others of his time, even those who, like himself, were in the process of developing infinite or infinitesimal analysis, Jacob Bernoulli when he used the word "analysis" frequently meant nothing more than algebra. Previous historians of probability theory, and in particular Ian Hacking, have questioned Bernoulli's intended interpretation of his law of large numbers, because his proof of the theorem presupposes that there are *a priori* ratios of cases and yet the theorem is supposed to justify discovering these ratios *a posteriori*. Bernoulli's world view, like that of De Moivre, indeed assumes that the universe displays design and that this design is incorporated in laws of nature that undergird observed frequencies. To God, Bernoulli says, all things are known and certain in the past and present and in the future as well. The law of large numbers shows that, despite temporary fluctuations, in the long run the structure of the world will manifest itself. The lemmas of Bernoulli's proof of the law of large numbers, that is the algebraic parts of the proof or the analysis, mirror this "God's eye" perspective on the universe in the sense that there is nothing prior or posterior, but all is equally present and evident. They are pure mathematics and self-contained. All that is required to apply them to prove the law of large numbers is to interpret them to apply to the outcomes of experiments. Thus, both Bernoulli's world view and the way in which he used algebra to prove his lemmas explain why he saw no problem in assuming the existence of *a priori* ratios of cases when he was proving the law of large numbers—and then boasting that the proof of the law of large numbers shows why one can reliably discover ratios of cases *a posteriori*. The ratios of cases so discovered were not necessarily ratios of fundamental underlying causes, but rather ratios of cases that could be prudently used, with the rest of the art of conjecturing, to make decisions in civic, moral, and economic situations.

North Carolina State University
Department of History

Notes

1 The translations of the *Ars Conjectandi* for this paper are my own, part of a joint project with Glenn Shafer to publish an English translation of *The Art of Conjecturing* with supporting materials. I shall quote in notes the original texts.

> "Verum enimvero alia hic nobis via suppetit, qua quaesitum obtineamus; & quod a priori elicere non datur, saltem *a posteriori*, hoc est, ex eventu in similibus exemplis multoties observato eruere licebit; quandoquidem praesumi debet, tot casibus unumquodque posthac contingere & non contingere posse, quoties id antehac in simili rerum statu contigisse & non contigisse fuerit deprehensum."

2 Nicholas Bernoulli was familiar with his uncle Jacob Bernoulli's work in mathematical probability long before the work was published. In 1709 Nicholas defended a mathematical-legal thesis *De Usu Artis Conjectandi in Jure*, that made use of his uncle's ideas, and throughout the period just before the publication of Jacob Bernoulli's *Ars Conjectandi*, Nicholas collaborated with Montmort in their work on probability theory, culminating in the publication of a number of letters from Nicholas to Montmort in the second edition of Montmort's *Essai d'analyse sur les jeux de hazard* (1713). These letters included an alternative approach to proving a law of large numbers. De Moivre's first publication in probability theory was his *De mensura sortis seu de probabilitate eventuum in ludis a casu fortuito pendentibus* in *Philosophical Transactions* (1711). This was followed in 1718 by his *The Doctrine of Chances: or, A Method of Calculating the Probability of Events in Play* (1718). De Moivre first dealt with Bernoulli's proof of the law of large numbers in his *Miscellanea Analytica de Seriebus et Quadraturis* (1730). In the first edition of his *Doctrine of Chances*, he only said, at the end of the preface:

> "Before I make an end of this Discourse, I think myself obliged to take Notice, that some years after my specimen was printed, there came out a Tract upon the Subject of Chances, being a Posthumous Work of Mr. James Bernoully, wherein the Author has shown a great deal of Skill and Judgment, and perfectly answered the Character and great Reputation he hath so justly obtained [...]."

The tone was set for all these later works by Christiaan Huygens, *De Ratiociniis in Ludo Aleae* as it appeared in Latin translation in F. Van Schooten, *Exercitationum mathematicarum* (1657). In 1692 John Arbuthnot published an English translation of much of Huygens's book (Arbuthnot 1692). Montmort said, in the first edition of *Essay d'analyse sur les jeux de hazard* (1708, iii-vi) that he was motivated to attempt to calculate expectations in games of chance by the reports about the manuscript of Jacob Bernoulli's *Ars Conjectandi*, made in the *éloges* at the time of Jacob's death.

3 Here what he writes (1975, ch. 17, 154-165):

> "Chapter 5 of Part IV of *Ars conjectandi* proves the first limit theorem of probability theory. The intended interpretation of this result is still a matter of controversy, but there is no dispute about what Bernoulli actually proved [...]. Bernoulli proves what is now called the weak law of large numbers [...]. Bernoulli's proof is chiefly a consequence of his earlier investigation of combinatorics, for it proceeds by summing the middle terms in the binomial expansion. Notice that this result is a theorem of pure probability theory, and holds under any interpretation of the calculus [...]. Bernoulli's exposition has a basic difficulty that has led to repeated misinterpretation. It is still a matter of

controversy [...]. Bernoulli plainly wants to estimate an unknown parameter p. His favourite example is the proportion of white pebbles in an urn. An *estimator* is a function F from data to possible parameter values, in this case, possible values of p. Bernoulli uses an *interval estimator* which maps given data onto a set of possible values of p, 'bounded by two limits' [...]. Inevitably [...] we come to consider his problem as one of estimating an unknown aleatory probability, or chance. Moreover, we wonder if he wanted to know the epistemic probability that a given estimate of chance was correct [...]. We are [...] confident that Bernoulli did not make any simply fallacious 'inverse' use of his theorem [...]. He thought [his theorem] had application to inverse inference, but does not make clear exactly why."

Stephen Stigler (1986, 66), also brings into question the correct interpretation of the significance of Bernoulli's theorem: "This modern synopsis is inaccurate in several respects, however, as is the occasional claim that Bernoulli presented the first example of an interval estimate of probability." Lorraine Daston (1988, 188-190), chides Hacking more generally for anachronism in his interpretation of Bernoulli's ideas:

"In Ian Hacking's thoughtful discussion of the *Ars conjectandi,* for example, Bernoulli emerges as both more prescient and more quaint than a less anachronistic reading would warrant. On the one hand, Hacking credits Bernoulli with anticipating a frequentist 'security level' for inductive inference [*corr. ex* influence] [...] and on the other, he saddles Bernoulli with a 'useful equivocation' between *de re* and *de dicto* senses of possibility and corresponding epistemic and physical senses of probability."

On Bernoulli's inverse use of his law, cf. also *ibid.*, 234ff. A general corrective to Hacking's history of the emergence of probability is to be found in Garber and Zabell (1978). The process of translating Bernoulli has made it clear to me that Bernoulli's use of the term "*probabilitas*" is always epistemic—the word is never used by Huygens or by Bernoulli in the first three parts of the *Ars Conjectandi* dealing with games of chance.

4 Cf. Bernoulli (W, vol. III, 88; from Bernoulli's notebook *Meditationes*, p. 91): "NB. Hoc inventum pluris facio quam si ipsam circuli quadraturam dedissem, quod si maximè reperiretur, exigui usûs esset."

5 "Quoniam enim omnes istae sortes differentes sunt et incognitae, earumque praecedens quaelibet a sequente et postrema vicissim a prima dependet, uti ex subjuncta operatione constabit, non poterit Problema istud Auctoris saltem methodo, per ea quae ad Propos. ult. annotata sunt, aliter quam mediante analysi algebraica expediri."

6 "Auctor in hoc Problemate primum adhibere cogitur analysin algebraicam, cum in praecedentibus sola synthesi usus fuisset: cuius differentiae ratio est, quod in illis omnibus expectatio quaesita fluebat ex aliis expectationibus vel in totum cognitis et datis, vel incognitis quidem, at natura prioribus ac simplicioribus, et quae ab hac vicissim non dependebant; quapropter incipiendo ab omnium simplicissimis earum ope gradatim pergere poterat ad enodandos alios casus magis magisque compositos absque analysi ulla. Secus vero se hic res habet; nam expectationem meam, quam possideo cum collusorem ordo jaciendi tangit, Auctoris more aestimare non possum, nisi cognitam habuero sortem, quam acquiro ubi vices jaciendi ad me devolvuntur: sed et hanc cognoscere nequeo, nisi priorem illam compertam habeam, quae tamen ea ipsa est quam quaerere intendo; unde cum utraque sit incognita, et altera ab altera vicissim dependeat, non possunt Auctoris vestigiis insistendo aliter quam analyseos ope ex se mutuo elici: id quod operae pretium est observasse, ut utriusque methodi discrimen, et quando haec illave in usum vertenda sit, perspicuo aliquo exemplo pateret."

7 "Dixi, Auctoris vestigiis insistendo non posse; datur enim adhuc alia peculiaris via, qua quaesitum consequi possum citra analysin ullam, et quam in sequentibus quoque utiliter adhibere licet. Fingamus loco duorum alternatim ludentium infinitos Collusores, quibus singulis ordine uni post alterum singuli tantum concedantur jactus [...]."

"Methodus porro nobis familiaris etiam in praesente hypothesi locum habet; neque enim hanc magis respuunt eae quaestiones, quae communiter sola synthesi solvuntur, quam quae analysi opus habent."

8 Huygens's Preface addressed to Franciscus Schooten (1657, 519) begins,

"Cum in editione elegantissimorum ingenii Tui monumentorum, quam prae manibus nunc habes, Vir Clarissime, id inter coetera Te spectare sciam, ut varietate rerum, quarum tractationem instituisti, ostendas quam late se protendat divina Analytices scientia, facile intelligo [...]."

And in ending (*ibid.*, 520) Huygens says,

"Horum Problematum nonnulla in fine operis addidisse me invenies, omissa tamen analysi, cum quod prolixam nimis operam poscebant, si perspicue omnia exequi voluissem, tum quod relinquendum aliquid videbatur exercitationi nostrorum, si qui erunt, Lectorum."

Bernoulli then echoes Huygens's reference to the working out of solutions to problems as "analysis" (1713, 49): "Coronidis loco Auctor Tractatui suo subjunxit sequentia quinque Problemata, sed omissa analysi vel demonstratione, quam Lectori eruendum reliquit."

9 "Si porro sensus Problematis sit, ut assumpti in commune calculi 12 non reponantur, postquam ex urna exempti fuerint; observandum est, quod per continuam eductionem calculorum nigrorum, primus quidem collusor transeat in locum tertii, tertius in locus secundi, secundus in locum primi, non idcirco tamen pariter sortes, quas ab initio ludi habuere, invicem permutent, ut factum fuit in praecendente hypothesi, sed quod subinde alias novas et a prioribus diversas ob mutatum calculorum numerum acquirant, easque tamen simpliciores quo plures calculi nigri educti fuerint, atque ita comparatas, ut tandem desinant in sortes omnino cognitas. Quapropter incipiendo consueta Auctoris methodo ab omnium simplicissimis, et pergendo retro per omnes intermedias, perveniemus ultimo sola synthesi utendo ad casum in quaestione propositum."

10 Cf. Boyer (1968, 97-98 ("Perhaps more genuinely significant is the ascription to Plato of the so-called analytic method [...]. Plato seems to have pointed out that often it is pedagogically convenient, when a chain of reasoning from premises to conclusion is not obvious, to reverse the process. One might begin with the proposition that is to be proved and from it deduce a conclusion that is known to hold."); 210 ("Pappus describes analysis as 'a method of taking that which is sought as though it were admitted and passing from it through its consequences in order to something which is admitted as a result of synthesis.' That is, he recognized analysis as a 'reverse solution,' the steps of which must be retraced in opposite order to constitute a valid demonstration."); 352 ("Viète had been one of the first to use the word 'analysis' as a synonym for algebra"); 418-419 ("One who has read our chapters on Greece will see that Wallis was far better as a mathematician than as a historian, for he equates algebra (or the analytics of Viète) with the ancient geometrical analysis.")).

11 "Nam si ex. gr. facto olim experimento in tercentis hominibus ejusdem, cujus nunc Titius est, aetatis & complexionis, observaveris ducentos eorum ante exactum decennium mortem oppetiisse, reliquos ultra vitam protraxisse, satis tuto colligere poteris, duplo plures casus esse, quibus & Titio intra decennium proximum naturae debitum solvendum sit, quam quibus terminum hunc transgredi possit. Ita si quis a plurimis retro annis ad coeli tempestatem attenderit, notaveritque, quoties ea serena aut pluvia extiterit: aut si quis duobus ludentibus saepissime adstiterit, viderit que quoties hic aut ille ludi victor evaserit, eo ipso rationem detexerit, quam probabiliter habent inter se numeri casuum, quibus iidem eventus praeviis similibus circumstantiis & posthac contingere ac non contingere possunt."

12 "Atque hic modus empiricus determinandi numeros casuum per experimenta neque novus est neque insolitus; nam et Celeb. Auctor Artis cogitandi magni acuminis et ingenii Vir Cap. 12 et seqq. postremae Partis haud dissimilem praescribit, et omnes in quotidiana praxi eundem constanter observant. Deinde nec illud quenquam latere potest, quod ad judicandum hoc modo de quopiam eventu non sufficiat sumsisse

unum alterumque experimentum, sed quod magna experimentorum requiratur copia; quando et stupidissimus quisque nescio quo naturae instinctu per se et nulla praevia institutione (quod sane mirabile est) compertum habet, quo plures ejusmodi captae fuerint observationes, eo minus a scopo aberrandi periculum fore. Quanquam autem hoc naturaliter omnibus notus sit, demonstratio, qua id ex artis principiis evincitur, minime vulgaris est, et proin nobis hic loci tradenda incumbit [...]."

13 "Ubi tamen parum me praestiturum existimarem, si in hoc uno, quod nemo ignorat, demonstrando subsisterem. Ulterius aliquid hic contemplandum superest, quod nemini fortassis vel cogitando adhucdum incidit. Inquirendum nimirum restat, an aucto sic observationum numero ita continuo augeatur probabilitas assequendae genuinae rationis inter numeros casuum, quibus eventus aliquid contingere et quibus non contingere potest, ut probabilitas haec tandem datum quemvis certitudinis gradum superet: an vero Problema, ut sic dicam, suam habeat Asymptoton, hoc est an detur quidam certitudinis gradus quem nunquam excedere liceat, utcunque multiplicentur observationes, puta, ut nunquam ultra semissem, aut 2/3, aut 3/4 certitudinis partes certi fieri possumus, nos veram casuum rationem detexisse."

14 I make this point because of Hacking's discussion of Bernoulli and the law of large numbers. Cf. Hacking (1975 as quoted above, note 3, and 159):

> "Remember, however, that at the time Bernoulli wrote, the problem of induction had not yet been stated as a central problem of philosophy [...]. One thing Bernoulli was *not* trying to do was to solve some publicized problem of induction, for when he wrote there was none."

15 Cf. Bernoulli (1713, 236):

> "*Propos. Princip.* Sequitur tandem Propositio ipsa, cujus gratia haec omnia dicta sunt, sed cujus demonstrationem sola Lemmatum praemissorum applicatio ad praesens institutum absolvet."

Cf. also (*ibid.*, 228):

> "Ut prolixae rem demonstrationis qua licet brevitate et perspicuitate expediam, conabor omnia reducere ad abstractam Mathesin, depromendo ex illa sequentia Lemmata, quibus ostensis caetera in nuda applicatione consistent."

16 He says, *e.g.*, at the start of his demonstration of lemma 3 (*ibid.*, 229): " Nota res est inter Geometras, quod potestas nt binomii $r+s$, hoc est $(r+s)^{nt}$ hac serie exprimitur [...]." Cf. also his use of "abstractam Mathesin" in the quotation at the preceding note (15).

17 "It may be objected against Lemmas 4 and 5, by those who are not accustomed to speculations about the infinite, that even if, in the case of an infinite number n, the factors in the expressions for the ratios M/L and M/Λ, namely $nr \pm n$m1, 2, 3, *etc* [...]. I cannot reply to this uneasiness better than by showing how to assign an actually finite number to n, or a finite power to the binomial, so that the sum of the terms within the bounds L and Λ will have to the sum of terms outside a ratio larger than a given ratio however large [...]. When this has been shown, it will be seen that the objection necessarily also collapses."

18 Cf. (*ibid.*, 229):

> "Every integral power of a binomial $r+s$ is expressed by one more term than the number of units in the index of the power. Thus a square is composed of 3 terms, a cube of 4, a biquadrate of 5, and so forth, as is known."

19 "*Dem.* Ponatur numerus capiendarum observationum nt, & quaeratur, quanta sit expectatio, seu quanta probabilitas, ut omnes existant foecundae, exceptis primo nulla, dein una, duabus, 3, 4 &c. sterilibus. Quandoquidem autem in qualibet observatione praesto sunt ex hyp. t casus, eorumque r foecundi & s steriles, & singuli casus unius observationis cum singulis alterius combinari, combinatique rursus cum singulis tertiae, 4 tae &c. conjungi possunt, facile patet, huic negotio quadrare Regulam Annotationibus Prop. XIII. [*sic*, should be XII] primae Part. in fine subnexam, & ejus Corollarium secundum, quod universalem formulam continet, cujus ope cognoscitur, quod expectatio ad nullam observationem sterilem sit $r^{nt} : t^{nt}$, ad unam $\frac{nt}{1} r^{nt-1} s : t^{nt}$, ad duas steriles $\frac{nt(nt-1)}{1 \cdot 2} r^{nt-2} ss : t^{nt}$, ad tres $\frac{nt(nt-1)(nt-2)}{1 \cdot 2 \cdot 3} r^{nt-3} s^3 : t^{nt}$ & sic deinceps; adeoque (rejecto communi nomine t^{nt}) quod gradus probabilitatum seu numeri casuum, quibus contingere potest, ut omnia experimenta sint foecunda, vel omnia praeter unum sterile, vel omnia praeter duo, 3, 4 &c. sterilia, ordine exprimantur per $r^{nt}, \frac{nt}{1} r^{nt-1} s, \frac{nt(nt-1)}{1 \cdot 2} r^{nt-2} ss, \frac{nt(nt-1)(nt-2)}{1 \cdot 2 \cdot 3} r^{nt-3} s^3$, &c. ipsissimos nempe terminos potestatis nt binomii $r+s$, in Lemmatis modo nostris excussae: unde jam caetera omnia oppido manifesta sunt. Patet enim ex progressionis natura, quod numerus casuum, qui cum ns sterilibus experimentis nr foecunda adducunt, sit ipse terminus maximus potestatis M, utpote quem ns termini praecedunt, & nr sequuntur, per Lemm. 3."

20 "Omnia, quae sub Sole sunt vel fiunt, praeterita, praesentia sive futura, in se & objective summam semper certitudinem habent. De praesentibus et praeteritis constat; quoniam eo ipso, quo sunt vel fuerunt, non possunt non esse vel fuisse: nec de futuris ambigendum, quae pariter etsi non fati alicujus inevitabili necessitate, tamen ratione tum praescientiae tum praedeterminationis divinae non possunt non fore; nisi enim certo eveniant quaecunque futura sunt, non apparet, quo pacto summo Creatori omniscientiae & omnipotentiae laus illibata constare queat."

21 "Objiciunt primo, aliam esse rationem calculorum aliam morborum aut mutationum aeris; illorum numerum determinatum esse, horum indeterminatum & vagum. Ad quod respondeo, utrumque respectu cognitionis nostrae aeque poni incertum & indeterminatum; sed quicquam in se & sua natura tale esse, non magis a nobis posse concipi, quam concipi potest, idem simul ab Auctore naturae creatum esse & non creatum: quaecunque enim Deus fecit, eo ipso dum fecit, etiam determinavit."

22 While I do not agree with every part of Daston's analysis, her basic point is soundly established, namely that there is no real chance in Jacob Bernoulli's universe.

23 Cf. (*ibid.*, 211), immediately following the passage quoted at note 19: "Let others dispute how this certainty of future occurrences may coexist with the contingency and freedom of secondary causes; we do not wish to deal with matters extraneous to our goal."

24 "Unde tandem hoc singulare sequi videtur, quod si eventuum omnium observationes per totam aeternitatem continuarentur, (probabilitate ultimo in perfectam certitudinem abeunte) omnia in mundo certis rationibus & constanti vicissitudinis lege contingere deprehenderentur; adeo ut etiam in maxime casualibus atque fortuitis quandam quasi necessitatem, &, ut dicam, fatalitatem agnoscere teneamur; quam nescio annon ipse jam Plato intendere voluerit, suo de universali rerum apocatastasi dogmate, secundum quod omnia post innumerabilium seculorum decursum in pristinum reversura statum praedixit."

25 For Latin see note 1 above.

26 On the urn model, cf. Daston (1988, 237 ff.)

27 Cf. Bernoulli (1686, 238): "Animadverto, subducto calculo, peritias Colllusorum esse incommensurabiles inter se, id est, veram illorum rationem nullo numero posse exprimi, tametsi id fierit prope verum possit." This incommensurability would follow if it was assumed that one player could concede the other a certain number of points in a fair game and then asked how much better the first player must be than the second if this concession of points makes their chances of winning the game equal.

28 Cf. Leibniz (GM, III, 1, 78; letter of Bernoulli to Leibniz, October 3, 1703):

> "Unde jam determinare possum, quot observationes instituendae, ut centies, millies, decies millies etc. verisimilius (adeoque tandem ut moraliter certum) sit, rationem inter numeros casuum, quam hoc pacto obtineo, legitimam et genuinam esse."

In (1713, 227), Bernoulli uses the word "inventam," or "found." Earlier (225), he uses the word "detexerit," and (226), he uses the word "determinare."

29 Cf. Stigler (1986), 66:

> "Bernoulli [...] dealt only with the case where the numbers of fertile cases (r) and sterile cases (s) were integers, not with the modern situation in which the proportion $\frac{r}{r+s}$ is allowed to range over all real numbers in the interval [0,1]. His aim was to show that, in essence, the exact ratio $\frac{r}{r+s}$ could be recovered with 'moral certainty' for a sufficiently large N [...]. He did view the ratio $\frac{r}{r+s}$ as possibly an approximation to the real state of affairs, and he knew that r and s were not identifiable ($r' = 10r$ and $s' = 10s$ would give the same ratio as r and s). But up to the order of approximation determined by a given $r+s$ he sought to determine the ratio exactly, as his statements and examples make clear."

Hacking, (1975, 158) assumes that Bernoulli uses the observed ratio of cases after some number of trials plus or minus some small error term as an "estimator" of the *a priori* probability of the outcome, and he is concerned that Bernoulli, not having a notation for conditional probability, has confused the probability that the *a priori* probability p falls within a small range around the observed ratio given that prior probability p, with the probability that the p falls within that same small range around the observed ratio given that observed ratio. When he quotes the passage in Bernoulli's letter to Leibniz quoted in the previous note, (163), reads his interpretation into the text by translating: "that the ratio between the number of cases which I estimate is legitimate and genuine [*i.e.* within some allowed error]." While Leibniz and Bernoulli in their letters (April 1703, October 1703) do write of estimating probabilities ("*doctrina de probabilitatibus aestimandis*"), they are thinking of epistemic probabilities rather than directly of frequencies. *The Emergence of Probability* has been very influential, but here and elsewhere Hacking reads later issues back into earlier authors in a way that creates rather than solves problems of understanding what the historical actors intended.

30 Cf, Bernoulli (1713, 226): "Si loco urnae substituamus aerem, ex. gr. sive corpus humanum, quae fomitem variarum mutationum atque morborum intra se, velut urna calculos, continent [...]." Cf. Leibniz, (GM, III, 1, 88). So the diseases could be the cases, but apparently Bernoulli is thinking that some diseases are more common than others, so that some would count for more cases.

31 "Vous me demandez, si ces Propositions renferment quelque realité qui puisse être démontrée, ou si elles ne sont fondées que sur de pures conjectures faites en l'air, et qui n'ont rien de solide; ne pouvant pas concevoir, à ce que vous dites, que l'on puisse mesurer les forces des joüeurs par nombres, et encore moins en tirer toutes les conclusions, que j'en ay tirées."

32 "Qu'il n'en est pas de même des jeux, qui dépendent uniquement, ou en partie, du genie, de l'industrie ou de l'adresse des joueurs, tels que sont les jeux de la paume, des échecs, & la pluspart des jeux de cartes; étant bien visible, que l'on ne sauroit déterminer par les causes, ou *à priori*, comme l'on parle, de combien un homme est plus savant, plus adroit ou plus habile qu'un autre, sans avoir une parfaite connoissance de la nature de l'ame, & et la disposition des organes du corps humain, laquelle mille causes occultes, qui y concourent, rendent absolument impossible. Mais cela n'empêche pas, qu'on ne puisse le sçavoir presque aussi certainement, *à posteriori*, par l'observation de l'événement plusieurs fois reiterée, en faisant ce qui se peut pratiquer dans les jeux même de pur hazard, lors qu'on ne sçait pas le nombre des cas, qui peuvent arriver."

33 Cf. Bernoulli (1713, new numeration, 3):

> "Je juge par là, avec assez de certitude, que le premier est deux ou trois fois meilleur joüeur que l'autre, ayant pour ainsi dire deux ou trois parties d'adresse, comme autant de cas ou de causes qui luy font gagner la bale, là où l'autre n'en a qu'une."

34 Cf. Hald (1990, 263):

> "His proof was worked out at the latest in 1690. It must have seemed rather unsatisfactory, to Bernoulli himself as well, when he included it in the manuscript 15 years later in view of the fact that the integral calculus had been developed in the meantime. In 1705 it would have been natural to evaluate the areas (sums) by means of integrals instead of limiting ordinates [...]. The need for a revision of the proof may have been another reason for Bernoulli's hesitation to publish."

I know of no evidence that Bernoulli thought his proof unsatisfactory.

35 John Arbuthnot, in his (1692), a work incorporating an English translation of large parts of Huygens's *De ratiociniis in ludo aleae*, makes this point in his Preface:

> "All a wise Man can do in such a Case is, to lay his Business on such Events, as have the most or most powerful second Causes, and this is true both in the great Events of the World, and in ordinary Games [...] that only which is left to me, is to wager where there are the greatest number of Chances, and consequently the greatest probability to gain [...] and tho it is possible, if there are any Chances against him at all, that he may lose, yet when he chuseth the safest side, he may part with this Money with more content (if there can be any at all) in such a Case."

CARLOS ALVAREZ JIMENEZ

MATHEMATICAL ANALYSIS AND ANALYTICAL SCIENCE*

I Introduction

The development of physical sciences during the eighteenth century is inconceivable without also taking into account a major development in mathematical analysis itself. The birth of a new analytical mechanics, of a new physical theory of sound, of a new capillarity theory or, at the beginning of nineteenth century, of the theories of heat and elasticity, all follow nearly the same procedure consisting in the definition of variables and equations to describe the phenomenon. This treatment of physical sciences, that took a long way from purely descriptive approaches, or even of geometrical models, has been qualified as "analytical". This style can be found in the great scientific treatises of eighteenth century as a kind of "method" of approaching natural phenomena.

At first glance it can be said that a physical science becomes "analytical" as soon as mathematical analysis is used to express the equations describing the physical phenomenon. This way of mathematisation contrasts with a previous one, where the description was done by geometry, for example, as was the case in the first science of movement by Galileo or even with Newton's *Principia* whose underlying mathematical style can be considered as a sort of "geometry of limit positions"(De Gant 1986). But the role played by mathematical analysis in the new physical theories is more than just a way for expressing physical concepts that were previously defined; the algorithms through which this new style of mathematisation is realized become also the means for the constitution of the concepts for an analytical science. The role played by mathematical analysis, as the privileged mathematical mean to describe a wide scope of physical phenomena, includes the first descriptions in mechanics up to those of heat theory. In the preface of his *Théorie Analytique de la Chaleur*, Fourier describes this wide application of mathematical analysis:

> "Les équations analytiques, ignorées des anciens géométres, que Descartes a introduites le premier dans l'étude des courbes et des surfaces, ne sont pas restreintes aux propriétés des figures, et à celles qui sont l'objet de la mécanique rationnelle; elles s'étend à tous les phénomènes généraux. Il ne peut y avoir de langage plus universel et plus simple, plus digne d'exprimer les rapports invariables des êtres naturels.

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 103–145.

Considérée sous ce point de vue, l'analyse mathématique est aussi étendue que la nature elle-même; elle définit tous les rapports sensibles, mesure les temps, les espaces, les forces, les températures; cette science difficile se forme avec lenteur, mais elle conserve tous les principes qu'elle a une fois acquis; elle s'accroît et s'affermit sans cesse au milieu de tant de variations et d'erreurs de l'esprit humain". (Fourier 1822, xiij-xiv)

But even if the so called analytical sciences, such as mechanics, probability theory or heat theory, come closer to a model where mathematical analysis plays the central role, it must be said that its particular status—concerning the model that it follows or the model that it imposes—and also the history of its birth and the history of its radical separation from the previous models of explanation, cannot fit into a general explanatory framework.

Let us take the example of mechanics. Since the Newtonian synthesis between celestial mechanics and terrestrial mechanics, it can be said that the two main scientific texts on mechanics from the eighteenth century, J. L. Lagrange's *Mécanique Analytique* (1788) and P. S. Laplace's *Mécanique Celeste* (1799-1725), represent the highest expression of that theoretical movement introduced by Newton in his *Philosophiae Naturalis Principia Mathematica* (1687). With this in mind it could be said that a common point of view ought to be shared by Lagrange and Laplace concerning their approach towards dynamics and its analytical treatment. But we find in Laplace one hypothesis that runs through his *Mécanique Celeste*, and also through other fields, such as capillarity, heat or light, that could hardly be found in Lagrange's texts: it is the hypothesis establishing that these phenomena are the result of the action through distance of certain attractive and repulsive forces between molecules. This model of explanation is clearly expressed in his historical notice of the XII book of his *Mécanique Celeste*:

"Au moyen de ces suppositions, les phénomènes de l'expansion de la chaleur et des vibrations des gaz sont ramenés à des forces attractives et répulsives qui ne sont sensibles qu'à des distances imperceptibles. Dans ma théorie de l'action capillaire, j'ai ramené à semblables forces les effets de la capillarité. Tous les phénomènes terrestres dépendent de ce genre de forces comme les phénomènes célestes dépendent de la gravitation universelle. Leur considération me paraît devoir être maintenant le principal objet de la philosophie mathématique." (Laplace, 1799-1825, V 99)

With Lagrange, on the other hand, the analytical treatment of phenomena seems to go against any hypothesis concerning any physical approach for them. Lagrange's *Mécanique Analitique* is a text where the formal expression of the main concepts, and the role they play therein, make possible the wide scope of applications they have. A remarkable example is given by the principle of "virtual velocities", treated as a kind of axiom of mechanics, which states that a system of forces is in equilibrium if these forces are in an inverse ratio to their virtual speed. Lagrange's general formulation of this principle states

"Si un système quelconque de tant de corps ou points que l'on veut tirés, chacun par des puissances quelconques, est en équilibre, et qu'on donne à ce système un petit mouvement quelconque, en vertu duquel chaque point parcoure un espace infiniment petit qui exprimera sa vitesse virtuelle, la some des puissances, multipliées chacune par l'espace que le point où elle est appliquée parcourt suivant la direction de cette même puissance, sera toujours égale à zero, en regardant comme positifs les petits espaces parcourus dans le sens des puissances, et comme négatifs les espaces parcourus dans un sens opposé." (Lagrange 1788, 11-12)

This principle is immediately expressed through a differential form:

$$Pdp + Qdq + Rdr + ... = 0$$

where $P, Q, R, \ldots$ are forces acting on different bodies and $dp, dq, dr, \ldots$ are the differentials of the quantities $p, q, r, \ldots$ which represent the line distances from the bodies where the forces act, to their centers of mass.

The great advantage of this formal expression for the principle of virtual velocities, is that in this way it might be used to solve all the problems that might appear towards equilibrium of forces. In this sense the principle plays the role of principle of unification of the, at least, static of solid bodies and the static of fluids. This unification will make use of a formal calculus particularly well adapted for this purpose, the calculus of variations, that will make possible the reduction of mechanics to analysis, before making the reduction of analysis to algebra. This theoretical reduction is already announced in the preface of the *Mécanique Analytique*:

"On a déjà plusieurs Traités de Mécanique, mais le plan de celui-ci est entiérement neuf [...]. Les méthodes que j'y expose ne demandent ni constructions, ni raisonnements géométriques ou méchaniques, mais seulement des opérations algébriques, assujetties à une marche régulière et uniforme." (Lagrange 1788, v-vi)

And he states also that

"Ceux qui aiment l'Analyse verront avec plaisir la Máchanique en devenir une nouvelle branche et ne sauront gré d'en avoir étendu ainsi le domaine." (Lagrange 1788, vi)

This way of understanding the analytic methods underlying mechanics as a sort of translation into algebraic means, could be identified with the synthesis which Descartes made between algebra and geometry.

At first glance it could be said that the analytical method can be declared as the inheritor of Cartesian thought: the subordination of geometry to algebra, a procedure well justified by *le Discours de la Méthode* and *la Géométrie*, states that the knowledge of geometric properties of bodies is obtained by an ascension in the order of magnitudes that, just as the order of reasons, follows a way moving from the complex to the simplest one. In this way algebra carries out the role of "reason" in its investigation of spatial "extension", and it also carries out a means of expression which, more than a mere description for phenomena, becomes the means for rational comprehension. By recognizing the origin of this tradition in

Descartes it is possible to say that the analytic methods all live in the theoretical frame of modernity created by him[1].

This treatment and diffusion of formal procedures within calculus, and therein through physical sciences, becomes an ideal which is more than a simple procedure to generalize certain properties; it is considered the most important means to propagate knowledge. A typical example for this is Condillac who conceived that "analytical" procedures were the most appropriate ones to guarantee an accord and fidelity with regards to the nature and methods of verification of ideas. But for the success of this project, a particular language able to transmit the research procedures as well as conceptual changes and transformations, was needed. This language is algebra, since, for Condillac, it is the only well-formed language where nothing is arbitrary (Dhombres 1982-1983).

We think that Laplace shares this point of view concerning the support that analytical-algebraic procedures give to the constitution of knowledge, as well. In his seventh lesson given at the *Ecole Normale*, and maybe because of the great influence that Condillac's thought had therein, Laplace states that

> "Pour bien connaître les propriétés des corps, on a d'abord fait abstraction de leurs propriétés, et l'on n'a vu en eux qu'une étendue figurée, mobile et impénétrable. On a fait encore abstraction de ces deux dernières propriétés générales en considérant l'étendue simple comme figurée. Les nombreux rapports qu'elle présente sous ce point de vue sont l'objet de la géométrie. Enfin, par une abstraction encore plus grande, on n'a envisagé dans l'étendue qu'une quantité susceptible d'accroissement et de diminution; c'est l'objet de la science des grandeurs en général, ou de l'arithmétique universelle, [...]. Ensuite on a restitué successivement aux corps les propriétés dont on les avait dépouillés; l'observation et l'expérience en ont fait connaître de nouvelles, et l'on a déterminé les nouveaux rapports qui naissaient de ces additions successives, en s'aidant toujours des rapports précédemment découverts. Ainsi, la mécanique, l'astronomie, l'optique, et généralement toutes les sciences qui s'appuient à la fois sur l'observation et le calcul, ont été créées et perfectionées. Vous voyes par là que ces sciences diverses s'enchaînent les unes aux autres, et qu'elles ont une source commune dans la science des grandeurs dont l'utile influence s'étend sur toute la philosophie naturelle. Cette méthode de décomposer les objets et de les recomposer pour en saisir parfaitement les rapports, se nomme analyse. L'esprit humain lui est redevable de tout ce qu'il sait avec précision sur la nature des choses." (Laplace LEN, 87)

So far we have talked only about those changes that took place within mechanics and its transformation into an analytical science, but what can be said about other *analytic sciences*? A quick look at Fourier's *Théorie Analytique de la Chaleur* shows clearly that the sense of what "analytic" means here—in what sense is the theory of heat an "analytic theory"—has partially changed. In Fourier's theory of heat there is no attempt to reduce the explanation of heat phenomena to algebraic deductions. Certainly the preface to the *Theorie Analytique* gives a clear idea of the role that mathematical analysis played in the general constitution of the theory, but it is also clear that in this treatise, mathematical analysis is by no means just a subset of algebraic methods. His approach to heat phenomena states that they are not reduced to mechanical theories, since they are not related with

the question of movement and of equilibrium of bodies, nor are they related with attractive or repulsive forces between bodies or molecules. This point of view, clearly different from "Laplacian molecularism" opens new horizons to mathematical physics: his main purpose is to give the mathematical description for the problem of diffusion of heat into a solid body, the question of transmission of heat from one body to another, the question of heat loss. In this sense the analytic questions to be solved are those of finding the correct expression of the "temperature function" v at each point of a solid body when a source of heat is applied at one point o of the body, the question of finding the heat flow after a time t, and the problem of the heat loss, after a time t, at each point of the body, when this source is no longer in contact and ceases its action over the body.

Considering that the value v of the temperature at each point of a body is given through a function $f(x, y, z, t)$ of the variables x, y, z which give the position of the point, and of the variable t which gives the time that a heat source has been in contact with one extreme of the body, the heat flow is given through the differential equation

$$\frac{dv}{dt} = \frac{K}{CD}\left(\frac{\partial^2 v}{\partial x^2} + \frac{\partial^2 v}{\partial y^2} + \frac{\partial^2 v}{\partial z^2}\right)$$

Where K gives the specific conductibility of heat of the body—the heat content transmitted through the body in a unity of time—C is the specific heat capacity—the necessary heat content needed to raise the temperature of a unity of the mass body from the temperature 0, the temperature of the melting ice, to temperature 1, the temperature of boiling water—and D is the density of the body. Now, considering that the heat flow is to be found using this equation, whose particular conditions justify the general solution given through a trigonometric (convergent) series, and considering Fourier's proof that not only this particular function of heat flow, but "any function" can be developed into a trigonometric series, it seems clear that the "mathematical analysis" working in this treatise is not to be identified with a branch of mathematics whose main advantage is its possible reduction to algebra. Already in the introduction to his *Théorie Analytique*, Fourier remarks that new methods, and not only "algebraic deductions", are needed in his treatise:

> "Les équations du mouvement de la chaleur, comme celles qui expriment les vibrations des corps sonores, ou les dernières oscillations des liquides, appartiennent à une des branches de la science du calcul les plus récemment découvertes, et qu'il importait beaucoup de perfectionner. Après avoir établi ces équations différentielles, il fallait en obtenir les intégrales; ce qui consiste à passer d'une expression commune, à une solution propre assujettie à toutes les conditions données. Cette recherche difficile exigeait une analyse spéciale, fondée sur des théorèmes nouveaux dont nous ne pourrions ici faire connaître l'objet. La méthode qui en dérive ne laisse rien de vague et d'indéterminé dans les solutions; elle les conduit jusqu'aux dernières applications numériques, condition

> néccessaire de toute recherche, et sans laquelle on n'arriverait qu'à des transformations inutiles." (Fourier 1822, xij)

It seems clear to us that between the two analytic treatises by Lagrange and Fourier respectively, the meaning, the role and the scope of "mathematical analysis" have changed. This change is not only related to any particular style of mathematisation, but it concerns the mathematical theory that constitutes the base and the possibility for all those analytical projects. In other words, we think that there have been some transformations in mathematical analysis, just like there have been some transformations in physical sciences.

In this text we will analyze some of the changes that took place in mathematical analysis in the period between these two analytic treatises, Lagrange's *Mécanique Analytique* and Fourier's *Théorie Analytique de la Chaleur*. However, we have to point out that we will not refer to the "underlying mathematics" of these two treatises; we will not refer to the *Calculus of Variations* nor to *Fourier's Series* or *Fourier's Analysis*. The problem we want to analyze is rather that of the emergence of some concepts of mathematical analysis, mainly those of "continuity" (of functions) and of "convergence" (of series), which determined the development of this branch of mathematics during the nineteenth century. We think that it is the emergence of these concepts which makes possible the dissolution of a link between "algebra" and "analysis", a link that is conceived, and valued by Lagrange, as a relation of "subordination" of the latter to the former. After the dissolution of this particular link, a new shape was given to mathematical analysis, creating a new branch of mathematics, valued "in itself" by Fourier.

The emergence and use of those new concepts will be followed through the evolution of mathematical analysis and the theory of functions, and it could be said that after their appearance algebra itself will not be able to overlook the new "analytic methods"[2].

II The Algebraic Foundation of Mathematical Analysis

Regarding the main transformations within mathematical analysis, it seems that the first great change in the eighteenth century was introduced by Euler, who made the concept of "function" the central one. The reorganization given by his *Introductio in Analysin Infinitorum* (1748) introduced a new attitude towards the field of "quantities". Up to that moment mathematical analysis had been conceived as a kind of algebra of infinitely small or vanishing quantities, out of which the mechanical or geometrical problems could be solved. Euler's *Introductio* gives a new treatment for quantities—constant, variable, infinitely small or infinitely large—through those "calculus expressions" which are "functions". The field of quantities is conceived as being formed out of constant and variable quantities—

which are "like the gender or the species towards the individual" (*ibid.*, I, 4). With a variable quantity it is possible to define another variable quantity through "an analytical expression made out of this quantity and other constant quantities" (*ibid*). The variable quantity obtained by this procedure is a "function" of the first variable quantity, and functions are classified according to the analytical procedures used to define them. The *Introductio* is above all a treatise that intends to give a complete classification for functions, and through them a classification of curve lines. It is in this general scope that algebra becomes the privileged mean to express a function and to develop the theory of functions itself. This algebraic treatment of functions, that constitutes a new branch of mathematical analysis namely "algebraic analysis", became a necessary background that preceded infinitesimal calculus.

Now, even if the main trends for Euler's mathematical analysis are to be found in the algebraic treatment of functions, it must be pointed out that the algebraic form is above all the way through which a variable quantity is transformed in order to define a function, and so a function is more than just an equation through which an unknown quantity is to be found. As a variable quantity, a function runs through different values, depending on the values given to the variable quantity. A function Z of the variable z might be "algebraic" or "transcendent".

> "The first ones are obtained through variable quantities that are combined among them by using only the common algebraic operations; the second ones depend on other operations [...]." (*ibid.*, I, 5-6)

Algebraic functions might be "irrational" or "rational", according to whether the variable z is submitted to root operations or is free of them. Another distinction between functions is given after the first one: "rational" functions are always "uniform"—only one value for the function is obtained for each value given to the variable quantity—while "irrational" functions are always "multiform"—many different values for Z might be obtained for each value given to the variable quantity. Now the way in which the quantity Z takes different values, as the variable z runs through different values, is given precisely through an algebraic, analytic, expression. If the algebraic expression is such that Z is a "multiform" function, it might happen not only that for some values of z, Z takes two or more different values, but also that for some values of z, Z might be no longer a real but an imaginary quantity. In this case, the way in which the quantity Z takes its corresponding different (possibly manifold) values might not follow a "continuous course" in the domain of quantities. But for uniform functions, Euler considers valid, because of its algebraic form, the following property: if a uniform function $Z(z)$ takes, for $z = a$, the value $Z = A$, and for $z = b$, the value $Z = B$, then while the variable quantity z runs through the values between a and b, the function Z must take, at least ones, each value between A and B. Euler's argument states that

"Since Z is a uniform function of z, for every real value of z, the function Z takes also a real value, and if the quantity Z, in the first case, when $z = a$, takes the value A, and in the second case, when $z = b$, the value B; then Z could not run from A to B without passing through all the intermediate values. Then if the equation $Z-A = 0$ and the equation $Z-B = 0$, have a real root, the equation $Z-C = 0$ will have also one whenever C lies between A and B." (*ibid.*, I, 20)

With this general property for uniform functions, Euler proves that for a uniform function Z of z, whose highest exponent is an odd number $2n+1$, the function Z has at least one real simple factor. In his proof, besides the intermediate value property, he uses a formal calculus for infinite quantities as if they were any real and finite quantities:

If the function Z is of the form

$$z^{2n+1} + pz^{2n} + qz^{2n-1} + rz^{2n-2} + \dots$$

when $z = \infty$, all the terms disappear in relation to the first one, and the function takes the form $Z = (\infty)^{2n+1} = \infty$; but when $z = -\infty$, the function takes the form $Z = (-\infty)^{2n+1} = -\infty$. Now for any real value C, since C lies between $-\infty$ and ∞, the theorem states that Z cannot run from $-\infty$ to ∞ without passing through C. That means that the equation $Z-C = 0$ has a real root. If $C = 0$ the conclusion is that the function Z has a real simple factor $(z-c)$, where c lies between $-\infty$ and ∞.

Before giving the intermediate value property, Euler stated the two following properties for an entire function:

1. The function given through an algebraic expression of the form

$$z^n + pz^{n-1} + qz^{n-2} + rz^{n-3} + \dots$$

is equal to the product of n simple (linear) factors[3].

2. The simple factors might be real or imaginary, but the imaginary simple factors are always in even number.

Clearly these two properties were sufficient to proof that an entire function whose highest exponent is an odd number has at least one real root, but as we have seen, Euler used the intermediate value property, as if some hidden reason, not explained in his *Introductio*, made the conclusion without this argument illegitimate.

Concerning properties 1 and 2, the first one is obtained directly from the statement that any equation of nth degree has n roots; the second one establishes that imaginary roots are always in even number. For the second property Euler gave no general proof, nevertheless he realized that in some sense this property was closely related with the fact that a polynomial is equal to the product of simple or double real factors. The only argument given by him to support this statement goes as follows: first he assures that if a function $f(x)$ has two simple imaginary factors, then the product of these two factors is a real double factor: without any hypothesis concerning the nature of imaginary roots[4], Euler states that if $P(x)$ denotes the product of the simple real factors of $f(x)$—and so $P(x)$ is real of degree $(n-2)$—, the product of the two imaginary factors is $\dfrac{f(x)}{P(x)}$ which is a real double factor. After this he assures that if a function is the product of four simple imaginary factors, then it can be given as the product of two double real factors; to prove this fact he takes as imaginary quantities those of the form $a + b\sqrt{-1}$. Once this property for functions that are the product of four imaginary factors is proven, Euler makes a generalization: for a function Z of the variable z it is always possible to combine in couples the imaginary factors to obtain a (double) real factor[5], For this argument Euler states simply that

"If there are only two imaginary factors, it is clear that their product will be real, and if there are four imaginary factors their product, as we have seen, can be given as the product of two double real factors of the form fz^2+gz+h. Even if the same proof is not valid for higher powers, it seems clear enough that this property holds for any number of factors, so that instead of $2n$ simple imaginary factors, there will be n double real factors. So any entire function of the variable z is equal to the product of simple or double real factors. If the truth of this proposition is not proved here completely, it will soon become stronger."[6] (*ibid.*, I, 19)

After this argument, which cannot be considered as a proof for the general case, Euler shows, using the two facts: that a polynomial of odd degree has at least a real root, and that those polynomials which are equal to the product of four imaginary factors are equal to the product of two double real factors, that the polynomials

$$a + bz^n + cz^{2n} + dz^{3n}$$

$$a + bz^n + cz^{2n} + dz^{3n} + ez^{4n}$$

$$a + bz^n + cz^{2n} + dz^{3n} + ez^{4n} + fz^{5n}$$

accept the same factorisation by real or double simple factors. These cases confirm the hypothesis that any entire function—any polynomial—is equal to the product of simple or double real factors[7].

"So if there were still some doubts concerning the factorisation of any entire function, they should vanish almost completely." (*ibid.*, I, 117)

In any case, as it will be clearly admitted by Lagrange (1798, note I, 111-113), the proof about the factorization of any polynomial in real factors, the hypothesis about the even number of the imaginary roots, and therefore the nature of the imaginary roots[8], is based on the property that any equation of an odd degree has a real root. Euler considered that the purely algebraic conclusion from the equality in number of roots and the degree of the equation, and of the fact that imaginary roots are always in an even number, could not be used as an argument to prove that an equation of odd degree has a real root. For Euler, the intermediate value property appears already as one which algebra could not ignore.

The intermediate value property which Euler proves for uniform-rational functions explicitly rests on the assumption that once the variable quantity Z has reached two different (real) values, it should run through all the values between them. Two facts of different kind are involved here; first the fact that Z is a uniform-rational function: because of the algebraic nature of the function Z—no roots for the variable z appear—while z runs through all the real values, Z takes only real values and no "jumps" might occur in this case, since the only possibility for a jump is when an irrational or multiform function takes imaginary values. Considering the general algebraic form for a multiform function Z of z:

$$Z^n + PZ^{n-1} + QZ^{n-2} + RZ^{n-3} + \ldots = 0 \tag{1}$$

where P, Q, R, ... are uniform functions of z, the different values of Z are given through the different n roots of the polynomial, but in this case each "root" of the equation is a function of z that might take only real values, or is a function that might take imaginary values for some values of z. Euler gives the example of a "biforme" function $Z^2-2PZ+Q = 0$ (where P and Q are uniform functions of z), where for each value of z the two values of Z are given, the first one by $Z_1(z) = P+\sqrt{P^2-Q}$, and the second one by $Z_2(z) = P-\sqrt{P^2-Q}$. So if the uniform function P is such that for every value of z $P^2 > Q$, the two values of Z are always real; but for those z where $P^2 < Q$, the values of Z will be imaginary. And he asserts, again from the intermediate value property, that when both conditions hold and there are some values of z such that $P^2 > Q$, and some other values of z such that $P^2 < Q$, then there must exist at least one value of z between them, such that $P^2 = Q$. In this case the two values of Z coincide and are given through the function P. From the algebraic theory of equations Euler assures that if n is an odd number, at least one of the root-functions is a uniform real function, and whenever a value of z gives an imaginary value for one of the root-functions, this same value of z will give imaginary values for at least another (always in even number) root-function. So if $Z(a) = A$ and $Z(b) = B$, but Z does not take the value C which lies between A and B, it is because while the variable z runs from a to b, Z takes imaginary values.

The second fact related with the intermediate value property deals with the nature of "variable quantities": since they are magnitudes which include all determined quantities, it is in their nature to take all values between two fixed ones.

That is why in geometry a variable quantity is representated correctly by a straight line, and a function can be represented by a curved line: a line all of whose points take as abscissa a value of z, and as ordinate the corresponding value(s) of Z. The remarkable fact in Euler's geometric interpretation of functions (when for each value of z the corresponding value of Z is given, then by taking the first one as abscissa and the second as ordinate, a line is obtained) is that a (curved or straight) line is obtained here with "all" the points out of which it is assembled; this makes possible to study geometric curves independent of the idea of "mouvement" or "fluxion" of a point. With this approach even "mechanical curves" might be studied as formed by functions.

> "Even if we can describe mechanically many curve lines by the continuous mouvement of a point which presents the whole curve to our sight, we will consider them as obtained by functions. This approach is more analytic, more general and appropriate to calculation. In this way any function of z will give some straight or curve line and, conversely, any curve line will be related to a function." (Euler 1748, II, 6)

When the function Z is uniform, the curve representing it, will be produced continuously and indefinitely, and at any point of the horizontal axis representing the values of the variable z, a perpendicular line will cut the curve exactly at one point. When the function is multiform, and is given by a polynomial of the previous general form, the curve representing it might be intercepted by a perpendicular straight line in n, $n-2$, $n-4$, ... points; making certain that if n is an odd number, any perpendicular will intercept this curve at least once; but when n is an even number, it may happen that at some points of the horizontal axis a perpendicular line does not intercept the curve representing the function at all, making clear that the intermediate value property "might" fail in this case.

Euler is certain that the intermediate value property depends only on the algebraic nature of the function: if the property fails it is because function Z takes also imaginary values. Besides, "continuity" for functions and for "curves" is conceived by him as a property related with the permanence of the analytic expression: no matter how the curve that represents it looks like, a function (and the curve) is continuous whenever it is obtained through a single analytic expression. That means that "continuity" is a property that is ruled by "analysis"—through the analytic expression—and not by geometry[9]. For a multiform function, even if the curve related to it might be formed by different branches and the intermediate value property does not hold, it is considered by him as a continuous curve (generated by one analytic expression). On the other hand, "discontinuous" curves are for him "mixed" curve, obtained with two or more different functions.

For Euler the way in which a variable quantity runs between two fixed values needs no further description to guarantee the fact that it does it "continuously". Considered as a variable quantity, the variable z bears no "jump" nor any "gap" in the domain of real quantities; and the same happens with the function Z, as long as it remains in the domain of real quantities. For Euler there is no need to state that if the variable z runs "continuously", the analytic law which defines Z also makes it follow a "continuous" path through the values it takes[10].

The continuity of functions—in Euler's sense—is a question that cannot be generally answered just by stating that a function is a variable quantity obtained from another (variable) quantity through an analytic expression; the analytic form has to be given in such a way that the permanence of the analytic expression could be identified without any doubt. Considering the classification given by Euler at the beginning of his *Introductio*, it seems that for algebraic-rational functions there is no problem at all: the polynomial form becomes the mean to express them. For algebraic irrational functions and for transcendent functions the generalization from polynomials to infinite power series becomes a necessary step to be given. Through the infinite power series it might be said that the difference between algebraic and transcendent functions almost vanishes: the possibility to reduce those functions which require the transcendental operations (mainly the logarithmic and the exponential functions) to power series makes them appear as "continuous" (always in Euler's sense) functions, too.

After having analyzed some features of Euler's *continuity of functions*, let us look closely at the expression of a transcendent function in power series. Two general hypotheses concerning the nature of real quantities, are made to justify the development of the logarithmic function in power series: first a formal calculus for infinitely small and infinitely large quantities is used as a generalization of the calculus for finite quantities; secondly a general hypothesis about finite quantities: the assumption that they all can be obtained as the product of an infinitely small and an infinitely large quantity. For the calculation of the power series for the logarithmic function another main algebraic principle is used by Euler: Newton's binomial formula for the case where the exponent is any real quantity. This formula, admitted without proof[11], is here justified as the result of a formal procedure that is already valid in the case of a positive integer exponent.

The series for the logarithmic function will be calculated also by Lagrange and Cauchy, and we will analyze the solutions given by them as a paradigmatic example that will help us to better understand the changes that took place within algebraic analysis from Euler's *Introductio* to Cauchy's *Cours d'Analyse*.

Starting from an arbitrary quantity a and an infinitely small quantity ω, since a^ω is > 1 if $a > 1$, then $a^\omega = 1+\psi$, where ψ is another infinitely small quantity. It is possible to write the last one as a function of the first one: $\psi = k\omega$ and $a^\omega = 1+k\omega$. If L denotes the characteristic for logarithms of base a, then $\omega = L(1+k\omega)$ and $i\omega = L(1+k\omega)^i$, and since "it is clear that when the number i increases, the value $(1+k\omega)^i$ goes beyond the value of the unity (*ibid.*, I, 88)", then $(1+k\omega)^i = (1+x)$ and $i\omega = L(1+k\omega)^i = L(1+x)$. Starting from $(1+k\omega)^i = (1+x)$, Euler states

$(1+k\omega) = (1+x)^{\frac{1}{i}}$, and then, by a simple algebraic substitution, $i\omega = \frac{i}{k}\left[(1+x)^{\frac{1}{i}}-1\right]$. By developing the term inside the parenthesis through Newton's formula

$$L(1+x) = i\omega = \frac{i}{k}\left[(1+x)^{\frac{1}{i}}-1\right] =$$

$$\frac{i}{k}\left[\left(1+\frac{x}{i}-\frac{(i-1)x^2}{i\cdot 2i}+\frac{(i-1)(2i-1)x^3}{i\cdot 2i\cdot 3i}-\frac{(i-1)(2i-1)(3i-1)x^4}{i\cdot 2i\cdot 3i\cdot 4i}+\ldots\right)-1\right].$$

When i becomes an infinitely large quantity, Euler establishes that a quotient of the form $\frac{ni-1}{(n+1)i}$, becomes equal to $\frac{n}{n+1}$ and so he finally gets

$$L(1+x) = \frac{1}{k}\left(x-\frac{x^2}{2}+\frac{x^3}{3}-\frac{x^4}{4}+\ldots\right) \tag{1}$$

From the equality $a^\omega = 1+k\psi$ Euler gets $a^{i\omega} = (1+k\omega)^i$ for any value i; and from Newton's binomial formula this one is equal to

$$1+ik\omega+\frac{i(i-1)}{2}k^2\omega^2+\frac{i(i-1)(i-2)}{2\cdot 3}k^3\omega^3+\frac{i(i-1)(i-2)(i-3)}{2\cdot 3\cdot 4}k^4\omega^4+\ldots$$

Euler takes a finite number z and makes $i = \frac{z}{\omega}$; so that number i be infinitely large. From this $a^{i\omega} = (1+k\omega)^i = \left(1+k\frac{z}{i}\right)^i$. And again from Newton's formula

$$\left(1+k\frac{z}{i}\right)^i = 1+kz+\frac{(i-1)}{2i}k^2z^2+$$

$$\frac{(i-1)(i-2)}{2i\cdot 3i}k^3z^3+\frac{(i-1)(i-2)(i-3)}{2i\cdot 3i\cdot 4i}k^4z^4+\ldots$$

Since i is an infinitely large quantity, the value of a quotient $\frac{(i-n)}{(n+1)i}$ becomes equal to $\frac{1}{n+1}$, so the series takes the value

$$a^{i\omega} = 1 + kz + \frac{k^2z^2}{2} + \frac{k^3z^3}{2\cdot 3} + \frac{k^4z^4}{2\cdot 3\cdot 4} + \ldots$$

When $i\omega = 1$, the expression

$$a = 1 + k + \frac{k^2}{2} + \frac{k^3}{2\cdot 3} + \frac{k^4}{2\cdot 3\cdot 4} + \ldots \tag{2}$$

gives the relation between the values a and k. With relation (2) it is possible to state that $a = e^k$, and so $k = \ln(a)$. The series expansion (1) for the logarithmic function is then equal to $L(1+x) = \frac{1}{\ln(a)}\left(x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4} + \ldots\right)$.

Euler's proof is a good example of what a formal procedure, given under the general frame of analytic thought, looks like: the series development for the logarithmic function is obtained through a purely algebraic calculus where the rules for the infinitely small and infinitely large quantities, and also a purely formal justification for Newton's formula, completely fill any conceptual gap that might appear.

Lagrange's point of view concerning algebraic analysis is close to Euler's ideas about the role that algebra has to play in the development of the theory of functions. The intermediate value property will play an important role in relation with the nature of the roots of algebraic equations; but it will play a central role also in the calculation of the remainder of an infinite series, out of which this series could be replaced by a finite polynomial. Besides these facts, the continuity property plays an important role in the proof of the binomial formula as a special case of the Taylor series.

In his *Discours sur l'objet de la théorie des fonctions* (Lagrange 1799), a short but deep manifesto for algebraic analysis, he states that the foundations of mathematical analysis are to be given by the new discipline defined through its relation with algebra: theory of functions. In a sense, Lagrange states that algebra is precisely a theory of functions, since those quantities algebra deals with appear as functions of other quantities. Through this theory of functions differential calculus becomes a particular branch that will no longer need to consider infinitely small quantities, vanishing quantities or fluxions: the methods introduced through the calculus of these infinitely small quantities just try to find out the first terms of the infinite power series development for the function.

> "Il est [...] plus naturel et plus simple de considérer immédiatement la formation des premiers termes du développement des fonctions, sans employer le circuit métaphysique des infiniments petits ou des limites; et c'est ramener le Calcul différentiel à une origine purement algébrique, que de le faire dépendre uniquement de ce développement." (*ibid.* 1799, 234)

This algebraic style rules not only over the power series development of functions, but introduces, above all, a "canonical form" that resumes in itself the reduction of mathematical analysis to algebra. In his *Théorie des Fonctions Analytiques*, all the possible applications of the analytic theory of functions are already contained in the canonical expression for a function given by its Taylor series: it is possible to proceed from the formal expression to the geometrical and mechanical domains. It is also through its formal nature that the theory of analytic functions includes all possible kinds of calculus; not only differential calculus, but also the calculus of variations, "this type of calculus which does not require a new analysis but only a special application of the theory of functions" (Lagrange 1797, 200-201).

Using this approach, Lagrange's theory of functions completes a theoretical program that includes mechanics and the calculus of variations as two moments to give a reduction of mechanics to a purely algebraic reasoning[12].

From the development in power series for the function $f(x+i)$: $f(x+i) = f(x) + ip(x) + i^2q(x) + i^3r(x) + \ldots$ [13], Lagrange obtains the canonical development given through the derived functions[14]

$$f(x+i) = f(x) + if'(x) + \frac{i^2}{2}f''(x) + \frac{i^3}{3\cdot 2}f'''(x) + \ldots \tag{3}$$

It is through this canonical form that Lagrange calculates the series development for the binomial formula and for the exponential and the logarithmic function $L(1+x)$. In order to give a proof for the binomial formula, Lagrange tries to give the development of $(1+x)^m$ with a power series as an application of the canonical form for the power function $f(x) = x^m$, since then $f(x+i) = (x+i)^m$. In this way, "by the simple rules of arithmetic or the first operations of algebra" (*ibid.*, 15) it is possible to show that the first two terms of $(x+i)^m$ are $x^m + mix^{m-1}$, so clearly, by equating with the series (3), $f'(x) = mx^{m-1}$, and he obtains[15]:

$$(x+i)^m = x^m + imx^{m-1} + \frac{i^2}{2}m(m-1)x^{m-2} + \frac{i^3}{3\cdot 2}m(m-1)(m-2)x^{m-3} + \ldots \quad (4)$$

In this way Newton's binomial formula (4) is obtained through the "canonical form" for the power function. Once "the first operation of algebra" led him to the first derived function, the series (3) justifies all the rest; there is no need to fall back on the principles of differential calculus for a justification of this formula.

Lagrange considers that formula (4) is valid for every rational number m, but in order to consider it valid when the exponent is any real number, two implicit assumptions are made: first an assumption about the "dense" distribution of rational numbers, secondly the assumption that considering the exponent as a variable, the power function behaves as a continuous function[16]:

> "Comme tout nombre irrationnel peut être renfermé entre des limites rationnelles aussi resserrées que l'on veut, on en pourrait conclure tout de suite la vérité du résultat précédent pour une valeur quelconque irrationelle de m, puis qu'on peut, en resserrant les limites, diminuer l'erreur à volonté." (Lagrange 1806, 16)

Once this binomial formula is proved, the series for the exponential and the logarithmic functions can be obtained. For the function $f(x) = a^x$, $f(x+i) = a^{x+i} = a^x \cdot a^i$, the problem is now to find the first two terms of the series for a^i. By putting $a = 1+b$, and by the binomial series, Lagrange gets:

$$a^i = (1+b)^i = 1 + ib + \frac{i(i-1)}{2}b^2 + \frac{i(i-1)(i-2)}{2\cdot 3}b^3 + \ldots$$

So after developing the products and rearranging the series for the increasing powers of i, it is easy to see that

$$a^i = (1+b)^i = 1 + i\left(b - \frac{b^2}{2} + \frac{b^3}{3} - \frac{b^4}{4} + \ldots\right) + \ldots$$

With these two first terms, Lagrange states that $a^{x+i} = a^x \cdot a^i = a^x(1+iA+\ldots)$, where $A = b - \frac{b^2}{2} + \frac{b^3}{3} - \frac{b^4}{4} + \ldots$, so by the development (3), he gets $f'(x) = Aa^x$,

and the algorithm to find the derived functions gives $f''(x) = A^2a^x$; $f'''(x) = A^3a^x$; with these functions, the complete series can be obtained:

$$f(x+i) = a^{x+i} = a^x\left(1 + Ai + A^2\frac{i^2}{2} + A^3\frac{i^3}{3\cdot 2} + \ldots\right)$$

From this equality, after dividing by a^x and changing i for x he obtains

$$a^x = 1 + Ax + A^2\frac{x^2}{2} + A^3\frac{x^3}{3\cdot 2} + \ldots \quad (5)$$

When $x = 1$, the value for a is given by $a = 1 + A + \frac{A^2}{2} + \frac{A^3}{3\cdot 2} + \ldots$

For the value $x = \frac{1}{A}$ $a^{\frac{1}{A}} = 1 + 1 + \frac{1}{2} + \frac{1}{3\cdot 2} + \frac{1}{4\cdot 3\cdot 2} + \ldots$ which is the number e; $a^{\frac{1}{A}} = e$ or $a = e^A$. Clearly $\frac{1}{A} = L(e)$ and $A = \ln(a)$; so $a = e^A = e^{\ln a}$. When $f(x) = e^x$ the series (5) gives $e^x = 1 + x + \frac{x^2}{2} + \frac{x^3}{3\cdot 2} + ..$ since in this case the value $A = \ln(e) = 1$.

By introducing now as a new function $f(x) = L(x)$, then $x = a^{f(x)}$, and $f(x+i) = L(x+i)$, so $x+i = a^{f(x+i)}$. Again, the series development is solved once the derived functions are found; in this case Lagrange finds[17] $f'(x) = \frac{1}{xA}$. By putting this last function in the form $f'(x) = \frac{1}{A}x^{-1}$, the algorithm already found for the derivation of a function of this form, gives $f''(x) = -\frac{1}{A}x^{-2} = -\frac{1}{Ax^2}$; $f'''(x) = \frac{2}{A}x^{-3} = \frac{2}{Ax^3}, \ldots$. The development (3) gives:

$$L(x+i) = L(x) + \frac{i}{Ax} - \frac{i^2}{2Ax^2} + \frac{i^3}{3Ax^3} - \ldots$$

By making $x = 1$ and putting x instead of i he finally gets:

$$L(1+x) = \frac{1}{A}\left(x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4}\ldots\right) = \frac{1}{\ln(a)}\left(x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4}\ldots\right)$$

since $A = \ln(a)$. This is the same series development already given by Euler.

With this series, Lagrange tries to find the logarithm of any real quantity y. By making $y = 1 + x$, the series for the logarithm gives

$$L(y) = \frac{1}{\ln(a)}\left((y-1) - \frac{(y-1)^2}{2} + \frac{(y-1)^3}{3} - \frac{(y-1)^4}{4}\ldots\right) \tag{6}$$

which is a convergent series only for those values of y "which are close to the unity" (Lagrange 1797, 20). So Lagrange now comes to the problem of finding the logarithm of any quantity y, even if it is not so close to the unity; that means, even if the series (6) does not converge. Since it is always possible to find another quantity r, big enough, so that $z = \sqrt[r]{y}$ is close to the unity, a new convergent series can be find to calculate the logarithm, no matter how big the quantity y might be. The series for this quantity z is

$$L(z) = \frac{1}{\ln(a)}\left((z-1) - \frac{(z-1)^2}{2} + \frac{(z-1)^3}{3} - \frac{(z-1)^4}{4}\ldots\right)$$

so that

$$L(z) = L(y)^{\frac{1}{r}} = \frac{L(y)}{r} = \frac{1}{\ln(a)}\left((z-1) - \frac{(z-1)^2}{2} + \frac{(z-1)^3}{3} - \frac{(z-1)^4}{4}\ldots\right)$$

and from this Lagrange gets

$$L(y) = \frac{r}{\ln(a)}\left(\left(\sqrt[r]{y}-1\right) - \frac{\left(\sqrt[r]{y}-1\right)^2}{2} + \frac{\left(\sqrt[r]{y}-1\right)^3}{3} - \frac{\left(\sqrt[r]{y}-1\right)^4}{4}\ldots\right) \tag{7}$$

Clearly Lagrange's aim is to make possible the transit from the formal expression of a series, obtained from the general development (3), to the numerical value of a function[18]. But the question raised goes farther and becomes a question about the series development (3). Since this series is obtained by substituting $(x+i)$ for x in $f(x)$, at each step a new function appears:

$$f(x+i) = f(x) + iP(x,i)$$
$$P(x,i) = p(x) + iQ(x,i)$$
$$Q(x,i) = q(x) + iR(x,i) \ldots$$

Each function iP, iQ, iR, ... is zero when $i = 0$, but when i is a very small quantity, these functions take also very small values. Already in the first step, when Lagrange affirms that if $i = 0$ then $f(x+i) = f(x)$, he suggests at the same time that when i is a very small quantity—the term "infinitely small quantity" has been explicitly proscribed from the *Théorie des Fonctions Analytiques*—the reminder iP becomes also a very small quantity and so is the difference between $f(x+i)$ and $f(x)$[19]. To make clear the behavior of these functions, Lagrange considers the curve whose abscissa is equal to i and whose ordinate is given by one of these functions. This curve has a continuous path, so:

> "[...] le course de la courbe s'approchera peu à peu de l'axe avant de le couper et s'en approchera, par conséquent, d'une quantité moindre qu'aucune quantité donnée, de sorte qu'on pourra toujours trouver une abscisse i correspondant à une ordonnée moindre qu'une quantité donnée, et alors toute valeur plus petite de i répondra aussi à des ordonnées moindres que la quantité donnée." (*ibid.*, 12)

This property is in fact a fundamental principle for the whole theory of functions, and it has been always assumed implicitly in the differential calculus and in the calculus of fluxions. With this property a bound for the reminder functions iP, iQ, iR, ... can be given so that more than their specific values, it is possible to have a clear idea of the error, when only a finite number of terms of series I are considered.

Series (3) gives the value of $f(x+i)$, in order to obtain a series development for $f(x)$, Lagrange takes $x-i$ in the place of x in (3) and he obtains

$$f(x) = f(x-i) + if'(x-i) + \frac{i^2}{2} f''(x-i) + \frac{i^3}{3\cdot 2} f'''(x-i)+...$$

and by making $xz = i$

$$f(x) = f(x-xz) + xzf'(x-xz) + \frac{x^2z^2}{2} f''(x-xz) + \frac{x^3z^3}{3\cdot 2} f'''(x-xz)+... \tag{8}$$

clearly if $z = 0$ this series reduces to the equality $f(x) = f(x)$, and for $z = 1$ it becomes[20]

$$f(x) = f(0) + xf'(0) + \frac{2}{2} f''(0) + \frac{x^3}{3\cdot 2} f'''(0)+... \quad . \tag{9}$$

Through this transformation Lagrange's aim is not only to give a series development for $f(x)$, but to obtain the value of $f(x)$ only with a finite number of terms of the series. The series (8) and (9) suggest that it is possible to obtain a value which will come closer and closer to $f(x)$ as more and more terms of the series are added; but the "meaning" of the equality sign in the series (3), (8) or (9) should be, Lagrange thinks, the same as in any equation where both terms are considered to represent exactly the same quantity—out of which the equality sign can be used to link them—, and so equations (3), (8) and (9) are exact only when "all" the terms of the series are really added. But to obtain the value of the function for a specific value of x, the quest for the reminder that could help to avoid the infinite series becomes necessary

> "Tant que ce développment ne sert qu'à la génération des fonctions dérivées, il est indifférent que la série aille à l'infini ou non; il est aussi lorsqu'on ne considère le dévéloppement que comme une simple transformation analytique de la fonction; mais, si on veut l'employer pour avoir la valeur de la fonction dans les cas particuliers, comme offrant une expression d'une forme plus simple à raison de la quantité *i* qui se trouve dégagée de dessous la fonction, alors, ne pouvant tenir compte que d'un certain nombre plus ou moins grand de terms, il est important d'avoir un moyen d'évaluer le reste de la série qu'on néglige, ou du moins de trouver des limites de l'erreur qu'on commet en négligeant ce reste." (Lagrange 1806)

Faced with this problem, Lagrange looks for the value of a "remainder" that helps to find the exact value for $f(x)$ with just a finite number of terms.

From the series development (8) it is possible to write

$$f(x) = f(x-xz) + xP(z)$$

Where $P(0) = 0$. In the case $z = 0$, the development reduces to the equality $f(x) = f(x)$. By deriving the two members of this equation with respect to the variable z, the following equality is obtained

$$f'(x-xz) = P'(z) \tag{10}$$

So the reminder P is obtained by looking for a function of the variable z whose derivative regarding this variable is equal to $f'(x-xz)$, and is such that $P(0) = 0$. Once this condition for the reminder P is given, and if $z = 1$, the equality

$$f(x) = f(0) + xP(1)$$

is obtained.

By following to the next term of the series (8) it is possible to write

$$f(x) = f(x-xz) + xzf'(x-xz) + x^2Q(z)$$

where $Q(0) = 0$. By repeating the process of derivation in both members of the equation, a value for Q' is obtained

$$Q' = zf''(x-xz) \tag{11}$$

Again, when $z = 1$ Lagrange obtains now

$$f(x) = f(0) + xf'(0) + x^2Q(1)$$

Repeating the process again for the expression

$$f(x) = f(x-xz) + xzf'(x-xz) + \frac{x^2z^2}{2} f''(x-xz) + x^3R(z)$$

the value for the reminder R is given through

$$R' = \frac{z^2}{2} f'''(x-xz) \tag{12}$$

and for $z = 1$, the value

$$f(x) = f(0) + xf'(0) + \frac{x^2}{2} f''(0) + x^3R(1)$$

Since the new functions $P(z)$, $Q(z)$, $R(z)$, ... are known through their derivatives—from the relations a, b, c, ...—Lagrange gives upper and lower bounds for them: by defining first a function $F(z)$ such that $F'(z) = z^m Z(z)$, where $Z(z)$ is another function such that $N \leq Z(z) \leq M$ when $a \leq z \leq b$, and if $f(z)$ is another function such that $f'(z) = z^m(M - Z)$, then $f(b) > f(a)$[21]. Since $f'(z) = z^m M - F'(z)$, then $f(z) = \frac{Mz^{m+1}}{m+1} - F(z)$ and the following inequality holds

$$\frac{Mb^{m+1}}{m+1} - F(b) = f(b) > f(a) = \frac{Ma^{m+1}}{m+1} - F(a)$$

from this inequality it is possible to write

$$F(b) < F(a) + \frac{M\left(b^{m+1} - a^{m+1}\right)}{m+1}$$

In a completely similar way, by taking now $f'(z) = z^m(Z - N)$, the following inequality is obtained

$$F(b) > F(a) + \frac{N\left(b^{m+1} - a^{m+1}\right)}{m+1}$$

giving finally

$$F(a) + \frac{N\left(b^{m+1} - a^{m+1}\right)}{m+1} < F(b) < F(a) + \frac{M\left(b^{m+1} - a^{m+1}\right)}{m+1} \tag{13}$$

This is applied to the functions $P(z)$, $Q(z)$, $R(z)$, ... First by assuming that $P = F(z)$, it follows that $P' = F'(z) = f'(x-xz)$, and since it has been assumed that $F'(z) = z^m Z(z)$, by making $m = 0$, then $Z(z) = f'(x-xz)$. Whenever $a = 0$ and $b = 1$, $P(0) = 0 = F(a)$ and $F(b) = P(1)$. In the case that $N \leq f'(x-xz) \leq M$ whenever $0 \leq z \leq 1$, it is possible to obtain from the inequality (13) the inequality: $N < F(b) = P(1) < M$.

In a similar way Lagrange obtains for the function $Q(z)$, by making $m = 1$, that if $N_1 \leq f''(x-xz) \leq M_1$, then $\frac{N_1}{2} < F(b) = Q(1) < \frac{M_1}{2}$. And for the function $R(z)$, by making $m = 2$, that if $N_2 \leq \frac{f'''(x-xz)}{2} \leq M_2$ then $\frac{N_2}{3} < F(b) = R(1) < \frac{M_2}{3}$.

If in the variable quantity $u = x-xz$, the variable z runs through the interval $[0,1]$, then u runs through $[0,x]$, Lagrange concludes then, with the help of the intermediate value theorem, that $N \leq f'(x-xz) = f'(u) \leq M$, and so any value between N and M can be given as $f''(u)$ for some u in $[0,x]$. So the value $P(1)$ takes this form. For the same reason there are values of u such that $Q(1) = \frac{1}{2} f''(u)$ and $R(1) = \frac{1}{2 \cdot 3} f'''(u)$. From these facts his conclusion is the following theorem: "En désignant par u une quantité inconnue mais renfermée entre les limites 0 et x, on peut développer successivement toute fonction de x et d'autres quantités quelconques suivant les puissances de x de cette manière

$$f(x) = f(0) + xf'(u)$$

$$f(x) = f(0) + xf'(0) + \frac{x^2}{2} f''(u)$$

...

$$f(x) = f(0) + xf'(0) + \frac{x^2}{2} f''(0) + \frac{x^3}{3 \cdot 2} f'''(u) + ... \qquad \text{(Lagrange 1797, 49)}$$

So the canonical form (3) and the formal series (9) might be replaced by a finite polynomial, but this substitution does not mean an "error" in the calculus of the value for $f(x+i)$ or $f(x)$.

Two levels in Lagrange's theory of functions become clear: first a purely formal representation for functions where the canonical form (3) carries the reduction of all the theory to the algebraic scope, as well as the application of this theory of functions to geometry and mechanics. The second level is given when the effective calculation is wanted; the fact that the "remainder" of the series exists and takes the general form $\frac{x^n f^n(u)}{n!}$, reduces the canonical form to a finite and effective process.

But as we have seen, Lagrange's arguments are strongly supported by a simple assumption that is hardly justified within the frame of this algebraic function theory: all functions are supposed to behave as "continuous" functions. This assumption was made in relation with the theorem concerning the possibility to bound the reminder functions iP, iQ, iR,... It was made also concerning the intermediate value property of the derived functions.

The property of continuity, treated up to now as an evident truth through a geometric image, is a main tool to justify the passage from the formal representation to the effective calculation. But the need for this property shows, as it was already the case with Euler's algebraic analysis, that a theory of functions can no longer ignore it. Even more, making now a deeper gap between the analytical ideal, identified as an algebraic foundation for function theory, and the means to carry out this ideal, the fundamental proposition for algebra is also involved and needs this continuity property.

The relation between the coefficients of an equation, the degree of this equation and the number of its roots, is a problem that goes back to the algebra of Cardano and Viète. In his *De aequationum recognitione et amendatione* (1615) Viete shows the possibility to write a general equation of third and fourth degree as a product of linear factors. Concerning the relation between the maximum number of roots for an equation and its degree, an important background is given in Girard's *Invention nouvelle en l'algèbre* (1629) and in Descartes' *Géométrie* (1637), related to fact that a polynomial of degree n has n roots and also that it can be divided by each one of the n linear factors formed by these roots.

So, behind the classical form for the fundamental theorem of algebra, stating that a polynomial of degree n with real coefficient has n roots, several problems are involved. Among them the most important are:

1. The problem related with "existence" of the roots for a polynomial.
2. The description of the "form" that these roots may have.
3. The "number" of roots that might exist for a polynomial.
4. To find the roots of the polynomial through the linear factors that divide this polynomial.

Historically the first problem to appear is the one related to the number of roots for an equation. This problem, treated in some sense by Girard and Descartes, also involves the fourth problem: if the nth degree polynomial $P(x)$ is equal to the product $(x-x_1)(x-x_2)...(x-x_n)$, it is because the polynomial admits as many roots as its degree. The equality $P(x)=\prod_{i=1}^{n}(x-x_i)$, states not only that we

can divide $P(x)$ by any of the factors $(x-x_i)$, and so each quantity x_i is a root for $P(x)$; but also that $P(x)$ can be divided by $(x-x_i)$ whenever x_i is a root[22].

Now, when the problem is not to "prove the existence" of the roots, but to justify the equality $P(x)=\prod_{i=1}^{n}(x-x_i)$, then it is necessary to establish which values make the equality possible. As we know, Descartes states that in order to assure that any polynomial $P(x)$ of degree n is equal to a product of linear factors, it might be necessary to "imagine" some of these quantities that make possible the factorization. All through 17th and 18th centuries, the controversy about the nature of these "imaginary quantities" was at the center of all the questions concerning algebraic equations and their roots, until D'Alembert's proof (1746), that these quantities can only have the form $x+y\sqrt{-1}$. An immediate consequence is then that the number of imaginary roots is even[23]. When the degree of the polynomial is odd, the only possibility to admit this fact, and the one stating that the number of roots is equal to the degree of the equation, is that in this case the equation must have at least one real root.

At the beginning of his *Traité de la résolution des equations numériques de tous les degrés*, Lagrange gave two theorems where "the foundation for the theory of equations is given" and for which the continuity becomes necessary:

> "Si l'on a une équation quelconque, et que l'on connaisse deux nombres tels qu'étant substitués seccessivement à la place de l'inconnue de cette équation, ils donnent des résultats des signes contraires, l'équation aura nécessairement au moins une racine rélle dont la valeur sera entre ces deux nombres.
>
> Si, dans une équation qui a une ou plusieurs racines réelles et inégales, on substitue successivement à la place de l'inconnue deux nombres, dont l'un soit plus grand et l'autre soit plus petit que l'une de ces racines, et qui différent en même temps l'un de l'autre d'une quantité moindre que la différence entre cette racine et chacune des autres racines réelles de l'équation, ces deux substitutions donneront nécessairement deux résultats de signes contraires." (Lagrange 1798, 6)

For the proof of the first theorem Lagrange proceeds as follows: if it is possible to write the equation $P(x)$ as the product of linear factors of the form $(x-\alpha_i)$, where a_i is a real or imaginary root, and if by substituting two values p and q in the place of x in the product $\prod_{i=1}^{n}(x-\alpha_i)$, $P(a)$ and $P(b)$ take different signs, then at least one of the factors $(x-\alpha_i)$ changes its sign when substituting x by a and b. But in the product $\prod_{i=1}^{n}(x-\alpha_i)$, whenever one of the roots α_i has the form $a+b\sqrt{-1}$, then another root α_j takes the form $a-b\sqrt{-1}$. Since the product of the two linear factors $\left(x-a-b\sqrt{-1}\right)\left(x-a+b\sqrt{-1}\right)$ is always positive for any value of x, if there is a change in the sign of $P(x)$, this change is produced in a linear factor $(x-\alpha_i)$ where α_i is real. But Lagrange recognizes that there is a circular argument: the theorem about the nature of the imaginary roots, and the form of the linear factors, depend in some way on the first theorem that was to be proven.

Because of this circular argument Lagrange uses a cinematic image which was also used in his lessons at the *Ecole Normale* (LEN), before becoming a "rigorous" proof given in the first note in his 1798 treatise on numerical equations. This new argument is considered a rigorous one since it follows "from the nature of the equation, independently of any of its properties" (Lagrange 1798, note I, 111): by dividing the equation into two parts P and Q, each one of them representing the sum of positive and negative terms, when the value of the variable x is augmented "by insensible degrees" the values P and Q also change by "insensible degrees". By doing this between two values of the variable x which give, the first one $P-Q<0$, and the second one $P-Q>0$, then between these two values there must exist at least one value that makes $P=Q$,

> "[...]comme deux mobiles qu'on suppose parcourir une même ligne dans le même sens, et qui, partant à la fois de deux points différents, arrivent en même temps à deux autres points, mais de manière que celui qui était d'abord en arrière se trouve ensuite plus avancé que l'autre, doivent nécessairement se rencontrer dans leur chemin." (*ibid.*, note I, 112)

The fact that a mechanical or geometrical image is used, shows that algebra is unable to introduce and give a theoretical place to this notion itself. This limitation will show exactly how mathematical analysis finds its own and specific scope. The revolution in mathematical analysis caused by Bolzano and Cauchy concerns the reorganization of mathematical analysis on the basis of those concepts that Lagrange already considered as necessary, but that were not clearly conceivable within the frame of a purely algebraic foundation for analysis: the concepts of "convergence" (of series) and of "continuity" (of functions). The introduction of these concepts will not only show a new stage for mathematical analysis, but also a new relation of analysis towards algebra.

III Convergence and Continuity as the Trends of the New Analysis

The introduction of a new concept in mathematics realizes the definition of a new kind of objects. In this case, the new changes in mathematical analysis at the turn of the nineteenth century could be characterized as the transformations that took place within the theory of functions when the new objects known as "continuous functions" and "convergent series" were introduced. To see how the introduction of these new concepts and objects gave a new structure to mathematical analysis, we will look closely at some aspects of the mutual relation between the already existing concepts and the new ones. If mathematical analysis reaches a new "modernity" with the concepts of "convergence" and "continuity", it is because it takes on a new structure once these concepts have been introduced.

The new structure given to mathematical analysis by these new concepts of continuity and convergence emerges from the fact that they introduce a new approach towards the domain of real quantities. The theory of curve lines, as given by Euler in the second part of his *Introductio*, assumes, as we have seen, that the course of values which the function runs through is, as well as the one which the variable runs through, a "continuous" path. This property was automatically assumed from the "analytic nature" (in Euler's terms) of the function; mechanical or geometrical curves could all be seen as the "graph" of an appropriate analytic function. In his theory of curve lines, and in the proof of the intermediate value property—a property which could be deduced from the algebraic nature of functions—there is no special approach towards the "values" that the function takes, as the variable runs through different values. The assumption that an extreme value could not be reached by a function without reaching before all the intermediate values, is enough to deduce the properties related with continuity. Contrary to this style, a new approach towards real quantities, considered as the main condition to articulate the new trends of mathematical analysis, is introduced by the works of Bolzano and Cauchy. Bolzano's *Rein Analytischer Beweis* (1817), and Cauchy's *Cours d'Analyse* (1821) state the basis of this new approach, and with this new approach they give a new sense to what the "analytical style" ought to be.

We think, for example, that the main point of the "purely analytical proof" for the intermediate value property, given by Bolzano, is the proof of the existence of a certain quantity: the "real root" of an equation that takes values of different sign. We want to underline that when Bolzano argues that a purely analytical proof for this theorem is needed, it is not because of some misleading fact about geometry or mechanics, but rather because they are unable to support an argument that is, or should be, a "fundamental" one. Geometry or mechanics could only support a plausible argument, whereas it is necessary to give a foundation for the "truth" of the proposition. In Bolzano's words a proof should not be only a "confirmation" but rather a "justification [*Begründungen*]" (Bolzano 1817, preface, 160). The property to be proved, equivalent to the fact that a function "never reaches a higher value without first going through all lower values" (*ibid.*, preface, 162), is a property of "continuous functions", even if it can be more immediately "seen" as a property of continuous curves. After the radical changes that Euler introduced, and that we have already analyzed, curve lines should be considered as emerging from functions and so the property has to be proved in the scope of (continuous) functions. Even more, since this property has always been admitted as an evident fact of "continuity", the concepts of "continuity" and of "continuous function" have never been explicitly given. Bolzano introduces the concept of continuous function; with this concept he introduces a new objet into mathematical analysis:

> "A function $f(x)$ varies according to the law of continuity for all values of x inside or outside certain limits [...] if [...] the difference $f(x+\omega)-f(x)$ can be made smaller than any given quantity provided ω can be taken as small as we please." (*ibid.*, preface, 162)

The property holding for algebraic equation, as describes by Euler or Lagrange, is a result of the following schema of argumentation; which is the correct way to prove that for any equation $P(x)$ taking values of different sign for two values a and b of the variable x, a real root exists:

> [1.] If two functions of the variable x, $f(x)$ and $g(x)$, vary according to the law of continuity either for *all* values of x or only for those which lie between α and β, and if $g(\alpha) > f(\alpha)$ and $f(\beta) > g(\beta)$, then there is always a certain value of x between α and β for which $f(x) = g(x)$. (ibid., §15, 177)
>
> [2.] Every function of the form
>
> $[P(x) =]\ a+bx^m+cx^n+...+px^r$
>
> in which $m, n,..., r$, designate whole positive exponents, varies according to the law of continuity for all values of x (ibid., §17, 180).
>
> [3.] If a function of the form
>
> $[P(x) =]\ x^n+ax^{n-1}+bx^{n-2}+...+px+q$
>
> in which n denotes a whole positive number, is positive for $x = \alpha$ and negative for $x = \beta$, then the equation
>
> $x^n+ax^{n-1}+bx^{n-2}+...+px+q = 0$
>
> has at least one real root lying between α and β. (ibid., §18, 181)

The purely analytical proof is based on the following auxiliary theorem, which states the existence of the least upper bound for an (upper) bounded set, and which also establishes the necessary relation between the property of "continuity" for function and the property of "continuity" for the domain of real quantities:

> "If a property M does not belong to *all* values of a variable x, but does belong to all values which are less than a certain u, then there is always a quantity U which is the greatest of those of which it can be asserted that all smaller x have property M." (*ibid.*, §12, 174)

By taking as M the property of all those values of x for which $f(x) < g(x)$ (if $\alpha < \beta$ and $f(\alpha) < g(\alpha)$), then for the quantity U, whose existence is guaranteed by the theorem, the continuity of the functions f and g will make $f(U) = g(U)$. For if $f(U) < g(U)$, since f and g are continuous functions, it could be possible to show the existence of a real quantity s, such that $f(U+s) < g(U+s)$, and so U would not meet the condition established by the theorem. By reasoning in a similar way, if

$f(U) > g(U)$ it would be possible to show that $f(U-s) > g(U-s)$ and the same conclusion is obtained about U.

The proof of the auxiliary theorem, which shows the "existence" of the quantity U, goes as follows: if the property M is satisfied for all the values $x < u$, but not for all the values of the variable x, then there exists one number $D > 0$ such that M is not satisfied for all $x < V = u+D$. By considering now the following sequence of values

$$\left\{V_n; V_n = u + \frac{D}{2^n}\right\},$$

with n an increasing number, and $V = V_0 > V_1 > V_2 > ... > V_n > ....$ Since M is not satisfied for every $x < V_0$, it is possible to ask if there is some V_n such that M holds for every $x < V_n$; if there is no such quantity V_n, then $U = u$ and the theorem is proved. But if there exists a number n such that the property M is satisfied for all $x < V_n$, but not for every $x < V_{n-1}$ (n is the first number with this property) the procedure starts again. Considering now the sequence

$$\left\{W_m; W_m = V_n + \frac{D}{2^{n+m}}\right\}$$

with an increasing number m, and $W_0 = V_{n-1}$—since $V_{n-1} = V_n + \frac{D}{2^n}$. Now M does not hold for every $x < W_0$. Since $W_0 > W_1 > ... W_m > ... > V_n$, if there is no integer number m such that M holds for every $x < W_m$, then $U = V_n$ and the theorem is proved; but if there is a number m with the desired property (and again it might be assumed that m is the first one), then M is satisfied for every $x < W_m$, but not for every $x < W_{m-1}$. In this case the procedure is repeated again. If it happens that after a finite number of steps the property M holds for every $x < Z_r = u + \frac{D}{2^n} + \frac{D}{2^{n+m}} + ... + \frac{D}{2^{n+m+...+r}}$, but there is no positive integer number s such that M holds for every $x < u + \frac{D}{2^n} + \frac{D}{2^{n+m}} + ... + \frac{D}{2^{n+m+...+r+s}}$, then $U = Z_r$. If, on the other hand, it is not possible to find such a value, then the sequence of values

$$u, u+\frac{D}{2^n}, u+\frac{D}{2^n}+\frac{D}{2^{n+m}}, ..., u+\frac{D}{2^n}+\frac{D}{2^{n+m}}+\frac{D}{2^{n+m+...+r}}, ...$$

represents a sequence whose terms increase while the difference between two consecutive terms decreases in a reason that is less than a geometric progression. The quantity U is in this case the "limit" of this sequence. Bolzano assures that:

> "If a sequence of quantities
>
> $F_1(x), F_2(x), F_3(x), \ldots, F_n(x), \ldots, F_{n+r}(x), \ldots$
>
> has the property that the difference between its n-th term $F_n(x)$ and every later term $F_{n+r}(x)$, however far from the former, remains smaller than any given quantity if n has been taken large enough, then there is always a certain *constant quantity*, and indeed only one, which the terms of the sequence approach, and to which they can come as close as desired if the sequence is continued far enough."[24] (*ibid.*, §7, 171)

Since the sequence u, V_n, W_m, ... Z_r,... has this property, the existence of the quantity U the limit of this sequence, is guaranteed by the last statement.

As we said before, a main point in Bolzano's argument is the proof of the existence of a certain quantity. The existence of the quantity U, which becomes the root for the equation, is given through the auxiliary theorem—stating the existence of the "least upper bound" for a bounded subset of numbers—, whose proof rests upon the convergence of a sequence having the so called Cauchy property[25]. For Bolzano a proof for this last property is possible, and his argument for the existence of a limit for a "Cauchy sequence" is that the assumption of the existence of such limit bears no contradiction:

> "The assumption of an *invariable* quantity with this property of proximity to the terms of our series is not impossible because with this assumption it is possible to determine the quantity as accurately as desired." (*ibid.*)

This means that the existence of the limit quantity can be asserted since its value can be approached as accurately as desired through the successive values of the sequence. The value of the limit of the sequence might not be known, but it is possible to approach this value through the sequence, and this possibility is the main reason to assure the existence of the limit. Otherwise, if there was no real quantity the sequence approaches, the terms of the sequence would not approach each other as they increase; "for anyone who has a correct concept of "quantity" the idea of this value is the idea of a real, *i.e.*, "actual", quantity". Clearly Bolzano's conclusion would not be valid if the domain of real quantities had a "gap"; but he considers that when a sequence behaves as the theorem says, then it is convergent, since for a non convergent sequence the "non approaching behavior" is essential. It is possible to accept the existence of a quantity, being the limit of the series, and then to consider this hypothesis among the rest of "truths" of analysis[26].

But if the existence of the limit of the sequence might be concluded, the property out of which this is deduced, the so called Cauchy property, is far from containing the existence of a limit. In other words, in a Kantian sense, the proposition "any sequence or series having the Cauchy property has a limit" is not "analytical" but "synthetical". Since in the statement "for any positive quantity, no matter how small this might be, there exists a positive number n such that the difference, in absolute value, between the term a_n and any other term a_{n+r} of the sequence, is smaller than the given quantity", there is nothing involving the "existence of a limit value"; and that is why *the existence of the limit must be proved.*

With regards to this point Cauchy's procedure is different. First he works with limits of functions: he proofs that if for the increasing values of the variable x, the difference $f(x+1)-f(x)$ "converges" to a limit k, then the function $\frac{f(x)}{x}$ converges to the same limit. In the proof of this statement, Cauchy makes clear the meaning of the sentence "the difference $f(x+1)-f(x)$ *converges* to a limit k":

> "On pourra donner au nombre h une valeur assez considérable pour que, x étant égale ou supérieur à h, la difference dont il s'agit soit constamment entre les limites $k-\varepsilon$ et $k+\varepsilon$. (si ε est un nombre possitif aussi petit que l'on voudra)." (Cauchy 1821, 54)

For any function, or any sequence which is to be considered as a function $f(1)$, $f(2)$, ... , it converges to a limit k if, given any positive value ε no matter how small it might be, there is a positive number h such that if $n > h$, the term $f(n)$ lies between the limits $k-\varepsilon$ and $k+\varepsilon$. After this explanation of the concept of convergence for sequences, Cauchy explains the convergence of series in detail: for a series $\sum a_i$, let $s_n = \sum_{i=1}^{n} a_i$ be the sum of the first n terms, if the terms of the form s_n form a convergent sequence whose limit is s, the series is convergent and its limit is s (and so it might be written $s = \sum_{i=1}^{\infty} a_i$). Now for a series to be convergent it is necessary that it satisfies the "Cauchy condition": for any positive quantity, no matter how small this might be, there exists a positive number n such that the sum of the terms $a_n+...+a_{n+r}$ of a series Σa_i, is smaller than the given quantity. But for the converse property Cauchy simply states that "when this condition is filled, it can be assured the convergence of the series" (*ibid.*, 126). So he finally considers that concerning the question which was "proved" by Bolzano, really there is nothing to prove.

The relation between convergent sequences and series and continuous functions is a basic one, since whenever the variable quantity x has X as a limit, and $f(x)$ is a continuous function, $f(x)$ becomes a variable quantity whose limit is $f(X)$. That means:

$$\lim_{x \to X} f(x) = f(x) \tag{14}$$

With this basic relation Cauchy proves the intermediate value property: if the function $f(x)$ remains continuous between the two limits $x = x_0$, $x = X$, and if the two values $f(x_0)$ and $f(X)$ have different signs, then it is possible to find a solution for the equation $f(x) = 0$, at least with one real value of the variable x between x_0 and X.

If $x_0 < X$, $h = X - x_0$, and $m > 1$ is an integer number, since the two quantities $f(x_0)$ and $f(X)$ have different signs, it is possible to compare two consecutive terms of the sequence

$$f(x_0), f\left(x_0 + \frac{h}{m}\right), f\left(x_0 + 2\frac{h}{m}\right), \ldots, f\left(X - \frac{h}{m}\right), f(X)$$

and there must exist at least two consecutive terms $f(x_1)$ and $f(X')$ having different signs. Clearly $x_0 < x_1 < X' < X$, and $X' - x_1 = \frac{h}{m} = \frac{1}{m}(X - x_0)$.

Once these consecutive terms x_1 and X' have been found, it is possible to find two values between them, x_2 and X'', giving values $f(x_2)$ and $f(X'')$ of different signs, and holding the conditions $x_1 < x_2 < X'' < X'$, and

$$X'' - x_2 = \frac{1}{m}(X' - x_1) = \frac{1}{m^2}(X - x_0) \quad .$$

By continuing in this way two sequences are given: an increasing sequence of values

$$x_0, x_1, x_2, \ldots \tag{15}$$

and a decreasing sequence

$$X, X', X'', \ldots \tag{16}$$

The terms of sequence (16) are all greater than those of sequence (15), and the difference between two respective terms of these sequences decreases: $X - x_0 = h$,

$$X' - x_1 = \frac{h}{m}, X'' - x_2 = \frac{h}{m^2}.$$

It must be concluded that the terms of the sequences (15) and (16) will converge to a common limit a. Since $f(x)$ is a continuous function, the terms of the sequences

$$f(x_0), f(x_1), f(x_2), \ldots \textit{ and } f(X), f(X'), f(X''), \ldots$$

converge also towards the limit $f(a)$ which must be equal to zero.

Nevertheless, Cauchy states another relation between convergence and continuity. When speaking of a convergent series, since the partial sums $s_n = \sum_{i=1}^{n} a_i$ indefinitely approach a certain limit s, the difference between the limit s and the partial sum decreases as the number n increases. This difference, the "reminder" of the series, is a variable quantity whose limit is zero[27]. The fact that the terms of the series are constant or variable quantities does not change this property of the reminder: to be an infinitely small quantity. Now, when the terms of the convergent series are all continuous functions—each term is a function for which an infinitely small variation for the variable produces an infinitely small variation in the value of the function itself—the variations for the value of the limit function, when infinitely small variations takes place for the variable, are proportional to the variation for the reminder itself, but this last variation must be infinitely small since the reminder itself is already an infinitely small quantity. From this argument Cauchy concludes that:

> "Theorem I: Lorsque les differents terms de la serie sont des fonctions d'une meme variable x, continues par rapport à cette variable dans le voisinage d'une valeur particulière pour laquelle la série est convergente, la somme s de la série est aussi, dans le voisinage de cette valeur particulière, fonction continue de x." (*ibid.*, 131-132)

The conclusion is obtained by stating the properties of a "fixed" object, the limit of the series, from the behavior of a "mobile" object, the reminder of the series; but the properties that can be stated about the reminder are obtained from the existence of the limit: it is the existence of the limit which determines that the reminder must be an infinitely small quantity, and this property is enough, in Cauchy's view, to state that the limit function is continuous when the terms of the series are all continuous functions.

Many articles and texts have been written around this famous "wrong theorem" proved by Cauchy. Some of them have pointed out "why" it is a wrong statement (since Cauchy does not give the precise condition on the way in which the series converges; *i.e.* that the series should be a "uniformly convergent" series); others have tried to point out in which sense Cauchy's argument could be read as a correct statement. But very few have remarked on the "place" that this statement takes in the whole text of 1821: it is used to justify a crucial step in the proof of the binomial formula.

As we said before, the introduction of a new concept is not reduced to the statement of a new definition, the role of continuous functions does not stop with the intermediate value property or with the relation between continuity and convergence. Besides Newton's binomial formula, another outstanding and well-known statement gets a new foundation through the concept of a continuous function: the fundamental theorem of algebra (FTA). And it is precisely through these two propositions that it could hardly be said, that mathematical analysis gets its foundation through algebra. Contrary to this, it will be algebra—and precisely its fundamental theorem—which will find a new proof, and so a new foundation as Bolzano affirmed, through mathematical analysis.

Cauchy's proof of the binomial formula, and the development of the logarithmic series, are given in the scope of the solution of functional equations. The problem is to find the continuous functions that satisfy the following conditions:

1. $\phi(x+y) = \phi(x)\times\phi(y)$
2. $\phi(xy) = \phi(x)+\phi(y)$

The solutions given by Cauchy for these equations are:

1. $\phi(x) = A^x$, with A a positive constant value.
2. $\phi(x) = aL(x)$, with a a constant quantity and L the characteristic of the logarithmic function.

For the solution of these equations the assumption that they should be continuous functions is necessary. As to the first one, Cauchy remarks that the function takes only positive values: from the equality $\phi(x+y) = \phi(x)\cdot\phi(y)$, he gets $\phi(2x) = [\phi(x)]^2$; and by taking $\frac{1}{2}x$ in the place of x he gets now $\phi(x)=\left[\phi\left(\frac{1}{2}x\right)\right]^2$.

By taking a positive number α and a positive integer m, it follows from equation 1 that $\phi(m\alpha) = [\phi(\alpha)]^m$. If now $\beta=\frac{m}{n}\alpha$, from the two equalities $\phi(m\alpha) = [\phi(\alpha)]^m$ and $m\alpha = n\beta$, it follows $\phi(\beta)=\phi\left(\frac{m}{n}\alpha\right)=[\phi(\alpha)]^{\frac{m}{n}}$. By the "density" of the rational numbers, and from the property of continuity of the function ϕ, Cauchy gets finally $\phi(\mu\alpha)=[\phi(\alpha)]^{\mu}$. The case $-\alpha=1$ gives $\phi(\mu)=[\phi(1)]^{\mu}$, and by taking the limit when $\mu\to 0$, $\phi(0) = 1$. From the initial condition it follows that $\phi(-\mu)=\dfrac{\phi(0)}{\phi(\mu)}=[\phi(1)]^{-\mu}$, which proofs that for any positive or negative value of the variable x, the equality $\phi(x)=[\phi(1)]^x$ holds. If $A = \phi(1)$, Cauchy gets the solution $\phi(x) = A^x$.

To find the proof of Newton's binomial formula, a problem related with the convergence of a series has to be solved: the only "legitimate" way to prove that the equality

$$(1+x)^{\mu} = 1+\mu x+\frac{\mu(\mu-1)}{2}x^2+\frac{\mu(\mu-1)(\mu-2)}{2\cdot 3}x^3+\ldots$$

holds (for every real number μ), is that the infinite series which is the right member of the equality—which is infinite unless μ represents a positive integer—"converges" to the value of the left member. The "root" tests for the convergence of series, when the terms of the series $\sum_{i=0}^{\infty}u_i$ are functions of the form $u_i(x) = a_i x^i$, takes the form:

Let A be the $\limsup_{n\to\infty}\sqrt[n]{a_n}$. The series converges for every value x between the limits $x=-\frac{1}{A}$ and $x=+\frac{1}{A}$; the series diverges for every x outside these limits (the value A defines the "radius of convergence" of the power series).

For power series, Cauchy proves also the algebraic closure related to the sum and the product: if the two series $\sum_{n=0}^{\infty}a_n x^n$, $\sum_{n=0}^{\infty}b_n x^n$ are convergent for some value of the variable x, and if their respective sums are s and s', the power series $\sum_{n=0}^{\infty}(a_n+b_n)x^n$ is also convergent and its sum is $s+s'$. Under the same conditions, if each one of the series is absolutely convergent, the series $\sum_{n=0}^{\infty}c_n x^n$, with $C_n=\sum_{k+l=n}a_k\cdot b_l$, is a new convergent series whose sum is ss'.

By taking as a general coefficients for the two series

$$a_n=\frac{\mu(\mu-1)(\mu-2)\ldots(\mu-n+1)}{n!} \text{ and } \qquad (17)$$
$$b_n=\frac{\mu'(\mu'-1)(\mu'-2)\ldots(\mu'-n+1)}{n!}$$

where m and m' are two arbitrary quantities, if $-1 < x < 1$, by the root test states they are ("absolutely") "convergent", and the general term of the "product series" is

$$c_n = \frac{(\mu+\mu')(\mu+\mu'-1)(\mu+\mu'-2)\ldots(\mu+\mu'-n+1)}{n!} \tag{18}$$

Cauchy writes $\phi(\mu)=\sum_{n=0}^{\infty} a_n x^n$, and when the coefficients take the values given in (17), it satisfies the equality

$$\phi(\mu) = 1+\mu x+\frac{\mu(\mu-1)}{2}x^2+\frac{\mu(\mu-1)(\mu-2)}{2\cdot 3}x^3+\ldots \tag{19}$$

Now for the sum of the second series, Cauchy writes $\phi'(\mu)=\sum_{n=0}^{\infty} b_n x^n$ and it satisfies

$$\phi(\mu') = 1+\mu' x+\frac{\mu'(\mu'-1)}{2}x^2+\frac{\mu'(\mu'-1)(\mu'-2)}{2\cdot 3}x^3+\ldots \tag{19'}$$

Clearly $\phi(\mu+\mu')=\sum_{n=0}^{\infty} c_n x^n$, when the coefficients c_n take the form (3); in this way the function $f(m)$ satisfies the equation

$$\phi(\mu)\cdot\phi(\mu') = \phi(\mu+\mu') \tag{20}$$

From equation (19), and by taking $-1 < x < 1$, theorem I assures that $f(m)$ is a continuous function for the variable m that satisfies the functional equation (20) and so $\phi(\mu)=[\phi(1)]^{\mu}=(1+x)^{\mu}$. That means,

$$(1+x)^{\mu} = 1+\mu x+\frac{\mu(\mu-1)}{2}x^2+\frac{\mu(\mu-1)(\mu-2)}{2\cdot 3}x^3+\ldots, \tag{21}$$

whenever $-1 < x < 1$ for any real value of μ. Newton's binomial formula is completely proven.

As an immediate consequence of this formula, Cauchy gives the series developments for the exponential function e^x, for the natural logarithmic function $\ln(1+x)$

and for the logarithmic function of any base a, $L(1+x)$. First by putting in the equation (21) $\mu=\frac{1}{\alpha}$ and substituting x with αx then

$$(1+\alpha x)^{\frac{1}{\mu}} = 1+x+\frac{(1-\alpha)}{2}x^2+\frac{(1-\alpha)(1-2\alpha)}{2\cdot 3}x^3+\ldots$$

if $-1 < \alpha x < 1$—or $-\frac{1}{\alpha} < x < \frac{1}{\alpha}$. Taking the limit when $\alpha\to 0$ the series

$$\lim_{\alpha\to 0}(1+\alpha x)^{\frac{1}{\alpha}} = 1+x+\frac{x^2}{2}+\frac{x^3}{3\cdot 2}+\ldots$$

is convergent for $-\infty < x < \infty$. When $x = 1$, the series

$\lim_{\alpha\to 0}(1+\alpha)^{\frac{1}{\alpha}} = 1+1+\frac{1}{2}+\frac{1}{3\cdot 2}+\ldots$ defines the number e, and

$$e^x = \lim_{\alpha\to 0}(1+\alpha x)^{\frac{1}{\alpha}} = 1+x+\frac{x^2}{2}+\frac{x^3}{3\cdot 2}+\ldots \tag{22}$$

By subtracting 1 to each member of equation (21), and then dividing by μ and taking the limit when $\mu\to 0$, he gets

$$\lim_{\mu\to 0}\frac{(1+x)^{\mu}-1}{\mu} = x-\frac{x^2}{2}+\frac{x^3}{3}-\ldots \tag{23}$$

and since $(1+x)=e^{l(1+x)}$,

$$(1+x)^{\mu} = e^{\mu l(1+x)} = 1+\frac{\mu l(1+x)}{1}+\frac{\mu^2[l(1+x)]^2}{2}+\frac{\mu^3[l(1+x)]^3}{2\cdot 3}+\ldots$$

and

$$\frac{(1+x)^{\mu}-1}{\mu} = \frac{l(1+x)}{1}+\frac{\mu[l(1+x)]^2}{2}+\frac{\mu^2[l(1+x)]^3}{2\cdot 3}+\ldots \tag{24}$$

From (23) and (24) he gets

$$\lim_{\mu\to 0}\frac{(1+x)^{\mu}-1}{\mu}=l(1+x)=x-\frac{x^2}{2}+\frac{x^3}{3}-\ldots \tag{25}$$

whenever $-1 < x < 1$.

For the function $L(1+x)$—the logarithms of base a—Cauchy uses the well-known equality $\frac{L(1+x)}{L(a)}=\frac{l(1+x)}{l(a)}$ and from (22) it follows that

$$L(1+x)=\frac{1}{\ln(a)}\left[x-\frac{x^2}{2}+\frac{x^3}{3}-\ldots\right] \tag{26}$$

Two conditions play a fundamental role in the developments of these functions and also in the proof of Newton's formula: the series must converge, and the functions represented through the series are continuous—the function $\phi(\mu)=[\phi(1)]^{\mu}=(1+x)^{\mu}$ is a continuous function for the real variable μ because of theorem I, the "wrong theorem". Those theorems and series developments that were proved before by Euler and Lagrange are here submitted to these conditions; from now on, mathematical analysis and "analytical style" will be related with them. Mathematical analysis was a branch of mathematics that under the conceptual basis given by Euler, became mainly a theory of functions, and made the natural means to develop functions out of polynomials and infinite series. With Lagrange, the development of functions by a Taylor series achieved the reduction of theory of functions to algebra. In the new scope of mathematical analysis given by Bolzano and Cauchy, the concepts of continuity and convergence rule the extent of the "algebraic generalizations" —the possibility to develop a function through an infinite series is necessarily submitted to the fact that the variable of the function should vary within the radius of convergence of the series.

The proof given by Cauchy for the binomial formula states another feature for the new analytic style: it is possible to finish with the vicious circle—already detected by Euler—, between the binomial formula and Taylor's series for a function. In Lagrange's algebraic theory of functions, the binomial formula appeared as a particular case of the Taylor series for $f(x) = x^n$, although for the justification of the Taylor series development, a proof for the relation $f'(x) = nx^{n-1}$ is needed. This relation is proved precisely by using the binomial formula. For Cauchy two facts are clearly stated: the binomial formula is based on the principles of purely "algebraic analysis"—which in the tradition opened by Euler states that there is no need to call for any principle of differential or integral calculus—and, because of that, it needs no other justification than those coming from basic concepts of continuity and convergence, as we have already seen.

As for the upcoming relation between algebraic analysis and infinitesimal calculus, these concepts state how algebraic analysis should precede infinitesimal calculus: let us just point out that without them it is not possible to define the two main concepts of calculus: in Cauchy's lessons on infinitesimal calculus (1823), and since then, the derivative and the integral of a function are defined as a "limit" (of a quotient or a series). The "definite integral" for a function, with this definition, becomes independent of the derivative of a function. This makes possible and necessary the proof of the fundamental theorem of the calculus.

The core of Cauchy's analytical ideal, as given through his *Analyse Algébrique*, is not only to introduce the concepts that will give the new foundation to infinitesimal calculus. We think that Cauchy's aim is, contrary to Euler and Lagrange, to present algebra as founded by analysis. This aim is finally reached with his proof of the fundamental theorem of algebra (FTA):

> "Theorem 1. For any real or imaginary values for the constants $a_0, a_1, a_2, \ldots, a_{n-1}, a_n$, the equation
>
> $$a_0x^n+a_1x^{n-1}+\ldots+a_{n-1}x+a_n=0 \tag{27}$$
>
> where n is an integer positive number ≥ 1, has always real or imaginary roots."

With this general theorem the following ones are also given

> "Theorem 2. For any real or imaginary values for the constants $a_0, a_1, a_2, \ldots, a_{n-1}, a_n$, the polynomial
>
> $$a_0x^n+a_1x^{n-1}+\ldots+a_{n-1}x+a_n=f(x) \tag{28}$$
>
> is equal to the product of the constant a_0 and n linear factor of the form $x-\alpha-\beta\sqrt{-1}$."

> "Theorem 3. For any real or imaginary values for the constants $a_0, a_1, a_2, \ldots, a_{n-1}, a_n$, the equation
>
> $$a_0x^n+a_1x^{n-1}+\ldots+a_{n-1}x+a_n=0 \tag{29}$$
>
> has always n real or imaginary roots, and it could not have more." (*ibid.*, 343)

According to (28), $f(x)$ is a real or imaginary, but always "entire" function. With this notation, equation (27) states that $f(x) = 0$. By taking $x=u+v\sqrt{-1}$ and by substituting this value in $f(x)$, then $f\left(u+v\sqrt{-1}\right)=\phi(u,v)+\psi(u,v)\sqrt{-1}$, where now $\phi(u,v)$ and $\psi(u,v)$ are real functions of the real variables u and v. Under this new form equation (27) becomes $\phi(u,v)+\psi(u,v)\sqrt{-1}=0$; and this is satisfied only when the two equations

$$\begin{cases}\phi(u,v)=0\\ \psi(u,v)=0\end{cases} \tag{30}$$

are satisfied at the same time, or when the equation $F(u,v)=[\phi(u,v)]^2+[\psi(u,v)]^2=0$ holds. So the proof for FTA becomes the proof of the existence of two real values, u and v, that satisfy the equation $F(u,v) = 0$. Two main properties of the function $F(u,v)$ are obtained: first that this function is not bounded when one of the two values u, v increases more and more

> "La fonction $F(u,v)$ ne peut conserver une valeur finie qu'autant que les deux quantités u, v reçoivent elles-mêmes des valeurs de cette espèce, et devient infiniment grande dès que l'une des deux quantités croît indéfiniment." (Cauchy 1821, 334)

The second property for $F(u,v)$ is that it is also a continuous function of the variables u and v. Now, since $F(u,v) \geq 0$, the two properties for this function, being continuous and becoming infinite whenever u or v become infinite, allow Cauchy to conclude that the function reaches its lower limit with finite values of u and v.

> "$F(u,v)$, variant [avec les variables u,v] par degrés insensibles, et ne pouvant s'abaisser au-dessous de zéro, atteindra une ou plusieurs fois une certaine limite inferieure qu'elle ne dépassera jamais." (*ibid.*, 334-335)

By calling A this lower limit and (u_0,v_0) one couple of values such that $F(u_0,v_0) = A$, Cauchy proves that $A = 0$. Clearly the main point here is the statement that the lower limit A is reached by the continuous function $F(u,v)$—out of which the "existence" of the couple (u_0,v_0) is obtained, and by this the existence of the root of the equation. Once again, as it happened with Bolzano, the goal is the proof of the existence of a quantity (which now could be not only real but also imaginary), and this existence is obtained through a property that the function $F(u,v)$ should hold as a continuous function: this function reaches its lower bound since whenever u or $v \to \infty$, $F(u,v) \to \infty$[28].

At the end of seventeenth century *Mathematical Analysis* was not a well-recognized mathematical theory. Certainly a new approach towards quantities, requiring the study of entire and infinitely small quantities, became the main attribute of a new *style* of working the algebra of quantities; the need for this new algebra was already justified by the works of Descartes and Leibniz. But as we said before, at the beginning of nineteenth century *Mathematical Analysis* was considered the core of the mathematical expression of physical phenomena. As Fourier stated in

the Introduction to his *Théorie Analytique de la Chaleur*, it is not only a well-recognized mathematical theory, but even the "heart" of all mathematics; the great development of this theory in the nineteenth century in some sense confirmed Fourier's vision. But as we have seen, the methods, the content, and the concepts of Euler or Lagrange that articulate this theory are not the same as in Cauchy or Riemann. Certainly the development of mathematical analysis after Cauchy is not conceivable without the concepts of "continuity" and "convergence", even if wider classes of functions were discussed after Riemann—the class of "integrable functions" which includes "continuous functions" as a particular subclass, the class of measurable functions, the "Baire" functions, etc.

The birth of a new physics in the eighteenth century happened because of an "analytical ideal" that made possible their treatment out of the purely descriptive explanations. Now, it seems to us that the main consequence the "analytical ideal" had for mathematical analysis itself was precisely the need for the production of the concepts of continuity and convergence, that support the theoretical structure for the new analysis and their distinction from purely "algebraic generalizations".

Universidad Nacional Autonoma de México

Notes

* The author would like to thank the support of grant UNAM IN 401294

1 As J. Sebestik says "Since Descartes up to the beginning of 19th century, modern science has lived under the regime of analytical theories". (Sebestik 1992, 25).

2 In her profound work, Hourya Sinaceur (1991) points out the differences between Lagrange and Fourier, with regard to the question of the resolution of algebraic equations, starting from their different conceptions of what the analytic methods ought to be.

3 "The simple factors of an entire function Z of z are found by equating the function to zero and by looking for the roots of this equation; since they give one a simple factor for the function Z." (Euler 1748, 17)

4 In 1746 Jean le Rond d'Alembert (1746) proved that any imaginary quantity is of the form $a+b\sqrt{-1}$. In 1749 Euler gave a proof of the same fact in his "Recherches sur les racines imaginaires des équations" (1749), although he had presented a previous version of his memoir in 1746. Concerning this proof given by Euler and d'Alembert cf. Gilain (1991).

5 Clearly if the imaginary quantities are supposed to be complex quantities of the form $a+b\sqrt{-1}$, the conclusion comes out immediately: if $a+b\sqrt{-1}$ is a root of the equation, then $a-b\sqrt{-1}$ is also a root, and the product of the two imaginary factors $\left(a+b\sqrt{-1}\right)\left(a-b\sqrt{-1}\right)$ is a real double factor.

6 "Quod quamvis non summo rigore sit demonstratum, tamen eius veritas in sequentibus magis corroborabitur".

7 The proposition that any entire function is equal to the product of double or simple real factors implies both properties: that any equation of odd degree has a real root, and that imaginary roots are always "complex" quantities.

8 In chapter IX of his *Introductio* (1748, 108), Euler says that

> "It is sometimes difficult to find the imaginary factors [...] but if the nature of imaginary factors is such that the product of two of them is real, it is then possible to find all of them by looking for the double factors that are real, but whose simple factors are imaginary; since it is clear that ones we know all the double factors of the form $p-qz+rz^2$ included in the function $a+bz+gz^2+dz^3+\dots$, we will have then all the imaginary factors".

9 This is one of the main differences between algebraic analysis in the scope of Euler's *Introductio* and that of Cauchy's *Cours d'Analyse*. Euler is certain that his definition of continuity is "analytic", and Cauchy thinks exactly the same about his definition.

10 This last condition towards the property of "continuity" of functions, which will be clearly given by Bolzano and Cauchy, cannot be stated in the algebraic frame for mathematical analysis given by Euler.

11 Euler's attempts to prove Newton's formula in the case of a non integer exponent are given later. Cf. Dhombres (1987).

12 As is clearly stated by Amy Dahan (1992, 186):

> "Ce que Lagrange veut accomplir dans la *Mécanique Analytique* [...] c'est un mouvement de double réduction: de la mécanique à l'analyse et de l'analyse à l'algèbre. Si la première partie du programme y est réalisée grâce au calcul des variations, la deuxième réduction est à l'œuvre dans la *Théorie des Fonctions Analytiques*".

13 Obtained, as it is well known, from the idea that when substituting the variable x for the variable $x+i$, $f(x+i)$ takes the place of $f(x)$, with the obvious condition that they must be equal whenever $i = 0$. In the expression for $f(x+i)$, it should be possible to separate those terms that do not depend on i, from those that are equal to zero when $i = 0$. That means that it is possible to write $f(x+i) = f(x)+iP$, where $P = P(x,i)$ is an expression depending on both x and i. By repeating his reasoning Lagrange states that also for the function $P(x,i)$ it is possible to separate that part which depends only on the variable x from another part which also depends on i and must be equal to zero when $i = 0$, that means $P(x,i) = p(x)+iQ$, so $f(x+i) = f(x)+ip(x)+i^2Q$. Continuing in this way a development of the form $f(x+i) = f(x)+ip(x)+i^2q(x)+i^3r(x)+\dots$ is obtained.

14 Where each "derived function" $f'(x), f''(x), \dots$ is obtained from the previous one and coincides with a differential quotient: $f'(x) = \dfrac{df(x)}{dx}$, $f''(x) = \dfrac{df'(x)}{dx}$.

15 Clearly Lagrange takes for granted that if $f'(x) = mx^{m-1}$, when $f(x) = x^m$, then the algorithm will give for the second derived function $f''(x) = m(m-1)x^{m-2}$; for the third derived function $f'''(x) = m(m-1)(m-2)x^{m-3}$, and so on.

16 Two assumptions that become explicit and clear in Cauchy's proof for Newton's binomial formula.

17 If $x+i = a^{f(x)+o} = a^{f(x)}\cdot a^o$, by writing $o = if'(x)+\dfrac{i^2}{2}f''(x)+\dfrac{i^3}{3\cdot 2}f'''(x)+\dots$ and substituting this value in (5), $x+i = a^{f(x)}\cdot a^o = a^{f(x)}\left(1+Ao+A^2\dfrac{o^2}{2}+A^3\dfrac{o^3}{3\cdot 2}+\dots\right)$. Dividing by x he gets $\dfrac{i}{x} = Ao+A^2\dfrac{o^2}{2}+\dfrac{o^3}{3\cdot 2}+\dots$. Dividing then by i, replacing then the value of o, and rearranging according to the increasing powers of i, leads to the expression:

$$\frac{1}{x} = Af'(x)+\frac{i}{2}\left[Af''(x)+A^2f'^2(x)\right]+\dots$$

All the terms that are multiplied by i disappear, since i is an indeterminate value which does not appear in the quotient $\dfrac{1}{x}$, and so $\dfrac{1}{x} = Af'(x)$.

18 In his *Leçons sur le Calcul des Fonctions*, he goes one step further and states that no matter how big the number y might be, a number r can be found so that the value of $L(y)$ lies between two values:

$$\frac{r}{ln(a)}\left(1-\frac{1}{\sqrt[r]{y}}\right) < L(y) < \frac{r}{ln(a)}\left(\sqrt[r]{y}-1\right).$$

19 This fact would give the prove of the continuity of $f(x)$.

20 This development takes the form $f(x) = A+Bx+Cx^2+Dx^3+\dots$ already known from the general theory of equations and, given in particular by Euler. Lagrange says that on the basis of the theory of derived functions from the development $f(x) = A+Bx+Cx^2+Dx^3+\dots$ it is easy to say that $f(0) = A$, $f'(0) = B$, $f''(0) = 2C, \dots$

21 Lagrange uses the theorem as a main tool stating that

> "Si une fonction prime de x telle que $f'(x)$ est toujours positive pour toutes les valeurs de x depuis $x = a$ jusqu'à $x = b$, b étant $> a$, la différence des fonctions primitives qui répodent à ces deux valeurs de x, savoir $f(b)-f(a)$, sera nécessairement une quantité positive." (Lagrange 1797, 45)

(This theorem says that a function $f(x)$ such that $f'(x) > 0$ is always increasing).

22 This problem, the converse of the first one, is treated by Cauchy in relation with the "interpolation" problem, the problem to determine completely an entire function once a certain numbers of values are given.

23 Since whenever $x+y\sqrt{-1}$ is a root of an equation, then so does the quantity $x-y\sqrt{-1}$.

24 This theorems affirms that a sequence of numbers having the so called "Cauchy property" is convergent.

25 These two propositions are, as it is well known, equivalent and they both characterize the continuity property for the set of real numbers.

26 Here we agree with Philip Kitcher (1975) when he assures that for Bolzano the hypothesis stating the existence of the limit for a Cauchy sequence is completely compatible with the "fundamental laws" of analytical quantities.

27 "An infinitely small quantity", according to the sense given to this notion in his *Cours d'Analyse*.

28 The only possibility that the continuous function $F(u,v)$ not reach its lower limit would be that this lower limit be reached "at infinity", *i.e.*, that whenever u or $v \to \infty$, $F(u,v) \to A$.

JEAN DHOMBRES

THE ANALYSIS OF THE SYNTHESIS OF THE ANALYSIS... TWO MOMENTS OF A CHIASMUS: VIÈTE AND FOURIER

I Introduction

Old as it is, the debate over analysis versus synthesis is not a foundational one in mathematics. By indistinctly referring to Plato and Theon or more precisely to book VII of the *Mathematical Collections* of Pappus—a text dating from the 4th century AD—most commentators assign a secondary position to the debate, even if they only do so in a rhetorical way[1]. Such a position mainly proves that the conscious surge of analysis, either as a rival to synthesis or a complement to it, is first of all a criticism of mathematical reasoning and its practice. In other words, it is as a historical move that the couple analysis/synthesis finds its way in epistemology and no further explanation is necessary. Yet very little would have been said, had we not simultaneously stated the strong evolution through centuries of the very acceptance of the two words. They even switch their parts, in a similar fashion to mask-plays in Elizabethan theatre. Paradoxically, in the same way as in this theatre Oberon acts in a timeless world, assigning the debate there is a risk of putting aside time. And therefore there is a risk of excluding history under the pretext that the opposition analysis/synthesis would just be a form taken by the eternal problem of what logically comes first and what comes second, but could arguably come first as well. Unfortunately this circuit is made all too easily by restricting this opposition to a philosophical one between induction and deduction, or even between empiricism and rationalism. The timeless nature of this opposition may therefore be due to the intellectual question of equivalences or, to use a less anachronistic expression, to the mathematical back and forth motion[2]. If this motion will be my principal object here, I do not wish to forget its historical insertion, precisely in order to reach its scientific meaning.

At least one should easily recognise, like Titiana under the influence of the philtre that generated the transformations, that the opposition between analysis and synthesis also depends on the tradition of teaching mathematics. Therefore, it depends on the way mathematics takes its grasp on societies, each one organizing the transmission of knowledge in its own way and therefore according a meaningful logic to the teaching of a science for which an added value is provided for what

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 147–176.

could remain a pure technique (as was, for example, the case in classical Chinese culture). Is not mathematics the oldest object of teaching in the Western world? From Boethius proposing the first book of Euclid's *Elements* as a model for school exercises to Antoine Arnauld's ruling through a *Géométrie* the *Petites Écoles* of Port-Royal[3]; from the Jesuit fathers' great expectations for the exemplary Collegio Romano[4] to the enthusiastic adepts of modern mathematics during the sixties[5] of our century, how many personalities have neglected mathematics for the sole benefit of its presupposed effects? If didactics at a given period is scarcely read as serving the description and the structure of a science, it unavoidably serves a culture. Then it makes history run. And, as a consequence, looking for history in our search concerning the analysis/synthesis debate, we may be tempted to restrict ourselves to text-books and to teaching methods. When a study of analysis and synthesis is intended to be historical, not one but many projectors must be used in order for it to be efficient; many questions have then to be selected and pursued. It may even form a structure. Then one must be aware that this structural multiplicity *ipso facto* overthrows the historical localization; each cause having its own particular historical rhythm. The teaching of mathematics does not have the same historical rhythm as mathematics! This is the reason why I decided to reduce observations strictly to two mathematical texts only.

Indeed, I do think that historians of mathematics—and sometimes mathematicians may play that role—contributed more to keeping alive the opposition between analysis and synthesis than to the individual meanings successively attributed to the two terms. It could be more interesting to shed light on the stability of the opposition built by an "historical" line of thought than to follow the commentaries of mathematicians themselves or of philosophers. One way would be to deconstruct some classical histories of mathematics. We only quote certain names to recall a long line of thought; Etienne Montucla, Abraham Gotthelf Kästner, Charles Bossut, Maximilien Marie, Moritz Cantor or Gino Loria, etc.[6] We do not intend to proceed in this analytical manner through historiography here, but at least we may recognize that the mobility of meanings of the two terms in the analysis/synthesis couple is the other side of the historical stability of the opposition. The paradox does not lie in the fact that the "mathematical" back and forth motion generates a "historical" back and forth explanation in mathematics, but that in the long term only one antagonistic couple was fixed by historians. I would like to argue that this perennial opposition finds its mathematical value via the inversions it generates. As this is the value I am looking for, the times of inversion must be privileged.

In spite of the different meanings, determinations and causalities linked with various historical and social contexts, and transient as it may be, the pure epistemological question of analysis and synthesis does not lose any of its dialectical interest. It can easily be seen in a universal way, with many historical concretiza-

tions. Without yielding to a facile *mise en abîme*—the analysis of the synthesis of the analysis...—we may suppose some depth to the couple in its game of transformations. And hence in its efficiency as a representation. The philosopher Maurice Blondel, who remarkably perceived the general role played by analysis and synthesis in the sciences—an abstract generality and a linking by way of necessity in one instance and for the other one a synthetic and quantitative individualized intuition—explains that this duality cannot be solved. At least, it cannot be solved through the sciences alone:

> "In their continuous work of integration, [the sciences] constantly appeal to a synthetic process; it is the only one able to provide a material which could be said to be a formal one. But even this initiative of the thought escapes the sciences; they are alien to themselves [...]. As for what they know, they do not *know* it the way they know *it*."[7] (Blondel 1893, 61)

By deciding to illuminate some moments precisely where meanings turn up, that is when analysis becomes synthesis and when synthesis constructs analysis as well, we try to specify the back and forth motion of mathematics; we reach the crossings of what we metaphorically call a chiasmus. Thus we may localize the strong thought of Maurice Blondel in order to show it is just an artefact.

In order to act on the analysis/synthesis opposition within the conditions of a historical view I tried to circumscribe in the preamble, my display of the moments of a chiasmus requires a temporal determination of at least two periods. But two moments already require a lot. Thus, I will speak of the end of the 16th century using François Viète's work, and of the early 19th century using Joseph Fourier's contribution. Two names, but as already stated two texts only and each treating quite different subjects: we look at a style and at a method, and less at specific objects. In order to examine two cases when analysis and synthesis exchange their meanings, the comparison is none too pleasant, as two different languages are at work. There is the pompous Latin of a Renaissance already influenced by the baroque, and there is the severe French style of mathematical physics looking for a style somewhere between the analytical description derived from the Enlightenment and the rigorous style of convergent series of the positivist period. We have to win over the heterogeneity of the two texts in order to build a meaning: its validity and its soundness should be measured by a critical appraisal which may give back their own fragrances to the two periods.

II Viète or Analysis Seen as an Appeal for a Constructive Synthesis

In a printed text of 1593, Viète works out the sum of all terms of an infinite geometric progression (1593, ch. XVII). Even though it is the first occurrence of such a formula, Viète wishes his explanation to be a very short one:

"The whole science of geometric progression almost reduces to one theorem only, for which four relations among the datas are naturally deduced." (*ibid.*, 28)

He then abruptly asserts:

"When magnitudes are in a continued proportion, the largest term of the ratio is to the smallest as the sum of all terms is to this sum to which the largest term has been subtracted." [8] (*ibid.*)

A proposition which, as Viète is its author, we immediately have to try to read using notations. By setting a first term as D, which is necessarily "the largest" of the progression[9], then its second term B, and the sum F, we write

$$\frac{F}{F-D}=\frac{D}{B}$$

It therefore comes as a surprise that in the specificative transcription of the theorem in letters, Viète introduces a supplementary notation, some X which is a somewhat restive "smallest term" of the progression as a whole. Its presence has the advantage to build a well-balanced proportion which can be visualized in a modern way by a formula and was appreciated by Viète's contemporary readers from the rhetorical expression:

$$\frac{F-X}{F-D}=\frac{D}{B}$$

A quite simple interpretation can be given, at least if we restrict ourselves to a progression with only a finite number of terms. In fact, in more modern terms, choosing an integer n (≥ 1) and letting the general term be $x_n = x_1 r^{n-1}$ (D then corresponds to $n = 1$, or to x_1, and B to x_2) the sum $F_n = \sum_{k=1}^{k=n} x_k$ for a geometric progression of ratio $r = \frac{x_1}{x_2}$ (in the modern sense) can be written as[10]

$$\frac{F_n - x_n}{F_n - x_1}=\frac{x_1}{x_2}$$

And it is easy to go to infinity by replacing F_n by F and therefore x_n by x_∞:

$$\frac{F - x_\infty}{F - x_1}=\frac{x_1}{x_2}$$

What is simple for us was as simple to Viète's readers in their time because they had read Euclid[11]. In this respect, the emphasis of this author writing the "smallest term" is surprising. In other words, it solicits some reflexion, for as he does not even provide proof of the theorem–in the expression of which we have to recall that the term X does not appear. Here lies our major observation. From a literary form to a literal one, something more is made apparent which is something less in terms of mathematical efficiency.

Unfolding a beautiful analytical process, Viète deduces some other formulations in his form using X, admitted for the duly accepted theorem. Precisely four formulations as there are four quantities being displayed, F, D, B and X. The last X from which we cannot escape is set up at the same level as the others. Four ways of expressing any one of the quantities in terms of the three other quantities. It is a display of analysis first referred to by means of a classification but Viète explicitely refers to analysis at the end: "*Vt hæc in Analyticis abunde demonstrata, & exemplificata sunt*"[12] (*ibid.*, 29). He organizes his material according to an algebraical script[13] and, moreover, he introduces the required formula by the word "δεδόμενον" each time. In the literary play of Renaissance texts, this is an allusion to Euclid's *Data* (Δεδόμενα); a typical text of analysis, for which some elements of a drawing are determined from other elements which are postulated as given. In short, Viète clearly proclaims analysis, and for our purpose we have no need to examine it in more detail.

The text does not stop here. Surprisingly—and the effect is deliberate—here there is a question in Viète's exposition: "Shouldn't we say that X will go down to nothing when magnitudes are in a continued proportion to infinity" (*ibid.*). If this is the first time that infinity is mentioned in the text, it was present ineluctably from the early lines. It was hidden in the literary expression used for the theorem: as it only mentions three things, the theorem cannot make any sense to any reader if conceived for a progression with a finite number of terms[14]. On the opposite side, using the game played by X from which infinity is revealed ("smallest term"), the literal transcription makes sense in both finite and infinite cases. Finally, with the notation X, a name is given to what provides an additional meaning to the literary form of the theorem. Then, abruptly, there is a change in the stylistic register of Viète's text. An opinion is given, as in any good scholastic text: "And Mechanists[15] will assure us that it vanishes as the smallest quantity subsides in the intellect only" (*ibid.*). In short, the reader is aware of what is suggested. In its literary form, the theorem sounds true for the reason that is suffices to make the smallest term of the literal form equal to zero. A form which can be said to be the indefinite writing of the sum of a geometric progression (n as a integer, the number of terms, is not specified and might as well be infinite). Isn't this the added value of algebra?

Viète's analysis could therefore end here with only the well regulated game of a computation: reduce to 0 an infinitely small quantity and obtain a formula quite close to the one we usually adopt when we reach for the sum of a convergent geometric progression,[16]

$$\frac{x_1}{F} = \frac{x_1 - x_2}{x_1}$$

The scholastic parenthesis might then just have been a stylistic effect. And analysis will have remained the main tool.

Indeed, the text proceeds further and from now on analysis recedes to give place to synthesis. A synthesis in the sense that there is a construction which answers the question: shouldn't we say that... The question is really about the maintenance of analysis. Synthesis symptomatically begins by a definition; in this case an original definition of an increment (*cremento*): "what the difference of [any] term of the ratio is to the [immediately] inferior term of the ratio, the smallest [magnitude] is to the increment" (*ibid.*, 29). For a progression with a finite number of terms, the increment Δ possesses a unequivocal definition $\frac{x_1 - x_2}{x_2} = \frac{x_n}{\Delta}$. But it obviously depends on the integer *n*, a parameter in a way too talkative in the literal form, and excluded by the literary one. We could better denote Δ_n, and write F_n as well, for the finite sum with n terms. In the case of an infinite progression, the definition of the increment can be read as $\frac{x_1 - x_2}{x_2} = \frac{x_\infty}{\Delta}$, or better said in the manner of proportions using then Δ_∞. Unfortunately, the second ratio is a quotient of two quantities, each one equal to zero (according to the "Mechanist" opinion); the quotient is therefore a non-assignable quantity. Equipped with such a definition, the result of a synthesis may however appear:

> "As the difference of [any] term of the ratio is to the [immediately] superior term of the ratio, so is the largest magnitude to the one composed of all terms plus the increment." (*ibid.*)

In algebraic notation,

$$\frac{x_1 - x_2}{x_1} = \frac{x_1}{F + \Delta}$$

To see this better, it is possible to rewrite it as:

$$F = \frac{x_1^2}{x_1 - x_2} - \Delta$$

The increment Δ corresponds to a failure; it measures what fails to an infinite sum when one stops after a finite number of terms. Nothing will fail once the infinite is reached. From a finite *n* to an infinite, from the literary meaning to the literal one, a continuity of meaning is restored, by means of a synthesis.

Proceeding further in this line of reasoning consists in establishing the need to put the so-defined increment to zero. At this step however, Viète is no longer looking for a complete reasoning: it seems enough for him to refer to a result which Archimedes splendidly and synthetically explained—"and there is a fact"— in the *Quadrature of the Parabola* (proposition XXIII; Archimedes OO, II, 310):

> "Let there be continuously proportional magnitudes to infinity[17], with an under-quadruple ratio, and let 3 be the largest of all. The composed magnitude will be 4. And there is a fact[18]; to these in continuous under-quadruple ratio magnitudes, the largest being 3, nothing as small as possible can be added without the composed magnitude being larger than 4." (Viète 1593, 29)

The allusive style is unequivocal: it is by a double *reductio ad absurdum* typical of the method of exhaustion that the increment can be verified to be zero. The only short way is to use the particular case of the Archimedean progression as if it were the general case. Continuity is restored on an historical order as well.

Viète still does not stop here. He went from analysis to synthesis; but he raised a question rather than having solved one. The reference to the tradition of the method of exhaustion of which Archimedes is the most celebrated artist, is in no way an authoritative argument. Viète does not even criticise this tradition; he merely states that it contains a type of satisfactory proof for which no sequence can be provided. Moreover, it seems impossible to follow an algebraical path, or rather, a filiation to the tradition would denature the algebraical way. Indeed, using an algebraical relation, Viète associates the smallest term of a progression to the increment. But there is no link with the double reasoning by contradiction alluded to, which would be enough to validate the theorem on the sum of an infinite progression. Then Viète essentially shows the requirement of a "new algebra". This algebra does not appear as a natural one. It has to deal with indefinite quantities like Δ or x_∞, for which a correct writing is available only in the case of a finite term progression.The new quantities can be combined in some algebraical way as their possible ratio is equal to a well defined ratio of finite quantities. And equating these quantities to zero according to the formula of likelihood, something true is obtained. Viète's is a testimony of this essential experience.

He then concluded by refusing an end and this is undeniably an appeal for a sequel. Viète explicitly says of the reduction to zero: "But Platonicians will agree with difficulty, as the whole of Geometry essentially lies in the intellect" (*ibid.*). Will the sequel be an analysis or a synthesis? Wavering has the value of erasing the differences. For our purpose, it is enough to have shown that in Viète's case the passage from one style to another in the direction of a necessary future, served to make us aware of the uselessness of a motion back and therefore helped to suspend the back and forth move. We recognize a suspended analysis in this text.

III Fourier or the Synthesis Appearing as an Analytical Necessity

With the appearance of the *Théorie analytique de la chaleur* (1822), the localization in analysis seems indisputable. Fourier at least displays the banner of an analysis, by using the specific adjective in the title of his book. Therefore, as there is no apparent ambiguity, we are compelled to present our study in a manner different from the one used for Viète's text. We first have to question the validity of the analytical reference. Using this title, couldn't Fourier mainly be displaying a stylistic filiation to Lagrange's *Mécanique Analytique* (1788). Published in 1811-1815, the second edition of this book, corrected by the famous author, was considered as the example of a mathematization of the real world. In fact, classifying the content of Fourier's book at an epistemological level, the analogy with Lagrange appears less deep than the title may at first suggest. It was Auguste Comte, a thorough reader of Fourier whom he was persistently inviting to attend his first course in positive philosophy during the year 1829, who understood that Fourier was competing with Newton's *Principia* (1687). For even if there are some traces of analysis, Newton's book openly maintains the genre of a synthetic composition which resulted in some stylistic obscurity as has so often been observed[19]. By endowing heat theory with its phenomenological and mathematical concept, the flux[20] (which is the analogous concept to velocity in mechanics, and even its exact mathematical counterpart as a derivative) and by using the technique of a thermal balance implying an invariance, Fourier succeeded in establishing a partial differential equation governing temperature. Thus is the so-called heat equation to which commentators usually reduce the Fourier's achievement from the point of view of physics[21]. In his turn and for the specific physics of heat, he thus realized the Newtonian program which had been exemplified by the derivation of differential equations of motion from universal laws of attraction.

> "I do not fear to pronounce, as if I were ten centuries from now, that since gravitation theory, no mathematical creation was more valuable than this one for the general progress of natural philosophy."[22] (Comte 1830-1842, l. 31, II, 592)

Thus Auguste Comte speaks of Fourier's achievements. And to increase the weight of this judgement, he adds something which is not far from the important distinction between a metaphysical era—Newton—and a positivist one:

> "even so, by seriously scrutinizing the history of those two great thoughts, we could find that the foundation of mathematical thermology by Fourier was less made ready than the foundation of celestial mechanics by Newton." (*ibid.*)

Such a judgement *ipso facto* states that Fourier's theory composes a synthesis: apparently it comes from nowhere and it is totally built and "positively" explained; it has therefore definitively acquired the status of a scientific and perennial work:

> "The new theories which are explained in our work are for ever united to the mathematical sciences and, like them, they rest on invariable foundations; they will preserve all the elements which they now possess, and will continuously grow in extention." (Fourier OD, I, xxviii)[23]

Thus Fourier did not hesitate to proclaim his achievements and he was taking advantage of a language which had been dominant for centuries, namely the language surrounding Euclid's *Elements*, always an admired model for synthetic presentation of the science of magnitudes[24].

Let us then give up the reference to Lagrange. The analytical way is perhaps not yet Analysis! This latter would then appear in the text of Fourier, not as a style subordinate to the explanation, but far better as a whole new branch of Mathematics. It is clearly during the 19th century that any specific denomination for Analysis was abandoned[25]: it is no longer *in Analysin infinitorum* as it used to be with Euler (1748), but forcibly without any adjective in Cauchy's *Cours d'Analyse* (1821). And this is more visible as the first part of the course accounts only for algebraical analysis. A contemporary of Cauchy, could not Fourier be the instigator of Analysis as well? For more than fifteen years, he had been refining the various aspects of his Theory: it is sufficient to read any page of the *Théorie analytique* at random to notice his chiselled wordings. A consultation of the long table of contents at the end of the book, where classification in the finest detail takes care of the very connections of the reasoning itself[26], would convince any reader that the literary structure of the text was deliberately chosen to adapt as close as possible both to the reasoning and to the part of the real which is investigated. "Looked from this point of view, mathematical analysis has an extension as large as Nature herself" (Fourier OD, I, xxiii), so he claims in his preliminary discourse to the *Theory*. If the word Analysis receives then a privilege, it stays in the book without any further definition. Darboux, later editing the *Théorie analytique* for the *Complete Works* of Fourier, will find himself obliged, in printing this sentence, to add a capital "A" to Analysis.

However, the organization of our quest would be upset if we were to pursue the building of Analysis on this path. We had far better go to the conclusion to his work provided by Fourier himself. There, he feels the need to explain that even though it is the main object of his *Théorie*, he has not chosen to derive in a unique form the various integrals found for the heat equation belonging to the various situations met within different kinds of solids subject to heat propagation. He claims that such "transformations require long computation and they suppose almost every time that the form of the results is known in advance" (Fourier OD, I, 525, n° 428). He thus affirms that he could not have purely followed an analysis, even in the sense Pappus acknowledged where analysis has to start from what has to be reached.

If we were to adopt the qualification of "historical" for Fourier's presentation we might avoid choosing between analysis and synthesis and reach some kind of equilibrium. This seems to be a valid statement to start with[27]. Using the word "historical" requires us to play with the double meaning this word usually takes in the sciences. It certainly means a narration, with its chronological and critical unrolling of a thought concerning an object of science, but it also means the account of a systematic look at the real world. This last meaning is precisely the one in "natural history", a familiar expression used throughout during the 18th century and early 19th century. Fourier is first of all an original thinker (or scientist) because while allowing to read history of his thought, he turns it into a history of Nature herself[28]. Individually neither an analysis nor a synthesis, but a history of the real to which reason belongs as well.

A history of thinking and a history of objects; this double function is an old one in the construction of science. The swinging implied by these meanings is certainly one of the major ambiguities of history of science as such, at least as an intellectual mode. And this explains why we are aiming at the stylistic swinging of a chiasmus. The *Théorie analytique* appears to be accomplished in the same way as any historical account which is always told using a past time; as any synthesis, the *Théorie* keeps no trace of a past and bears no error before a future. If the *Théorie* has to be an analytical discourse, it is because so is Nature herself; not only in the interpretations given of the efforts made to analyse it, but in the very way those natural effects are produced. At the end of a section "the object of which almost entirely belongs to Analysis", when he evokes the structure of a differential equation, Fourier aptly qualifies it as the equation of the phenomenon, because this equation represents "in the most distinct manner the natural effect. This is the principal condition we always had in view"[29] (*ibid.*, I, 525, n° 428). The equation is not a model, or a reduction. For Fourier, there exists no middle locus between a mathematical thought and the real; fiction is not a resource which, even through the assumed risk of a logical fault, might account for the adequation of a thought.

Could we say then that we have a synthesis of the analysis! Such a genitive case is used too rashly. In order that the expression might have a meaning which convenes to Fourier's work, we should have to consider, as at any cross-road with no sight-of-crossing sign, that the order of the two words, analysis and synthesis, is indifferent. If Fourier calls the motion which animates his Theory 'analysis', *in fine* he summarizes what explicitly is a synthesis[30]. He turns up the older definitions of the two names; he locates himself at the crossing of the chiasmus.

As a question, the adequation of the analytical style to the synthetic content makes the purpose of our inquiry. We have to understand why analysis only, by sheer accumulation of deductive signs, could not have been sufficient in Fourier's eyes to build the *Théorie*. It could have achieved the status of synthesis only once it was entirely accomplished, that is once ended. Synthesis would have been the result of the unrolling of analysis. However, Fourier himself prevents us from adopting such a compromise which would provide an orientation for the branches of the crossing by explicitly naming each one. His exposition of facts, so he claims, coincides with the discovery of the facts; it is an invention as such and therefore his account cannot be smelt into a synthesis, the unrolling of which necessarily requires some axiomatic method. Even a man like David Hilbert would never state that the axioms precede thought in an inquisitive mind: they have to become the frame for intuition as a construction of the mind. Nevertheless, it is a history of the inquisitive mind of a natural philosopher which is the true account of Fourier, and he claims that it is the account of Nature herself. Analysis and synthesis are unequivocally mixed.

Analysis and synthesis are combined in the fate of Fourier's work. Those two words intervene directly in his intellectual and objectal filiation, and they are to be simultaneously written. They are endowed with a precise meaning, and fortunately there is no questioning about it: it is simply decomposition and recomposition. It is after Fourier, in a way rather a long time after him but in an explicit reference to his work, that everybody spoke of the harmonic analysis of a function and of its synthesis[31]. In the same manner as for the adjectivation of Analysis, even the word function had to disappear when a branch of mathematics was finally organized—Harmonic analysis—but this is no restriction but a metonymy as this branch contains harmonic synthesis as well. The maintenance of the expression "Harmonic analysis" is a rare phenomenon in mathematics, a science which is generally chary of distinctions among its various enterprises; the expression of Fourier Analysis is less common, but with the same metonymy that implies synthesis as well. The last expression follows from the fact that elementary functions are necessarily associated with the very idea of a periodic function: they can be called "simple" modes[32], the obtaining of which for a given function comes from a computation of integral coefficients, the so-called Fourier coefficients[33]. Such is analysis. Once the coefficients associated to the modes are known, according to an infinite addition naturally induced by a numbering by integers—this is the numbering of simple modes—a function is entirely found again or reconstructed: this is its synthesis[34].

Even if we positively follow the mathematical practice of the domain launched by Fourier, we have not yet reached a clear and distinct explanation concerning an analysis which should be followed by a synthesis, in the sense that, historically and epistemologically, we cannot use for long the apparently nice but "frivolous" distinction made by Condillac who places a "before" and an "after" in order to point up the link between the two operations of decomposition and recomposition. Fourier's operations show this clearly. First obtained from a laborious algebraical technique proceeding through the elimination of variables, a computation of Fourier's coefficients acquires a rational transparency only once orthogonal relations intervene[35]. These orthogonality relations exhibit such properties of simple modes that each one may reach an independent existence; each one is taking advantage of the freedom and therefore of the status of a dimension in geometry. These relations provide analysis with its own legitimacy and shape analysis as an independant moment of the reasoning, *i.e.* of the proof. However, as efficient operations, such orthogonality relations are available at the very moment of the synthesis of a function only; and practically as well as formally they can be omitted from what could be seen as the pure moment of the analysis. In short, orthogonality relations cannot be metaphorically viewed as the knuckle-joint linking in this order analysis and synthesis. But curiously we have to ascertain that analysis does offer an explanation in its own right only once synthesis is concluded[36]. Contrary to what has so often been said with good reason by classical epistemologists for whom roads without crossings are the best warrant for a scientific construction—it is the no noise syndrome—, synthesis is not the justification for analysis. Synthesis is certainly not the occurrence of a formalization according to an accepted mathematical canon, from which we can absolve those scientists who are not looking for rigour[37]. In fact, it happens as a crucial experience, and possibly as the main mathematical activity, that the computation yielding Fourier's coefficients works correctly even if, at the moment of synthesis, we were to "forget" certain simple modes[38]. As the conclusive example requires some technical preparation, it will be given somewhat later. Fourier proceeds in the same way, giving it at the very end of his book (this is a supplementary proof, if such is required, that his display is not a linear one; we already used the qualification of enveloping display). Before proving, we go to the consequences. Analysis has its own independence; but it is not automatically conducive to truth. Synthesis is not a conclusion which functions as a validation; it is an interpretation of an earlier analysis which, in this very process, changes for a new meaning: a cycle begins.

Fourier has not underestimated the aporetic conclusion which confuses the order for intellectual operations, analysis/synthesis. He even cancels the opposition. An aporia, which etymologically is what prevents an idea from providing a

path, conspicuously gave him the possibility of creating a theory: he had to erase the opposition, or the parallelism and lack of a meeting point of analysis and synthesis. This seemed necessary to turn "simple" modes into "proper" modes. The adjective has a value of reality. The path followed by Fourier is a thought in itself. The chiasmus analysis/synthesis is no longer the algebraical effect of a presentation: it is part of the work of science.

Such modes, so Fourier explains, are intrinsically linked with a periodic function conceived as a mathematical object; the reason being that Nature so constructs them. The study is that of heat propagation in solids. Ineluctably, at least from an analytical study, periodic functions do appear in the case of heat.There exist "waves" of heat. Mathematically, a "wave" is a mixing of a periodic oscillation and of a decreasing exponential in the variable describing the distance from the heating source. Therefore, analysis reveals a phenomenal property in its own right. Proper modes make their appearance from physics analytically pursued, and they go far beyond periodic functions; they are appearing under the inventive pen of Fourier in many other circumstances, for example with the so-called Bessel functions if we wish to point out only one other example[39]. We find the essential fact which instaures a generality: proper modes are present in all phenomena of heat propagation, and this is why the word "proper" is physically valid. But they "properly" too happen with the harmonics in sound propagation or in the explanation of tides. Both are quite distinct physical phenomenoms. If Harmonic Analysis becomes a mathematical theory, it is because of its universality. But this brings no loss of a "proper" property: the simple character of a mode is not changed into proper by the technical play of the mathematical game which is unable to confer such a quality to its objects. Even by folding analysis into synthesis. Fourier has eliminated any "middle", even mathematics, between a thought—his thought—and the world.

The nature of these modes has to be the object of a proof, for which we are at the active cross between analysis and synthesis. However, if the chiasmus is not yet discernable, it is because we have not sufficiently enveloped it with mathematics. Fourier is not providing a rhetorical discourse; he intends to speak like Nature herself.

In which sense, in fact, could one prove the "proper" property of an object which is deduced or built from an analysis? As a form has been exhibited, there can be no doubt about the very existence of proper modes; synthesis does not play the somewhat restrictive part of an ontology. By the way, in the case of periodic functions, such modes are reduced to the brave functions sine and cosine for integral multiples of the variable and are quite elementary functions. Clearly, by leading to a reconstruction of a function from its proper modes, synthesis gives credit to modes in their status of proper modes. It is not sufficient enough as a proof. Here synthesis appears for what it is etymologically, just an addition. It is not sufficient for a good reason: in its own proof, synthesis shares the defect of analysis. It works, but it does not help the understanding. A scandalous situation, a contradiction indeed to the purpose of providing a proof of a "proper" property.

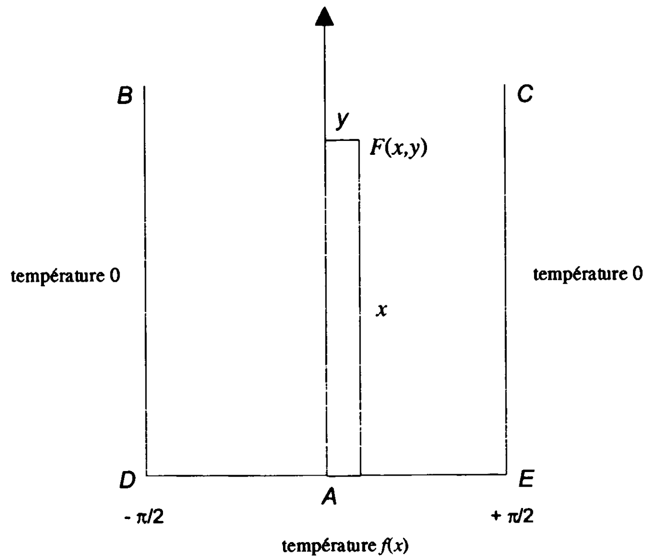
To get rid of the contradiction, the first way chosen by Fourier is just a bias in order to prove synthesis, *i.e.* the sum of a function developed into a trigonometric series (Fourier's series). It relies on the development of the function in power series. He then uses what was more or less called Taylor series, manipulated all through the 18th century, but certainly not rigorously proved, and eventually made the very basis of Analysis by Lagrange in his *Théorie des fonctions analytiques* (1797). Long as it is, with even a strange formal play on a typical constant like π used as a variable for differentiation—a game no longer authorized by acceptable science during the early nineteenth century—Fourier's proof sufficiently shows that he conferred on his manipulation no more value than a linking one. Fourier just helps to join his new mathematics with already known mathematics[40]. His bearing is a normal one for someone introducing an invention when one does not locate it as a revolution. The intention of this proof is not to mathematically fix what "proper" means; but this is the purpose of the theory!

There is no difficulty in proving or ascertaining the adjective "simple" for a mode. For the partial differential equation which governs heat propagation, a simple mode appears as a solution whose variables are separated: it has to be the product of a function of one of the variable by a function of another variable. This is, by the way, how from a computational point of view, such modes are obtained. It is a pleasant and efficient analytical characterization which the first year students usually are compelled to undertake. However, this characterization is a formal one; it cannot "prove" anything "proper"; it is a trick to reach such modes. Guile cannot provide a proof of what "proper" is!

There is another way which tempted Fourier, but it led him to nowhere. This failure is rather surprising to our modern eyes, in that the way is the one which will lead to proper vectors and proper values. Here the usual language adopted in English is unfortunately improper, and we have to think of the original German meaning of Eigen in Eigen-vectors or Eigen values. At least in French or in German, the maintenance of the adjective 'proper' or 'eigen' in linear algebra as well as in linear analysis, has a historical meaning. Fourier, effectively, shows some stability, and this stability is no longer a formal situation like the one where 'simplicity' just meant separation of variables. To explain this, we have now to enter some mathematics and at least a drawing, even if Fourier, as a presumed analyst, is rather parsimonious of such graphic representations.

We consider an infinite rectangular lamina: thus we have a two-dimension problem, with two space variables x, y and a physical mind may fancy that the lamina has an indeterminate depth. The two long lateral sides of the lamina are at a fixed temperature, melting water being a good choice in order to suggest the

idea of a muffler isolating the lamina at the sides, isolating it to the point of suppressing even the unavoidable dilatation which the lamina has to undergo. At the bottom of the lamina freedom reigns for the fancy of the experimentalist mathematician. He may impose a constant temperature—and this is how first Fourier began an analytic computation[41]—or he may impose any function. That is, he may decide any ordering of values of temperature along the internal DE, but only on this real interval where a real variable y is running: in other words, a free function $f(y)$ is available (variable x runs on the oriented median edge of the lamina). As we are at an intermediate moment of the analysis, time is no longer playing a role.



It is presupposed that the regime of heat is a permanent one, temperature is stationary as an equilibrium has been achieved between the lateral muffler and the given and generous source of heat at the base. Temperature at every point of the lamina is a function F of the space variables x and y only.

Fourier establishes a connection between the two functions, $f(y)$ at the bottom of the lamina—the given function—and $F(x,y)$ which is the sought for temperature in the lamina. Physically speaking, the connection seems obvious: only one regime of temperature is obtained. Fourier takes the opportunity to prove this uniqueness from the physics of the flux he has launched. Mathematically speaking, there is also a connection, and this is original as well. Function F is altogether a solution of a partial differential equation of the second order: the Laplacian of F is equal to zero

$$\frac{\partial^2 F}{\partial x^2}+\frac{\partial^2 F}{\partial y^2}=0$$

and it satisfies three more conditions:

$$F\left(x,-\frac{\pi}{2}\right)=F\left(x,+\frac{\pi}{2}\right)=0$$

$$F(0,y)=f(y)$$

$$\text{Lim } F(x,y)=0 \text{ for all } y \text{ in } \left[-\frac{\pi}{2},+\frac{\pi}{2}\right] \text{ where Lim } x=\infty$$

The indissoluble association of boundary conditions to the very partial differential equation is an innovation due to Fourier: it helped him to understand the correspondence between f and F, even at a moment when the concept of function was the prey of transformations to which the work of Fourier was to contribute[42]. It happens that proper modes are such that, if such a mode is an input at the bottom of the lamina, in the form of some function f, any trace of F at any horizontal segment of the lamina is equal to the given f (up to a constant multiplying factor). As an example[43], if $f(y) = \cos(11y)$, then $F(x,y) = \lambda f(y) = \lambda\cos(11y)$, where $\lambda = \frac{4}{11\pi}e^{-11x}$. From this remarkable stability, which we call to-day a proper property in a mathematical sense, Fourier deduces no mathematical action; he let it stay as a physical determination. In other words, he does not try to characterize "proper" modes functionnally as the invariants of the correspondance from f to F (up to a multiplying factor which we learned nowadays to call an eigen-value). The lamina remains as an intermediate object of the correspondance: it has not been identified through a relation. For Fourier, the proper character is not yet proven.

In a sense, we have not to regret Fourier's failure to detect the "proper" mathematical character in the invariance of a direction in a functional space. The irrepressible need of the determination led him to where what he brought is formidable: he affirms that synthesis of a function from the addition of its proper modes covers all thinkable functions. What prevails is the "arbitrary" character of the function; the adjective is thoroughly used by Fourier and associated with the expression

fonction générale. Sure enough, a combined mathematical and historical criticism may eventually say that this character was brought about by the pure analytical computation of Fourier's coefficients, in the sense that, for this computation, just the integral of a function operates, if we multiply the function by a proper mode[44]. In Fourier's time an integral was conceived as an area, therefore any "arbitrary" function possessed an area. However, our account of Fourier's display would not be sufficient if we were to restrict ourselves indicating a necessity due to the form of the computation; or, as could be said using an other description, we are too sensitive to the architecture of Analysis as it becomes independant of Geometry. Historically we think in terms of the building of Analysis. The possibility of the arbitrariness of a function, independently of the computational technique, is precisely for Fourier where the foundation of a mode as a proper mode lies.

We should less emphatically say that Fourier had the capacity to link two concepts, the one of proper mode and the one of arbitrary function. But this is not the knot of the whole situation.

In order finally to justify our description, the proof (which we consider now in order to show from what defect synthesis is suffering), is more remarkable because it plays with oblivion. Let us suppose that a "proper" mode, or better "simple" mode has been forgotten, for instance some $\sin(n_k x)$ for a certain integer n_k. Nothing would have been changed concerning the analytical computation of all other coefficients: we already said that the first part of analysis was independent of any synthesis. Strong as he is thanks to the orthogonal relations, Fourier however takes notice that any function synthetized with all the other proper modes would at least be orthogonal to this, willingly forgotten, mode. Forgotten, but still perpetuated by a sign

$$\int_0^{2\pi} f(x)\sin(n_k x)\,dx=0$$

The fact that an integral is zero is really a condition imposed on the function f. Therefore f is in no way an arbitrary function. Synthesis forgetting a mode is then a false synthesis. To give warrant to the arbitrariness of the temperature function at the bottom of the lamina is the way to offer to modes their "proper" property. "Proper" properly means an unavoidable property and thus it is an intrinsic property. Nature, which governs heat, cannot avoid proper modes: it is Nature who compels the mathematician, or better the natural philosopher, to think the abstraction of an arbitrary function, a function upon which no condition can be imposed. Obtained *via* analysis, the nullity of an integral helps to understand why forgetting some mode makes synthesis wrong: but this understanding comes only once synthesis is viewed as working for an arbitrary function. This condition of the arbitrariness—I dare call it that way—*ipso facto* intervenes for the practise of analysis itself. We were to eager to find a knuckle-joint between two styles and in fact we have found arbitrary functions as a general condition for both styles; we have acknowledged the shift from one style to the other. This is precisely what orders the Theory as constructed by Fourier; and it is the localization of a chiasmus.

In this move, the whole construction of the *Théorie Analytique* is at stake. To ensure the arbitrary character of the functions used or to avoid using just a name, Fourier has to exhaust all possible cases. He undertakes a systematical journey through different cases of heat propagation in quite different solids. Totality of the journey is necessary to fill the freedom provided by the arbitrariness of functions. From to-day, the word "total" precisely refers to the concept ruling mathematically proper modes, at least once some functional spaces are specified. A system of modes is total when there exists no function outside the zero function which may be orthogonal to all modes. Fourier did not have this ingredient at his disposal and was therefore obliged to verify the exhaustivity of proper modes by totalizing all possible cases. Analysis could provide a convincing proof of the proper character of a mode, only once all cases are synthetized. Each case, individually, is then a renewed analysis, and not simply a reproduced one. The risk of a chiasmus is not a unique risk in the theory: its very moment is therefore a scientific creation. With each case the theory can be falsified; the synthesis of one case helps the analysis of its successor. It also renews the analysis of the previous ones.

No redundancy at all[45]! Fourier organises its presentation according to an ordering of successive solid forms where heat propagates—lamina, prismatic beams, cylinders, armillas, or cubes—and each case provides, not only a confirmation, but its contribution to an understanding of propagation. This is an unavoidable proof that analysis alone is insufficient. Here is the answer to our original question. By specifying for each body a particular form, heat draws its proper geometry. This is this "reality", which has to be drawn for each case, and analyzed to each occurrence, from which at the end a structure —thermogeometry—is found. Each case has to be recomposed and informs the analysis of the previous case, thus modifying the meaning of analysis already made. Solved case by case, Fourier's thermogeometry is not the result of a synthesis: it is, in its ordered multiplicity, a direction for an analysis always reformed by synthesis.

As in any analysis properly done, there is the problem of the end of the theory, that is the moment where the back and forth move has to be stopped. It is here signalled by pure repetition, when any new case only brings computations but no renewed analysis. Fourier does not theorize, perhaps because he judges repetition

as not being sufficiently objective. And he was right, as his intellectually richest experience came long after he had thought his Theory ended.

IV Fourier's Transform: an Erasing of Synthesis

The most remarkable example of the efficiency of this style is provided by Fourier's transform, for which we first of all have to recall the extraordinary success in contemporary sciences, from solid-state physics to pseudo-differential operators, from wavelets and magnetic nuclear resonance, to a spectacular spread out in chemistry or medicine. It is the last case considered by Fourier in his quest for mere heat propagation[46], a case which he considered only in his text of 1822 almost without manuscript preparation. It is moreover a case for which the geometry is the flattest, just presenting an indescernible diffusion of "heat motion in an homogenous solid mass whose dimensions are all infinite" (*ibid.*, I, 387, n° 342)[47]. A case which would not be the possible focus of an analysis had not previous results shown the role of proper modes. The indiscernible geometry of the space can now be structured into a thermogeometry and therefore made analyzable: by a feed-back, in this process the mirror effect from the apparently dull geometry helps in turn to better "see" previous analyses of more particular cases.

By separation of variables, proper modes are easily found for the general "spatial" case which can be summarized by a partial differential equation (for which there exist a constant k, obviously a positive one which reflects physical parameters). This equation rules temperature allocation $T(x,t)$ where x runs through all real values—this is spatial freedom—and time t runs through real positive values only[48].

$$\frac{\partial T}{\partial t} = k\frac{\partial^2 T}{\partial x^2}$$

Right away, the case is a functional one as Fourier allocates an initial distribution of temperature—he writes $F(x)$—and makes clear, in his rigorous manner, that this function has to be an arbitrary one, under the specification that the function is defined over an (arbitrary) segment. A purely mathematical analogy is thus prepared with the case of the lamina for which the bottom temperature—involving a repartition on another segment—was also thought of as an arbitrary function on a given segment. Such a situation gave place to Fourier series (developed in a cosine series). Strong as he is from this result, Fourier may now begin by imposing a symmetry property to function F: it will be an even function ($F(x) = F(-x)$) as is the cosine function and the definition segment will have the origin as its middle point. But this is pure commodity.

Proper modes are many, $e^{-kq^2t}\cos qx$, with a positive real parameter q, and the trick for the computation is just to look for "simple" modes: Fourier no longer tries to prove their "property"; it has been seen in the lamina case, in the armilla case, etc. The passage from the discrete situation—that is all previous cases with a enumerable numbering of proper modes—to the continuous situation of the new geometry imposed by the freedom offered to parameter q, presents no difficulty; neither to Fourier nor to any mathematician of his time[49]. All have learned how to manage the passage by precisely using Calculus and by replacing a discrete sum by an integral. Without batting an eye, and by sheer analogy with the formula obtained in the lamina case, Fourier writes for the temperature T at point x and time t

$$T(x,t)=\int_0^\infty Q(q)e^{-kq^2t}\cos qx\,dq\,,$$

where Q is a function of the only variable q, the integral being extended to the whole domain of q, that is from 0 to ∞. This domain is not a fiction invented by the mathematician: it really is the space of what is "proper" and it does not depend upon the nature of function F or of the segment where it is defined. In the same way as with the lamina where one was compelled to suitably compute coefficients relative to the discrete family of proper modes, here "the difficulty lies in suitably determining function Q" (*ibid.*, I, 390, n° 345). The initial condition ($t = 0$) indeed yields a functional equation for Q.

$$F(x)=\int_0^\infty Q(q)\cos qx\,dq$$

In this equation, function F is known and function Q is the unknown. In other words, analysis has its object. But this is not the last aspect. In its turn, synthesis will change the object in order to present a new object to analysis: this will be the Fourier transform. But everything in its own order. In a suggestive fashion, Fourier speaks of an "inverse problem" as he is confronted to what, after I. Fredholm and D. Hilbert, we call an integral equation of the first class. He is conscious of the novelty and the interest of this "singular problem" (*ibid.*, I, 391, n° 346). In order to solve it, he reinterprets the result obtained in the lamina case: such a back and forth move is the main component of his method. For the lamina, the n-th order Fourier coefficient of the even function is obtained through an integration by summing the product of the temperature allocation by function cos nx. Then, multiplying this computed coefficient once more by function cos nx, and summing this time over all integers n, the original allocation f is found once again. Such is the lesson given by an investigation of the formula for even and 2π-periodic function. In order to avoid the exception of the coefficient of zero order, and precisely to avoid putting the analogy to come at a disadvantage, Fourier uses all integers, positive and negative, to exhibit a formula for the lamina case:

$$a_n=\frac{1}{2\pi}\int_0^{2\pi} f(x)\cos nx\,dx$$

and

$$f(x)=\sum_{n=-\infty}^{n=+\infty} a_n\cos nx$$

Thus, in the new case Q where the "proper" domain for q is no longer the set of integers but the interval of all real numbers from 0 to ∞, Q has to be obtained by an inversion

$$Q(q)=\tfrac{1}{\pi}\int_{-\infty}^{+\infty} F(x)\cos qx\,dq$$

Symmetry of the roles played by F and Q is now apparent: up to a constant, the same formula links the two. Judiciously, Cauchy (1817) speaks of "reciprocal function". An explicit involutive relation is available. This is equation (E) as Fourier calls it (OD, I, 408, n° 36) in order to magnify its importance[51].

$$F(x)=\tfrac{1}{\pi}\int_{-\infty}^{+\infty} F(\alpha)\,d\alpha\int_0^\infty \cos q(x-\alpha)\,dq \tag{E}$$

The straightforward meaning of (E) is an absurd one: an interpretation. But this task appears to Fourier more as the duty of his posterity than his own[51]. To award the merit of the invention of (E) possibly to Cauchy does not in fact modify Fourier's office. Not only was his part to provide a unique meaning to the word "sum" appearing in two occurrences in the lamina case—integration and discrete summation—but also to show that the two opposite functional operations of harmonic analysis and of harmonic synthesis were the same operation of a "sum" after a multiplication by a proper mode. Summation in the sense of integration in one occurence, summation in the sense of series in the other: the difference is a technical one, not a basic difference. This is what function Q brought to attention, and what the "spatial" case of heat propagation brought back to all other cases: Q is obtained from F by an "inverse" operation of the one which yields F from Q. An inverse operation, but as well a similar operation. Analysis and synthesis in this sense are formally identical operations. We already underlined the back and forth motion from analysis to synthesis; their formal identification, in some way, is the final result of the philosophical quest of Fourier.

He knows that the process he followed cannot replace a satisfactory mathematical proof: an analogy is no proof. But nevertheless the formula gives the general allocation of temperature. Fourier is eager to give an integral which, due to an exponential term, obviously converges:

$$T(x,t) = \frac{2}{\pi}\int_0^\infty F(\alpha)d\alpha\int_0^\infty e^{-kq^2t}(\cos qx)(\cos q\alpha)\,dq$$

Such a representation, without any doubt, is the aim of the *Théorie*, as the concrete numerical computation is never forgotten: it is the only way to get a verification. However, this concretization does not hid the main idea, a functional one, which is the "equivalence" between functions F and Q. This very idea moulds a second one, the idea of a transformation: so occurs the Fourier transform[52]. A transform for which, after what may be called experimental computations for special and elementary functions[53], Fourier individualizes a property. It is the transfer of a derivation or an integration operating on a function into a multiplication of the transformed function by a power of the variable, either positive or negative. This transfer is directly linked to the arbitrariness of the functions in order to fix a regulating principle:

> "By this transform, a function in some way acquires all the properties of trigonometric quantities; differentiations, integrations, summations of series are as well performed on general functions in the same way as they apply to trigonometric or exponential functions." (*ibid.*, I, 505, n° 419)

This is the use of such a principle which gives its value to distribution theory, a large and powerful generalization of the concept of function which was organized in the 20th century by Sobolev and Laurent Schwartz. The direction which has to be taken by posterity appears therefore as obvious for Fourier: "the use of such a proposition gives at once solutions of partial differential equations with constant coefficients" (*ibid.*). The solutions are precisely obtained using the method of "proper modes"; in the instance of these equations they are exponentials on which it is now possible to work inasmuch as "theorems of which we speak give to general and arbitrary functions the qualities of exponentials" (*ibid.*). "Representation" is thus an extraordinary tool for the "expression of complete solutions". Nowadays, it makes the kernel of pseudo-differential operators, an expression which wonderfully adheres to the idea of Fourier "representing" as well differentiation and generalizing it[54].

If technically speaking, for trigonometric series as well as for integrals, Fourier has shown that the analogy between analysis and synthesis lies in their being reciprocal, at the same time he justified the necessity of the back and forth motion followed in the *Théorie analytique de la chaleur*. His theory is altogether an analysis and a synthesis.

V The Scientific Sufficiency of a Chiasmus

In the two historical cases we investigated—Viète, Fourier—the passage from analysis to synthesis is no stylish pride of the author: it seems a required one, due to the nature of the mathematical objects and to the project of the inventor. Therefore it may be appraised as a scientific style. Moreover, in both cases, *de facto* there is a calling into question of what analysis is. But in both cases we find no soothing substitutions through synthesis. A synthesis may certainly be sought for by Viète, but he has not achieved it, which is an acknowledgement in itself. For Fourier, synthesis is viewed as impossible, or better not useful. In both cases, a criticism is dispatched in the mathematical way, that is on the edge of a problem, and not for itself. This is precisely the *in concreto* which Kant judiciously assigned to mathematics.

As such a mathematics is a culture, the question immediately arises of the relation between such criticism and more general thought. At the time when Fourier wrote, simultaneously a particularly severe criticism of the analytical way had been made by Kant and the scientific world itself was questioning its efficiency[55]. Kant invented the synthetic judgment *a priori* in order to maintain the idea of a progress, a progress which professionals themselves were no longer seeing as an inexorable chase[56]. One might think that this was the end of an era, and this was thought by contemporary thinkers[57]. In the time of Viète, the questioning was no less active; but it was in a context of analysis perceived as a new way, a way which may then stumble over tradition.

A suspensed analysis with Viète, a synthesis by analytical exhaustion by Fourier, the dissolution of differences between analysis and synthesis is striking in the two texts we have chosen. And the dissolution is independant of the particular meanings the concepts of analysis and synthesis may have had. What makes history then, is that in order to solve a problem—and I take the word in its general epistemological meaning—no appeal was made in either cases to some other intellectual resource. It thus ascertained that science is self-sufficient. The judgement which Blondel gave about the imposssibility of science to know itself is not always justified by the history of mathematics. It may be a valid judgment of the value of science in some times of restlessness, but not of all times of restlessness.

Centre F. Viète of the History and Philosophy of Sciences,
University of Nantes

Notes

1 The constant reference to Pappus as an origin for the meaning of the analysis/synthesis opposition, is certainly fascinating. We may think that François Viète has some responsibility for this reference in modern times (using perhaps the recent Latin edition of Pappus by F. Commandino (1588)):

> "Il y a une voye aux Mathématiques pour enquerir et rechercher la verité, laquelle est dite avoir ésté premièrement trouvée par Platon, et par Theon appellée Analyse; et d'icelles définies l'Assumption du requis comme concedé, par les consequences au vray concedé." (Viète IV, 13)

However, one should not neglect the following, also historical, fact: Viète explicitly refers to the Ancients in his *Isagoge in artem analyticam* (1591*a*) in order to offer a new kind of analysis of epistemological thought. He coins a specific name for this new analysis (exegetics). Therefore, Viète interprets past mathematics in order to justify the advent of a new approach. *Mutatis mutandis*, we could say the same for Pappus: by exploring analysis he was obliged to locate it opposite to synthesis and he also claims his novelty. Isn't it true that mathematics is an action?

2 To qualify the opposition between analysis and synthesis as part of a back and forth motion seems a natural conclusion once the usual reference to Pappus has been stated. We use a translation from the French version of Ver Eecke in order to emphasize Pappus' choice ("that is called the domain of analysis, as I conceive it..."):

> "Now analysis is the path from what one is seeking, as if it were admitted, through its consequences to something that is admitted in synthesis. That is to say, in analysis we suppose what is sought as if it had been achieved, we look for the thing from which it follows and again from what comes before that, until by regressing in this way we come upon some of the things that are already known, or that occupy the rank of a first principle; and we call this kind of method 'analysis', as if to say a reduction backwards." (Pappus VE, II, 477)

3 More Cartesian than it was possible to be, in his *Elémens de Géométrie* (1667) Antoine Arnauld imposes a "natural order" to the display for the various objects of mathematics; he was, paradoxically, aiming at shaping a "natural" thought. Cf. Gardies (1984, ch. 4) and Dhombres (fc *a*).

4 A general feature of mathematics as it was fervently taught in the first Jesuit colleges was to develop reasoning according to Euclidean synthesis. But no effort was made to render synthesis as an objective of the teaching. Cf. Dhombres (1996*a*).

5 In his thesis, P. Trabal (1995) tries to describe the move around modern mathematics using a sociological approach. He gives perhaps too much credit to the novelty of an event without inserting it into the long history of teaching mathematics.

6 By contrast, one could underline the weak part played by analysis/synthesis opposition in histories of mathematics which emphasize technical aspects. An example is provided by the *Eléments d'histoire des mathématiques*, according to Nicolas Bourbaki (1974). Cf. Dhombres (fc *b*).

7 It may be useful here to add a quotation from I. Kant, which Blondel certainly refers to, but he refutes the idea it implies:

> "[...] all the steps that Newton had to take from the first elements of geometry to his greatest and most profound discoveries were such as he could make intuitively evident and plain to follow, not only for himself but for every one else." (Kant 1790, § 47, quoted from Kant (CJM))

8 "Si fuerint magnitudines continuè proportionales, Erit vt terminus rationis maior ad terminum rationis minorem, ita composita ex omnibus ad differentiam compositæ ex omnibus & maximæ".

As I do not intend to enter here upon philological explanations, I will not explain why the word 'ratio' does not denote here the quotient of two successive terms of the progression, but, by metonymy, the progression itself.

9 That the progression is convergent to provide a sum is guaranteed by the decrease of the successive terms.

10 For a mind of the Renaissance, the intervention of F_n in a proportion is the equivalent of an exact equality providing F_n.

11 A possible reference is proposition VII, 12 of Euclid's *Elements*.

12 Viète's bibliographical reference is unfortunately obscure to us inasmuch as we find no identical algebraical computation in an earlier book of Viète (1591*a*). But some works of Viète are lost; cf. Grisard (w. d.).

13 In his use of letters, at least in geometry, Viète makes a distinction between vowels used for known quantities and consonants used for the unknown ones. In the text under scrutiny, only consonants appear. It must be understood that each quantity, in its own turn, is an unknown to be computed from the three others. One of the relations fixes the value of X and states "On the contrary if, D, B, F are given, X will be given. In fact it is certain

$$\frac{\begin{array}{c} B \text{ times } F \\ +D \text{ square} \\ -D \text{ times } F \end{array}}{\text{B}}$$

will be equal to X" (1593, 29).

In modern notation, this reduces to $x_\infty = \dfrac{x_2F+x_1^2-x_1F}{x_2}$.

14 If there is such a sophisticated literary composition, it means that Viète's reader is considered by him as his equal. Such a reader cannot fail to notice that in its litteral form the theorem uses only three imputs and this is contradicted by its transcription through four relations.

15 That is the way we chose to translate "*mechanici*". ("Et euanescere afferent Mechanici...")

16 This is the usual form of this result during the 17th century which is equivalent to our modern formula $\sum_{n=1}^{n=\infty} ax^{n-1} = \dfrac{a}{1-x}$. Apparently three traditions exist for the proof and in each one it is proved that something goes to 0. One tradition, a logistic one, is Viète's way which will be used by Fermat; a second one, a geometrical approach which inscribes computation in a drawing was founded by Gregory of Saint-Vincent; the last one, using a mechanical device, is chosen by Isaac Barrow (Dhombres 1995).

17 In Greek in the original (εως ἄπειρον). Archimedes' sums $3+\frac{3}{4}+\frac{3}{4^2}+\ \ldots$, and for this he establishes the formula for the remainder $3+\frac{3}{4}+\frac{3}{4^2}+\ldots+\frac{3}{4^n}+\frac{1}{3}\frac{3}{4^n}=3\left(\frac{3}{4}\right)$. A double reasoning by contradiction yields $3+\frac{3}{4}+\frac{3}{4^2}+\ldots+\frac{3}{4^n}+\ldots=4$. On this example, Viète's notations can be interpreted with $F=4$ and $\Delta_n=\frac{1}{4^n}$.

18 "*Composita ex omnibus fiet 4—Neque enim magnitudinibus* [...]."

19 Roger Cotes is explicit in his preface to the second edition of the *Principia* (1687; 2nd ed. 1713), when he describes the third class among those who cultivate natural philosophy:

> "They proceed therefore in a twofold method, synthetical and analytical. From some select phenomena they deduce by analysis the forces of Nature and the more simple laws of forces; and from thence by synthesis show the constitution of the rest." (quoted from Motte-Cajori translation)

20 The name with its meaning is due to Fourier.

21 With K, C and D being constants having a physicical meaning, heat equation in written in the form:

$$\frac{\partial T}{\partial t}=\frac{K}{CD}\left(\frac{\partial^2 T}{\partial x^2}+\frac{\partial^2 T}{\partial y^2}+\frac{\partial^2 T}{\partial z^2}\right)$$

where $T(x,y,z,t)$ is the temperature at point (x,y,z) and at time t. This equation is the kernel of the Theory. Analysis can then be described as all that has to be developped in order to make use of this equation. In the *Mécanique analytique*, Lagrange was putting to test a different ambition: he tried to interpret the whole science of motion from a unique abstract theorem, the so-called principe of virtual velocities. For sure, he found both Newton's law and velocity in its mathematical acception, but these two notions were not coming first. There *is* therefore a great temptation to attribute to Lagrange the organization of the analytical way, which has to be distinguished from Analysis.

22 An edition of Comte's *Cours*, unfortunately a critical one, was prepared by M. Serres, F. Dagognet, H. Sinaceur (Comte SDS).

23 References to the *Théorie* will be quoted from the edition of *Œuvres de Fourier* (OD), edited by G. Darboux. We add a numbering due to Fourier himself, in order to help references to the original book or to the English translation by A. Freeman.

24 To make a comparison with the perennial quality of the *Elements* does not imply that Fourier adopted an axiomatic method. In the *Théorie analytique de la chaleur*, we have no unfolding from propositions to propositions and from common notions to definitions. The construction is of a very different kind, for which the qualification of an enveloping movement is far better. We can but evoke this construction here, at least in the aspect which may concern analysis and synthesis.

25 To answer such a question, or rather to see its meaning, we should have to go back to the old debate on mathematical rigor. It is historically and mathematically well known that the qualification of rigor was given to Cauchy, for his *Analysis* (1821), but refused to Fourier for his *Théorie* (1822). Is it possible to conceive any kind of rigor if no construction project is at stake?

26 The table of contents takes twenty one pages of the *Théorie* in the edition of the Complete works of Fourier, for a text totalling five hundred and sixty three pages. Sometimes, this table shows more than what is explicitly proved in the corresponding article; as if Fourier had written his table in the manner of a programme to be completed and, later, would have had to reduce his ambitions. One would, at least, admit that such an ambivalence leads us to the trail of an analysis corrected by some kind of synthesis. But we will have to take a far longer path in our study of analysis and synthesis as organized by Fourier.

27 We cannot properly justify here such a description of the work done by Fourier. Many authors have thoroughly described the *Théorie analytique de la chaleur*, first of all Auguste Comte whom we already quoted. There is also Gaston Bachelard (1928). Among historians, we may quote I. Grattan-Guinness (1972) and J. Herivel (1975) and, with the ambition to deal simultaneously with the biography and the scientific work, J. Dhombres and J.B. Robert (1996).

28 Properly speaking, history of science, *i.e.* history of what was done before Fourier, almost never intervenes in his *Théorie*. Probably this refusal of a past is based on the fear that it may bring a kind of contingency to the construction; it may generate unjustified images contradicting the objective of unrolling a proof which pretends to be as close to Nature as possible. In other words, anything concerning a past history will appear under Fourier's pen as a counterpoint. It thus has two purposes; one is to measure the progress made by Fourier himself and the second is to make past errors conspicuous, in order to avoid them. In a very concrete way, we find here the attitude of Auguste Comte about the positive interest of history of science. And this is precisely where he mentions analysis and synthesis:

> "Various sects of metaphysical philosophers so abused, for a century, of those two expressions, using such a variety of logical and deeply different acceptions, that any righteous mind to-day should loath to introduce them in the discourse, at least when the circumstances of their use do not specify in a natural way their positive meaning." (1830-1842, l. 35, vol. III, 33)

29 Perhaps we should linked this with an expression which Newton used, "the nature of things".

30 Although commentators frequently overlook its meaning, the synthetic aspect is very strong in the remarkable *Remarques générales sur la méthode qui a servi à résoudre les questions analytiques de la chaleur* (*General remarks on the method which has been used in order to solve the analytical questions of heat*; Fourier OD, I, 524-531, n° 428). We cannot avoid noticing that the method itself is not stated as being an analytical one: the qualification is only used for the questions which the *Théorie* arouses.

31 The history of the expression "harmonic analysis" is a curious one: it started from the domain of mathematical instrumentation during the 19th century (*Harmonische Analysatoren*) to the theory during the 20th century (as in the title *Harmonic Analysis* used by Norbert Wiener (1930 and 1938)).

32 For a 2π-periodic function, if we add the unit function, those simple modes are $\cos nx$ and $\sin nx$ where the integer n runs from unity.

33 To do the harmonic analysis of a 2π-periodic function is to associate to this function its Fourier coefficients

$$a_n=\frac{1}{\pi}\int_0^{2\pi} f(x)\cos nx\,dx \text{ and } b_n=\frac{1}{\pi}\int_0^{2\pi} f(x)\sin nx\,dx \text{ for } n\geq 1 \text{ and } a_0=\frac{1}{2\pi}\int_0^{2\pi} f(x)\,dx$$

Fourier was obliged to explicitly state the boundaries of a definite integral: his notation is so instrumentalized that the integral becomes an operator. In order to explain Fourier's integrals, he later will use a_n for negative integers n.

34 The synthesis of a 2π-periodic function is, using its Fourier coefficients, to reconstruct f from the infinite sum $\sum_{n=0}^{\infty}(a_n \cos nx + b_n \sin nx)$.

35 Such orthogonality relations are of the form $\int_0^{2\pi} \cos nx . \cos mx dx = 0$ for $n \neq m$. J. B. Pécot (1992) provides an excellent historical and epistemological presentation of these relations over two centuries.

36 I am not pretending to reconstruct the genesis of invention in the case of Fourier in a few lines; I am not trying to confirm or to refute what he himself claims. I already said that the genesis he describes is presented by Fourier as a part of his *Théorie*; both as a tale and as an account: therefore I mainly keep the order he has given. Whatever is the computation leading to Fourier's coefficients, in the precise case of orthogonality relations obtaining them is always a second move. Even if such relations were unconsciously copied by Fourier from Euler, Fourier first presented analytical computation for the coefficients, both in his early manuscripts as well as after he has had time to synthetically polish his *Théorie analytique de la chaleur*. The book issued in 1822 is the last form of many earlier manuscripts, a first and complete one finished in 1807, a second in 1811, part of which was published by the Academy of sciences (Fourier 1819-1820) later after obtaining a "Grand Prix" in January 1812.

37 If I willingly omitted to stipulate as a preamble that Fourier's work was inscribed in physics, it was to avoid, at least for a modern mind, the anachronistic opposition between pure and applied mathematics. I wanted to avoid a too easily thought prejudive of a weaker kind of rigor for a mathematician working on real objects and on the real world, for whom the distinction between analysis and synthesis could have been minimal, distinctions seemingly relevant to the pure world of mathematics only.

38 The example of the so-called Bessel's function is an important one for Fourier. The reason of the emphasis is that it helps him universalizing his method by removing it from the too restrictive category of trigonometric series. Orthogonality of the Bessel functions, which is certainly not an obvious result as in the case of trigonometric functions, becomes therefore both a tool and an explanation. This orthogonality interprets the orthogonality of trigonometric functions: it is not only viewed as a generalization but, as an understanding.

39 Once more, we have to rely on what the reader knows of Fourier's mathematics (see bibliographical list); we are in no way attempting to describe the originality of his treatment of the so-called Fourier series, Bessel functions or of the Fourier integrals.

40 Both in physics and in mathematics, Fourier's theory is literally unchanged; it has been the subject of a considerable formalization by the practise of teaching. Therefore, the objective of the proof for a "proper" character no longer appears as essential: it seems already known. This is often the result of the conjugate weight of history and objectivity: this is also the main difficulty in any history of objectivity.

41 With a function $f(y) = 1$, Fourier was compelled to express 1 as a trigonometric expansion:

$$1 = \frac{4}{\pi}\sum_{n=0}^{\infty}(-1)^n \frac{1}{2n+1}\cos(2n+1)y$$

It gave him the way to express temperature $F(x, y)$ at any point (x, y) of the lamina.

$$F(x,y) = \frac{4}{\pi}\sum_{n=0}^{\infty}(-1)^n \frac{1}{2n+1} e^{-(2n+1)x}\cos(2n+1)y$$

42 This transformation of the function concept is certainly one important part of the constitution of Analysis as a domain. The fact that Fourier is linked with it is not just a chance. It is part of his project: the *Discours préliminaire* of his Theory is explicit.

43 In general, for $f(y) = \cos(2n+1)y$, $F(x, y) = \lambda f(y)$ with $\lambda = \frac{4}{\pi}(-1)^n\left(\frac{1}{2n+1}\right)e^{-(2n+1)x}$.

44 In the twenties of the 19th century, Cauchy has ended this conception by defining a definite integral from "Riemann's sums". In the process, area becomes a property but not a universal one. Thus, a continuous function possesses an area, but not necessarily an arbitrary function. Fourier took no notice of this change.

45 Contrary to what has been claimed by some positivist commentators, even like G. Bachelard: they regret that Fourier renews his analysis in each case, and therefore forget the "proof" by exhaustion provided by Fourier. In other words, they take for granted the claim of Fourier's adequation to the world, whereas the author makes efforts to prove it. In this sense, scientific positivism is not a defect of Fourier!

46 Sumptuously entitled "On diffusion of heat", the last chapter of the *Théorie analytique* signals that no particular geometrical body overtightens the spread of heat.

47 The ordering of cases where heat propagation is to be studied is an important part of the construction of the theory; it is neither an organization issued directly from the empirical world; nor an organization ruled by the criterium of Cartesian simplicity as the simplest case, the purely spatial one, is the last. The ordering has as its objective to let analysis and synthesis interact.

48 For reasons of symmetry, the three space variables are reduced to one only. As usual with Fourier, even with a final case, a first step begins by an analysis and therefore by a reduction of the problem. This simplified model has many possible interpretations: one is the diffusion of heat in the space when the temperature is known in a band (portion between parallel planes) and constant on each intermediate plane.

49 Is it necessary to recall here that, concerning sizes, there is no difference made during the time of Fourier, between an enumerable infinite and a continuous one. Cantor will exhibit the difference in the 1870's, opening a new era for mathematics as a whole, and for analysis in particular.

50 Equation (E) is written in the general case and F is no longer required to be an even function; this explains only $\cos qx \cos qd$'s replaced by $\cos q(x-a)$

51 Posterity will work as Fourier predicted: it only took far more years than we expected and in the process the memory of Fourier as a decent mathematician will suffer. We have attempted to "tell the story" in the last chapter of Dhombres and Robert (1996).

52 Let us give a standard definition of Fourier's transform.

53 Thus, he computes the Fourier transform for power functions and is led to

$$\int_0^{\infty}\frac{\sin u}{\sqrt{u}}du = \int_0^{\infty}\frac{\cos u}{\sqrt{u}}du = \sqrt{\frac{\pi}{2}}$$

Many other formulae are given, a sort of first dictionary for Fourier transform.

54 The main difference between to-day's attitude and Fourier's way is that he realizes the transform as describing the operations duly made by Nature. On the contrary, the modern point of view is a formalist one: it is just the adaptation of a theory, using an analytical form subjected to algebraical handlings, in order to find solutions to partial differential equations.

55 B. Timmermans (1995) remarkably pointed this philosophical inquiry, and doubt, about analysis at the end of the 18th century.

56 To recall the existence of a restlessness, it is enough to mention some sentences of Evariste Galois. He, around 1830, proposed to jump over computations, as analytical deductions were no longer inventive tools.

57 In a collective way, as it represents the opinion of the members of the First Class of the Institute, the impression of having to create the conditions of a new era can be seen in Delambre (1810).

MORITZ EPPLE

STYLES OF ARGUMENTATION IN LATE 19TH CENTURY GEOMETRY AND THE STRUCTURE OF MATHEMATICAL MODERNITY

I Introduction

In this paper, the distinction between analysis and synthesis in mathematics will be related to a second distinction, that between concrete and abstract forms of mathematical argumentation or, more generally, of mathematical practice.

As discussed in other contributions to this volume, the distinction between analysis and synthesis in mathematics has a long history, involving topics of a rather different nature. There is the proof-theoretical aspect, which appeared first in the ancient Greek uses of the term. There is the aspect of epistemology, which played a central role in Descartes' *Discours de la méthode* and Kant's *Kritik der reinen Vernunft*, bearing on central issues in the philosophy of mathematics; and there is the aspect of two different research styles in geometry, made possible by the merging of geometry and algebra in early modern times and which evolved into a great controversy in 19th century projective geometry.

The situation with regard to the distinction between concrete and abstract concepts, knowledge, or argumentations is similar. Again, this distinction has a long history, including its connections with mathematics. Suffice it here to say that Aristotle used the Greek counterparts of abstraction (ἀφαίρεσις and χωρισμός) to describe the ontological status of the objects of mathematical knowledge as well as the epistemic perspective which mathematicians make their own in looking at real (that is for him: concrete) objects as mathematicians[1]. And even more than is the case with the terms 'analytic' and 'synthetic,' the expressions 'concrete' and 'abstract' have often been used in a rather intuitive way, without explicitly introducing them as notions with a clear meaning. (Even though there is at least one technical sense to which one could refer: namely the technique of defining mathematical terms "by abstraction", *i.e.*, by means of invariance under an equivalence relation[2].)

Here I will not try to give a comprehensive history or philosophy of the role of this distinction in mathematics or even in modern mathematics. Instead, I want to

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 177–198.

begin my discussion with a rather limited historical question, namely: what became of the controversy between the analytic and the synthetic style of geometry towards the end of the 19th century? If one uses the term 'mathematical modernity' for the period *after* the great changes in 19th century mathematics (as I shall do), then the controversy about analytic and synthetic geometry seems to be a *premodern* affair. Later there arose a new, *modern* difference in geometrical style, exemplified by the geometric writings of Felix Klein on the one hand, and David Hilbert on the other. It is a difference of this latter type which I want to describe in the following, using the distinction between a concrete and an abstract style of mathematical reasoning.

After a few remarks on the historical developments in question, I will try to make my use of the terms 'concrete' and 'abstract' a little more precise philosophically. It will turn out that, as in the case of the analysis-synthesis distinction, the difference between an abstract and a concrete mathematical argumentation is not confined to geometry, but represents a rather general difference in the style of mathematical reasoning. Finally, I want to relate this difference to the historical reconstruction of mathematical modernity due to Herbert Mehrtens. My proposal will be *to use the distinction between abstract and concrete mathematical styles as an internal criterion to judge the modernity of a piece of mathematical research.* In the course of the discussion, a historical example—the invention of the braid group—will be discussed in some detail in order to bring out how this criterion could work in historiographical practice.

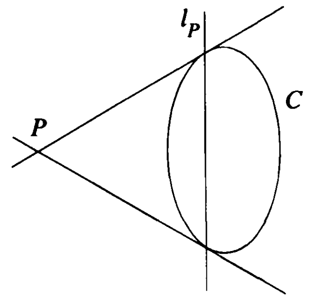
II From Synthesis and Analysis to Concrete and Abstract Styles of Mathematical Argumentation

II.1 Concerning the development of geometric argumentation during the 19th century, I shall restrict myself to some rather general remarks, most of which are due to the historical writings of Felix Klein. Certainly, they do not really capture the complexity of the historical development. However, they may serve the purpose of setting the stage for the discussion that follows. Let me begin by recalling some aspects of the controversy between synthetic and analytic geometers in the early 19th century.

It is well known that a revival of a "pure" approach to geometry was advocated by important pupils of the French mathematician Gaspard Monge[3]. This approach avoided the algebraic formulation of geometric relations which had proved so successful since the appearance of Descartes' *Géométrie* (1637). Instead, a research program gradually evolved which aimed at finding and using purely geometrical techniques to investigate properties of various geometrical objects in the plane or in space. A typical example was Poncelet's use of the machinery of the polar correspondence between points and lines with respect to a given conic section in order to translate theorems about point configurations into theorems about lines and vice versa. This research program, which eventually also found supporters in Germany, was particularly successful in the investigation of projective properties of geometric figures. For instance, Jacob Steiner had shown in 1832 how to generate conic sections and certain surfaces by means of projective correspondences between pencils of lines or planes[4].

On the other hand, some French and German mathematicians immediately realized that the projective properties which had become the focus of geometrical research could equally well be treated by means of algebraic equations. The main step in this direction was the introduction of adequate systems of coordinates by Möbius and Plücker in the late twenties of the last century. The relation between pole and polar with respect to a given conic thus appeared, for instance, as a simple consequence of a bilinear equation in homogeneous coordinates. It did not take long before mathematicians like Plücker and Hesse handled the formulas of projective geometry quite masterfully and could use them to establish astonishing facts like the configuration of inflection points of a general curve of third order. Their achievements contributed essentially to the rise of the new field of algebraic geometry.

synthetic *analytic*



$$C: f(x,y,z)=0$$
$$P:(x',y',z')$$
$$l_p: \frac{\partial f}{\partial x}x' + \frac{\partial f}{\partial y}y' + \frac{\partial f}{\partial z}z' = 0$$

Figure 1: Pole-polar correspondence

II.2 It soon became clear that most parts of projective geometry could be formulated either synthetically or analytically, and both parties competed in re-proving results of the other party in their respective idioms. Thus it is obvious that these were not two different branches of mathematical knowledge but rather two different modes of presenting, acquiring and justifying this knowledge. Modern theories of scientific knowledge have furnished us with a number of concepts to describe such differences. Ludwig Fleck's notion of a thought style or Gerald Holton's notion of a pair of methodological themata would apply here (Fleck 1980 and Holton 1978).

This view was expressed already by Klein in his *Elementary mathematics from a higher standpoint* of 1908:

> "Synthetic geometry is that which studies figures as such, without recourse to formulas, whereas analytic geometry consistently makes use of such formulas as can be written down after the adoption of an appropriate system of coordinates. Rightly understood, there exists only a *difference of gradation* between these two kinds of geometry, according as one gives *more prominence to the figures or to the formulas*. [...] In mathematics, however, as everywhere else, men are inclined to form parties, so that there arose *schools of pure synthesists and schools of pure analysts*, who placed chief emphasis upon absolute 'purity of method.'" (Klein 1908-1909, II, 55)

To show that the controversy lay in fact on this level, we may look at the critical objections of the synthetic geometers against analytical arguments. One such objection ran as follows: In a sequence of algebraic manipulations of a formula, it may be impossible to keep track of a sequence of geometric steps to which the formal manipulations should correspond. Therefore, we arrive in the end at a geometrical statement without knowing what its place in the system of geometric truths is. As Chasles put this argument: "Is it then sufficient in a philosophic and basic study of a science to know that something is true if one does not know why it is so and what place it should take in the series of truths to which it belongs?"[5] Obviously, Chasles refused to consider an analytic derivation as a *adequate justification* of geometric knowledge, even though he allowed for the *correctness* of the result of such a derivation.

Synthetic geometry thus appeared as a form of methodological purism. A particular argumentative context was specified—for example, the geometry of systems of projection rays[6]—and criteria were given which singled out the accepted types of questions and arguments relative to that context. The same was true for geometers with strong analytic commitments: here the argumentative context was the manipulation of algebraic equations in the space of homogeneous coordinates[7].

II.3 In the second half of the 19th century, the most fruitful lines of geometrical research were no longer structured by the research programs of analytic and synthetic geometry. These lines were, first, the one leading to the development of algebraic and differential geometry, and, second, the line leading to a strictly axiomatic approach to geometry. Klein's later geometrical writings were intended to convey to the reader some main ideas of the first line, ideas which were due to people like Clebsch, Riemann, or Lie. To Moritz Pasch and David Hilbert we owe the classics of the second line[8]. Let me briefly illustrate this reorientation with

some remarks pertaining to Klein's *Vorlesungen über höhere Geometrie* (1893) and Pasch's *Vorlesungen über neuere Geometrie* (1882).

Felix Klein had been one of the first to make clear that the opposition between analytic and synthetic geometry had lost its importance. In a note to his *Erlanger Programm* he had written in 1872: "The difference between recent synthesis and recent analytic geometry has no longer to be considered as an essential one, since the ways of reasoning on both sides have gradually evolved into quite similar forms." (Klein 1872, 74) Later he spoke of a "certain petrifaction" in geometry, due to the exaggeration of purist orientations[9].

Klein himself avoided a commitment to one of the sides. Early in his *Vorlesungen über höhere Geometrie* he said: "We pronounce it already here as a principle that we shall always combine the analytic and the geometric treatment of our problems and will not take a one-sided point of view." (1893, 26) In fact, Klein himself built both aspects simultaneously into his own unifying conception of geometry. If he proposed to study geometric properties in terms of invariants under a group of transformations, he also combined new algebraic notions with typical synthetic questions. For the topics presented in his *Lectures on Higher Geometry*, he favoured the name "algebraic geometry," making explicit his interest in the geometric properties of algebraic objects, from zero sets of polynomials to differential equations. The list of topics mentioned is—as with most of his writings—impressive. It includes, besides traditional material of analytic and synthetic geometry, multilinear equations and determinants, quadratic forms, rational and algebraic functions, algebraic curves and surfaces, Gaussian differential geometry, differential equations, invariant theory, group theory, Riemann surfaces, and some of Lie's ideas. But also he hinted at subjects like graphical statics or the theory of cogwheel profiles.

II.4 Like Klein, Moritz Pasch acknowledged the importance of synthetic as well as analytic points of view. In the Preface to Pasch's *Lectures* of 1882, we find the remark: "Analytic geometry has learned from synthetic geometry, and in case of a further fusion, there may emerge a higher geometry of a unified nature." (1882, 2) Perhaps, Pasch would have accepted Lie's or Klein's geometrical writings as a candidate for that higher, unified geometry. However, his own conception of geometry was directed at different aims. As is well known, he strove for a "pure," axiomatic development of elementary geometry, making it a rigorous mathematical theory by establishing its theorems on the basis of the smallest possible set of "core notions" and "core propositions" (*ibid.*, 4 and 15). His basic notions and propositions are synthetic notions like points, planes, and incidence, and Pasch even placed his work in the tradition of synthetic geometry (*ibid.*, 1). Only at the end of the book do we find a discussion of coordinates and of the continuum of real numbers, by which, as he says, analytic geometry is made available for the field of projective geometry (*ibid.*, 179). However, it is quite clear that Pasch's central aim was not intuitive, but conceptual, logical clearity. This comes out in his extension of the use of the basic notions, *e.g.* the use of "point" for a "bundle of rays" which reduces the number of necessary basic propositions, or his famous criticism of the logical gaps in Euclid's *Elements*.

In Pasch's book, we find again a consciously cultivated purity of method. We do not, on the other hand, find the wealth of connections to other mathematical disciplines present in Klein's lectures. Neither do we see Pasch switching constantly between algebraic, geometric or even intuitive arguments. He remains strictly within the conceptual framework set out at the beginning of his presentation.

In this methodological respect, there is but a small step to Hilbert's *Grundlagen der Geometrie* (1899)[10]. Certainly, in Hilbert's text the interpretation of the axiomatic method is rather different from Pasch's view. (For the latter, geometry is still to be considered as part of "natural science" (*ibid.*, 3); the basic notions and propositions encode empirical evidence (*ibid.*, 16).) Moreover, the mathematical treatment is complete in a quite different sense. But the style of Hilbert's text, the strict adherence to a well-defined argumentative context and method, is quite close to Pasch's and indeed very far from Klein's.

II.5 The difference between the two lines of geometrical thinking connected to the names of Klein (or Riemann or Lie) on the one hand and Pasch or Hilbert on the other is not merely a difference in style but also a difference in the topics investigated. The inquiry into the relations between curves, surfaces and algebraic function theory leads to different mathematical questions than those concerned with the relations between the different groups of geometrical axioms. However, it is obvious that there is still an important difference in style between Klein's *Lectures on Higher Geometry* and Pasch's *Lectures on Recent Geometry*. It is a difference in style of this kind which may be understood as replacing the issue of a synthetic or an analytic treatment of geometry in the context of mathematical modernity[11]. In order to mark this shift, let me propose to use the distinction between a "concrete" and an "abstract" style of geometrical argumentation. For the moment these are but two names. I want to explain my choice in the following, making the notions of a concrete and an abstract argumentative style more precise at the same time.

Let me begin by noting two rather obvious features of the shift from the analysis-synthesis opposition to that of the concrete and the abstract. *i*) While the beginning of the century had seen a controversy between two competing, more or less purist methodologies, the interesting opposition by the end of the century is better described as one about methodological purity *vs.* methodological diversity. Pasch and Hilbert made a deliberate choice of methodological purism. Klein, on the other hand, explicitly favoured the use of different methods, and most of his

mathematical achievements are closely related to this diversity of methods. *ii*) The second feature is a very different view of the generality of a piece of mathematics. The axiomatic style of Pasch and Hilbert sought to guarantee the general applicability of its results by reducing the argumentative context to its uttermost minimum. (It is only implicitly encoded in the axiomatic basis of a mathematical theory.) In Klein's style, on the contrary, it was precisely the density of the argumentative context, the rich variety of topics and points of view discussed, which was intended to show the general relevance of the ideas presented.

III A Philosophical Analysis of Concrete and Abstract Arguments

III.1 At this point I would like to sketch a philosophical analysis of the relationship and differences between an abstract and a concrete style of argumentation. Thus I leave history aside for a moment and make a digression into the philosophy of mathematics.

It seems that a more precise description of abstract and concrete arguments can start from two premises. The first is that mathematical arguments are pieces of mathematical practice, *i.e.*, we have to deal with a question of the pragmatics of mathematics. The second premise is that one should begin with a consideration of the relation in question from a local point of view. That is to say, one should look at a small piece of argumentative practice and try to explain the difference there.

I take the practice of mathematical argumentation to be a complex of actions, such as defining, conjecturing, proving, etc.[12] (These *mathematical actions* are immersed in communicative and *social actions* like publishing, giving talks, applying for positions, organizing meetings, and the like.) Argumentative practice is organized in smaller units, which I shall call '*mathematical games*', using a notion for complexes of actions going back to Wittgenstein[13]. In the first half of the 19th century, synthetic geometry was guided by a set of methodological constraints that defined a certain mathematical game, and similarly, analytic geometry may be viewed as another, though related, argumentation game. Such games may be described by specifying the possible situations belonging to the game and the rules guiding possible actions in these situations. A part of the rules is determined by, or rather, determines the mathematical subject of the game (*e.g.* geometrical objects), and another part fixes the techniques, types of arguments etc. considered legitimate. Thus the games of analytic and synthetic geometry show a partial, but not a complete correspondence of action-rules. For instance, the pole-polar correspondence could be used in both games to derive dual theorems. (A closer look shows, however, that we have in fact two rules here: a purely geometric construction, on the one hand, and a correspondence determined by a bilinear equation, on the other[14].)

We can immediately translate the two features of an abstract and a concrete mathematical style noted above into this language. A domain of mathematical argumentation is methodologically pure if it belongs to a single, well-defined argumentation game. Diversity on the other hand means playing more than one game at a time, or switching frequently between different argumentative contexts[15]. Whether a context of argumentation is (relatively) "poor" or "rich" may be judged by the degree of detail of the descriptions of situations and rules of the game(s) in question. Still, this may not be clear enough. Let me thus turn to my example, by means of which I can complete the local description of the distinction between an abstract and a concrete argument.

III.2 The example was included by Wilhelm Blaschke in the third edition of Klein's *Lectures on Higher Geometry*, published posthumously in 1926, as one of five topics under the heading "Examples of geometric research of the last decades" (Klein 1893). In fact, it is a topological example, namely Artin's *Theory of braids*, which had appeared in 1925 in the *Hamburger Abhandlungen* (Artin 1925-1926). The inclusion of this example into Klein's book is revealing for several reasons. First, it shows how broad the conception of geometry was which Blaschke ascribed to Klein, and in fact I think he was essentially correct. Second, Artin's work on braids was rooted in Klein's favourite subject, the geometric theory of algebraic functions. (For details concerning the history, of the next §IV.6.) Third, it was one of the few topological problems which could in some sense be solved completely by group-theoretic methods at the time. This last feature makes the example particularly suited for my purposes.

Artin defined his braids as follows:

> "By a braid Z of n-th order we understand the following topological object: Let a rectangle with opposite sides g_1, g_2 and h_1, h_2 (the 'frame' of Z) be given in space. Let n points $A_1, A_2, \ldots, A_n$ and $B_1, B_2, \ldots, B_n$ be given on each of the sides g_1 and g_2, counting from h_1 to h_2. With every point A_i we associate uniquely a point $B_{r(i)}$ with which it is connected by a curve m_i without double points and without intersections with any other curve m_k. Let the curve m_i be oriented from A_i to $B_{r(i)}$." (*ibid.*, 47; see fig. 2.)

In addition, Artin required that every curve cuts a plane orthogonal to h_1 and h_2 at most once.

Two such braids are considered "equal" (says Artin), if they can be deformed into each other without self-intersection. Obviously, Artin introduces here an equivalence relation between braids without being too explicit about that, as was still common practice at this time. (In fact, definitions by abstraction had been analyzed logically only some 20 years earlier, by Peano (1901) and Weyl (1910 and 1913)[16].) Further on, he sometimes speaks of the topological objects as braids, and sometimes of the equivalence classes under isotopy. Only in his second, more

rigorous attempt to deal with braids in the late 1940's Artin did draw a clear distinction between "weaving patterns" and "braids," which are equivalence classes

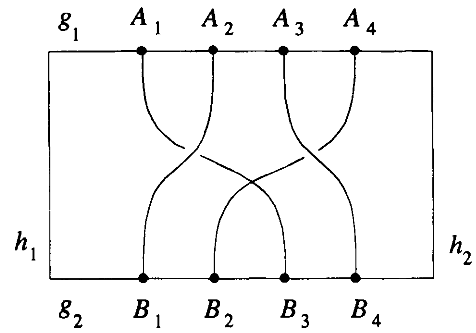


Figure 2: A braid of 4th order

of weaving patterns (Artin 1947, 101-126 and Artin 1950, 112-119). Let me call the weaving patterns "concrete" braids, and equivalence classes of weaving patterns "abstract" braids.

By joining two concrete braids and removing the joining line, we get a third braid (cf. fig. 3).

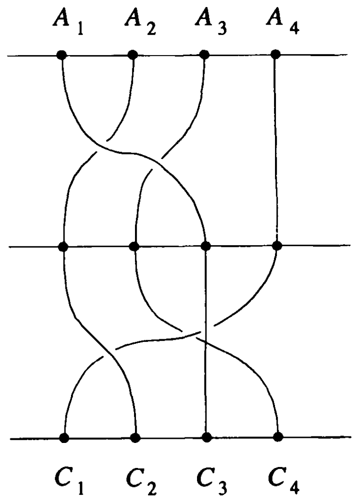
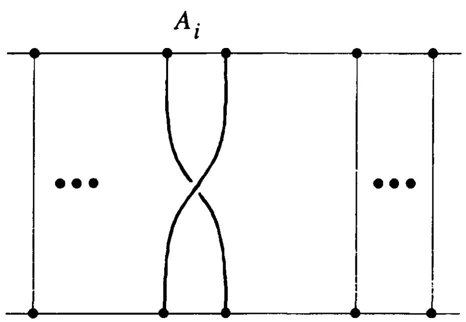


Figure 3: Joining braids

This makes abstract braids into a *group*. Artin's first step is to "arithmetize" braids, *i.e.* to give a symbolic presentation of the group of abstract braids. This is achieved by looking at the elementary braids in which only the i-th curve crosses the $(i+1)$-th (cf. fig. 4 below). These braids generate the whole group. In this way, Artin finds a new definition of the group in question (in fact it would be more precise to say: of an isomorphic group): It is the group with symbolic generators $\sigma_1, \sigma_2, \dots \sigma_{n-1}$ and relations



$$\sigma_i \sigma_j = \sigma_j \sigma_i \text{, for } |i-j| \geq 2,$$
$$\sigma_i \sigma_{i+1} \sigma_i = \sigma_{i+1} \sigma_i \sigma_{i+1}.$$

Figure 4: The elementary braid σ_i

By this argumentative move, Artin had related the topological problem of classifying isotopy classes of concrete braids to problems of combinatorial group theory. In fact it turned out that the topological problem is equivalent to the word problem in the braid group, and Artin's main theorem presents a solution of the latter.

III.3 Now what is really going on here (and in the wealth of similar examples)? At first sight, we have a situation very much similar to the situation in early 19th century projective geometry. We may compare the topological point of view to the synthetic approach, and the group-theoretical standpoint to the analytic approach. In the language introduced above, we have two mathematical games, the game of weaving patterns, and the game of the symbolically defined group. However, what really matters for a description of Artin's argumentative practice is not the difference between these two mathematical games but *the way they are related.* What Artin showed is that the group-theoretical game may be *embedded*, as I shall say, into the topological one. *I.e.*, we can redescribe certain situations, rules and moves of the topological argumentation game in such a way that they appear as situations, rules and moves of the group-theoretical game. (This I take as a definition of the notion of embedding of games[17].) This embedding of group theory into topology allows Artin to change his perspective during his arguments from one to the other. In particular, and this seems to me the essential point, he has two ways

at his disposal to deal with the braid group. Either he can deal with it as a purely symbolically defined object, disregarding its topological interpretation. Or he can look at the group elements as equivalence classes of concrete braids and use the whole topological context to make arguments (provided he does not violate the necessary invariance under isotopy)[18].

Now there is clearly a significant difference between the two possibilities. The first involves only a single game. In this sense, arguments restricted to it are (relatively) abstract: they are methodologically pure, and their argumentative context is (relatively) poor. Arguments of the second alternative, however, are (relatively) concrete: they use the methods of two mathematical games, and thus also the argumentative context is (relatively) rich.

Let me give you examples of an abstract and a concrete argument about the braid group.

a) By a sequence of symbolic calculations, we may deduce that the braid group is generated by the two elements σ_1 and $a := \sigma_1 \sigma_2 \dots \sigma_{n-1}$.

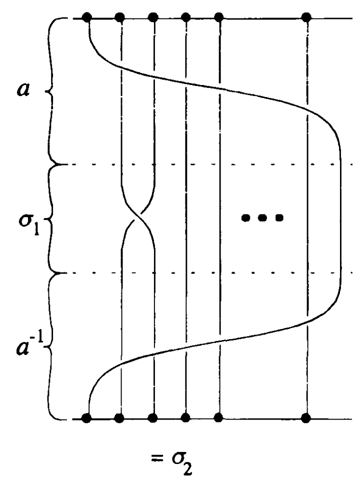


Figure 5: A braid equation

b) The same fact may be learned from the fig. 5. Iterating the idea of this figure we understand that $a^k \sigma_1 a^{-k} = \sigma_{k+1}$ holds. Therefore, a and σ generate the braid group. (Here we face a typical situation: The concrete arguments seem to be intuitive. This is interesting from the pragmatical point of view, but not logically essential: Imagine the argument formulated in a rigorous language, say of piecewise linear topology. Hence it is more adequate to say: By the game change from combinatorial group theory to topology, more intuitions are made accessible which may eventually be turned into rigorous arguments.)

III.4 The situation which we encountered in the example (the embedding of an abstract game into a concrete one) is the elementary, local situation in which the difference between an abstract and a concrete argument, as I propose to use it, may be illustrated. Before I proceed to extend my description to a more global level, let me add some remarks concerning the explanations given so far.

i) It is now possible to relate the names chosen for my distinction to the formal notion of abstraction. The embedding of the example depended on a definition by abstraction in the technical sense of the term. In fact, definitions by abstraction always lead to an embedding of an abstract mathematical game into a concrete one, so that the distinction introduced above may be applied. However, this situation is only a special case of the relation between mathematical games which I called "embedding".

ii) Certainly, the above example is mathematically rather simple. Nevertheless, modern mathematical experience tells us that similar examples abound – on the elementary as well as on more advanced levels. The possibility of embeddings of mathematical games has, in fact, itself become a subject of modern mathematical research. This shows that there is no difference in rigor between an abstract and a concrete argument insofar as my present analysis is concerned. Thus it is clear that the question of using abstract or concrete arguments may again be (as in the case of an analytic or synthetic treatment of geometry) a question of style, of methodology, and not a question of substantively different mathematics.

iii) Finally, it should be emphasized that the distinction introduced above turns out to be a *relative* one. In the elementary situation of the example, concreteness comes about by means of a relation between two games. Only relative to these two games (or a more complex interrelation of mathematical games) is it reasonable to distinguish between an abstract and a concrete approach *to the same questions*.

III.5 I am now in a position to sketch a reconstruction of the global difference between an abstract and a concrete argumentative style. It is clear that modern mathematics consists of a whole network of mathematical games. The mutual embeddings provide, so to speak, the links between these games. An author like Klein seeks systematically to exhibit such embeddings, and he does not hesitate to change the game continually in order to form a convincing argument (like in the concrete argument of the example). A text like Pasch's or Hilbert's, on the other hand, restricts itself as far as possible to a single, mathematically well-defined game (in the extreme case: a single axiomatic system) and argues strictly within the context thus defined. On this level, an abstract orientation produces with great probability theorems of a rather different type than those that arise

from a concrete orientation. The search for algebraic invariants of topological objects was motivated by the wish to enrich the argumentative context available for the treatment of topological (and algebraical) problems. The proof of the independence of a specific axiom with respect to a given system of axioms, on the other hand, is motivated by the intention to clarify the logical structure of a single, restricted mathematical game.

One may even go one step further towards a global picture of the abstract and the concrete mathematical styles. The games of mathematical argumentations are not only linked by internal embeddings. They are also embedded into external, non-mathematical domains of scientific and social practice. In the light of such embeddings, there is also a scale of concreteness ranging from the pure to the applied. (Think of Klein's discussion of graphical statics and of cogwheel profiles.)

III.6 Are there other mathematical disciplines in which the distinction between an abstract and a concrete argumentative style played a role in late nineteenth and early twentieth century mathematics? I think there are. I have discussed a topological example above. In fact, the development of algebraic topology provides a wealth of examples which could be analyzed in terms of abstract and concrete argumentative styles. Another field of mathematics where the distinction seems to have been relevant is number theory. Dirichlet and Riemann had shown how to embed number theory into complex analysis (by means of Dirichlet series and Riemann's ζ-function). Thus the argumentative context of number theory became richer, and Hadamard's and de la Vallée-Poussin's success in proving Gauss's conjecture on the asymptotic distribution of primes motivated a whole generation of number theorists to employ the concrete style of analytic number theory. On the other hand, an elementary, abstract approach finally succeeded in proving the prime number theorem, too (Erdös and Selberg). A revival of elementary number theory was the consequence (Echeverria 1992, 249ff.). As in the case of geometry it seems to be the analytical side which tends to methodological diversity, while the synthetical, elementary side is committed to methodological purism.

It is an interesting question whether the shift which I described in the development of geometry could be related to the shift in the philosophical conceptions of mathematics from Kant to the end of the 19th century. Whereas Kant's philosophy of mathematics was centered on the analysis-synthesis distinction, two of the most important thinkers in philosophy of mathematics of the end of the century, namely Frege and Husserl, tried hard to make clear the second distinction as applied to mathematics.

IV The Role of Concrete and Abstract Argumentative Styles in Mathematical Modernity

IV.1 It seems that the shift from the controversy about analytic and synthetic geometry to that between a concrete and an abstract style of geometrical argumentation described in the first part of this paper is related to the formation of what has been called "mathematical modernity". Let me now turn to explaining briefly how the distinction introduced above could contribute to a better understanding of the modernity of modern mathematics.

Herbert Mehrtens has drawn an impressive and detailed picture of the process of mathematical modernization in his book, *Moderne–Sprache–Mathematik* (1990). Mehrtens tries to show that there are two fundamentally different types of reactions to the changes in 19th century mathematics. The first, in an emphatic sense modern reaction, was to fully accept the new autonomy and to pursue mathematics as a free, creative enterprise, with no bounds on mathematical production other than internal coherence and success. Among the modernists, Mehrtens points to pure mathematicians like Cantor, Hausdorff, and Hilbert as the "general director." On the other hand, there is a second type of reaction which tries to re-establish the threatened ontological basis and epistemic certainty of mathematical knowledge and the links of mathematics to science under the new conditions. A typical representative of this counter-modern type of reaction is Felix Klein, who was engaged in reforming mathematics at technical universities, and who favoured applied mathematics while constantly emphasizing the role of intuition as a basic pre-requisite for doing mathematics.

Mehrtens' thesis is that the modern and the counter-modern attitudes together provided a framework for mathematicians' sense of self-identity at the beginning of the twentieth century. These attitudes helped to justify mathematical research, and played a role in the fight for positions and prestige. The professional politics of the two Göttingen leaders, Hilbert and Klein, was determined by the difference between modern and counter-modern attitudes as well as the later *Grundlagenkrise* between "formalists" and "intuitionists." While in the case of Hilbert and Klein, their different attitudes did not preclude the possibility of "forging of an intellectual alliance" between the two in the fight for Göttingen mathematics (Rowe 1989, 195 ff.), after the take-over by the German National "Socialists," there appeared, according to Mehrtens, a fatal connection between radical counter-modernists and the fascist ideology.

IV.2 In order to draw his picture, Mehrtens needs criteria which allow him to place his actors on the modern/counter-modern scale. In fact, his historical narrative tries to exhibit such criteria along the way. The autonomy of modern mathematics is best described, so he claims, by viewing mathematics as the production

of a language, the meaning and uses of which are not determined beforehand. ("That, by which the discipline of mathematics identifies itself, is the self-referential language Mathematics in the products of the mathematicians, *i.e.* the texts." (Mehrtens 1990, 404)) Consequently, the difference between the modern and the counter-modern attitude must be expressible in terms of the attitude towards mathematical language. Mehrtens uses the linguistic distinction between "signifying" and "signified" to describe this difference. He writes: "The modern and the counter-modern conception give rise to different conceptions of the realm of mathematical language. Modernity is oriented in the Hilbertian formalism at the signifiers which it interprets as the empirically treatable signs on the paper. Counter-modernity resorts to an *a-priori* psychology by postulating a unifying subjectivity with the gift of an original intuition, in which all mathematicians partake" (*ibid.*, 414). And due to this *Ur*-intuition, there is a guarantee of access to that which is "signified."

The main criterion for being a modern is thus, in Mehrtens' view, whether one is prepared to dispense with an explanation of what the meaning of mathematical language is, be it the meaning of mathematical expressions like "point", "line", "field" etc., or even the cultural meaning of mathematical discourse as a whole. A counter-modern, on the contrary, would insist on precisely that. Mehrtens illustrates this criterion with Hilbert's *Foundations of Geometry*, which in fact does without an explanation of the meaning of the basic notions like point, line, etc. From this standpoint, Frege's critique of Hilbert's axiomatic definitions may be the philosophically most self-conscious counter-modern attack on modernism. It revealed that not only questions of the semantics of mathematical language are concerned but also questions of mathematical truth and questions pertaining to what mathematics is really *about.*

IV.3 Mehrtens' book is an example of a very elaborated kind of external historiography. His sources are mainly the programmatic declarations of the mathematicians involved and the documents of their institutional activities. Mehrtens does not attempt to analyze some of the more advanced productions of modernist or counter-modernist mathematicians, and, in fact, he makes no claims about the internal construction of modern mathematics. Thus we are left in a somewhat unclear position if we accept his narrative. Was the struggle between moderns and counter-moderns only a meta-mathematical drama, staged for reasons of self-interpretation and disciplinary politics? Or does the conflict also manifest itself in the "regular discourse of mathematics," as Mehrtens described it, *i.e.*, in the research activities and programs, in the mathematical writings of the period under consideration? Apart from some rather general remarks on the semiotic structure of modern mathematical texts (*ibid.*, ch. 6.3), Mehrtens leaves this question entirely open.

In any case, Mehrtens' thesis would lose much of its attractiveness, if it could not be complemented by an analysis of the modernity or counter-modernity of pieces of mathematical research. Thus we may ask: is there a difference between Hilbert's and Klein's, or between Landau's and Bieberbach's mathematics? That is, between the styles of their mathematical texts, the mathematical games they played? As Mehrtens is silent on this point, we are free to look for our own answers to these questions.

IV.4 Evidently, there is a difference between a text such as Klein's *Lectures on Higher Geometry* and Hilbert's *Foundations of Geometry*. I have tried to describe this difference in the second part of this article and I ventured at a philosophical analysis of its core in the third. Thus the question arises whether we could reasonably use the distinction between an abstract and a concrete argumentative style as an internal criterion for the degree of modernity of a mathematical text. A typical modern piece of mathematics should then argue in a strictly abstract fashion, while counter-modern texts should be written with a concrete style of argumentation. For the two texts of Klein and Hilbert, the statement holds.

In fact there is some evidence in favour of such a proposal. The form of mathematical texts and the type of mathematical questions discussed in the first decades of the twentieth century show strong variations on the scale concrete/abstract. To mention two other names: Henri Poincaré, a counter-modern according to Mehrtens' classification, introduced the fundamental group and the homology groups of a manifold. In this way, he established a far-reaching embedding of the games of group theory into those of geometry, or rather, topology. Felix Hausdorff, placed among the moderns, became famous for his axiomatization of the game of set-theoretical topology.

Let me add immediately that a schematic thesis of the type: "Moderns only wrote abstract texts, counter-moderns only concrete ones" seems very problematic. Counterexamples are too obvious. Frege's *Fundamental Laws of Arithmetics* (1893-1903) are evidently abstract in the sense introduced here, and hence should be called a modern text according to my criterion. On the opposite side, one could mention Hausdorff's very concrete proof that there exist non-measurable subsets of the circle and the sphere (Hausdorff 1914, 428-433), not to speak of much of Hilbert's mathematical work. Rather, the use of this criterion to judge the modernity of a piece of mathematics will lead to modifications of Mehrtens' picture. A grey scale will appear between the white moderns and the black counter-moderns. And I think it will also become clear that (and how) concrete and abstract argumentative styles stimulated each other.

IV.5 Nevertheless, differences in mathematical style existed, and often they corresponded to the metamathematical views of the authors. This correlation would find a partial explanation if we could relate Mehrtens' semantic criterion for being a modernist to the internal criterion of an abstract argumentative style.

In order to establish such a relation we have to ask whether the use of abstract or of concrete arguments leads, or may lead, to different attitudes toward the meaning of mathematical language. Let us go back to the example of the braid group. In fact the difference between viewing the group elements *a*) as words in the symbolic generators σ_i, or *b*) as isotopy classes of weaving patterns, can be described as a difference in semantics. Disregarding the topological game means considering braid words as uninterpreted strings of symbols. The only possibility of ascribing meaning to them is to explain the rules governing their use in the argumentation game we play. If we connect the group theoretical game to the topological game, we open up the possibility of an interpretation of the group symbols: we may call the "isotopy class of concrete braids with one positive twist between the first two threads" the ***meaning*** of the symbol σ_1[19]. Thus the passage from an abstract to a concrete perspective on a mathematical game creates meaning, while the converse passage suspends it.

In this way, we have found, on the local level, a counterpart to Mehrtens' criterion of meaning. The language of abstract arguments is, relative to the given embedding of mathematical games, devoid of that element of meaning which a concrete argument exploits to enable game changes. It seems quite probable that mathematicians who strove for axiomatizations developed a distaste for the varieties of meaning alluded to in concrete argumentations. These meanings occupied the mathematical mind, tending to obscure the logical structure of an argument or a theory. Authors like Klein or Weyl, on the other hand, must have been fond of every new facet of meaning which they could exhibit in mathematical language.

The relativization of Mehrtens' criterion of meaning to an embedding of mathematical games even allows one to reconstruct some of Mehrtens' statements about the attitude of mathematicians towards the cultural meaning of mathematical discourse. If mathematical argumentation moves in a complex network of mathematical games, the outer ends of which are embedded into non-mathematical practice, then a concrete argumentative style in the outer parts of the net creates meaning *outside* the cultural system called 'mathematics'. Klein's love for concrete arguments goes a long way toward embeddings of mathematical argumentations into non-mathematical contexts. (Again I come back to the cogwheels.) The least one can say is that this corresponds to his conviction that mathematics had a meaning for physicists, or for engineers.

IV.6 To finish, I want to discuss once again Artin's braids, but now from a historical point of view. This is meant to illustrate the use of the concrete-abstract distinction as a criterion for the modernity of mathematical argumentations in historiographical practice.

The original context for the topological objects called 'braids' by Artin was the theory of Riemann surfaces, viewed as branched coverings of the complex plane. After some earlier results on two-sheeted surfaces, Hurwitz investigated in 1891 n-sheeted Riemann surfaces with a finite number k of branch points. In particular, he counted the number of inequivalent surfaces for low n and k which had only simple branch points, *i.e.* points where exactly two sheets of the branched covering meet. Hurwitz' text is certainly concrete in my sense: he defined the surfaces by the then usual cutting and pasting techniques, thus aiming at a *topological* definition of Riemann surfaces (without explicit reference to complex function theory). In the next step, he translated the problem of classifying these surfaces into a *group-theoretical* problem. (To every surface, there corresponds a transitive subgroup of permutations of the sheets, generated by the permutations arising at branch points. The associated presentation of this group determines the surface.) Thus he established an embedding of mathematical games.

In the course of his arguments, he came to consider the following situation: Suppose that, for a given surface, we move the branch points in the basis of the covering in such a way that they never meet, but reach a permutation of the original point configuration in the end. By continuously deforming the surface along the way, we arrive at a new surface with the same number of branch points and sheets in the end. Viewing time as a third dimension, we see that the movement of the branch points in the base plane forms a braid! (Imagine the branch points originally on a line; cf. fig. 6.) In fact, Hurwitz showed that (isotopy classes of) these movements form a group, and that they induce a transitive action of this group (to be called braid group only later) on the set of Riemann surfaces with n sheets and k simple branch points.

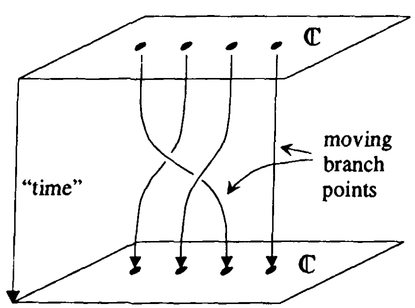


Figure 6: Moving branch points of Riemann surfaces

Thus, the original context of the study of braids is a typical, rich context of argumentation, involving geometric, complex analytic, and group-theoretic ide-

as[20]. This rich argumentative context almost completely disappeared in Artin's definition of the braid group in 1925. There are few doubts that Artin was aware of Hurwitz's work and that it was his deliberate decision not to mention it. (Rather, he placed his braids into a more recent context, namely the problem of classifying knots.) Artin's move even led to a quite common opinion that he had actually invented the braid group.

Artin not only restricted his argumentative context by cutting off the connection to Riemann surfaces and function theory, but originally he even aimed at a purely abstract argumentation on the local level described in III.2. He hoped to solve the classification problem for braids by solving the word problem of the braid group using only methods of the group-theoretical game. This hope is documented in his acknowledgements to his colleague Schreier: "My special thanks are due to Mr. Otto Schreier, who forcefully supported me in the writing of this paper, in particular with the complicated calculations by means of which we first hoped to get through" (Artin 1925-1926, 47). Thus the argumentative strategy seemed clear enough: *i*) define braids topologically, *ii*) "arithmetize" braids, *i.e.* introduce the argumentation game of group theory, and then *iii*) solve the classification problem exclusively in the latter. It is even possible that the main intention of the paper was not to contribute to knot theory by classifying topological braids, but rather to find an interesting example of a group presentation with a non-trivial, but solvable word problem[21].

The tendency toward abstract argumentation makes Artin's paper on braids a modern piece of mathematics. This is in agreement with his general position in German mathematics in the twenties. His lectures on algebra were one of the sources of the strictly axiomatic approach of van der Waerden's *Modern Algebra* (1930-1931); the other being Emmy Noether's work, of course. From 1926 to 1937, when he was dismissed by the Nazis, he held one of the chairs at the Mathematical Seminar at the University of Hamburg, which certainly was one of the liveliest centers of mathematical modernity in Germany during the decade before 33. (The other chairs were held by Blaschke and Hecke. The activities of the seminar are documented in the very successful journal of the seminar, the *Hamburger Abhandlungen.*)

However, the abstract strategy of solving the word problem of the braid group did not quite work. The symbolic calculations which Artin and Schreier undertook turned out to be tedious, and the solution which Artin gave in the paper rests essentially on topological arguments and on frequent changes between the group-theoretic and the topological argumentation game. A close analysis of the proof even shows a certain "return of the repressed": the topological methods employed have a strong connection to the methods which had been used earlier in the context of complex function theory. (In particular, this holds for the method of calculating the fundamental group of a closed braid, which was essential for Artin's argument. The method was due to Wilhelm Wirtinger, a Viennese mathematician who specialized in algebraic function theory.) Hence, counter to his original intention, Artin was forced into a concrete argumentative style.

In later years, Artin was completely dissatisfied with the argument he gave in 1925. He felt it was too intuitive, and the proof of the main theorem was, as he said, "not even convincing" (Artin 1947, 101). Again we see the abstract impulse of modernity. Nevertheless, even the second attack on the braid group did not achieve a purely group-theoretical treatment. Instead, concrete braids were defined more cautiously in order to make rigorous topological arguments available. When Artin wrote a popular article on braids in 1950, he emphasized that "the theory of braids shows the interplay of two disciplines of pure mathematics—topology, used in the definition of braids, and the theory of groups, used in their treatment" (Artin 1950, 112).

Perhaps these remarks mirror some general features of the fate of the abstract style in mathematical modernity. The tendency towards abstract reasoning probably revealed more about the hopes of committed modernists than it did the structure of the actual arguments at the cutting edge of mathematical research. The rigorous axiomatization of mathematical theories even made it possible to clarify the relations between different mathematical games in such a way that concrete arguments lost the flavour of being intuitive and imprecise, as was the case in the braid example. Some of the deepest research of modern mathematics concerned the relations between different mathematical games (or structures, if you wish), but there are few examples where a single mathematical game was carried on for a long time *without* being related to other ones.

Of the two modern lines of geometry at least, the strictly abstract approach of Pasch and Hilbert seems soon to have lost its fertility, while the branches of differential and algebraic geometry lead to exciting results and open questions up to the present day. Not only the strict adherence to the methodological purism of analytic or synthetic geometry, but also the adherence to the methodological purism of abstract argumentations led, as Klein had said, to a "certain petrifaction".

University of Mainz
Department of Mathematics

Notes

1 Cf. *e.g.* Aristotle, *Metaphysics*, 1029*a*, 1061*b*; *Second Analytics*, 92*b*.

2 Cf. the survey by Thiel (1988). See also below, III.4.

3 On Monge, compare Glas (1985).

4 For general historical information about the development of projective geometry, see *e.g.* Kline, (1972, ch 35). A wealth of information is contained in Felix Klein's *Vorlesungen über die Entwicklung der Mathematik in 19. Jahrhundert* (1926-1927).

5 Cited after Kline (1972, 836).

6 This characterization eventually evolved in a modern mathematical notion of projective space: the projective space of a vector space is the set of its one-dimensional linear subspaces.

7 Two "purist" classics of 19th century geometry are mentioned in Klein's *Lectures on Higher Geometry* of 1893 (to be discussed below): "Hesse (1861) purely analytic; Reye (1866-1867) (purely synthetic). Both methodically one-sided, but in their treatment very elegant." (Klein 1893, 5).

8 Certainly, the discovery of the non-Euclidean geometries contributed essentially to the need for a clarification of the logical foundations of geometry. However this can hardly be the "only" reason for axiomatic thinking in geometry (which was then a common trend in other parts of mathematics as well).

9 Klein (1908-1909, II, 55 f.):

> "The analytic geometricians often lost themselves in blind calculations, devoid of any geometric representation. The synthesists, on the other hand, saw salvation in an artificial avoidance of all formulas, and thus they accomplished nothing more, finally, than to develop their own peculiar language formulas, different from ordinary formulas. Such exaggeration of the essential fundamental principles into scientific schools leads to a *certain petrifaction*; when this occurs, stimulation to renewed progress in the science comes principally from 'outsiders'."

10 For the relations between Pasch's and Hilbert's work, see Toepell (1986, in particular 51 ff.).

11 Certainly, it may be objected that Klein's *Lectures on Higher Geometry* represented a rather singular way of treating geometry. However, I hope it will become clear in the following that the stylistic differences on which I focus here are characteristic not only for texts like Klein's and Pasch's.

12 Unfortunately, questions of mathematical pragmatics are still rather unexplored in recent philosophy of mathematics. This is partly due to the fact that the Fregean tradition has focused on parts of mathematics which are far from actual mathematical practice (such as elementary arithmetic). With the revival of methodological and epistemological questions (Lakatos, Benacerraf, Kitcher), the situation has changed to some extent. There seem to be quite a number of valuable ideas still waiting to be unearthed in the non-logicist classics of twentieth century philosophy of mathematics as, *e.g.*, Husserl or Wittgenstein.

13 For an account of the history of the comparison between mathematical practice and games, see my (1994). David Bloor has developed an "anthropological" perspective on mathematics as a system of language games in his (1983). Although I doubt that my view of mathematical games coincides with his notion of language games, some of the remarks below might contribute to his perspective.

14 It seems possible to formalize the notion of a mathematical game: one would then be led to a pragmatic interpretation of the formal systems which Hilbert introduced in his metamathematical work. However, a rigid notion of mathematical games certainly would restrict the range of phenomena in mathematical practice to which it could be applied in an instructive way.

15 Bloor speaks of a "superposition of language games" (1983, 110ff).

16 Cf. Thiel (1988).

17 Or of "superposition", cf. note 15. Whether or how this definition applies to the embedding of mathematical into social games—the situation which interests Bloor most—will be left open here. Also Lakatos stresses the importance of embeddings of contexts of argumentation into each other, cf. *e.g.* (1976, ch. 1, section 2).

[18] In fact, we have three ways to play a braid game (that is, to investigate braids): 1) We may only look at the topological definitions, disregarding the embedded group structure. This we could call the "synthetic braid game". 2) We may only look at the group presentation, disregarding the topological context. The "analytic braid game". 3) We may interpret the group sometimes topologically, sometimes symbollically, using both methods as it suits in studying braids. The "mixed braid game." Only in the last two cases, the object of argumentation is really the braid *group*. Thus the alternative above.

[19] We are not compelled to interpret this type of meaning as reference. We may equally view it from the standpoint of a "use" theory of meaning: by embedding the group-theoretical game into topology, we can make a different use of the symbol σ_1 than without. It is this extension of its possible use which gives a new "meaning" to the symbol, not necessarily its connection to an object.

[20] To Hurwitz' ideas, one must still add the connection between braids and the mapping class group of the complex plane with n points removed, which appeared in Fricke and Klein (1897-1912, I). Cf. Magnus (1974).

[21] Combinatorial group theory was still in its beginnings, and there was considerable need for good examples. Cf. Magnus (1974), and Chandler and Magnus (1982).

II. Philosophy

PETRI MÄENPÄÄ

FROM BACKWARD REDUCTION TO CONFIGURATIONAL ANALYSIS

I Introduction

Ancient Greek geometers devised the method of analysis and synthesis for solving construction problems. According to Pappus (*ca.* 300 *AD*), it was also used for proving theorems, the other class of propositions conceived by the Greeks. He gave the only extensive ancient methodological account of analysis that survives. The term "analysis" has a variety of usages, but only this mathematical one is studied here.

Pappus described analysis as the reduction of a proposition to be solved or proved successively backward to its antecedents until arriving at a proposition whose solution or proof is known (Section II). This is the "directional interpretation" of analysis.

Modern studies of analysis in terms of the directional interpretation have focused on its logical character. The question has been whether the analysis of the ancients is deduction or reduction, which is not deductive in general. Hintikka and Remes (1974), notably, try to read the latter interpretation into Pappus's description. This is forced, because almost all examples of analysis in the Greek mathematical corpus are in fact deductions. Of course, these deductions are also reductions, because they are to be convertible into syntheses, but there is little evidence of non-deductively reductive analyses. I shall call such analyses "purely reductive".

The few examples of Greek purely reductive analyses were devised by commentators rather than mathematicians with original contributions (Knorr 1986, ch. 8). The first purely reductive directional interpretation of analysis in a methodological description that I know of is by Duhamel (1865, ch. X and XI). He goes so far as to regard the deductive analysis of the ancients as defective, because it ignores concerns of convertibility of an analysis into a synthesis. He says further that modern analysis, which is (purely) reductive, does not suffer from this defect. It is trivially convertible.

But purely reductive analysis appears in mathematical practice much earlier: Galileo's manuscripts on mechanics contain a purely reductive analysis (Mäenpää

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 201–226.

1993, section 7.2). Nevertheless, it doesn't seem to appear in the methodological discussions of the 1600's. Analysis was discussed extensively then, notably by Descartes and Newton. Their conception of analysis is deductive in methodological accounts as well as in mathematical practice.

Mathematical language and method changed decisively around 1600 in the hands of Viète and Descartes. They introduced a new kind of algebra, explicitly based on the ancient Greek method of analysis (Section III). The main innovation of their algebraic language was the introduction of variable symbols for all given and unknown quantities. The Greeks used no variable symbols before Diophantus introduced one in his *Arithmetic* in *ca* 250 AD. (The present account deals with Descartes only, see Mäenpää 1993, ch. 5-7 for Diophantus, Viète, and Newton.)

At the same time, Descartes's methodological description of his algebraic method of analysis introduced an important novelty with respect to Pappus's description. Descartes said that analysis serves to determine how the unknown quantities of a problem depend on the given ones. Instead of seeking a deductive connection between the proposition to be solved or proved and propositions whose solution or proof was known, Descartes sought to determine the dependencies of the unknown quantities on the given ones. This is the "configurational interpretation" of analysis.

On the face of it, the configurational interpretation is a simple specification of the directional one. The analyst works backwards by reduction from the sought conclusion to given premisses (Pappus). More specifically, he thereby establishes a dependency of the sought quantities on the given ones (Descartes).

But this specification has deeper methodological and logical significance. It shifts the focus of the analytical method from the analysis of a deductive connection to the analysis of what is in more modern terms a "functional" connection. Analysis is, according to the configurational interpretation, a study of the functional dependencies in a mathematical configuration with known as well as unknown constituents.

In the Greeks' twin method of analysis and synthesis, synthesis served to put together the sought objects from the given ones, making use of their functional dependencies uncovered in analysis. This concerns problems. In the case of theorems, the task of synthesis was to convert the analysis into a demonstration of the proposition to be proved from ones known to be true.

This informal description gives the impression that the configurational interpretation suits problem solving better, while the directional interpretation suits theorem proving. To get a more precise and deeper understanding of the situation, we shall describe the analytical method in formal terms. This is intended as a theoretical explanation of the configurational and directional interpretations. It aims at finding a theoretical structure behind the phenomena, so to say, of the examples of analysis in the mathematical literature and of informal methodological accounts. Devising such a theoretical explanation is quite clearly a task that calls for a logical formalism as a conceptual tool. The resulting reconciliation of the two interpretations of analysis serves to spell out in relevant theoretical (logical) terms what they are and how they relate to one another.

The question of how the analysis of problems relates to the analysis of theorems is also a logical question. This is why it is answered in the most satisfactory way, from the systematic point of view, in terms of a logical formalism. In particular, the formalism must describe adequately functional dependencies between configurations (=constructions) as well as deductive connections between propositions.

Descartes's algebraic analysis has had a remarkable success due to its problem-solving power. It soon became the *lingua franca* of the exact sciences, and that it remains today. What is more, it has served as a standard system of forms of understanding ancient historical materials in mathematics beginning from Zeuthen in the late 1800's (cf. *e.g.* Zeuthen 1893). Yet the reduction of ancient historical materials to Cartesian algebra does not preserve mathematical content. One possibility of dealing with this difficulty is to refrain from using anachronistic concepts as forms of historical understanding. Another possibility, which is made use of here, is to employ a system of concepts that is general enough to preserve mathematical content in full.

We shall then be in a position to see, for instance, the precise difference in meaning between the informal expressions

"deduction of a construction",
"deduction of a proposition"

current in modern studies of ancient mathematics. This has not been possible before, because there has been no conceptual system for relating the notion of construction to the notions of deduction and proposition in a satisfactory way before constructive type theory (from now on: type theory), which we shall employ here (Section IV). Type theory (Martin-Löf 1984) is one of the main current approaches to the foundations of mathematics and computing science.

This formal system of concepts helps us to understand the systematic source of the heuristic usefulness or problem-solving power of analysis. Furthermore, it lets us see new things in historical and informal mathematical materials, using the new forms of understanding.

Hintikka and Remes (1974 and 1976) brought the configurational interpretation into recent methodological discussion, and coined the names of the two interpretations. They described analysis in terms of predicate logic, both the configurational and the directional interpretation. They also refuted conclusively

Mahoney's (1968) claim that the analysis of problems is not a method that can be described in logical terms, in contrast to the analysis of theorems.

Analysis is, in their configurational interpretation, a study of the functional dependencies among the constituents of a definite mathematical configuration. In the case of geometry, for example, the configuration is a geometrical figure. They also introduced the term "constructional interpretation" as a synonym for configurational interpretation, and "propositional interpretation" as a synonym for directional interpretation.

Besides bringing the modern methodological study of analysis to a new, theoretical level of precision, by employing modern logical concepts, they identified the crucial heuristic role of "auxiliary constructions" in analysis. Taking apart a definite configuration into its constituents is routine compared to inventing the auxiliary constructions that are needed to amplify the configuration in order to find the solution to nontrivial problems (Section V). Auxiliary constructions are in fact indispensable also in finding the proof of nontrivial theorems, and this is one important logical connection between the analysis of problems and of theorems. Hintikka and Remes describe also auxiliary constructions in terms of predicate logic.

Now it has turned out that the logical tools used by Hintikka and Remes do not suffice for a natural logical description of the configurational interpretation and of auxiliary constructions (Mäenpää 1993). The systematic reason for this is that predicate logic does not recognize constructions. In its stead, I use type theory, which enriches predicate logic with a functional hierarchy that exactly captures on the formal level the informal notion of synthesis as functional composition of constructions of various types, like points, circles, and line segments in geometry, and of analysis as its inverse operation, functional decomposition of a construction into its constituents.

Despite its introduction of quantifiers and individuals, predicate logic is still too close to propositional logic in order to serve as a formal tool for describing the analysis and synthesis of constructions adequately, which the configurational interpretation of analysis requires. Propositional and predicate logic suit the directional interpretation better. It turns out that predicate logic fails, for instance, to describe geometrical construction postulates, which are used in solving geometrical problems.

Auxiliary constructions receive a logical description that is eminently natural in view of the informal way of understanding them as constructions that are not constituents of the configuration originally subjected to analysis (Section VI). That is, auxiliary constructions are constructions that are constituents of neither the given nor the sought objects.

II The Directional Interpretation of Analysis: Pappus's Description

The directional interpretation of analysis runs as follows in Pappus's classical description in the seventh book of his *Mathematical Collection* (the English translation is from Jones's edition of Pappus (CJ, 82-85); I have added the Greek terms in square brackets). Part of the description may originate in older sources, probably in Euclid (Knorr 1986, 354-360).

> "That which is called the Domain of Analysis, my son Hermodorus, is, taken as a whole, a special resource that was prepared, after the composition of the Common Elements, for those who want to acquire a power in geometry that is capable of solving problems set to them; and it is useful for this alone. It was written by three men: Euclid the Elementarist, Apollonius of Perge, and Aristaeus the elder, and its approach is by analysis and synthesis.
>
> Now analysis is the path from what one is seeking [*zetoumenon*], as if it were established, by way of its consequences [*akoloutha*], to something that is established by synthesis. That is to say, in analysis we assume what is sought [*zetoumenon*] as if it has been achieved, and look for the thing from which it follows, and again what comes before that, until by regressing in this way we come upon some one of the things that are already known, or that occupy the rank of a first principle. We call this kind of method 'analysis', as if to say *anapalin lysis* (reduction backward).
>
> In synthesis, by reversal, we assume what was obtained last in analysis to have been achieved already, and, setting now in natural order, as precedents, what before were following, and fitting them to each other, we attain the end of the construction of what was sought [*zetoumenon*]. This is what we call 'synthesis'.
>
> There are two kinds of analysis: one of them seeks after the truth, and is called 'theorematic'; while the other tries to find what was demanded, and is called 'problematic'. In the case of the theorematic kind, we assume what is sought [*zetoumenon*] as a fact and true, then, advancing through its consequences [*akoloutha*], as if they are true facts according to the hypothesis, to something established, if this thing that has been established is a truth, then that which was sought [*zetoumenon*] will also be true, and its proof [*apodeixis*] the reverse of the analysis; but if we should meet with something established to be false, then the thing that was sought [*zetoumenon*] too will be false. In the case of the problematic kind, we assume the proposition as something we know, then, proceeding through its consequences [*akoloutha*], as if true, to something established, if the established thing is possible and obtainable, which is what mathematicians call 'given', the required thing [*protathen*] will also be possible, and again the proof [*apodeixis*] will be the reverse of analysis; but should we meet with something established to be impossible, then the problem too will be impossible. Diorism is the preliminary distinction of when, how, and in how many ways the problem will be possible. So much, then, concerning analysis and synthesis."

The translation of certain Greek terms deserves comment. Issues of translation depend on how the logical character of analysis is understood.

Pappus calls analysis as applied to theorem proving "theorematic' and as applied to problem solving "problematic" in Jones's translation. I use the terms "theoretical" and "problematical" instead, because they have become standard, although Jones's terms avoid the ambiguity inherent in "theoretical" between the terms "theorem" and "theory".

Jones translates "*anapalin lysis*" as "reduction backward", whereas Heath (in his translation of Euclid's *Elements*, I, 138-139) translates it as "backward solution". Hintikka and Remes (1974, 8-10) follow Heath. I find Jones's translation preferable, because Pappus describes analysis as a method that applies also to theorem proving, not only to problem solving. Note however that Pappus does not use the technical term "*apagoge*" for reduction here. The term "*lysis*" is nontechnical (Knorr 1986, ch. 8). Knorr translates "*lysis*" as "resolution", but I prefer not to do so, because I shall use resolution as a technical term for the second part to be distinguished in analysis.

In sum, Pappus says that if the end-point of analysis is an impossible problem (or absurd theorem), then synthesis is not needed, and the original problem is also impossible (or the original theorem absurd). That is, analysis constitutes a *reductio ad absurdum*. This is quite conclusive evidence for the interpretation that Pappus conceives analysis as deductive, because a purely reductive analysis could not constitute a *reductio ad absurdum*.

If analysis leads to a problem whose solution is known (or a theorem whose proof is known), a synthesis is needed. The synthesis reverses the analysis and yields a solution to the original problem (or a proof of the original theorem). Pappus's description of synthesis as complementing analysis would be pointless if he regarded analysis as purely reductive, because this would make synthesis trivial and superfluous.

Strangely enough, Pappus does not have anything to say about the nontriviality of this reversal. He does mention that the analyst must in general determine the conditions of solvability of a problem, the "diorisms [*diorismos*]". They are part of establishing reversibility, because they are conditions under which an analysis is reversible.

Hintikka and Remes translate "*akoloutha*" as "concomitants" in order to leave room for their interpretation of analysis as a purely reductive procedure. Previously "*akoloutha*" had been translated as "consequences". The evidence provided by the Greek mathematical corpus renders Hintikka and Remes's translation implausible, because the extant Greek analyses are deductive, with the few exceptions devised by commentators. It is hardly conceivable that Pappus, in describing the analytical works of the corpus, should have described analysis in a way that is not consistent with those works.

On the other hand, an important precursor of analysis was the method of reduction (*apagoge*), which was not deductive (Knorr 1986, 23-24). A well-known application of *apagoge* is Hippocrates's (pre-Euclidean) reduction of the problem of duplicating a cube to the problem of finding two mean proportionals between two given line segments. Proclus, who flourished in the fifth centrury *AD*, says in his commentary of Euclid's *Elements* (PEEL, 212-213) that

> "'Reduction [*apagoge*]' is a transition from a problem or a theorem to another which, if known or constructed will make the original proposition evident. For example, to solve the problem of doubling the cube geometers shifted [*metethesan*] their inquiry to another on which this depends, namely, the finding of two mean proportionals; and thenceforth they devoted their efforts to discovering how to find two means in continuous proportion between two given straight lines. They say that the first to effect reduction of difficult constructions was Hippocrates of Chios, who also squared the lune and made many other discoveries in geometry, being a man of genius when it came to constructions, if there ever was one."

(I have inserted some Greek terms in square brackets from Friedlein's edition.) Neither Pappus nor anyone else of the ancients seems to relate analysis to *apagoge* methodologically. The only testimony we have is their mathematical practice. Judging from that, analysis and *apagoge* seem to have been distinct methods, and the modern purely reductive interpretation of analysis applies to *apagoge* rather than to analysis in Greek mathematics.

III The Configurational Interpretation of Analysis: Descartes's Description

Descartes introduced his algebra as a new tool for solving mathematical problems. It turned out so powerful that those problem domains that it applies to were studied in great depth, while those falling outside its scope received less attention after Descartes. Its application in geometry, in particular, required that geometry, as practised in the tradition established by Euclid and his contemporaries, be abstracted to algebra.

The non-algebraic aspects of geometry gradually fell out of the scope of what is now known as analytic geometry. A case in point is an elementary construction problem like the first proposition in Euclid's *Elements*, to construct an equilateral triangle on a given line segment. This is why Descartes's method of algebraic analysis is not a general mathematical method. To study analysis in all its generality requires a system of concepts that does not reduce mathematical content. This requirement concerns the systematic as well as the historical point of view. Cartesian algebra has been widely used as a system of concepts for studying ancient geometry historically, but this approach falls short of describing the historical materials in full, because it abstracts the geometrical materials to algebraic forms.

Here is how Descartes describes his analytical algebraic method. Rule Seventeen of his *Rules for the Direction of the Mind* (ROP) reads as follows (quoted from Descartes PW, I, 70-71).

> "We should make a direct survey of the problem to be solved, disregarding the fact that some of its terms are known and others unknown, and intuiting, through a train of sound reasoning, the dependence of one term on another.
>
> [...] the trick here is to treat the unknown ones as if they were known. This may enable us to adopt the easy and direct method of inquiry even in the most complicated of problems. There is no reason why we should not always do this, since from the outset of this part of the treatise our assumption has been that we know that the unknown terms in the problem are so dependent on the

known ones that they are wholly determined by them. Accordingly, we shall be carrying out everything this Rule prescribes if, recognizing that the unknown is determined by the known, we reflect on the terms which occur to us first and count the unknown ones among the known, so that by reasoning soundly step by step we may deduce from these all the rest, even the known terms as if they were unknown."

And in his *Geometry* (1637), Descartes says in his classical description of analytical algebraic problem solving (quoted from Descartes GSL, 6-9) that:

"If, then, we wish to solve any problem, we first suppose the solution already effected, and give names to all the lines that seem needful for its construction,—to those that are unknown as well as to those that are known. Then, making no distinction between known and unknown lines, we must unravel the difficulty in any way that shows most naturally the relations between these lines, until we find it possible to express a single quantity in two ways. This will constitute an equation, since the terms of one of these two expressions are together equal to the terms of the other. We must find as many such equations as there are supposed to be unknown lines; but if, after considering everything involved, so many cannot be found, it is evident that the question is not entirely determined. In such a case we may choose arbitrarily lines of known length for each unknown line to which there corresponds no equation. If there are several equations, we must use each in order, either considering it alone or comparing with the others, so as to obtain a value for each of the unknown lines; and so we must combine them until there remains a single unknown line which is equal to some known line [...]."

Descartes shifts the focus of analysis from the deductive connection between propositions known to be true and the proposition to be proved to the dependencies, that is, the functional connections, between the known and unknown terms of a problem. Notice also the shift in terminology: where Pappus connects "something established" or "known" or "given" to "what is sought", Descartes connects "known terms" to "unknown terms", the "terms" now obviously referring to quantities, not propositions.

Descartes is concerned with problem solving exclusively. Pappus, on the other hand, uses the word "*zetoumenon*" for what is sought neutrally with respect to theoretical and problematical analysis.

In introducing the configurational interpretation into modern methodological discussion, Hintikka and Remes don't seem to have been aware that it was introduced by Descartes. They even say that "Descartes insists on discussing methodological matters in propositional terms or at least in terms of sequences of steps of thought" (Hintikka and Remes 1974, 103).

IV Logical Form in Analysis

Consider the elementary geometric construction of a circle from a point and a line segment, by a compass as it were, using the point as the centre of the circle and the line segment as its radius. This is the third construction postulate of Euclid's

Elements, with the slight generalization that Euclid uses one end-point of the given line segment as the centre of the circle rather than any given point.

In the formalism of type theory, this can be represented as the rule

$$\frac{a : Point \qquad b : LineSegment}{c(a,b) : Circle}$$

Progressing here from premisses to conclusion by deduction, we synthesize the circle $c(a,b)$ in the conclusion from the point a and the line segment b given in the premisses. This rule establishes at the same time a deductive connection from the premisses to the conclusion and a functional dependency from the constructions in the premisses to the construction in the conclusion.

Suppose we seek the construction of a circle. We thus have a variable y: *Circle*. Now if we match this with the conclusion of the type-theoretical rule, and reduce the conclusion to the premisses, we get to know that the unknown circle y can be composed from a point a and a line segment b, that is, that

$$y = c(a,b) : Circle$$

in the formal terms of type theory.

In predicate logic, the same construction postulate could be represented as the rule

$$\frac{\vdash Point(a) \qquad \vdash LineSegment(b)}{\vdash Circle(c(a,b))}$$

This does codify the same informal step of construction, but the forms of expression of predicate logic do not allow systematizing rules of construction in a natural way, in contrast to type theory. A type-theoretical rule like the one above simply composes a sought construction functionally in synthesis or decomposes it functionally in analysis. Thus a circle $c(a,b)$ decomposes into a point a and a line segment b. This reconciles the configurational and the directional interpretations of analysis on the level of a single step of construction.

The predicate-logical rule, on the other hand, infers properties of individuals from other properties in a way that lacks this natural compositionality, which is at the heart of the informal conception of analysis. There is no natural way to analyze the predication $Circle(c(a,b))$ into the predications $Point(a)$ and $LineSegment(b)$.

Another approach in terms of predicate logic is presented by Mueller (1981, 1-3) in his formal rendering of Hilbert's axioms for Euclidean geometry. He denotes lines by upper case letters and points by lower case ones instead of distiguishing them by predicates.

There is no way to formalize geometric construction postulates like the one above for circles. Predicate logic thus reduces geometry to theorem proving, because problem solving requires construction postulates.

The only way to reason about constructions in this predicate-logical approach is to lay down existence axioms and then infer existence theorems from them. In the tradition of geometry established by ancient Greeks, on the other hand, constructions are more primitive than existence propositions. Constructions can be used for proving existence propositions, but the former do not reduce to the latter, as in this predicate-logical codification. Thus, it is not adequate.

Indeed, Hilbert's notion of abstract axiomatization, as exemplified in his *Grundlagen der Geometrie* (1899), reduced geometry to theorem proving by reducing the existence of mathematical objects to the consistency of the axiomatic system that defines them implicitly. In the tradition of the Greeks, in contrast, mathematical objects were defined explicitly by construction postulates, as in type theory. Hilbert's model has spread throughout mathematics in this century, reducing it to theorem proving. Problem solving, which was the primary concern of Greek mathematicians (Knorr 1986, ch. 8), has been ruled out.

One can conclude, then, that predicate logic is a logic of theorem proving. It serves to describe the directional interpretation of analysis but not the configurational one. To describe the configurational interpretation and problem solving adequately requires a richer system of logical concepts.

Already Kolmogorov (1932) proposed developing a logic of problem solving and applying it to geometric construction problems. He saw that this requires constructive logic, but no one seemed to have taken up the task before my (1993). This is surely because an expressive enough logical language, type theory, was conceived only in the 1970's. Kolmogorov gave a problem interpretation for constructive propositional logic.

In natural deduction terms, the above rule of type theory is an introduction rule for the set of circles. So introduction rules of natural deduction in type theory serve to analyze a sought construction into its immediate constituents, by regressing from conclusion to premisses. Elimination rules, correlatively, serve to analyze a given construction into its immediate constituents. Introduction rules are used for defining a set by telling how its elements are constructed. They represent formally the construction postulates of Greek mathematicians.

We have employed the two type-theoretical forms of judgement

$$a : A$$

$$a = b : A$$

that represent, respectively, the informal judgements

a is an element of the set *A*

a and *b* are equal elements of the set *A*

Let us now consider judgements and inferences where constructions have no intrinsic interest. In case the construction a has no intrinsic interest, the form of judgement

$$a : A$$

can be abbreviated to the form

$$\vdash A$$

The distinction between these two forms of judgement is already present in ancient Greek mathematics in the distinction between problems and theorems. The solution to a problem consists of a construction of the sought objects from the given ones and a proof that the construction satisfies the condition of the problem. The proof of a theorem, on the other hand, is just a proof that the given objects satisfy the condition of the theorem. Solutions to problems thus contain a construction with intrinsic interest and a proof that has no intrinsic interest. A proof of a theorem is just the latter, thus without intrinsic interest as a construction (cf. Mäenpää 1993, ch. 3 for further information).

Zeuthen (1896) identified problems in ancient Greek geometry with existence propositions, proved by constructions. Knorr (1983 and 1986, ch. 8) refutes this by displaying and discussing Greek theorems that have explicit existential form. In them, existence was not proved by construction, and on the other hand, problems were understood quite simply as tasks of construction rather than as existential propositions. Hintikka and Remes (1974 and 1976), in the same vein as Zeuthen, distinguish between problems and theorems in terms of existential form. Problems are for them propositions that have existential form.

Type theory allows us to distinguish problems formally from theorems in a more satisfactory way. This requires enriching the forms of judgement of predicate logic. Recall that the form of judgement $a : A$ can be used to express that a is an element of the set A. More generally, it expresses that a is a construction of type A.

Another particular case of a construction besides an element of a set is a proof of a proposition. The above form of judgment can be used to express also that a is a proof of the proposition A. In case the proof a has no intrinsic interest as a construction, the form of judgement can be abbreviated to the form $\vdash A$ by suppressing a. This abbreviated form expresses that the proposition A is true. It can be used when we are not intrinsically interested in how A is proved, that is, in what construction proves it, but only in its truth.

The form of judgement $a : A$ can now be used to formalize the "deduction of the construction" a, and the form $\vdash A$ to formalize the "deduction of the proposition" A.

Thus, the two forms of judgement $a : A$ and $a = b : A$ can also represent the informal judgements

a is a proof of the proposition A,

a and b are equal proofs of the proposition A.

Proof here is to be understood in the sense of construction, as employed in constructive logic. For example, the conjunction introduction rule

$$\frac{\vdash A \qquad \vdash B}{\vdash A\&B}$$

of propositional logic in natural deduction formulation is seen in type theory as an abbreviation of the rule

$$\frac{a : A \qquad b : B}{(a,b) : A\&B}$$

where the proof (a,b) of the conjunction proposition $A\&B$ in the conclusion is a pair composed of the the proofs a and b of the propositions A and B, respectively, in the premisses.

From the point of view of the traditional distinction between problems and theorems, we can now see the abbreviated rule as a rule for theorems and the full type-theoretical rule as a rule for problems, because the latter rule displays constructions.

Corresponding to the usual natural deduction rules of conjunction elimination

$$\frac{\vdash A\&B}{\vdash A} \qquad \frac{\vdash A\&B}{\vdash B}$$

type theory has the rules

$$\frac{c : A\&B}{p(c) : A} \qquad \frac{c : A\&B}{q(c) : B}$$

that take apart the proof c of the conjunction in the premiss into the left projection $p(c)$ and the right projection $q(c)$, which prove the left conjunct A and the right conjunct B, respectively.

The values of expressions obtained by applying elimination rules are determined by "computation rules" of type theory. Each proposition and set has its rules of computation. There is nothing corresponding to them in predicate logic. Conjunction, for instance, has the following computation rules that determine how to evaluate effectively left and right projections:

$$\frac{a : A \qquad b : B}{p((a,b)) = a : A} \qquad \frac{a : A \qquad b : B}{q((a,b)) = b : B}$$

Constructions may thus have intrinsic interest already on the level of propositional logic. Representing them as individuals of predicate logic is an artificial codification. This strengthens the conception that predicate logic is suitable for a logic of theorem proving but not for a logic of problem solving.

Proofs in this type-theoretical sense of constructions are formal functional representations of proof trees. They are brought into the formal language as objects that can be reasoned about like any other objects. This is why they are also called "proof objects".

Each step of constructing a proof tree is at the same time a step of constructing a proof object. There is nothing restrictive from the point of view of classical logic in this formal procedure, because classical logic uses proof trees just like constructive logic. Proof trees of classical and constructive logic have representations on a par as proof objects in type theory. Type theory just enriches predicate logic by bringing proofs into the formalism as objects. Trees that form elements of sets, generated by rules like the introduction rule for circles above, are treated on a par with proof trees.

Type theory and predicate logic differ in the formalization of sets, like those of points, line segments, and circles. The type theorist represents them directly as sets, whereas the predicate logician represents them indirectly by codifying them as predicates over the single domain of individuals. Correlatively, the terms a, b, and $c(a,b)$ are formalized as individuals of the single domain in predicate logic, but as elements of the sets *Point*, *LineSegment*, and *Circle*, respectively, in type theory. This is because type theory enriches predicate logic so that instead of the one domain of individuals, each set is a domain of individuals in type theory.

This account of sets in predicate logic followed our first formalization above. In the second formalization above, that presented by Mueller, sets are distinguished from each other only by representing their elements by different variable symbols. There is thus no real distinction, on the formal level, between a point and a line, for instance. Nothing prevents forming meaningless predications like $Intersects(A,a)$, where A is a point and a is a line.

To conclude, predicate logic does not recognize constructions and cannot formalize them naturally, although they can be artificially codified in terms of predicates over the single domain of individuals. It is not an adequate system of concepts for relating the configurational to the directional interpretation of analysis in formal terms. Hintikka and Remes (1974 and 1976) understand the analysis of a configuration formally as taking apart propositions into their constituents (by making use of the subformula property of natural deduction systems), although they describe it informally as taking apart a construction into its constituents.

Now let us consider the parts of a proposition in the Greek sense. A problem has "given" objects, "sought" objects, and a "condition" that relates them. A theorem, on the other hand, has only given objects and a condition on them. There are no things sought. A theorem is thus the limiting case of a problem with no sought objects. This distinction of the parts of a proposition is introduced by me (Mäenpää 1993, ch. 3) in order to discuss analysis in precise logical terms—it was not made explicitly by the Greeks. Their *zetoumenon* was the combination of what is here called the sought for objects and the condition. Thus for theorems it was just what is here called the condition.

An example of a problem is the first proposition of Euclid's *Elements*, to construct an equilateral triangle on a given line segment. Here the given object is a line segment, the sought object is a triangle, and the condition is that the triangle must be equilateral and constructed on the line segment.

An example of a theorem is proposition 32 of the first book of Euclid's *Elements*. It states that the angle sum of a triangle equals two right angles. Here the given object is a triangle, and the condition is that the sum of its angles is equal to two right angles.

In type-theoretical terms, we can represent the given objects, the sought objects, and the condition schematically as

$$x : A$$

$$y : B(x)$$

$$\vdash C(x,y)$$

respectively. Here x and y are (possibly empty) vectors of objects, because there may be any number of given and sought objects. For theorems the vector y is empty, so the scheme reduces to

$$x : A$$

$$\vdash C(x)$$

A problem or a theorem need not even have a condition, as in propositional logic, where predicates cannot be used. The existential form of proposition $(\exists y : B(x))C(x,y)$ in the context $x : A$ can be used to represent a problem that has a condition. However, problems are not to be identified with existential propositions, because problems that lack a condition are not be represented as existential propositions. And on the other hand, a theorem may be just as well be an existential proposition. This is the case when the condition of the theorem is an existential proposition.

Now analysis can be conceived of as a succession of two parts, "transformation" and "resolution", following Hankel (1874). Using our distinction between the given objects, the sought objects, and the condition, we can refine Hankel's proposal with type-theoretical form. First, transformation reduces the condition $C(x,y)$ to a transformed condition $T(x,y)$ that the analyst knows how to satisfy. The transformed condition of a problem must also determine some constituent of the sought objects $y : B(x)$ in terms of the given objects $x : A$. Then, resolution determines all of the sought objects in terms of the given ones. As theorems have no sought objects, their analysis has no resolution.

Synthesis has two corresponding successive parts, already distinguished by the Greeks, "construction [*kataskeue*]" and "demonstration [*apodeixis*]". Construction corresponds to resolution, because it constructs the sought objects $f(x) : B(x)$ from the given ones $x : A$. Demonstration corresponds to transformation, as it deduces the condition $C(x,f(x))$ from the transformed condition $T(x,f(x))$. The synthesis of a theorem has no construction, because there are no sought objects. Schematically, analysis and synthesis have the following form.

Analysis

$$
\begin{array}{cc}
\text{resolution} & \text{transformation} \\
x : A & \vdash T(x,y) \\
 & \uparrow \\
\vdots & \vdots \\
 & \uparrow \\
y : B(x) & \vdash C(x,y)
\end{array}
$$

Synthesis

$$
\begin{array}{cc}
\text{construction} & \text{demonstration} \\
x : A & \vdash T(x,f(x)) \\
\downarrow & \downarrow \\
\vdots & \vdots \\
\downarrow & \downarrow \\
f(x) : B(x) & \vdash C(x,f(x))
\end{array}
$$

In case there is no condition, analysis reduces to resolution and synthesis to construction. Analysis uncovers the functional dependency

$$y = f(x) : B(x)$$

of the sought objects on the given ones, and synthesis then constructs the sought objects from the given ones, using this knowledge.

Downward arrows in the scheme indicate deduction, and upward ones reduction. No direction is indicated for resolution, because it does not have any fixed direction. It may proceed either deductively from the given objects to the sought ones or reductively in the converse direction (cf. Mäenpää 1993 for further information).

Ancient Greek mathematicians deduced the sought objects from the given ones in resolution, beginning with the dependency of a constituent of a sought object on the given objects that was uncovered in the transformed condition. Thus Pappus's account only describes the transformation part of analysis.

In Greek mathematics transformation was a deductive reduction, that is, a chain of equivalent conditions. In synthesis, the transformation was converted into a demonstration.

No ancient methodological account seems to exist that discusses the restrictions that ensue from the limitation to deductive transformations in analysis. Quite evidently a large class of propositions admit only a successful analysis whose transformation is not deductive. This may be one reason why some Greek mathematical works were exposed only synthetically. If analysis is restricted to deductive transformations, its universality as a mathematical method of discovery is considerably restricted.

In the case of theorems, the scheme for problems reduces to the following special case.

$$
\begin{array}{cc}
\text{Analysis} & \text{Synthesis} \\
\text{transformation} & \text{demonstration} \\
\vdash T(x) & \vdash T(x) \\
\uparrow & \downarrow \\
\vdots & \vdots \\
\uparrow & \downarrow \\
\vdash C(x) & \vdash C(x)
\end{array}
$$

V The Heuristic Role of Auxiliary Constructions

The configurational interpretation construes analysis as a study of the functional dependencies in a definite configuration. This configuration consists of the given and the sought objects, assumed to relate to one another as specified by the condition.

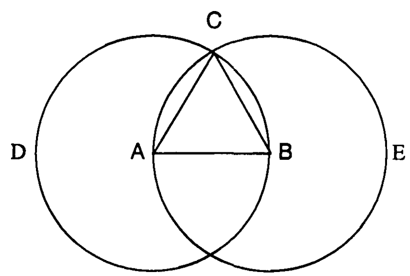
There is one proviso to this description. Determining the sought objects in terms of the given ones will not in general succeed by analysing just this definite configuration. It must be amplified by auxiliary constructions in the course of analysis. This is the heuristically crucial and unpredictable factor of analysis, as Hintikka and Remes (1974 and 1976) pointed out forcefully.

Descartes ignores auxiliary constructions in the account of analysis in his *Geometry* cited above. All he has to say is that in solving a problem the analyst is to "give names to all the lines that seem needful for its construction,—to those that are unknown as well as to those that are known."

To understand the systematic role of auxiliary constructions in analysis, Hintikka and Remes described their introduction in terms of quantifier instantiation rules of predicate logic. They contrast such instantiation steps to other steps of analysis, which take apart a proposition into its constituents as prescribed by the subformula principle of natural deduction systems of predicate logic.

However, as we have seen, analysing a configuration is more naturally formalized as the functional decomposition of a construction in type theory. Introduction rules decompose sought objects into their constituents, and elimination rules decompose given objects.

Let us look informally at a few examples of how auxiliary constructions function in solving problems and proving theorems of Euclidean elementary geometry. First, consider the proposition I, 1 of the *Elements*, which is the problem of constructing a sought triangle on a given line segment satisfying the condition that the triangle is equilateral and constructed on the line segment.



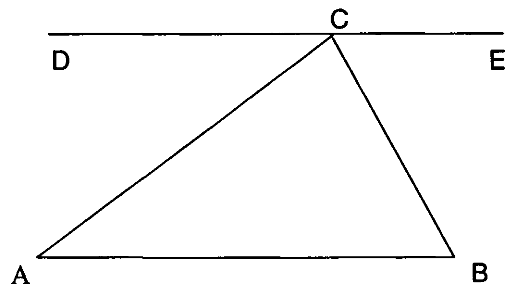
Euclid gives only the synthesis of the solution. In its construction part, he constructs two circles on the given line segment *AB*, one centered on point *A* and the other on point *B*, using *AB* as the radius. These steps apply his third construction postulate, for circles. Then he connects the points *A* and *B* to *C*, which is one of the two intersection points of the circles. These steps apply his first construction postulate, which allows constructing a line segment connecting two given points.

A well-known deficiency of the *Elements* from the point of view of modern standards of axiomatic systems is that Euclid does not justify the construction of a point by means of two intersecting lines, like point *C* here, by any construction postulate. Rather, he takes it for granted that one may construct a point by letting two lines intersect, whether two straight lines or two circles or one of each.

In the demonstration part of his synthesis, Euclid shows that the condition of the problem holds by appealing to the definition of a circle. As *AC* and *AB* are radii of the same circle, they are equal in length. The same goes for *BC* and *BA*. By his axiom that "things that are equal to the same thing are also equal to one another", the first "common notion" of the *Elements*, *CA* is equal to *CB* and hence the sought triangle *ABC* is equilateral. It is also constructed on the given line segment *AB*, as required.

This solution required the auxiliary constructions of the two circles, carried out in the construction part of the solution. Without them, Euclid could not have determined the sought triangle in terms of the given line segment, because the vertex *C* of the triangle was constructed from the line segment *AB* by intersecting the circles.

Now consider the proof of a theorem, proposition I, 32 of Euclid's *Elements*, which states that the angle sum of a given triangle equals two right angles (this proof is in fact a version handed down by Eudemus).



First Euclid draws a straight line *DCE* through the vertex *C* of the given triangle *ABC* parallel to its base *AB*. Then he argues that $\angle ACD$ is equal to its alternate angle $\angle A$, and likewise $\angle BCE$ equal to $\angle B$, so the angle sum of the given triangle *ABC* equals the sum of $\angle ACD$, $\angle ACB$, and $\angle BCE$, that is, two right angles.

The auxiliary construction of the line *DCE* is the heuristically crucial part of this proof. Without it, the proof would not succeed. This shows that even though theorems have no construction part in their synthesis, auxiliary constructions are in general needed in order for their proofs to succeed.

Auxiliary constructions serve to bring forth new relations among the constituents of the configuration that is analysed, so that new propositions can be applied in the proof or solution. In the synthesis of theorems, auxiliary constructions are performed in the demonstration part, because there is no construction part.

In the present case, the auxiliary construction *DCE* allows mobilizing the theorem that a line intersecting two parallels makes the ensuing alternate angles equal to one another. This is proposition I, 29 of the first book of Euclid's *Elements*.

Now to gain a more general understanding of the significance of auxiliary constructions in mathematics, consider the elementary algebraic problem

$$a^2x^4 + abx^2 = c$$

for reals, assuming we know the standard solution to a quadratic equation. Algebraic equations are equality propositions, to be distinguished from definitional equalities, which are represented in type theory as judgements of the form $a = b : a$. This problem has the following parts.

given	$a,b,c : R$
sought	$x : R$
condition	$\vdash a^2x^4 + abx^2 = c$

The solution by analysis starts with a transformation. First, substitute the fresh variable y for ax^2 in the condition. This reduces the condition to the equivalent one

$$\vdash y^2 + by = c$$

Then transform this into the equivalent condition

$$\vdash y^2 + by - c = 0$$

We have now hit upon the transformed condition, because this equation is solved by the known general solution to a quadratic equation.

The resolution first applies this known solution, which yields the value

$$y = \frac{-b}{2} \pm \sqrt{\frac{b^2}{4} + c} : R.$$

There is a condition of solvability for y, a diorism in Greek terms, that

$$\vdash \frac{b^2}{4} + c \geq 0$$

In the second step of resolution, we determine the original thing sought x in terms of y as

$$x = \pm\sqrt{\frac{\frac{-b}{2} \pm \sqrt{\frac{b^2}{4} + c}}{a}} : R$$

by means of the known solution for x in terms of y from the equation $y = ax^2 : R$ corresponding to the substitution. (Any value of x is a solution if $a = c = 0 : R$.) Here, too, we have diorisms,

$$\vdash \frac{\frac{-b}{2} \pm \sqrt{\frac{b^2}{4} + c}}{a} \geq 0 \qquad \vdash a \neq 0$$

As algebraic equations of this Cartesian kind are more formal than the above propositions of geometry, this example shows more clearly in formal terms how auxiliary constructions enter into the a solution or a proof by analysis. They are introduced by substitution. We substituted the fresh variable y for the expression ax^2 in order to find a solution for x in terms of a, b, and c.

VI The Logical Role of Auxiliary Constructions

As auxiliary constructions are so central heuristically in analysis, let us discern their role in logical terms. Hintikka and Remes characterize them in terms of quantifier instantiation, but type theory allows us to represent them logically in a way that preserves their informal character faithfully.

In informal mathematics, auxiliary constructions are brought into analytical proofs and solutions by substitution. Our algebraic example, for instance, showed no trace of quantifier instantiations in bringing in the auxiliary construction. Recall their other informal characterization, as constructions that are constituents of neither the given nor the sought objects.

The substitution rule of type theory

$$\frac{\begin{matrix}(x:A) & \\ b:B & a:A\end{matrix}}{b(a/x):B(a/x)}$$

can be seen reductively as a generalization of the usual cut rule of natural deduction systems (Mäenpää 1993, ch. 2). Its first premise means that $b:B$ in the context $x:A$, that is, the premise $b:B$ may depend on the hypothesis $x:A$. Proceeding deductively from premisses to conclusion, the rule allows substituting a for x in b and B.

In analysis, this rule can be used reductively for introducing the auxiliary construction x of type A into the problem of proving some specified proposition C. The point is, heuristically, to see C as a substitution instance $B(a/x)$ of a more general proposition B that is defined in terms of the auxiliary construction $x:A$. Proving the proposition C, that is $B(a/x)$, reduces then to proving the proposition B in terms of $x:A$ and to constructing an object a of type A.

In our algebraic example the proposition C was the equation $a^2x^4 + abx^2 = c$ whose solution, the value of x in terms of a, b, and c, we sought. The heuristically crucial step in the analysis was the first step of transformation, where we saw this problem as a substitution instance of the problem $y^2 + by = c$ by applying the above cut rule reductively. This introduced the auxiliary construction y, which matches x in the rule. Furthermore, the object a in the rule matches the expression ax^2 in our example.

This rule of cut or reductive substitution has a special role in our logical description of configurational analysis. As introduction rules serve to analyze sought objects and elimination rules given objects, there must be some rule for introducing auxiliary constructions, because they do not arise from analysing the given and sought objects. This is what the cut rule is for.

In Hintikka and Remes's logical characterization of analysis in terms of predicate logic, the cut rule violates the rules of analysis. They forbid its use altogether, because it does not enjoy the subformula property of predicate logic. Instead, they see auxiliary constructions as entering by quantifier instantiation. This conception is the only reasonable one in predicate logic, where the cut rule is less general than in type theory. It has nothing to do with substitution, because it does not deal with individuals at all. In fact, predicate logic has no rule of substitution. Substitution operations of informal mathematics are artificially codified by means of quantifier rules. This is why predicate logic does not allow representing the

introduction of auxiliary constructions in a way that is faithful to informal mathematical practice.

Let us now enrich the schemes for analysis and synthesis by taking auxiliary constructions into account.

Analysis

$$\begin{array}{cc}
\text{resolution} & \text{transformation} \\
x:A & \vdash T(x,y,z) \\
 & \uparrow \\
\cdot & \cdot \\
z:G(x) & \cdot\ (g(x,y)/z) \\
\cdot & \cdot \\
 & \uparrow \\
y:B(x) & \vdash C(x,y)
\end{array}$$

Synthesis

$$\begin{array}{cc}
\text{construction} & \text{demonstration} \\
x:A & \vdash T(x,f(x,h(x)),h(x)) \\
\downarrow & \downarrow \\
\cdot & \cdot \\
h(x):G(x) & \cdot \\
\cdot & \cdot \\
\downarrow & \downarrow \\
f(x,h(x)):B(x) & \vdash C(x,f(x,h(x)))
\end{array}$$

Analysis introduces the auxiliary constructions z of type $G(x)$ by substituting $g(x,y)$ for them reductively in the transformation (here z and $g(x,y)$ denote, again, vectors of objects). The original configuration that consists of x and y is thereby amplified by z.

Resolution determines the auxiliary constructions z in terms of the given objects x alone. This is why the type $G(x)$ must not depend on the given objects x, in contrast to the expression $g(x,y)$ substituted for z reductively in transformation (for the logical role of auxiliary constructions in analyses where the proposition has no condition, cf. Mäenpää 1993, ch. 3; in this case they have to be both introduced and determined in terms of the given objects in resolution).

Analysis uncovers the functional dependencies

$$z = h(x) : G(x)$$
$$y = f(x,z) = f(x,h(x)) : B(x)$$

that are constructed in synthesis. The determination of the things sought y in terms of the auxiliary constructions z and the given objects x in resolution must respect the equation

$$z = g(x,y) : G(x)$$

that corresponds to the substitutions in transformation.

For instance, in our algebraic example we introduced the auxiliary construction y by substituting the expression ax^2 reductively for it. This expression depends on the given object a as well as the sought object x. Yet in resolution, we determined y *in* terms of the given objects alone. Then we determined the thing sought x in terms of the given objects and the auxiliary construction y so that the equation $y = ax^2 : R$ corresponding to the substitution was respected.

Notice that substitution doesn't figure in synthesis. This is why we did not discern it in the geometric examples of employing auxiliary constructions from Euclid's *Elements* (for a discussion of the analyses corresponding to these syntheses, cf. Mäenpää 1993, ch. 5).

In the case of theorems, we have the following special case of the above scheme.

$$\begin{array}{cc}
\text{Analysis} & \text{Synthesis} \\
\text{transformation} & \text{demonstration} \\
\vdash T(x,z) & \vdash T(x,g(x)) \\
\uparrow & \downarrow \\
\cdot & \cdot \\
\cdot\ (g(x)/z) & \cdot \\
\cdot & \cdot \\
\uparrow & \downarrow \\
\vdash C(x) & \vdash C(x)
\end{array}$$

Here the auxiliary constructions do not have to be determined in terms of the given objects alone, as in the analysis of problems, because there are no sought objects that the auxiliary constructions could be made to depend on in transformation. We can thus directly use the same expression $g(x)$ in demonstration that was used in transformation.

Nevertheless, this scheme shows how auxiliary constructions figure in the analysis of theorems, and not only in the solution of problems. They do not arise by taking apart the given constructions. Rather, the given configuration x must be amplified by z.

The source of the heuristic usefulness of analysis is that it gives the possibility of making systematic use of the things sought and the condition as well as of the given objects. Plain synthesis, without an antecedent analysis, has to proceed from the given objects to the sought ones and then demonstrate the condition blindly, so to say, without making systematic use of the sought objects and the condition.

Auxiliary constructions, in particular, function in a subtle way. They can be based on the sought as well as the given objects in the substitutions performed in transformation. In resolution they are then determined in terms of the given objects alone. This functioning is spelled out in precise formal terms in the above schemes.

As theorems have no sought objects, analysis is less useful heuristically for proving them than for solving problems. In particular, auxiliary constructions function in a less subtle way. They are introduced outright in transformation in a way that need not be justified in resolution or in synthesis in another way, because they can depend only on the given objects. This explains in part why analysis was above all a method for solving problems for ancient Greek geometers (compare to the account of Knorr 1986, ch. 8).

Let us reconsider the question whether predicate logic is adequte for describing theoretical analysis. As proving theorems requires auxiliary constructions in general, it depends on solving construction problems. Auxiliary constructions are, after all, solutions to problems. Therefore predicate logic suffices for describing theorem proving adequately only in those scarce trivial situations where auxiliary constructions are not needed.

Moreover, auxiliary constructions and constructions in general require a constructive logic for their logical description, because constructions are formed constructively by definition. In the mathematical tradition established by the Greeks, the construction part of synthesis was constructive in terms of modern constructivist standards, whereas indirect proofs were allowed in the demonstration to prove properties of constructions. As mathematics has reduced to theorem proving during this century, classical predicate logic suffices to describe it, if auxiliary constructions are not taken into account.

In contemporary mathematics, indirect proofs of existence are allowed everywhere instead of constructions, which are carried out by means of construction postulates. In particular, auxiliary constructions are replaced by objects whose existence is proved indirectly. Then classical predicate logic suffices to describe them and theorem proving in general.

In another direction, the constructivists of this century have gone further than the Greeks—they require constructive reasoning even in demonstrations of the properties of constructions, not only in constructions.

As a surprising recapitulation of ancient mathematical history, computing scientists have in recent years started to solve problems in exactly the same sense as Greek mathematicians. They carry out constructions (computer programs) and demonstrate that they satisfy specified conditions. As programs are formal by definition, the need for an adequate formalism has been crucial in computing science, in contrast to mathematics.

Type theory has become one of the main theoretical approaches in computing science, because it is a programming language as well as a logical system (Martin-Löf 1982, Nordström, Petersson and Smith 1990). Programs are constructed by rules of introduction and elimination and evaluated by rules of computation. This allows constructing programs and demonstrating their properties in one formalism, which is a considerable advantage over traditional programming languages. Predicate logic does not suffice for this, nor does classical logic, because they don't recognize constructions.

Programming was until the 1970's in a pre-theoretical stage in the way mathematics was before the Greeks made it a science. In the 1970's the need arose to prove that programs satisfy their specified conditions, that is, do what they are supposed to do. This is how programming evolved into a science in the sense established by the Greeks. Characteristic of mathematics before the Greeks as well as programming before the 1970's was a stage of algorithmic constructions with no specifications or demonstrations of conditions of correctness imposed on the constructions. The Greek method of geometrical analysis can be generalized into a method of solving all kinds of mathematical problems in type theory by taking into account inductively defined problems, which are characteristic of programming. The method known as top-down programming turns out to be a special case of analysis (Mäenpää 1993, chs. 3 and 8).

University of Helsinki
Department of Philosophy

JEAN-MICHEL SALANSKIS

ANALYSIS, HERMENEUTICS, MATHEMATICS

I Introduction

In this article, we would like to study the importance of the concept of analysis for mathematics from three points of view:

– Firstly, we will be attentive to what, since the Greeks, since Pappus if what I have learned is correct, is called "analysis", and which, as a characteristic procedure of geometric reasoning, is put forward by Platonic and Aristotelian philosophy as a universal model of thought.

– Secondly, we would like to understand analysis as the regressive method of all transcendental inquiry, following Kant's suggestion, and to reinterpret this transcendental inquiry as necessarily hermeneutical.

– But finally, we will aim to elucidate, starting from these sorts of considerations, the unity of meaning of analysis, that contemporary branch of mathematics whose prodigious development in modern times is well known.

There is little doubt that a certain degree of failure in such an undertaking is likely. The method which will be followed to attain some results despite the scope of the questions raised and the unsettling character of the comparisons we wish to establish will consist in a straightforwardly personal reconstruction of certain elements of the tradition.

Let us therefore begin with Greek analysis.

II Greek Analytical Suspension: Hermeneutics and deliberation

In the *Republic* (510 *b-d*), Plato clearly opposes mathematical and philosophic approaches: he considers the latter as essentially regressive, consisting in an upward move from any given to the "non-hypothetical principle(s)" belonging to the purely intelligible realm. The former is essentially suspensive and progressive, laying down certain hypotheses, passing through their consequences while breaking once and for all with any questioning of them. But on the other hand, the method of geometers is readily called up as an argumentative model for philosophy. Notably in *Meno* (86*b*-87*d*), when a provisional phase of the research into the essence of virtue must be justified, Plato cites a relatively obscure (in the

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 227–241.

words of Plato's French editor and translator Léon Robin[1]) example which does seem to be a case of classical "analysis".

We therefore wish to reflect on Greek analysis to know if it is regressive or progressive, suspensive or interrogative, philosophical or mathematical.

The discussion may commence not with Greek sources, but with what, in the mathematical tradition, has been defined as the method of analysis. In my case, analysis was taught to me, more than two thousand years after the Greeks, as the first phase in the treatment of a problem of geometric construction. Faced with the problem of constructing a figure, a straight line or a point with such and such a property with respect to geometric givens—which are in turn simple figures (point, straight line, triangle, circle, etc.)—we are advised to begin by "assuming that the problem is solved": by tracing in a tentative and approximate manner a figure in which what must be constructed is present and whose construction we assume to be correct (generally speaking, moreover, we know how to adjust distances and angles intuitively so as to actually experience the construction as correct or slightly incorrect). We may then, on inspection of the figure, begin the work of deduction, whose premises are acquired through considering the properties of the entity under construction as satisfied. The process of deduction naturally gives up a series of properties, certain of which will be the relations of the entity under construction—or more generally the constituents of this entity—to the given entities. At a certain point, these relations may be able to indicate and prescribe in transparent fashion a possible construction. There then remains, in the phase called "synthesis", the task of demonstrating that what has been constructed in the discovered procedure indeed satisfies the stipulated properties of the "problem of construction".

Thus, one assumes that the relations to be satisfied are satisfied (the relations of the entity to be constructed to the given entities) so as to deduce other relations out of which a construction is possible and recommendable.

How must this procedure be described?

First of all it is suspensive, for it consists in a hypothesis; but the hypothesis is the elimination of what is at stake, of the aim, of the problematic originary orientation. Thus, there is indeed suspension, at least apparently, suspension at a certain level of the drive toward the goal.

The method is obviously progressive as well: one derives conclusions from the hypothesis that the problem is solved, instead of working down from the hypothesis to its unquestionable sufficient reasons, or attempting such a philosophic regression from the encompassing conditions of the problem. Yet these eventualities of a philosophic treatment of the geometric problem have a false ring to them, because the context of the problem is immediately non-philosophic: the regression to nonhypothetical principles referred to by Plato clearly deals with lexical indicators of conceptual signification, rather than with those configurations, leading to a decision, that the problems are. Therefore analysis does appear suspensive and mathematical, but suspensive in the sense that it is less the evaluation of a thesis that is suspended as the tension of a problem, that is to say a sort of strategic meta-thesis.

Let us proceed now to the purely logical plane. The procedure of analysis begins by laying down a phrase such as:

$\exists x\, P(a,x)$ [there exists an entity x such that it has a relation P to the given objects a].

We now move on the logical deduction, finishing with a phrase such as the following:

$\exists x\, Q(a,x)$ [there exists an entity x such that it has a relation Q to the given objects a].

One imagines that there must be a way to attest this new phrase "effectively", to construct the entity(ies) mentioned in the phrase existentially. And one imagines as well that this construction is in fact, through certain simple mediations, the *ipso facto* construction of entities x such that $P(a,x)$, which is to say one imagines this to be the solution to the original problem.

This procedure of analysis seems circular: one assumes the existence of an x satisfying P to be able to demonstrate the existence of an x satisfying P. This circularity has nothing to do with a vicious circle, because the presupposition is logico-existential, and because what is achieved at the end is an effective construction. Such a construction can be achieved because the existential description *à la* Russell of the object to be constructed has been transformed into that of another object, with the property that a constructive counterpart to it is immediately given, and because the passage from the construction of this new object to that of the original can be accomplished.

I am led to conclude that analysis, seen in this angle, is a thoughtful elaboration allowing for the transition from the logico-predicative precomprehension of an entity to practical comprehension. The underlying presupposition is that certain logico-predicative precomprehensions have always contained their practical counterparts: this is but to name and to grasp the traditional idea of a "guiding" geometric intuition. The geometric intuition consists in there being practical correspondents of the constructive order to certain simple, defined descriptions, providing that the constant parameters of these descriptions themselves be given in intuition.

In any case, the drift of my argument is now clear: the procedure of analysis in the classical, technical sense of the term that it has acquired since the Greeks in the field of geometry is closely related to hermeneutics. It must be pointed out in passing that this hermeneutics is opposed to the hermeneutics Heidegger adumbrates in section 63 of *Sein und Zeit* (1927): for Heidegger, the precomprehension of being is practical, ante-predicative, and hermeneutical elucidation consists in a bringing into view through predicative speech, whereas here, the passage is from a saying that articulates the object to be constructed in such and such a way to a realisation of the geometric construction which exhibits the object with the same determinations.

This line of thought on Greek analysis can be completed with an account of the following passage from the *Nicomachean Ethics* in which Aristotle conceives of the reasoning of the practical understanding as stemming from the model of analysis:

> "We deliberate not about ends but about means. For a doctor does not deliberate whether he shall heal, nor an orator whether he shall persuade, nor a statesman whether he shall produce law and order, nor does anyone else deliberate about his end. They assume the end and consider how and by what means it is to be attained; and if it seems to be produced by several means they consider by which it is most easily and best produced, while if it is achieved by one only they consider how it will be achieved by this and by what means this will be achieved, till they come to the first cause, which in the order of discovery is last. For the person who deliberates seems to investigate and analyse in the way described as though he were analysing a geometric construction (not all investigation appears to be deliberation—for instance mathematical investigations—but all deliberation is investigation), and what is last in the order of analysis seems to be first in the order of becoming."[2] (1112*b*, 12-25)

Analysis seems here to be characterised by regressive reasoning, which, *prima facie*, is in total contradiction with Plato's divide between mathematics and philosophy. The connection to the traditional notion of analysis in geometry mentioned earlier is easy to establish; Aristotle perceives that in practical deliberation, the problem is assumed solved as in problems of construction. But the deliberation is not analogous to progressive research into the conditions of construction, for it is in fact regressive: the regression it enacts is at one and the same time purely logical and empirical, conditions are introduced as perfectly regular logical premises of the previously considered condition, and the mind remains constantly watchful over the possibility of adjusting practically the world to the present condition.

A type of extremely simple mathematical reasoning conforming to this model can be cited. Moreover, this type of reasoning is of the greatest importance in contemporary mathematical analysis, be it real or complex. I refer here to processes of reasoning adapting α to ε, to attest a property of continuity or limit following the definition prevailing since Weierstrass: let us say, for example, that I wish to establish the continuity in 1 of the function $x \rightarrow x^2$; $\varepsilon > 0$ is given, and I will seek $\alpha > 0$ such that the condition $|x-1| \leq \alpha$ implies $|x^2-1| \leq \varepsilon$; what is to be obtained is in fact $|x-1||x+1| \leq \varepsilon$, which follows from $|x-1| \leq \varepsilon/2$ and $|x+1| \leq 2$, this last condition resulting from $|x-1| \leq 1$, so that $\alpha = \mathrm{Min}(1,\varepsilon/2)$ agrees. It is clear that the "deliberation" involved in this proof requires that a "means" be found of a prior (double) "means", therefore the deliberation already possesses a certain depth. Those familiar with contemporary real and complex analysis may witness that

this sort of procedure, with its essential estimative aspect, is omnipresent therein, not necessarily as a global scheme of what is accomplished (modern technicity having introduced other general modes of mathematical reflection), but quite often as the decisive and necessary local manipulation.

The question here is whether this deliberative regression *à la* Aristotle makes it "philosophic" in the Platonic sense. Once again, it seems that the distinction is marked in Platonic regression being semantic and lexical, aiming for the nonhypothetical principle, while Aristotelian regression is logical and phrastic, aiming at the effectuation of the hypothesis. In the case of ethico-practical deliberation, this is the pure and simple concrete faculty instituting a state of affairs in the world. In the case of Weierstrassian "deliberation", the effectuation comes about in the mediate discovery of a condition of a type set down in advance, ultimately implying the condition taken as final theme.

This other type of analysis can no longer be attached to the hermeneutical model, as was suggested above in bringing to light the procedure of analysis in the solution of a problem of geometric construction. The two relevant orders, that of the logical phrase and its implication on the one hand, that of its effectuation on the other, are no longer related in such a way that what takes place in one order can be considered as satisfying what is anticipated in the other. Moreover, must the hermeneutical path not be an uncertain progression, a drift? Is there in fact elucidation if one simply strives through accumulative stages toward a point of resolution and actuality? Aristotelian analysis has something in common with problem-solving , and nothing of the sort with the hermeneutical circle: the "problem is assumed solved", but this is not to make it a premise, nor to acquire it as a pregiven, but quite simply to make it one's goal at the end of a logico-rationally polarised interval. It will become clear by the end of this paper that this logically regressive analysis may however be considered, and doubly so, as a hermeneutics. But for the moment we lack the means of grasping this possibility.

At this point of our presentation it is difficult not to want to deal with that other historically claimed form of analysis: Kantian transcendental regression.

III Transcendental Analysis

In the "Methodology of pure reason", Kant sets up a famous demarcation between mathematics and philosophy, the procedure of philosophy being that of knowledge gained through concepts, and that of mathematics as knowledge gained through the construction of concepts. His essential aim is to explain how the deduction of the principles of pure understanding, which appears *a posteriori* as the philosophic result of the *Critique of Pure Reason*, is not and could not be a part of mathematics. The motive of this divide lies in the nature of the concepts worked through in the transcendental inquiry: they are strictly discursive concepts, thus with no generic instance in intuition (the procedure of "concept construction", so typical a move in mathematics, is in their case impossible). In a logic of auto-justification which is one of the essential stakes of this passage, Kant explains that what may be learned about them is limited to their function as "rules" for the synthesis of sensible manifolds in excess of them and under the extraneous legislation of the pure forms of intuition. But he also says something else, apparently gratuitous and intrinsic, about these concepts: that they are present in ordinary human usage, in such a form however that their content is not delimited. And he names "analysis" the procedure explicating a norm of correct signification for such concepts, opposing this procedure to that of mathematical "definition".

Once again we then meet with the collusion between the mathematico-philosophic divide and the figure of analysis, and that between the latter and the idea of regression, as will be seen more clearly below. Husserl, reading these passages, retained the idea that the regressive method was characteristic of the transcendental spirit *à la* Kant. In order to refute the Kantian transcendental, he retains as its positive principle a partially Cartesian formulation: the transcendental thesis consists in saying that all knowledge is knowledge of a subject and is only valid as knowledge following the certification of the subject—there can be no meaning to the idea of knowledge dictated and validated by the object. Husserl attributes this thesis to Kant as a major insight and progress for thought, but he parts ways with him over how to describe these subjective formations governing all knowledge. According to Husserl, Kant obtains his transcendental invariants, the categories, space and time with their own constraints "by regressing from *de facto* discourse", from *de facto* thought of the subject in general and of the subject of science in particular. But his judgement is that Kant's method issues in opaqueness of the resulting transcendental factors. In Husserl's view, what is discovered by regression, what is identified as the condition of possibility of a *de facto* exercise, even if it never be present in the exercise, has on principle the right not to have either intuitive grounds or evidence for its subject, and ultimately it is likely not to have any sense. Whereas, for Husserl, what we name the transcendental character of what affects our knowledge must appear as such to us in an examination of our subjective performance "on the path" of knowledge. The transcendental factors must not be merely linked in a logical relation to the experience of knowledge, but must themselves be able to be experimented with their functions within that experience. Husserl's position interests us for its negative lesson on what could be called "conceptual analysis", the regression from a fact not toward Platonic non-hypothetical principles, but to guiding notions, conditions of possibilities, a regression that always thinks a logico-significant link: this analysis does not conquer evidence, but rather leads us to contents whose strangeness is maintained at the very moment their guiding quality is acknowledged.

But let us listen to the expression of such a conceptual analysis in Kant:

> "In the second place, it is also true that no concept given *a priori*, such as substance, cause, right, equity, etc., can strictly speaking, be defined. For I can never be certain that the clear representation of a concept, which as given may still be confused, has been completely effected, unless I know that it is adequate to its object. But since the concept of it may, as given, include many obscure representations, which we overlook in our analysis, although we are constantly making use of them in our application of the concept, the completeness of the analysis of my concept is always in doubt, and a multiplicity of suitable examples suffices only to make the completeness probable, never to make it *apodeictically* certain. Instead of the term, definition, I prefer to use the term, *exposition*, as being more guarded term, which the critic can accept as being up to a certain point valid, though still entertaining doubts as to the completeness of the analysis."[3] (Kant A, 729; B, 757)

It is thus clear that the philosophical procedure of analysis starts with a concept given in usage, then attempts to decompose it at the level of signification, without however being certain of ever having a complete semantic portrait of the concept. This procedure is opposed to that of the definition, characterised in the following terms:

> "There remain, therefore, no concepts which allow of definition, except only those which contain an arbitrary synthesis that admits of *a priori* construction. Consequently, mathematics is the only science that has definitions. For the object which it thinks it exhibits *a priori* in intuition, and this object certainly cannot contain either more or less than the concept, since it is through the definition that the concept of the object is given—and given originally, that is, without its being necessary to derive the definition from any other source." (*ibid.* A, 729-730; B, 757-758)

It is then essential to the notion of analysis that it imply the relationship to a given, whereas the definition "gives" itself:

> "We shall confine ourselves simply to remarking that while philosophical definitions are never more than expositions of given concepts, mathematical definitions are constructions of concepts, originally framed by the mind itself [...]." (*ibid.* A, 758; B, 730)

Kant insists strongly on the provisional, perfectible character of analysis. Thus, in a footnote:

> "Philosophy is full of faulty definitions, especially of definitions which, while indeed containing some of the elements required, are yet not complete. If we could make no use of a concept till we had defined it, all philosophy would be in a pitiable plight. But since a good and safe use can still be made of the elements obtained by analysis so far as they go, defective definitions, that is, propositions which are properly not definitions, but are yet true, and are therefore approximations to definitions, can be employed with great advantage. In mathematics definition belongs *ad esse*, in philosophy *ad melius esse*. It is desirable to attain an adequate definition, but often very difficult. The jurists are still without a definition of their concept of right." (*ibid.* A, 731; B, 759)

Therefore I would like to know and ask to what point this figure of analysis is a figure of hermeneutics. The word "exposition" appears for the first time in the *Critique of Pure Reason* in the transcendental aesthetic, where Kant presents a "metaphysical exposition", clear though not detailed, of space. In this case as in that above, the exposition sets forth a content in ignorance of that completeness which is its aim, to the point of despair of ever being able to reach such a goal. With the problem of space, this impossibility has something principled about it, since the very infinity of space, revealed by the exposition, is opposed to its completeness. But this is only one of its aspects: the incompleteness is related as well to what the exposition sets forth of what is "anticipated" of space, to what of space is "prejudged", to what geometry will systematise, but which is not yet in itself formal or exact, thus displaying an essential incompleteness of determination, calling for diverse elucidations. The investigation here called "analysis" has common characteristics with the metaphysical exposition. The principle difference being that it is nevertheless a "decomposition": it works on a word of the language, a word corresponding to a concept, and attempts to elucidate it in what would appear to be the only possible way, *i.e.* through a list purporting to be complete of the semantic contents in which the concept exhausts its meaning. But this work is open and incomplete, consisting in a dialogue with the given which is at one and the same time a way of prescribing this given, as in the case of the metaphysical exposition. In that case, the donation is the celebrated intuitive donation, that of the pure forms of the sensibility to the subject, a donation supposed to precede *de jure* all experience, and which is called pure intuition. While in the case of analysis of a concept such as "substance", the given is that of a semanticism already shared by the circle of the thinking community. Hermeneutics in its most classic concept can only apply to this sort of given, which is easily conceived as equally "not given". This is the structure of the "envelopment of meaning", a sort of *a priori* structure governing the region of meaning, according to which everything having meaning withholds additional meaning that, in one way or another, has to be explicated or activated. On the other hand it is not self evident to conceive of the mathematical theorisation of space, for example, as a hermeneutic: this is nevertheless what I wished to propose as the best epistemological scheme of mathematical activity in my *L'herméneutique formelle* (1991), whose point of departure was indeed the presentation of the relation to space as a relation at once of familiarity and of dispossession, a relation to a given-not given of the same sort as that to a lexical unit in which meaning is enveloped. My complete thesis, whose main argument I have just in part reproduced, is that the relation named by Kant "intuition" is a relation of this sort.

But, as for the usage of the word analysis, there is an important distinction to be made. Analysis as a procedure of finite and controlled decomposition is the hermeneutical method when it has as its object the natural opaqueness of lexical meaning. On the other hand, the mathematical interpretation of space does not follow the path of analysis, but rather proceeds by axiomatic enunciation, "synthetically", the exact inversion of hermeneutics. Judgements prescribing space are made, inscribed and aligned; they are supposedly inspired by our familiarity with space but, whatever the case, they set and delimit that space, enabling a regulated logical usage of the representations that will implement the knowledge of space. The synthetic character resides in the fact that these judgements predicate subjects of determinations that do not figure in their concept, in conformity to the Kantian definition, but we could take a step further in considering modern axiomatic experience, and conclude that axiomatisation is synthetic insofar as it establishes, prejudgementally, a world of objects in its coherence and universality. Whereas conceptual analysis limits itself to deploying problematically the wealth of possibilities of a locus of meaning, of a condensation of thought.

In any case, the mere consideration of analysis as the characteristic method of transcendental investigation and of the metaphysical exposition of the transcendental aesthetic as both belonging to the hermeneutical attitude suffices to show that each factor of the Kantian transcendental structure in fact receives its identity as a hermeneutical conquest: space, time, and the categories constrain knowledge *a priori* only as figures of themselves to which access is given in a dispossessive familiarity. These figures have the status and the composition of non-given givens, objects allowing analytic work in the case of conceptual elements, and, as for intuitive elements, permitting mathematical synthesis which is nonetheless hermeneutical.

Can this preliminary two-headed reflection afford insight into the project of expressing the essence of contemporary mathematical analysis?

IV The Identity of the Branch Analysis of Contemporary Mathematics

How is analysis to be identified today? There is of course J. Dieudonné's *Elements of Analysis* (1963-1982), which gives us a sketch of the complex tree of the subdisciplines of analysis, claiming to expound them one after the other, volume after volume. General topology, theory of topological spaces, theory of analytical functions, functional analysis, algebraic topology, differential geometry, theory of dynamical systems, differential topology: all these headings, of different implicit or explicit levels, coming together and crossing each other in various ways, compose the figure of analysis. At a glance, the unity of these procedures is in the dependence of the objects treated on the $\mathbb{R}$ and the $\mathbb{C}$ of the Cantorian construction, together with the play of the topological element of these structures. Having said that, there are certain cases in which the disciplines of analysis confine with algebra, for various reasons: in the case of analytic geometry, it is because this branch makes use of constructions generally given as algebraic in a geometry itself known as algebraic; for the case of differential equations, the motive would be more strategic, because the solution of equations is an algebraic heuristic and, consequently, despite the topological nature of its objects and situations, many aspects of the theory come from algebra.

The discussion undertaken here revolves naturally around the opposition between "analysis" and "algebra". But this is not the only possible discussion: another one is oriented towards the distinction between "analysis" and "geometry". It seems self-evident to me that the theory of topological vectorial spaces should belong to analysis, but I would much less spontaneously call this theory "geometric". Dieudonné seems to classify in analysis everything in which topology plays a decisive role, thus evincing a particular conception of the branch. But in the diffuse sentiment of contemporary mathematicians, there is also a more restrictive idea of analysis, according to which it would be defined as the study of set-theoretical complexity—that is, above all, functional complexity—developed on the basis of $\mathbb{R}$ and $\mathbb{C}$, indeed from a topological viewpoint, without ever attaining a geometric perspective on these entities. From this point of view, differential geometry would contain numerous aspects outside the field of analysis strictly speaking.

As for the concept of "geometry", it is in a problematic inter-definitional state with that of topology: not all study of topological structure is geometric—there is another diffuse sentiment according to which geometry begins only when the topological structures studied are sufficiently affinitive to classical Euclidean structures. One possible criterion is the presence of a sheaf, that is, that readily operational entities be given above the localisations offered up by the topological space.

Lastly, the concept of "algebra" is difficult to distinguish from that of "arithmetic": the classic "algebraic structures"—group, ring, field—have for their simplest examples the objects $\mathbb{N}$, $\mathbb{Z}$, and $\mathbb{Q}$, which proceed immediately from $\mathbb{N}$, the presumed theme of all mathematics from the constructive point of view. "Arithmetic" may be a word for the designation of the intuitive-constructive base that all mathematics ultimately refers to, and from this viewpoint the notion of the algorithm becomes the decisive notion of arithmetic. Or else arithmetic concerns an interest in the qualitative distribution of integers and for their related operational configurations, which generally ushers us into algebra. Arithmetic thus appears to be linked in two ways: on one side to discrete constructive mathematics, on the other to modern algebra. Research on Fermat's theorem brilliantly underscores the second link. And I recall my teacher Claude Chevalley saying that algebra as a whole was a lemma for proving Fermat.

A few words are in order here in response to the characteristic aggravation with which mathematicians react to these sorts of considerations. They state that it is of no importance to reach an agreement on problems of classification and definition of the major "branch names". One of these mathematicians once said to me: here I am considered a geometer, there a topologist, elsewhere an analyst, but

as this has no incidence on my work it is unimportant. It may in fact be the case that these labels are devoid of operational value. It is notably certain that mathematicians may put any instrument to work from out of the laboratory of any subdiscipline, and do not in fact hesitate to do so in the context of Bourbakian inter-theoreticity. Is that tantamount to concluding that branch identities are no longer subject to questioning? I have serious doubts. The enlargement of the meaning that geometry has experienced since the nineteenth century has for instance clearly functioned as a conquest from which mathematicians have profited: none have scruples over introducing, each time they wish to, their procedures as "the introduction of geometric considerations", referring to the new identity of geometry, to one or another aspect of what today is classed as geometry but which never would have "before". Mathematicians themselves use branch classification in order to measure what is happening in their field, as an instrument of evaluation of research events. This can be done providing that the identities which stand behind branch names are important, that is "can be called into question". Conversely, it may be held that one of the stakes of mathematical development is the increasingly in-depth understanding of branch identities. This is moreover one of the titles under which my work published in 1991 established mathematics as a thinking discipline, as "hermeneutics", concerned with enigmas of various levels.

To return to mathematical analysis, we would also like to see what light historical knowledge might shed on what has been understood as "analysis" throughout history. From this vantage, it does seem that the word "analysis" and its corresponding adjective "analytic" first meant something quite closely attached to what today is understood as "algebra", unless these words designated literal calculus in general. Viète's *ars analytica* is algebraic calculus, literal symbolism with its procedures. When "analytic geometry" becomes the standard designation for coordinate geometry *à la* Descartes, the adjective once again denotes the symbolic level of numeric-literal calculuses, here opposed to that of spatial intuitions. This notion of analysis seems to me closely connected philosophically to the sememe decomposition. Literal calculus is based on the discrete character of the units of language, and the forms gathered within it are gathered on the basis of this presupposed analysis which offers the simple constituents. The numeric coding of geometry likewise appears as a reduction of the spatial-continuous synthetic nature of figures, to those perfectly individualised and mutually distinct determinations that numbers are. Even if $\mathbb{R}^n$ is, following modern discourse, an interpretation of the continuum, the critical vantage sees in this construction a set of ideally distinguishable points, which can be manipulated as independent particulars. This is an insult to the profound intuition of the solidarity of the continuum with itself, ruling out any autonomization.

Thus would we naturally retain the idea that analysis is the theory of the local, a theory whose intention aims at nameable and separable identities in a place.

This description would go to explain the large acceptation which assigns to analysis everything essentially turning on topological structures, and also the limited acceptation, which only assigns to analysis that which deals with the study of numericity and its functional complications in the framework of a topological questioning. This description would also be coherent with the originary given of an analysis whose meaning is equivalent to that of algebra, and with the specialization of the adjective "analytic" to the evocation of the coding of space, or more generally with the use of "analytic" to designate any numerico-formulary explication.

And therefore contemporary mathematical analysis would have no relationship to the hermeneutical part of analysis, if I may use this expression: I am referring to the part I began to situate in my commentary on the Greek method of analysis, or of the activity of analysis identified by Kant as proper to philosophic procedure. There would be no relationship between the fact that analysis—considered from the vantage of that branch of contemporary mathematics—accomplishes and/or presupposes the hermeneutics of the continuum and the fact that a particular logical type of analysis as method—explication, regression, or other types—be affinitive to the hermeneutical spirit.

Unless an attempt was made to think the homology of everything that has been said to this point, necessarily at a more radical level. We will begin by stating that there is a relationship between the theme of the continuum and, for instance, the regressive nature of reasoning attesting the property of a limit, of which an example was given above. Why is this reasoning regressive in exactly the way it is? Because we are in a problematic of "control": the continuum, here carried to the power of itself through the taking into account of a function, calls into play an excessively infinite profusion of information; thought then adapts itself to this excessive situation by concentrating on regions and by reflecting on how one aspect of the local information allows it to be controlled by another. To know what is in excess is to assign determinations to it, is to analyse it and to understand the analysing determinations themselves in their mutual relations. Regression responds to the metaphysical pragmatics of willing: an analysing determination of the continuum is a willing, my knowledge of what is in excess is will, to such an extent that the systematic thought of these determinations is no longer the progressive thought of consequences—a thought that would be adequate to the idea that the determinations reflect what is, and that what is "has" consequences, to be taken up in turn in new determinations derived from the originals—but rather the regressive thought through which the excess becomes known as I acquire the understanding of what I wanted in it, in terms of what I should have or could have desired implying the already desired or what is assumed desired.

At the very least such an image of the mathematics of the continuum makes sense, providing that it be corrected and relativised as is required: of course entire

segments of the reasoning in what is normally called "analysis" today is of another type, taxonomic, algebraic, calculative, etc., of course excess is in fact involved in practically all mathematical procedure, at least in the figure of the so-called potential infinite of indefinite enumeration. Therefore the trope of analysis here can justifiably intervene. But is that a reason for denying that the branch of analysis has a privilege with respect to logical procedures inspired by the idea of control? Is this not what J. Dieudonné suggests in formulating his famous adage "increase, decrease, approach" in the preface to his treatise on *Infinitesimal calculus* (1969, 9) (thus in the form of a "maxim for mathematical analysis")?

But with this we have still to reach the hermeneutical element itself. Is there a profound link between thought that decomposes and regresses and the project of interpretation of what is the "stance of the question" presented as such by the tradition? We would like to succeed in thinking this technique as already interpretative in a minimal but radical sense of the term. Analysing what needs to be analysed, that is what is itself enveloped, strictly speaking I am not calculating or thematising. I am not calculating, for calculus presupposes the dis-implication of the individuals that it acts upon, and thus cannot be the operation accomplishing this dis-implication. Neither do I thematise, for thematisation presupposes the subject of enunciation, whereas the situation requiring analysis is not a situation wherein such a subject is available, and it is rather the result of the analytical act to have themes appearing: analysis operates on an envelopment but otherwise than on a predicate. Likewise, something like the procedure of logical regression eliminates the notion of calculus: on the one hand, the simple fact of being on the logical plane keeps us under the dependence of phrases as concerns truth, whereas calculus is originally and once and for all a manipulation of the etymological elements of calculus, that is "pebbles", thus a treatment of objects (and that, in the modern context, phrastic connecting can be considered as calculus or algorithmic modalities as texts in logical theories does not seem in my view to change anything of importance in this difference, which is principled, and moreover these "transgressive" interpretations rely on it); on the other hand, calculus re-elaborates the objective material that it works upon in an essentially progressive fashion; in principle it is a question of reaching another arrangement and not to reach behind the arrangement facing the mathematician (although this intention is possible, it is yet symptomatic of the type of relation to symbolic objects that we are here calling analysis). Logical regression does not mesh well either with the apophantic declaration of the object's determinations: this declaration is presupposed by all logics, there would be no logical connections, thinkable or to be thought, if determinations had not already been assigned to objects, in order to generate phrases. Logical regression is moreover associated—at the onset (Kant A, 331; B, 387-388) of the transcendental dialectic—with the movement that Kant calls "prosyllogistic", consisting in the search for an attribution of the determinations that condition the one already given, the new attribution remaining suspended as for its truth, known only as the condition of the first attribution: this is as much as saying that regression denounces the apophantic act by linking it to a suspensive condition.

To decompose and to regress are however actual operations belonging to the field and to the traditional method of interpretation: as was set out above, to decompose into a number of sememes is the most classic of acts in the explication of lexical contents—the interpretation of texts consists notably in this explication, which in truth is the fundamental operation therein. The interpretative tension results from the fact that on the one hand the analysis depends on the situation and the context, on the other that it is never complete and certain, for "in fact" meaning is not additive, but rather enveloped or affecting, it has its being in a restraint or a transition which is repugnant to the analytical hunt. Logical regression is also an operation of interpretation: the envelopment of meaning, if it is thought at phrase level, is restored as the complete group of phrases implying the given phrase. The field of consequences of a phrase is readily considered the attestation of the opening of its meaning. But this development is in fact the incremental effectuation of meaning, as is well known despite the logical aporia in which deduction would be either tautology or loss of information. On the "textual" plane—unquestionably the pertinent plane for all questions of meaning—the list of axioms of ZFC, for example, does not mean the opened infinite totality of mathematical theorems. However, all elucidation of the logical preconditions of a logico-linguistic situation is always valid as the explication of its meaning. The theories of presupposition in linguistics have highlighted this point.

The conclusion may then be drawn that the modern unity of analysis may be understood in light of the congruence between the hermeneutical situation of analysis—as a theory of the continuum it is linked to the ageless question "what is the continuum?"—and a certain discursive technique that could be called "analysis" which Greek methodological reflection and Kantian thought of a demarcation between mathematics and philosophy have differently described and defined. "Analysis" would essentially be the name of the relation to what is in general enveloped in itself, and this relation is necessarily, in the same stroke, one of decomposition and interpretative explication. The strange doubling produced in the case of mathematical analysis is that the "stance of the question", that which is enveloped in itself, is but the presentative concept of, as it were, envelopment as such (the continuum). Thus the analysis of the continuum is so to speak a double analysis: it is the analysis of the envelopment of the meaning of the enigma of the continuum, but also of the continuum itself as a presented coherence. This doubling also means that the analysis is part and parcel of an interpretation of the continuum and simultaneously, its representative display. In other words, the move of analysis

explicates the continuum while at the same time symbolically repeating its presentation.

At this point of our reflection, we may return to the Greek geometric analysis that was characterised, at the beginning of this paper, as a procedure moving from the logical precomprehension of an object to its practical comprehension, its constructive effectuation. Analysis in this sense is clearly the name of a hermeneutical rhythm lodged in the totality of contemporary mathematics, which is throughout the anticipation of objects such that their structure is given through logical stipulations. This anticipation furnishes a relation to what I have called elsewhere "correlative objectivity". But it is always assumed that within this objectivity there will be realisable, presentable objects, participating in what I have called, rightfully so, "constructive objectivity". Present day mathematics never ceases, repeating the way of Greek analysis, to determine, in the objectivity obtained on the correlative way, the constructive objectivity that may be recovered, or to think the excess of correlative objectivity over constructive objectivity, by any and all technical means. This is the level at which mathematics as a whole becomes hermeneutical as analysis, and this level must be distinguished from the position and the task of analysis according to Dieudonné, which the preceding paragraph was an attempt to examine and comprehend.

University of Lille III
Department of Philosophy

Notes

1 Cf. the note on page 1322 of Robin's French edition of Plato's dialogues: Plato (OC).

2 I quote from Aristotle (WMK).

3 I quote from Kant (CS).

RICHARD TIESZEN

SCIENCE WITHIN REASON: IS THERE A CRISIS OF THE MODERN SCIENCES?*

I Introduction

In this paper I shall discuss and defend a position on the nature of scientific reason with a view to shedding light on the question of whether there are fundamental crises in the modern sciences. I shall argue that, broadly speaking, it is possible to distinguish science within reason from science without reason. I claim that one source of the view that there is a crisis of the modern sciences stems from the historically recent possibility of practicing science without reason. The phenomena I discuss can be found across the spectrum of the sciences, from mathematics to social science. I invite the reader to think about the argument in connection with his or her favorite science. I will not attempt to discuss details about specific sciences but I will make several remarks about how the argument should be understood in connection with mathematics.

As I see it, my concern here is related to the analytic-synthetic distinction in the following way. According to a central tradition in philosophy, analytic truths are truths of (pure) reason. According to this same tradition, reason is distinct from intuition or observation. I would like to align this view with the idea that analytic truths are true by virtue of meaning alone (which is not, for example, to say that they are true by virtue of form alone). On the other hand, synthetic scientific truths involve reason but they are not truths of pure reason. They are instead to be viewed as truths with respect to which reason is conditioned by experience or intuition. I will also say that they are truths in which the "meaning" (*Sinn*) under which we think objects is conditioned by evidence. It is natural to require, in particular, that there be evidence for existence claims in order to say that it is "known" that those existence claims are true. One might hold, under this condition, that knowledge of the truth of existence claims is synthetic. If we can keep the analytic-synthetic distinction at all, then perhaps it can be kept in this guise. I shall suggest below how this view can be developed, and I will link it to the broader issues with which I shall be concerned throughout the paper.

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 243–259.

II Rationality, Intentionality and Everyday Experience

I will not attempt to present a theory of reason in this paper. I only wish to note that the idea that human inquiry can be informed to a greater or lesser extent by reason has a long tradition in philosophy. In Aristotle's *Posterior Analytics*, for example, one finds the idea that mere observation does not suffice for scientific knowledge, for it gives us mere collections of "facts", without any order, coherence, or purpose. Philosophers like Aristotle and Kant hold that what reason brings to the data of sense experience is unity, a kind of universality, and purpose. I will to some extent follow this classical line, but first I will focus on what I think is a key feature of human reason: intentionality. It is difficult to deny that human reason exhibits intentionality. I will explain this claim, and then draw some consequences from it.

Some basic structural features of the intentionality of human reason can be captured in figure 1.

We can say that a person is directed toward a particular domain of investigation consisting of objects and/or states of affairs by virtue of the contents or "meanings" of her acts of reason. These acts of reason may be of different types, *e.g.*, believing, knowing, remembering, etc. What they have in common is their "directedness" by way of their content. The notion of content can also be thought of as the "meaning" associated with the act, in that we simply take it to be the meaning of the expression that is substituted for S in the diagram. Once a particular expression is substituted for S, a person will automatically be directed toward a particular domain of investigation in a more or less determinate way. Content plays an important role in the objective, non-arbitrary categorization and identification of objects, and in the description and explanation of change. It should be noted that the diagram picks out structural features of the intentionality of reason. The actual contents substituted for S may to some extent be bound to particular times, places or cultures.

The object or state of affairs toward which one is directed in an act of reason is placed in brackets in the diagram because it is essential to the notion of intentionality that human subjects may be "directed" toward objects even if those objects fail to exist, or if they are not completely or properly understood. The logical counterpart to the possibility of nonexistence of the object is found in the failure of existential generalization in the context of verbs of propositional attitude.

Consider an example of everyday experience of the type that motivates the idea of "bracketing" the object. Suppose it is your intention to clean the attic of a house. To reach the attic, you must crawl through a small trap door in the ceiling. As you begin to do this, you find yourself face to face with a large, furry, dangerous-looking spider. As a consequence, you back out of the trap door in order to consider your next move. After some time has elapsed, you again approach the

$$\text{Person } P\ \overbrace{\text{believes}}^{\text{act type}}\ \overbrace{\text{that } S}^{\text{act content or } \textit{meaning}}$$

$$\Big|$$

refers to
(is directed toward)

$$\downarrow$$

[object or state of affairs]

Figure 1

trap door, this time with a small net to snag the spider should it be necessary to do so. This time, however, you see that it was in reality not a spider that you saw, but a tangle of dark thread, shaped in a way that looks like a spider.

At the earlier stage of your experience, say t_1, you saw a spider and acted accordingly. At some later stage, t_k, you saw that what you took to be a spider is actually a tangle of thread. At t_1 you saw the object under the content or meaning of "spider", but at t_k the meaning under which you see the object shifts to "tangle of dark thread", and this shift is brought on by your further experience with the environment in which you are situated. You make a correction or adjustment of your belief in light of further experience.

What you take to be the object of your belief will be the thing that stabilizes in your experience, that is, the thing that remains invariant through your different experiences with it. It will be the thing to which you (and others) can return over and over again, and which remains the same through these different acts. What the object is taken to be, however, will always be a function of the sequences of acts carried out thus far. The future could bring further adjustments or even surprises. In the worst case, there just might not be an object. The "bracketing" in the diagram is meant to indicate this conditional nature of knowledge of the object.

Suppose that at some further stage of your experience, t_n, you come to see that it was actually not a tangle of thread you were experiencing but a small, shredded piece of black cloth. It is possible that this could happen, but the phenomenon of persistent misperception is atypical. Our experience usually settles down into a stable state in various ways. We do not persistently misperceive objects. Or, to put it another way, consider the conditions under which we persistently misperceived objects. This is the kind of situation in which we might begin to raise questions about our sanity, especially if it should constantly turn out that there are no objects (*i.e.*, that objects are hallucinated).[1] What kinds of expectations could we have in circumstances like this? What could we hope to predict or control in our experience? It is precisely in these situations that we begin to apply the notion of irrationality. This shows us how a form of irrationality (or the absence of reason) is associated with the absence of stabilized objects in our experience. Reason and a form of "objectivity" appear to be mutual conditions for one another. It appears that there can be unity on the side of the subject if and only if there is unity on the side of the object. To be more precise, we might say that reason and the possibility of objectivity mutually condition one another. The example suggests that objectivity, at least as a regulative ideal (in a Kantian sense), is a condition for rationality.

The description in this simple example has the following consequences. First, it shows that we need to be fallibilists about knowledge, or at least about knowledge that depends on sensory input. The picture we have presented precludes certain "absolutist" or "foundationalist" claims about our knowledge of objects of experience. We might find, in further experience, that we were under illusions at earlier stages of our experience. Fallibilism, however, need not imply complete skepticism. It is common for our experience in many domains to settle into a fixed state which serves us well for practical purposes. In any case, we do not doubt everything we believe in situations like this, as is shown by actual experience.

Second, the example shows that there is a kind of continuity through the stages in our experience of objects. The different stages are not radically discontinuous with one another, as if there were no connections between the stages. Indeed, if this were so there would be no possibility of making corrections in the experience we portrayed. A particular kind of continuity is, in other words, a condition for the possibility of identifying misperceptions and illusions.

This point is closely related to a third consequence we can draw from the example: there is a progressive character to the experience. It can be claimed that some progress has been made as the person proceeds through the stages of experience envisioned in the example, in contradistinction to the claim that there is merely change from one stage to the next. "Mere change" suggests discontinuity or incommensurability between the stages, as if at the various stages we had discrete, independent, atomic units of information. It would be as if there were no memory (or history) from one stage to the next, as if nothing about a past stage could be contained in the present stage. The example shows, on the contrary, that there is a kind of cumulativity to the experience. At least some of the content that was present at the earlier stage must be present at the later stage if there is to be a correction in the experience.

I am not arguing that later stages in the temporal sequence always represent progress over earlier stages. The point is rather that there has to be an ongoing stability of the object and a development of further sequences of acts with respect to the particular domain. This is what makes future-oriented thinking possible, and helps to fix our expectations. It leads to the possibility of prediction and control that would otherwise not be present. The notions of "progress" and "correction" here are not absolute. What is judged to be progress or correction is itself relative to what is given in the sequences up to a particular stage in time. In other words, the example shows that it is possible to avoid commitment to an absolutist notion of progress without redounding to the view that there is mere change from stage to stage.

These considerations have direct implications for issues about relativism. Let us say that, by definition, "evidence" is acquired in the sequences of acts in time that we pictured. "Strong" relativism may be defined as the view that there is no evidence that will help us to choose between rival (sets of) propositions. Note that in our example "There is a spider behind the trap door", believed at t_1, and "There is a tangle of dark thread behind the trap door", believed at t_k, are rival propositions. Now is it really true, in our example, that there is no evidence that will help us to choose between the propositions? This seems to be patently false. First, it seems that in the kind of case we are considering we do not typically "choose" what we want to believe. We are forced to some extent to change the content of our belief by conditions in the environment. This often happens automatically and without any deliberation of the type associated with choice. We cannot just as readily believe at t_k that the object is a spider as we can that the object is a tangle of dark thread, as if this were like flipping a coin. It would be absurd to think that we could actually do this sort of thing in our experience. Our experience does not work this way, and it is not clear how it could work this way. We would not get on in the world and behave as we do were strong relativism true. The upshot is that by embedding our rival propositions in the kind of intentional contexts that make up our actual experience, we see that strong relativism is baseless. The example suggests that strong relativism is a philosopher's abstraction that has nothing to do with actual experience.

Our position may, however, be compatible with forms of weak relativism. This follows from the fact that what we know at a given stage is "relative" to the sequences we have carried out up to that stage, along with the fact that we typically do not know everything we could know in these sequences. The future could hold surprises, or we may have to make various adjustments and corrections. This kind of epistemic relativity holds at various levels for the individual perceiver, groups of perceivers, cultures, and for historical periods. Following Edmund Husserl, we could say that truth for us at a given stage is always "truth within its horizons" (Husserl 1929, section 105). It is compatible with this view, however, to distinguish truth or objectivity within its horizons from truth or objectivity as it is. Indeed, the latter idea appears to operate as a regulative ideal in the kind of example we have considered. It is by virtue of possessing this ideal that we realize that our knowledge at a particular stage is imperfect and can be improved. We really do think we are coming to know more about the object. I have no objection to the claim that the notion of a perfect identity through difference (in the case of either the object or subject) is ultimately to be understood as a norm. Similarly, the idea of perfect truth can be understood as a norm. If the notion of intentionality is accepted then norms are part of the package. Thus, our weak epistemic relativism is qualified by a kind of objectivism.

As I said earlier, it appears that reason and the phenomenon of reference to "objects" require one another. On my view, we must think of the content or meaning of an act of reasoning as having a regulative function. It directs us toward a domain of objects or states of affairs which we can then proceed to investigate in sequences of acts in an effort to fill in our knowledge. Reason thus carries within itself at least an ideal of "objectivity" in this sense, and this ideal has a regulative function in our experience. In other words, if reason exhibits intentionality then it also exhibits referentiality. As our diagram indicates, we are directed toward or referred to objects in acts of reason. It is not trivial to note this fact about referentiality, for I will later contrast the referentiality of reason with what I will call "relational" views of scientific thinking.

III Scientific Rationality

Scientific rationality, it seems to me, is founded on and has its origins in the kind of everyday use of reason we considered in our example (Husserl 1936). The example provides a sensible description of how some elements of human experience actually work. Scientific reason is just an extension and development of the use of reason that we see in everyday contexts. In this section I would like to briefly indicate some elements of this extension and development.

We can carry the model of the intentionality of reason over directly to the case of scientific reason. Of course scientific reasoning is more systematic, deliberate and reflective, and we may need to distinguish between direct and indirect evidence, and so on. Scientific theories are just sets of propositions that are believed, as in our diagram, except that they are often believed by groups of people. Groups of people come to see problems under the same contents or meanings and pursue their research accordingly. They are directed or referred to domains of investigation in this way. There will just be different acts, contents and objects in different sciences. Scientists are in the business of finding regularities in these domains, of finding identities through difference. Groups of people could be under illusions about what they are doing, and are susceptible to misperception. They may need to make corrections as research proceeds, and so on. In other words, we are simply dealing here with group intentionality.

I am arguing that our experience in science is founded on everyday experience, and that the various consequences we have noted above will therefore also apply in the case of scientific rationality. To deny this is to hold that scientific

rationality and everyday rationality are disanalogous in the relevant respects, but I see no grounds for such a claim. It could not be the case that one exhibits intentionality and the other does not. Suppose both exhibit intentionality. Then it could not be the case that one exhibits continuity and the other does not, that one exhibits cumulativity and the other does not, and so on. We should therefore be able to say that scientific reason exhibits fallibility, continuity, cumulativity, a particular form of progress, and a weak relativism tempered by a kind of regulative objectivism. Much more could be said by way of defending and developing these claims, but it seems that we cannot give up the basic ideas involved in them without also rejecting what appears to be the sensible and innocuous picture presented in our example.

We can also note that it will be all the better to make corrections and to more closely approximate objectivity if as many voices as possible are heard. The perceptions of specific groups of people can be corrected on this basis. Corroboration is generally important in matters of knowledge, but it seems that in the pursuit of objectivity, rationality demands pluralism about who P in our schema could be. True identities will be those that stand out through multiplicities of persons, places and times. They are multi-cultural. They transcend differences in gender. This view of reason and "objectivity" implies that we should maximize difference in order to obtain true identities. To put it another way, it is not reasonable to monopolize reason. This is also not to say that it is always unreasonable to place some constraints on who or what P could be.

Perhaps there are some principles about which we do not have to be weak relativists. For example, the principle of noncontradiction may be a boundary condition on scientific rationality, in the sense that there is no S for which we can have $S \wedge \neg S$ at a given stage of our experience. We might be able to hold that this principle is necessary, relative to our condition on scientific rationality. We can of course have S at one stage and $\neg S$ at another stage. On the other hand, we can have $S \vee \neg S$ at a stage for a particular S. The idea that $S \vee \neg S$ holds for all S at a stage, however, seems to represent the regulative ideal of the decidability of all questions that permit of "yes" or "no" answers. We might take it to represent truth at the limit of our research.

Truth or objectivity, understood as a regulative ideal, is arguably what motivates the rationalistic optimism about problem solving that characterizes the scientific spirit. Consider for a moment the notion of a scientist who is pessimistic about solving any scientific problem. Perhaps no one has expressed this rationalistic optimism better than David Hilbert. As Hilbert puts it, mathematicians are convinced that every mathematical problem is solvable:

> "In fact one of the principal attractions of tackling a mathematical problem is that we always hear this cry within us: there is the problem, find the answer; you can find it just by thinking, for there is no 'ignorabimus' in mathematics."(Hilbert, 1926, 200)

Problems in some sciences certainly cannot be solved by thinking alone, but Hilbert has nonetheless captured something essential to the scientific spirit here.

What I would like to focus on at the moment is the fact that, as a founded structure, science depends upon a variety of additional developments. Everyday reasoning is, for example, typically informal. The content of our everyday acts in the lifeworld does not involve much by way of formal, structural or mathematical elements. We do not spring from the womb thinking in mathematical formulas. We learn these things later, if at all. We can and do separate the formal or structural elements from the content of our acts as we engage in higher cognitive tasks. It is exactly these formal, structural, mathematical and technical elements that are involved in many varieties of scientific thinking.

Some features of formal or mathematical thinking are especially striking. To take a simple example, consider the following possibility. Suppose I give you a particular rule for computing a number, along with some initial values. Here is the rule:

$$P(B|A) = \frac{P(A|B) \cdot P(B)}{P(A)}$$

The values for $P(A|B)$, $P(B)$ and $P(A)$ will be supplied and they will always fall between 1 and 0. It is your task to compute $P(B|A)$. For example, let $P(A|B) = .33$, $P(B) = .75$ and $P(A) = .25$. You will simply plug these values into the formula, compute, and give me the output. It is clear that you can perform this task without knowing anything about what $P(B|A)$ is, what the numbers represent, what the rule is, where it came from, what the purpose of this task is, and so on. I will call this "relational" thinking.[2] This simple procedure might form only a small part of a very large procedure, consisting of many input values and many rules, in which one obtains some output at the end of the procedure.[3] One could operate, or could imagine operating in a vast environment of this type.

There are many different kinds of examples of relational thinking and its use in the modern sciences. What is characteristic of relational thinking in science is that formulas or symbols are related to other formulas or symbols on the basis of sets of rules, and there is no need to reflect on or to understand the meaning of the formulas or symbols.[4] There have been especially striking examples of relational thinking in the sciences since the rise of formalism and its development into the newer forms of mechanism that are part of computer science. The very idea of computation, which is so dominant in our age, is characterized in terms of formal manipulations of finite sign-configurations on the basis of finite sets of rules which take us from input to output. What makes it generally possible to do scientific work in this formal, relational way is the rise of formalization, mechanization, technization and a practical instrumentalism. These trends have been accompanied by a greater division of labor in and professionalization of the sciences.

The formal, mathematical and technical activities that make up what I am calling relational thinking are rigorous, precise, exact. Rigor and exactness are old and venerable goals of science, and with them we obtain a kind of clarity and distinctness we would otherwise not possess in our knowledge. In fact, it is not difficult to see how one might come to believe that only rigorous and exact technical work could count as science, or could count as giving us genuine knowledge. If one begins to take this very seriously, then everything else that seems to be a part of science or scientific knowledge, more broadly construed, will come to be seen as just a prelude to the real thing. That is, it will be a goal of science to bring everything into this rigorous, exact, technical form if it is to count as genuine knowledge. What is informal, in any context, may then come to be viewed as unreliable. One can see this attitude, for example, in the work of Frege, Hilbert and Tarski. One might come to think that informal reasoning must always involve chance-like guesses or "intuition". Here we have the seeds of a particular form of reductionism that may come to be coupled with eliminativism. It might be argued that whatever is not in this form at a particular stage cannot count as knowledge. Eliminativism goes even farther. Once a science is regimented in this form, why not shed the informal, fuzzy reasoning that led to it? For the hard-nosed scientist of this kind, the notion of something like "informal rigor" would be an oxymoron. It would follow, on this view, that to really know anything you must be a technician. I will use the term "scientism" for the view that only the formal, exact, technical part of our relational thinking can count as genuine knowledge.[5]

IV The Analytic-Synthetic Distinction

In a relationalist climate it would be natural for analyticity to be thought of in terms of form (or formal logic) alone, as if we should understand reason itself in purely formal or relationalist terms. The idea, put bluntly, is that there are only symbols and there is no real content or meaning toward which we might be directed. Meaning or content drops away. In particular, one might think this is true in mathematics. Kurt Gödel has noted two different concepts of analyticity that are relevant to this point. Analyticity (of proposition), he says, can be defined in the "purely formal sense":

> "[...] the terms occurring [in an analytic proposition] can be defined (either explicitly or by rules for eliminating them from sentences containing them) in such a way that the axioms and theorems become special cases of the law of identity and disprovable propositions become negations of this law." (Gödel 1944, 150)

In a second sense, a proposition may be called analytic if it

> " [...] holds 'owing to the meaning of the concepts occurring in it', where this meaning may perhaps be undefinable (*i.e.*, irreducible to anything more fundamental)." (*ibid.*, 151)

This second definition of analyticity appears to be much broader than the traditional Kantian definition. Indeed, much of mathematics would appear to be analytic on this definition.[6] It might be possible to explicate this wider notion in terms of our diagram of intentionality. We take meaning to be specified in terms of our notion of the content (or meaning) of our acts. Analytic truths will be truths in which we can proceed from content to content without mediation by experience of the objects the contents are about. Analytic reasoning is reasoning without intuition of these objects. This would, however, require reflection on or intuition of meaning:

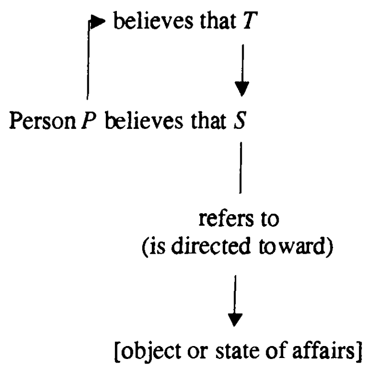


Figure 2

We are now directed toward meanings, not just meaningless formulas or symbols. This notion of analyticity requires that meaning itself be analyzable. It requires the notion of informal rigor. In attempting to clarify our understanding of meaning in acts of reflection we typically turn to the concepts of S in order to clarify them. We may then need, upon reflection, to further clarify the concepts used in that effort at clarification, and so on. This is arguably how some parts of our knowledge are developed. Gödel, for example, thinks that we need to analyze the meaning of the general concept of set more deeply in order to solve open problems in set theory, like the continuum problem.

It could not be the case, on this view, that only logic is analytic and that all of mathematics is synthetic. Instead, what would now distinguish logic from mathematics is the fact that logic is content-(or topic-)neutral while mathematics is not. We would need to distinguish different meaning categories in mathematics, in addition to recognizing a form-content distinction.

As I am construing this broad notion of analyticity, it will sometimes be possible to hold that content-to-content links are true or false without needing to have evidence for individual objects toward which we may directed by these contents. But it is exactly such an appeal to our "experience of objects" that is needed for synthetic truths. For synthetic truths, the meaning or intention under which we think of an object must be at least partially fulfilled. In other words, the meaning or intention needs to be conditioned by evidence for objects. I have argued elsewhere that we should understand the fulfillment of "mathematical" intentions in terms of the general notion of construction (as in constructive mathematics) (Tieszen 1989, 1995). It is when we possess constructions that we can be said to have evidence for the existence of the objects our mathematical intentions are about. It follows from these remarks that what is analytic in mathematics will be what is believed to be true (owing to the meaning of the terms involved) but which cannot (at least at present) be understood as constructive. Some parts of mathematics will, accordingly, be analytic (but not content-neutral) and some parts will be synthetic. Impredicative set theory, for example, might be construed as analytic in this sense.

I am somewhat skeptical about the idea that the analytic-synthetic distinction will be important in future philosophical and mathematical work. On the other hand, the issue of what the distinction amounts to seems to involve complications that are not yet understood very well. The view I have suggested may be worth exploring. It is obviously quite far from a relationalist understanding of analyticity.

V Crisis?

If only the formal, rigorous and exact scientific work of the relationalist kind is taken to constitute genuine science or knowledge then we approach a crisis state in science. It follows from what we have said about science within reason that scientism is a form of science without reason.[7] Why?

We are viewing scientific knowledge as a founded creation and in this founded creation we may have purely relational thinking. It is inherent in the formal, technical, structural or mathematical thinking we have called relational that we need not know what it is about. Formalism is often portrayed as a viewpoint according to which we are to abstract away from meaning or content. This also means that we can or even should forget about the origins of meaning in the lifeworld. In relation to our earlier model, if we abstract away from meaning and the informal deep background of the meanings in our experience, then the directedness of our acts drops away or shifts. As I said above, it does not need to be denied that we are, in a sense, "directed" in relational thinking. There is, nonetheless, a significant shift, and we are certainly not directed in the same way. The "objects" toward which we are now directed are symbols or formulas. We are not directed toward the objects the formulas are about in a particular context. We are not directed toward the objects to which the formulas are being applied. We are in a different environment. There may still be a regulative function in this context but now the goal or purpose has shifted. For example, the goal may be to simply obtain the output of a procedure given the input, quite independently of what the procedure is about. All of one's energies may then go to this end and it is possible to become submerged in this kind of work. There can be a complete displacement of concern. Consider, for example, how this has actually been used in various top-secret projects, such as the atom bomb project at Los Alamos. In this kind of situation there is a sufficient division of labor so that many people may work on a project while only a few actually know what it is about. Note that it is not the division of labor itself which makes this possible. Specialization does not by itself preclude the referential model. I am describing a particular alignment of specialization and relationalism. There are also other differences. The work of specialists in the humanities, for example, is not likely to have significant consequences for nature or the environment and is therefore unlike the work of specialists in the natural sciences.

The extent to which a referential or relational model is adopted determines the extent to which various skills and abilities are valued. In a worldview dominated by scientism it is more likely to be held that a person does not really know anything unless this knowledge takes a technical form. The skills and knowledge of technicians will be more highly valued, *e.g.*, the expert's knack for application of technique, or the ability to devise or acquire familiarity with relational systems. Understanding and discovery of the type associated with the referential model will be valued less than formal elegance and pragmatic success. Once goals are understood in a relationist way, it would be natural to see the rise of a kind of pragmatic instrumentalism about how to obtain such goals.

In short, there can be a shift to a very different account of reason, meaning, directedness, objects, knowing, and the like.[8] These concepts, at the level of relationalism, may in fact be reducible to mechanism by way of something like the Turing test. It might even be argued that Turing has captured a relationalist notion in his well known analysis of computation.[9] It is not clear at all, however, how we would have thereby captured the referential versions of these notions. On the relationalist view, we lose sight of (non-symbolic) objects or objectivity, and so we also lose the notion of evidence described above. The structure we pictured in our diagram changes considerably if we focus exclusively on the form or structure of S, in abstraction from content. What remains in place of S at a stage is a formula which we can try to relate to other formulas on the basis of existing or discoverable sets of formal rules.

I maintain that the following claims look all the more plausible from the viewpoint of relationalism: strong relativism, the claim that there is no progress in science, and the claim that different scientific theories are discontinuous and incommensurable. It is not difficult to see why. If objects drop out of our earlier diagram, even as regulative ideals, then we arrive at the position of strong relativism. There are only distinct or rival propositions at different stages of experience and we can make no appeal to the notion of evidence to motivate (justify?) the adoption of one over the other. It would be natural to hold that there is no notion of evidence which could motivate the adoption of one proposition over another. On the relationalist model, it will not be claimed that we are constrained or forced in some ways by our experience of objects. Once objects are out of the picture, it is easy to think that there is mere change from one stage to the next, for then we are only entitled to say that there are different propositions or formulas which need not have any apparent relation to one another. Even if we keep the content of the acts at the different stages of our example, we can ask what spiders have to do with tangles of dark thread. These concepts appear to be discontinuous, and the networks of propositions of which they respectively form parts are arguably incommensurable. Different propositions that appear at different stages might now look like (logically or semantically) independent and discrete units of information. On the purely relationalist picture, the propositions that appear at different stages of our experience will simply be different. There is difference without continuity. No connections between the formulas or even the meanings at the stages can be seen because it is not possible to recognize mediation through the experience of objects. On the referential model, there is difference with continuity.

I am not claiming that formal, mathematical and technical work is not important or not needed in science. Quite the contrary. Formal, mathematical and technical work is a necessary condition for science. It should also be apparent from the comments above that I am not claiming that mathematics is without reason. One can hold that formalization is very important without being a formalist. Similarly, it can be held that mathematization and technization are important without reverting to scientism. I do not think that mathematics is purely formal or relational. It does not exist only to serve the other sciences. It is also contentual. I would argue that the model of intentionality described above also applies to the (founded) science of mathematics. Thus, there are acts, contents and objects appropriate to mathematics. We are directed toward mathematical objects through the meanings of our mathematical acts. Mathematical objects are distinct from sensory objects, and there will clearly be some differences in the ways that we come to know about these kinds of objects. The meanings or contents have a regulative function in our mathematical experience. It is possible to become more reflective about these meanings, and more conscious and systematic about our understanding of them. Perhaps the motivation for reflection arises most clearly at the boundaries of the science of mathematics, where there are difficult open problems. Mathematics receives its meaning and direction through its own distinctive content, and not primarily through its applications in or its services to the other sciences. In order to know about the objects toward which we are directed, our acts have to be at least partially fulfilled. We can certainly say that science within reason involves relational thinking, but we do not need to hold that only this kind of thinking counts as knowledge or genuine science. It is a matter of balance.

If there have been excesses in the direction of scientism, then there have also been excesses in the direction of anti-scientism, to the extent of being anti-scientific. It has been suggested, for example, that this is the plight of Heidegger's work, and the suggestion could perhaps be extended to much of the post-modernist theory that has followed in Heidegger's wake.[10] After all, what has happened to the notion of reason in this work? The answer to this question is closely linked to what has happened to the notion of intentionality. The notion of intentionality or directedness has disappeared or been radically reinterpreted. It is supposed to be a virtue of Heidegger's position, for example, that the act-content-object model is undermined and replaced by appeals to practices and skills. There are no objects. (It is even a question whether there are any subjects.) Just as one can speak about propositions without objects on some of the views we have been considering, so one can speak about practices without objects. But if there are no objects, then at different stages we have only a motley of distinct or rival practices and we can make no appeal to the notion of evidence to motivate (justify?) the adoption of one practice over another. It would be natural to be of the opinion that there is no notion of evidence which could motivate the adoption of one practice over another. Once objects are out of the picture, it is easy to think that there is mere change between one stage and the next, for then we are only entitled to say that there are different practices which need not have any apparent relation to one another. These may appear to be discontinuous and incommensurable. Practices that appear at different stages of history or in different cultures will simply be different. There is difference without continuity. We can see no connections between the practices at the stages because, counter to the referential model, there is no mediation through the experience of objects. On the referential model, there is difference with continuity. Some post-modernist authors arguably embrace just such a notion of difference without continuity, or of difference without objectivity. In an interesting parallel with scientific relationalism, some post-modernist writers suggest that everything is symbolic, everything is a text. There are also other variations on this theme: everything is just a "language game", or there are just narratives without objects.[11]

Thus, I am arguing that strong relativistic claims about meaning, objectivity, progress, continuity and the like appear to be more plausible not only from the viewpoint of scientism, but from any viewpoint that rejects the notion of the intentionality of reason described in section II.

VI (Un-) Intentional Knots

In the account I have presented, scientific activity is taken to be founded on basic "lifeworld" activities of human beings. In the founded structure of modern science we create a viewpoint which we then turn upon various phenomena in the world. Suppose it is held that only the formal, rigorous, technical work that is part of relational thinking can count as genuine scientific knowledge. Suppose, in other words, that scientism is true. When we turn this viewpoint back around to ourselves it should come as no surprise that reason, meaning, and indeed consciousness itself disappear. We live in an age in which it is fashionable to talk about the disappearance of these things. We hear this talk everywhere. It is, for example, reflected in work in cognitive science, where the concepts of intelligence, thought, etc. are understood in a formal, mechanical, and computational way. There is nothing more to these phenomena. And to "know" anything about these phenomena one has to be a technician. Everything short of technical knowledge in this domain is relegated to "folk psychology".[12]

I am claiming that in all of this we are interpreting ourselves through a (founded) viewpoint that we have created. This viewpoint is itself an interpretation. It is not some neutral, theory-free, value-free, "correct" viewpoint. It is itself a "content" or "intention" under which groups perceive the world. Some investigators may then try to fulfill this intention. They may, for example, try to fulfill the intention according to which we are machines, or even the intention according to which there are no intentions. But is it possible to fulfill the intention according to which there are no intentions? If the analysis above is correct, then we cannot pretend to eliminate the semantic notion of an interpretation by appealing to the sciences. There are also reasons for believing that it is not necessary to interpret ourselves exclusively in this way. Perhaps there is no point of view prior to or superior to that of natural science, as is sometimes claimed in efforts to naturalize epistemology, but if the argument of this paper is correct then it also does not follow that an uncritical natural science can occupy a privileged position.

In the situation of the modern sciences that we have described there is a particular irony that borders on paradox: the extent to which we apply science without reason to ourselves is the extent to which we come to believe that reason is not intentional and, hence, that science is without reason.

VII Conclusion

The main argument of this paper can be summarized as follows: the use of reason in everyday experience exhibits intentionality. Scientific rationality exhibits intentionality but it is founded on everyday reasoning and is more complex and systematic. Some scientific thinking is relational. Many concepts may come to be thought of in a relationalist way, including the concept of analyticity. Now suppose, as in scientism, that only relational thinking in science can count as genuine science or knowledge, on the grounds that only this kind of thinking is rigorous, reliable and exact. It follows from the claim that reason exhibits intentionality that relational scientific thinking by itself, as in scientism, is without reason. The fact that it is possible to practice science without reason in this sense is one source of the view that there is a crisis of the modern sciences. Science within reason must involve relational thinking, but it cannot be held that only this kind of thinking counts as genuine knowledge or science.

San Jose State University
Department of Philosophy

Notes

* Versions of this paper were presented at a seminar on the nature of science at the University of Oslo and in the philosophy department colloquium at San Jose State University. I thank the members of both audiences for helpful comments, and especially Dagfinn Føllesdal and Alastair Hannay. Some of the work on this paper was supported by N.W.O. (Dutch National Science Foundation) Grant # 22-266. I thank Dirk van Dalen for sponsoring the grant. Finally, I am grateful to Nancy Tieszen for a number of important suggestions and comments.

1 Since we are on the matter of spiders, consider also whether or to what extent you would take the activity of dreaming about spiders to be rational.

2 The points I wish to make about what I call "relational" thinking are similar to some points made by, among others, Husserl (1935-1936), Cassirer (1923-1929), and more recently, O'Neill (1991).

3 Note to those for whom this rule is purely relational: this happens to be a very important rule. It is one of Bayes' rules for computing conditional probabilities.

4 It does not need to be denied that we are "directed" in relational thinking. We can say that we are directed, but it is now toward the formulas involved and toward obtaining the output from the given input. This, is not, however, the same thing as being directed toward the objects the formulas are about in a particular context (cf. section V).

5 I argue in unpublished work that scientism or relationalism is closely related to some viewpoints that Gödel criticizes in Gödel (1961) and other papers. Gödel can therefore be seen as making some similar points about science without reason. In particular, see his comments on the imbalance of "leftward" directions in philosophy and his objections to Hilbert's program and to Carnap's "syntactical" program. In addition, Hao Wang suggests that Gödel sympathizes with Husserl's claim that we must consider the origins of science in everyday experience (Wang 1987, 62, 122 and 239).

6 This idea has been explored to some extent, in relation to Quine and others, in Parsons (1995). It is worth noting that Bolzano also recognizes narrower and broader notions of analyticity (Bolzano W, sect. 148).

7 An immediate corollary is that scientific rationality, as described above, is not itself the source of crisis in the sciences. I note this consequence because there appear to be views on which it would be denied.

8 One fairly clear example of this can be found in Hilbert's conception of metamathematics. The objects toward which we are supposed to be directed in metamathematics are finite sign configurations. What is taken to be meaningful, reliable, and knowable in mathematics is to be understood on this basis. Hilbert then seems to construe properties like decidability in purely formal or mechanical terms, although some of his appeals to Kant's views about reason obscure elements of his conception of metamathematics. On the basis of what we have said above, it is not surprising that Hilbert's program has been interpreted as a form of instrumentalism.

9 See Turing (1936). I discuss some related ideas in section 5 of Tieszen (1994). Could there be a "referential" notion of computability? Such a notion would refer to what intentional systems do when they are computing *and* know what the computation is about.

10 It is on this kind of point that Husserl and Heidegger parted ways.

11 This view about narratives is arguably appropriate to literature and fiction, but it is not clear to me that it extends to other domains. See the section of Tieszen (1995) entitled "Against Fictionalism".

12 Compare, for example, the work of Dennett (1991) and Searle (1992) on consciousness.

MICHAEL OTTE AND MARCO PANZA

MATHEMATICS AS AN ACTIVITY AND THE ANALYTIC-SYNTHETIC DISTINCTION

I Intensional and Extensional Theories

Frequently, in modern discussions in philosophy of sciences, science—that is the object of the discussion—is intended as a class of (scientific) theories and a (scientific) theory is conceived as a linguistic system, or even as a class of propositions. Moreover, scientific theories (in this sense) are intended either as purely "intensional theories" or as purely "extensional theories".

By "intensional theory" (in the previous sense of the term "theory") we understand a theory that, as a set of postulates (or by means of a set of postulates), determines the intensions of its terms and in which (if you accept that there are extensions, in a proper sense) the extension of each term, that is its referential domain, is not only delimitated by its intension, but it is also constitued by it, as a sort of logical counterpart of it. The elements which belong to such an extension are not given independently of the theory, they are nothing but what the terms of the theory denote (if we accept that such terms are denotative terms). As Gödel says, "the existence of a class" depends "on the content or meaning" of the propositional functions (Gödel 1944, 132). Thus, an intensional theory is not really open with respect to the growth of knowledge and to the changes of our understanding of something that is not fully determined by the theory but exists outside of it.

In a proper sense it does not realize, as such, any form of knowledge or objective understanding; it is a closed domain, which provides no more than synoptic tables or something like that. Even if a some people have conceived empirical theories in purely intensional terms, the privileged model of an intensional theory is provided by a mathematical axiomatic theory, intended as a purely formal system. A classical example is provided by the Hilbertian axiomatic reconstruction of Euclidean geometry. Here, if the terms "straight line", "point" or "plane" are intended as denotative terms, they denote nothing but the arguments of the conditions expressed by the axioms. This idea was expressed by G. G. Granger by means of the notion of "formal content": if the terms of an intensional theory denote something, they denote formal contents (Granger 1982).

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 261–271.

Of course a lot of people have denied that the terms of an axiomatic Hilbertian theory (or of a formal theory in general) were denotative terms. They simply are, it is claimed, symbolic characters in a syntactical game or expressions of concepts without objects, as in the conceptualist account of mathematics (for example: Tharp 1989-1991). Even if in such cases the terms "intensional theory" could be misleading, we propose to maintain it, providing the term "intension" with a more general meaning than would be required in order to be able to speak of intensions as we have done up to now. We will come back to this point later. Let us pass now to the notion of "extensional theory".

By an "extensional theory" (in the previous sense of the term "theory") we understand a theory that speaks about something that is already given otherwise. The terms of such a theory have an intension as well as an extension, but neither the term "meaning" nor the terms "intension" and "extension" are understood in a way that would necessarily depend on the particular theory. Rather, the extensions are given by a sort of reality, intended as a system of things (acting upon the subject), and intensions are nothing but the means by which such things are introduced in the theory. Intensions seem to relate to extensions by grasping their "essential" characteristics in an unspecified manner.

The privileged model of an extensional theory is provided by a physical theory conceived as a realistic account of the external world. Nevertheless, a lot of people—the Platonists, as they are generally called—have advanced the idea of also interpreting mathematical theories as extensional theories. But in order to do so—without abandoning the idea that a mathematical theory is a formal theory—we have to accept something like an ideal reality that, in principle, is describable by means of a convenient set of definitions or axioms, expressing the "essential" characteristics of a domain of things (even if, purely formal things).

II Analytical and Synthetical Judgments

If we understand mathematics as a class of theories and these theories either as intensional theories or extensional theories, we are confronted with a number of difficulties when trying to make sense of the classical Kantian analytic-synthetic distinction. Let us consider this point in some detail.

By considering mathematics as a class of theories in the previous sense, many people have understood this distinction as primarily concerning the (logical) properties of mathematical propositions or the (logical) nature of their justification. As a consequence of such an understanding, the hard core of the Kantian thesis has been located in the assertion of the syntheticity and apriority of mathematical judgments, as explained according to the criterion advanced by Kant in the *Introduction* to the first *Critique*: a subject-predicate judgment is analytical if and only if the predicate does not assign to the subject any properties other than those that

it has to have in order to be just that subject, otherwise it is synthetic (Kant, A, 6; B, 10).

In order to apply such a criterion to the judgment "q is P", we have to understand the subject as something that is q and not simply as something that we call "q" : "q" is not a name here, but is already a way of specifying the nature of the subject itself. This is the reason why the examples that are generally presented to illustrate the Kantian criterion are not of the previous form, "x is F", but of the form "all G's are F", or, better, in the usual Fregean translation, "for all x, if x is G, then x is F". In this way, the subject-predicate judgment is interpreted as a way of connecting not really a subject to a predicate, but a predicate (that is G) to another predicate (that is F). Predicates play two different roles here. The first (that is G) specifies the domain to which a generic subject belongs (and in this way it specifies the subject, completely or partially) the second assigns to such a subject a certain property. It is only if a subject-predicate judgment is intended in such a way, that we can apply Kant's criterion: such a judgment will be analytic if and only if F expresses a sub-specification of the property expressed by G. The judgment "all congruent triangles are similar" is analytic—we could say—because the predicate "to be congruent" is a sub-specification of the predicate "to be similar" (for a triangle). But, here another presupposition is required. The properties expressed by our predicates have to realize a partial order with respect to a meta-relation of inclusion. And, in order to say that a certain judgment "is" analytical or synthetical, we have to assume that the configuration of such a partially ordered space of properties is fixed.

From such a point of view, to be something means (or has to be intended as) to satisfy a certain property and to satisfy a certain property implies that a certain set of other properties is met or fulfilled. Thus, the problem of analyticity or syntheticity of a judgment is the problem of connection between different properties : a mathematical judgment, as "all Q's are P", or "for all x, if x is Q, then x is P" would be synthetic if and only if it was logically possible to satisfy the property Q, without fulfilling the property P. But a mathematical judgment has to be proved in a mathematical theory (except if it is an axiom or a definition). So, a mathematical judgment would be synthetic only if it was possible to prove that to satisfy the property Q means to satisfy (among other) the property P, even if it is logically possible to meet the property Q, without meeting the property P.

The problem concerns, of course, the notion of "logically possible". In the previous context this notion refers to the partially ordered space of properties to which the properties P and Q belong. Such a possibility takes place if and only if the property Q does not include the property P. But how is the configuration of such a space fixed?

II.1 Mathematical Theories as Intensional Theories

If a mathematical theory is intended as an intensional theory, such a configuration can not be fixed outside or independently from the theory itself. Outside the theory there is properly speaking nothing concerning the theory itself. Therefore, if a judgment is a theorem (an axiom or a proposition) of the theory (that is, if it is a proposition of the theory and, thus, a mathematical judgment), it cannot be but analytic.

This seems immediately obvious, but we prefer to insist a little bit more on this point. In our characterization of an intensional theory we have not really specified what kind of things intensions are and this could cause problems in order to understand the point.

What then are intensions? With respect to our problem of deciding whether a judgment is analytic or synthetic, we need only answer this question up to relations of difference and equality of intensions (and in fact we can only answer it so). Using an informal language of sets and in particular interpreting equality as mutual inclusion of sets (and the latter in turn as logical implication) we realize immediately that, whatever intensions might be, in an intensional theory all statements are analytic, because they just state relationships of inclusion between intensions (interpreted as sets here). Therefore, the analytic-synthetic distinction makes no sense with respect to a mathematical theory intended as an intensional theory.

Perhaps it makes sense as a correlative distinction with respect to the other distinction between a mathematical judgment and an empirical one: all mathematical judgments being analytic, it could be possible that all empirical judgments are synthetic, because empirical theories are not intensional theories, as the terms of the theory cannot be complete descriptions of their referents. Otherwise for such a theory to have referents would equal its being true and *vice versa*. Now, in conceiving of (mathematical or empirical) theories as intensional theories, one negates a fundamental insight of Kant's *Critique*, namely that "no general description of existence is possible, which is perhaps the most valuable proposition that the *Critique* contains" (Peirce CP, 1.35). Thus this view amounts to denying the essential Kantian idea, namely that synthetic *a priori* judgments are possible and they take place in mathematics (even if not only there). Therefore, even if we could make sense of the Kantian distinction, with respect to mathematics (although not "within" mathematics[1]) it would fail its essential aim.

II.2 Mathematical Theories as Extensional Theories

It might appear that the situation changes essentially if we conceive mathematics as an extensional theory, but this is not really the case. For a long time it has generally been accepted that predicates bearing on empirical extensions could be connected analytically—providing logical truths, rather than genuine empirical judgments—as well as synthetically—providing genuine empirical judgment. But—as Quine has shown, in *Two dogmas of empiricism* (Quine 1953, 20-37)—even if this distinction can be maintained, from the point of view of an extensional theory, it does not express anything but our decisions on the internal organization of our language.

The arguments and conclusions of Quine are well-known and it is not necessary to repeat them. We would only like to insist on one point that seems to be connected with our problem concerning mathematical extensional theories. If we accept the Kantian criterion of the *Introduction* to the first *Critique*, as Quine does, essentially, we are compelled to assert, as we have seen, that a (true) subject-predicate judgment—let us say "all Q's are P"—is synthetic if it is not necessary to be P, in order to be Q, even if, contingently, all Q's are just P. Even though, it would seem to be a very natural situation from an extensional point of view, it is not.

Let us consider an example. We can argue, it is not necessary to weigh less that 200 pounds in order to be a swan, even if, contingently, all swans weigh less than 200 pounds. But, how are we sure that it is not necessary to weigh less than 200 pounds in order to be a swan? This is possible only if we have in our hands a precise and objective definition of what a swan is and if such a definition does not include that a swan weighs less than 200 pounds. Nevertheless, if we intend a swan as a "real external object", that is how it is independently from all possible definitions that we could give, it is possible only if our definition grasps what is "essential" in a swan, without specifying all properties of a "real swan", so that we can imagine genuine swans different from real ones, for example swans weighing 300 pounds, or even 30.000 pounds. But how do we know what is "essential" in a swan? has some God given the required definition? Certainly, in a proper sense, we cannot know it, we can only decide it. Thus, it is clear that the the analytic-synthetic distinction makes sense for an extensional theory (according to the criterion of the *Introduction* to the first *Critique*) only if the predicates are introduced into the theory by means of a definition which determines their logical range, according to a certain decision. This shows that an extensional theory—as well as an intensional one—depends on the choice of a perspective. A judgment like "all swans weigh less than 200 (or even 30.000) pounds" is then either analytic or synthetic, according not to the "objective extension" of the predicate "to be a swan", but to the perspective that has been chosen in fixing the logical range of such a predicate.

In order to make this point clearer, let us assume, provisionally, that properties are nothing but (names of) classes of objects. This is exactly the content of what is called generally the "axiom of extensionality" (Gödel 1944, 137):

$$\forall Q, P\{[Q = P] \Leftrightarrow \forall x[Q(x) \Leftrightarrow P(x)]\} \tag{1}$$

Once this axiom is given, let us consider two predicates G and F, such that $\neg(G \subseteq F)$. As G is then distinct from F, according to (1) these predicates satisfy the condition:

$$\exists x\{[G(x) \wedge \neg F(x)] \vee [F(x) \wedge \neg G(x)]\} \tag{2}$$

Let us consider now the domain of G and determine the range of the free variable x relatively to it, such that:

$$\forall x G(x) \tag{3}$$

As from (2) and (3) it follows

$$\neg \forall x[G(x) \Rightarrow F(x)] \tag{4}$$

we have,

$$\neg\{\neg[G \subseteq F] \wedge \forall x[G(x) \Rightarrow F(x)]\} \tag{5}$$

Thus, the judgment "all G's are F" cannot be synthetic, according to the criterion of the *Introduction* to the first *Critique*: the distinction between analytic and synthetic judgements like "all Q's are P" makes sense in an extensional theory, according to such a criterion, only if the space of the predicates occurring in it is partially ordered, independently from the partial order of the classes which constitute the extension of these predicates.

But, if so, how the partial order of the predicates is fixed? From an extensional point of view—different from an intensional one—we can try to answer in a number of ways, all of which do not provide however meaning for the Kantian distinction (according to the criterion of the *Introduction* to the first *Critique*).

First, we can imagine that it is an aim of our theory (or of a part of it, for example of the "meaning postulates", as Carnap proposed (Carnap 1952)) to provide the configuration of such a space. But if this is the case the distinction between analytical and synthetical judgments is nothing but an expression of the organization of the theory itself. Second, we can imagine that such a configuration is fixed once and for all, as if it were the configuration of the mind of God. In such a case, the "real" distinction between analytical and synthetical judgments rests on foundations unknown to us and our distinction is nothing but a conjectural representation of it[2]. In the first, as well as in the second case, it seems rather

arbitrary and open to points of view whether a statement is considered analytic or synthetic and the analytic-synthetic distinction does not lead to much.

But there is a third possibility: we can accept the Leibnizian idea according to which things are to be distinguished on basis of the sum of all their actual properties, so that it is not possible to be a Q, without having all the properties that a Q has. The configuration of the space of properties is then imposed by the real nature of things. All true judgments are analytical in this case.

Someone has imagined that, with respect to mathematical extensional theories, we are necessarily in such a case. From such a point of view formal theories, like mathematical theories, are in fact considered as meta-linguistic theories dealing with linguistic extensions, for which the "sum" of their actual properties is finite, and to be a (mathematical object) Q is exactly to have all these properties and only them. We can justify in such a way the neopositivistic thesis, according to which all mathematical judgments are analytic. Thus, the thesis of analyticity of mathematics can be defended by intending mathematical theories as intensional ones as well as by conceiving them as extensional theories. Whatever the choice may be, by accepting such a thesis one denies the essential content of Kant's thesis.

If we deny, in contrast, that mathematical extensional theories are meta-linguistic theories the situation for such theories is not really different from that for empirical extensional theories. Thus Quine's argument can be applied *mutatis mutandis*.

We may suppose that to be a certain formal thing is to satisfy certain properties $\{Q_i\}$ (that is, in a more convenient interpretation, certain conditions), expressed by certain definitions or axioms, in such a way that without any additional axiom it is not possible to prove that the fulfillment of these properties (or conditions) entails the satisfaction of certain other properties (or conditions) $\{P_i\}$. But we can introduce some additional axiom (and passing, for example, from absolute geometry to Euclidean geometry or from finitary arithmetic concerning numbers $\{0, 1, 2, ..., 100\}$ to infinitary usual arithmetic, or from an algebra without associativity for a certain operation to an algebra with associativity for that operation) and then prove that to satisfy the properties (or conditions) $\{Q_i\}$ entails the fulfillment of the properties (or conditions) $\{P_i\}$. We can interpret such a case in different ways and if our reasoning capabilities are strong enough, we may arrive at a justification of the syntheticity of a certain mathematical judgment. We can even interpret in this way the thesis of Poincaré according to which arithmetical infinitary judgments are synthetic (the additional axiom being the fifth axiom of Peano) or Cassirer's claim, according to which all the usual arithmetical judgments, like $n+m = v$, are synthetic (the additional axioms being the associative law of addition) (Poincaré 1894 and Cassirer 1907). But it is clear that there is no possibility to show that a certain mathematical judgment is, in such a framework, definitely synthetic. In order to make such a claim, we should justify that the real ideal things, of which the theory is speaking, are completely described by the first axioms only. And, we certainly cannot do that.

III Cassirer and Poincaré

But, of course, neither Poincaré nor Cassirer presented their theses exactly in such terms. Rather it seems that, when they state that arithmetical judgments are synthetic (and *a priori*) they do not refer to the Kantian criterion of the *Introduction* to the first *Critique*. But it is very difficult to say what their criteria for the distinction between the analytic and synthetic really are.

Cassirer (1907, 41) considers the proposition "7+5 = 12", quoted by Kant in the *Critique of Pure Reason*, to be synthetic, because its proof contains "a synthetic assumption", namely "the theorem that $a+(b+1) = (a+b)+1$". But, what Cassirer terms a synthetical assumption here is a special case of the associative law, which functions as a definition of the addition on the basis of the successor operation of ordinal numbers in the normal axiomatic characterization. Thus, even if we accept that the proposition "7+5 = 12" could be intended as a subject-predicate judgment, it would be very difficult to justify that it is possible to intend the subject of this proposition—that is the sum-number 7+5—without characterizing the operation of addition by means of the associative law or in a way that entails such a law. In case we characterize the operation of sum in terms of the cardinality of sets the situation is completely different. The associative law is in fact in such a case a consequence of our definition of addition (and not a part of it), and people could claim that such a consequence does not follow by a formal proof, but is to be observed by experience of concrete sets and their unions ; thus, it is nothing but a (quasi-empirical) generalization. If we accept that, we might conclude that the judgment expressing this law is synthetic, and the related proposition "7+5 = 12" as well. But the question is completely open to points of view. Thus, it is clear that, if the criterion of syntheticity of a judgment is that of the *Introduction* to the first *Critique*, Cassirer fails in asserting that "7+5 = 12" is definitely a synthetical judgment. This conclusion depends on our definition of addition and on our point of view with respect to the way in which the properties of the operations on sets are stated.

The same is true for Poincaré. Poincaré (like Hölder 1924) called (infinitary) arithmetic synthetic (and *a priori*) because arithmetical propositions—being founded on the axiom of recursion—are just expressions of the free activity of the human mind, they represent the structure of the subject itself. According to Poincaré, recursion cannot be reduced to the principle of contradiction: it is "the affirmation of a property of the mind itself" (Poincaré 1894, 12-13). The fifth axiom of Euclid in contrast is nothing but a "definition in disguise" (Poincaré 1891, 50), Poincaré believes, and it has been chosen only for reasons of our convenience. Thus, according to Poincaré arithmetical propositions are synthetic (and *a priori*) because they are founded on something we can intend as an *a priori* assumption to which the human subject is compelled by its very nature. Let us try to get rid of recursiveness—Poincaré says—and "let us construct a false arithmetic analogous to non-Euclidean geometry. We shall not able to do it" (*ibid.*, 49).

Clearly, Poincaré, as well as Cassirer, refer here to a different criterion for syntheticity than that of Kant's *Introduction* to the first *Critique*. But what is this criterion? This is really not very clear. Perhaps, we have to see in this lack of clarity one of the reasons for the success of the neopositivistic attitude concerning mathematics.

IV Mathematics as an Activity

Thus, we have to conclude that, both from the point of view of an intensional theory and from the point of view of an extensional theory, a logical distinction between analytical and synthetical judgments, founded on the criterion of the *Introduction* to the first *Critique*, makes no real sense. Do we have to conclude from this also, that the Kantian distinction as such, makes no sense logically ? We think not. We believe in fact: *i*) that the Kantian criterion of the *Introduction* to the first *Critique* is nothing but a bad illustration of a deeper idea; *ii*) that, in order to understand such an idea, we have to abandon the presupposition according to which mathematics (and science, in general) is to be understood as a class of theories (a theory in turn being a class of propositions); *iii*) that, by abandoning such a presupposition, we could gain a new perspective on the nature of logic, usually intended as an inquiry into the formal characters of our knowledge; *iv*) that, by assuming such a perspective, we can get rid of the dichotomy between intensional and extensional theories and conceive a scientific theory as something else.

The situation in fact changes radically if a theory is considered in the context of its genesis or application, or, in other words, if the notion of theory is transformed to incorporate activities that represent the epistemic subject-object relation. Extensions and intensions enter into varying and flexible relations with each other and this means that we have to base our considerations on the evolutionary process of cognitive activity, rather than on the idea of a theory as a class of propostions. Bolzano, among others, had accused Kant of having confounded mathematics with its development. Kant was right, although his ideas, with respect to the question of "the objectivity of the subjective" were insufficient and ahistorical.

From our point of view, mathematics (like science in general) has to be understood as a human activity, namely the activity of producing mathematical (or generally scientific) theories (in the previous sense). The aim of logic is not merely to study the internal structure of such theories, or even the formal nature of their propositions (either taken in isolation or jointly). Rather it implies the study of the modalities of human activity that produces them. Such an activity is a concrete and historical phenomenon. It is in terms of this phenomenon only that, we believe, it becomes possible to explain all other phenomena or entities. Nevertheless, logic has not to be confounded in our perspective with psychology. The latter treats the human activity producing our knowledge as a particular activity proper to each singular subject and tries, if it is possible, to isolate some constant features of it. The former treats of the general categories that may be used in describing such an activity and tries to understand the way by which it realizes intersubjectivity and founds the external world with respect to each subject.

Such a perspective is not to be confounded with a solipsistic point of view. Every realism, we believe in fact, has to be a constructive realism. Neither subject nor object exist in isolation and activity marks the essence of the subject-object relation, that is fundamental with respect to both relata. We do not suppose that only the individual subject exists, all the rest being pure appearance, but, to the contrary, we think that the notion of existence, or reality, is not a primitive notion, but has to be intended in terms of the modalities of the subject's activity.

Now to describe or explain the activity itself—and this is the only way for explaining a lot of subjective evidence (for instance the phenomenon of intuition)—one may conceive it as a system of means-objects relations. No activity exists without means and without objects. And neither internal experiences nor objective constraints can be understood but in terms of means and contents of activity. External conditions for the subjective activity or for consciousness are just to be intended as contents of intentional acts. The form they take is thus the form of these acts and the objectivity of such a form—that is the fact that it can be assumed as the same in our communication or along our life—is nothing but the effect of our capacity of connecting evidences in classes of equivalence and of inducing intentional acts in similar subjects. Of course, such a capacity is, once again, an hypothesis we advance in order to explain our evidences, that is: it is part of the intentional acts directed to pose ourselves as a subject or the external subjects as such.

Now, in such a context, science is nothing but a specific way of producing objectivity and the problem of a philosophy of science is essentially the problem of explaining this objectivity in terms of the activity that produces it. A scientific theory is nothing but a way for expressing this objectivity. We can recognize in it an intensional as well as an extensional component. The former is connected with the fact that the objectivity is the result of an activity, that is an act of consciousness. The latter is connected with the fact that this activity is an intentional one: it is just that which produces an objectivity.

But, what new sense can we give, from such a point of view, to the analytic-synthetic distinction, with respect to mathematics? We will try to answer to such a question in our following two papers.

Institute for Didactics of Mathematics,
University of Bielefeld

Centre F. Viète of the History and Philosophy of Sciences,
University of Nantes

Notes

1 For Kant, all mathematical judgments seems to be synthetic, and therefore for him also such a distinction—if it is applied to judgments—takes sense with respect to mathematics, but not in mathematics.

2 If we were realists, we could argue that certain properties are necessarily connected to other properties being particular specifications of them. For example, the property of being red—we could say—is a particular specification of the property of having a color (of reflecting the light). Thus a judgment as "all reds are colored" should certainly be analytic. However, this argument has two main defects: not only does it not prove that objectively synthetic judgements exist, but it is false also. In fact, if we were realists concerning properties, we should make a distinction between real and ascribed properties, an ascribed property being a property compounded by real properties or a generalization of certain real properties. According to such a sense, to be colored is certainly an ascribed property, because a body does not simply reflect the light, but reflects it in a peculiar way. Thus the necessity of the connection between the real property of being red and the ascribed property of being colored is, once again, a question of definition of the second property.

MARCO PANZA

MATHEMATICAL ACTS OF REASONING AS SYNTHETIC *A PRIORI**

I Introduction

My paper pursues two aims. First, I would like to argue that mathematical activity deals with pure objects, or even that mathematics is the human activity dealing with mathematical (that is pure) objects. In my view, this means that mathematical activity essentially consists of synthetic acts of reasoning and, as mathematical objects are pure objects, these acts are also *a priori*. This thesis should not to be confused with the standard thesis generally ascribed to Kant, according to which mathematical judgments are synthetic *a priori*. Nevertheless, I think that my thesis could be presented as a development of some of Kant's views on mathematics: as such, it is not a Kantian thesis, but I believe it is a "quite natural" consequence of Kant's views. Thus, my second aim is to trace a path leading from Kant's premises to my own conclusions.

II Standard Accounts

According to section V of the *Introduction* to Kant's *Critique of Pure Reason*, "All mathematical judgments [*Mathematische Urteile*[1]], without exception, are synthetic [*synthetisch*]" and "mathematical propositions [*Sätze*], strictly so called, are always judgments *a priori*" (Kant B, 14). If we accept that every mathematical judgment is a (mathematical) proposition, we have to conclude that:

(T_1) Every mathematical judgment is synthetic and *a priori.*

Since Kant certainly agreed with the auxiliary premise, (T_1) is certainly a Kantian thesis, and it is advanced by Kant in the section V of the *Introduction* to the first *Critique*. Thus, it is very natural that (T_1) is presented as an important Kantian thesis concerning analysis and synthesis in mathematics. Generally, this thesis is explained by referring to the following passage contained in section IV of the same *Introduction*:

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 273–326.

"In all judgments in which the relation of a subject [*Subjekt*] to the predicate [*Prädikat*] is thought [...], this relation is possible in two different ways. Either the predicate B belongs [*gehört*] to the subject A, as something which is (covertly) contained [*enthalten*] in this concept [*Begriff*] A; or lies outside the concept A, although it does indeed stand in connection with it. In the one case I entitle the judgment analytic [*analytisch*], in the other synthetic."(Kant A, 6-7; B, 10)

According to Kant, a judgment is not merely a "representation [*Vorstellung*] of a relation between two concepts" (Kant B, 140), but it is "the manner in which given modes of knowledge [*Erkennntnis*] are brought to the objective unity of apperception" (Kant B, 141). This is a very difficult definition and it is not my task to explain it here. However, it is clear that a judgment according to Kant, does not express any sort of possible association between two (or more) concepts: as long as it expresses a relation between a subject and a predicate, it expresses the appurtenance (*Zugehören*) of what is individuated by means of a certain concept S—the concept of the subject—to the sphere (or to the domain) of another concept P—the concept-predicate. In other words: as long as it expresses a relation between a subject and a predicate, a judgment says of a certain "representation" that if it is S, then it is P. Thus, we can reformulate the previous distinction in the following way:

(D_1) A judgment of the form $< \forall x[S(x) \Rightarrow P(x)] >$—where S and P are concepts and $S(x)$ and $P(x)$ mean that x belongs respectively to the domains of S and P—is analytic if and only if P belongs to S, and it is synthetic if and only if P does not belong to S.

(D_1) supposes that it possibly makes sense to say of two concepts α and β that α belongs to β. Such a possibility depends on a compositional (classic) notion of concepts, according to which concepts—or at least certain sorts of concepts—can be treated as collections of other concepts. Definitely, this seems to be an idea of Kant. Nevertheless, I am far from certain whether (T_1) is the hard core of Kant's philosophy of mathematics, and whether according to Kant, the essential epistemological relevance of the opposition between analysis and synthesis is expressed by (D_1), at least when such a distinction is meant literally. However, before I will give my own interpretation of Kant's views, I would like to present some standard reactions to (T_1) and (D_1). This will help to make my point clear.

In order to use (D_1) for justifying (T_1), we have to state the following lemmas:

(L_1) If S and P are respectively the concept of the subject and the concept-predicate of a mathematical judgment of the form $< \forall x[S(x) \Rightarrow P(x)] >$ (and it makes sense to say that P does not belong to S), then P does not belong to S.

(L_2) Every mathematical judgment is of the form $< \forall x[S(x) \Rightarrow P(x)] >$ (where S and P are two concepts such that it makes sense to say that P does not belong to S).

Therefore, it is very easy to refute (T_1) without denying that (D_1) is a good and useful distinction: you can deny (L_1), (L_2) or both. However, it is also possible to deny (L_1), (L_2) or both, without rejecting (T_1): if you want to do that, you have to look for an argument based on a distinction between analytic and synthetic judgments different from (D_1). The history of discussions about Kant's philosophy of mathematics contains a number of different examples for all these points of view.

It is very easy, for example, to refute (L_2) by quoting appropriate counterexamples and then reject Kant's philosophy of mathematics as a whole. This was done by Couturat (Couturat 1893, 84), for instance.

A more interesting position is Frege's (1844, particularly §88). According to Frege, the distinction between analytic and synthetic judgments cannot refer to the logical relation of inclusion between the concept-predicate and the concept of the subject, since this relation does not apply to arithmetical judgments, where the subject is generally a singular object. Moreover, an arithmetical judgment should not to be taken as an isolated one, for it is a consequence of a deductive proof. Thus, it is analytic if and only if it is "deducible solely from purely logical laws" (Frege 1884, §90), and it is synthetic if its proof depends on an appeal to intuition.

Though Frege speaks, like Kant, of mathematical judgments, his position can easily be generalized as one referring to mathematical sentences, to mathematical systems, or even to mathematics as a whole. For that, we only have to replace (D_1) by a more general and explicit distinction:

(D_2) A sentence is analytic if and only if it is part of an analytical system, otherwise it is synthetic; a system (of sentences) is analytic if and only if it is deductively closed with respect to purely logical rules and (eventually) purely logical axioms, otherwise it is synthetic.

Referring to (D_2), many have argued that "mathematics is analytic". (D_2), however, is a very problematic distinction, since it bears on the problematic notions of purely logical rules and axioms.

If we intend these notions in a strict sense, it follows from (D_2) that only propositional and predicative calculus are analytic systems. Hence—since it is obvious that, due to the occurrence of proper axioms in its deduction, no mathematical theorem, as it is generally enunciated, can be intended as a theorem of propositional or predicative calculus—we can assert that a mathematical theory is an analytic system only if we are ready to acknowledge that a mathematical theorem is nothing but an implication, where the antecedent is just formed by a suitable class of proper axioms. This was one of Russell's ideas (Russell 1903*a*), for example.

However, even though it would be possible to intend any mathematical theory as a system of implications of this sort, someone could argue that this is not the point, since these implications have generally to be of the form < if *A*, then *B* >, where *A* and *B* are not yet implications of this sort. Thus, by asserting, according to such an argument, that a mathematical theory is an analytic system, we are just saying that its theorems form a system of logical consequences of the given axioms, which Kant himself would have accepted. Here is what he writes just after the passage I quoted at the beginning:

> "[...] For as it was found that all mathematical inferences [*Schlüsse*] proceed in accordance with the principle of contradiction (which the nature of all apodeictic [*apodiktischen*] certainty requires), it was supposed that the fundamental propositions of the science can be themselves be known to be true through that principle. This is an erroneous view. For though a synthetic proposition can indeed be discerned in accordance with the principle of contradiction, this can only be if another synthetic proposition is presupposed, and if it can then be apprehended as following from this other proposition; it can never be so discerned in and by itself." (Kant B, 14)

Thus, to argue that a mathematical sentence or system (or even mathematics as a whole) is analytic is not enough—according to Kant—to show that mathematical rules of inferences are purely logical; if we want to deny (T_1) on the base of (D_2), we have to argue that mathematical axioms are purely logical too, even though they are proper axioms. There was a time when Russell and Whitehead dreamed to show that just this is the case: that every mathematical theory could be reduced to a system of logical consequences of axioms that we should take as logical, since they express nothing but general properties of sets (Whitehead and Russell 1910-1913)[2].

However, what is a logical axiom in this sense is really a disputed question and it is certainly not in this manner we can hope to decide whether (T_1) has to be accepted or not. If this is the problem, the question of analyticity or syntheticity of mathematics is simply a question of subjective views. According to Cassirer (Cassirer 1907), proper mathematical axioms and definitions are synthetic, for example, and every mathematical theory is then a synthetic system, even though it uses only logical rules of inference.

At first glance, we might believe that Poincaré advanced a similar thesis with respect to arithmetic (Poincaré 1894), but it seems to me that Poincaré's view is essentially different from Cassirer's. Poincaré is interested in the nature of "mathematical reasoning", rather than in the character of mathematical axioms. Thus, when he claims that the mathematical principle of induction is synthetic, he wants to say that mathematicians proceed by non-logical inferences in arithmetic, that is: they "proceed by construction" and "mathematical induction".

A similar point has been made by Hintikka (Hintikka 1973). According to him, Kant's distinction applies to "modes of reasoning", namely, the modes of reasoning "which are now treated in quantificational theory" (*ibid.*, 182). These "modes of reasoning are synthetic if the inferences or arguments that occur in them are synthetic", and an inference (or argument) is synthetic if it does not deal "with general concepts only", but needs "the introduction of an intuition" (*ibid.*, 194). A part of Hintikka's notion of reasoning in its relations to inferences or arguments in quantificational theory, this is exactly the thesis I will ascribe to Kant in the next paragraphs III and IV. However, according to Hintikka, this means that "for Kant the reason why mathematical arguments are synthetic is that they are constructive", that is: they proceed by introducting "new individual mathematical objects" (*ibid.*, 206). In other words:

> "Synthetic steps are those in which new individuals are introduced into the argument; analytic ones are those in which we merely discuss the individual which we have already introduced." (*ibid.*, 210)

Moreover:

> "In a suitable formulation, arguments of the former kind can be boiled down to existential instantiation." (*ibid.*, 210-211)

If the previous thesis is ascribed to Kant, I do not think this is a good explanation of it. I think that for Kant an "analytical argument" (to use Hintikka's terminology) does not "discuss individuals" at all (at least, if the term "individual" means "object"), and a synthetic one does not ask for "existential instantiation" and does not deal properly with "mathematical objects".

III A Provisional Reformulation of Kant's Distinction

By shifting attention from judgments or sentences to inferences or even to reasoning, Poincaré and Hintikka move, as I believe, in the right direction. Moreover, when Hintikka states that, according to Kant, mathematics is a constructive affair, and syntheticity is concerned with intuition, he points to a crucial aspect of Kant's philosophy of mathematics.

Nevertheless, in my opinion, for Kant the syntheticity of mathematics does not depend on the occurrence of constructive, or generally non-deductive, inferences. As a matter of fact, it depends on the role of intuition, but intuition has to be intended neither as a condition of construction (in the usual sense), nor as a sort of psychological capacity: the capacity of "seeing" some hidden relations, or to be convinced by some particular evidence or even to switch on a mental light in the darkness of doubt or ignorance. *A fortiori*, intuition has not to be taken as a (logical or psychological) condition of non-deductive or constructive inferences. Thus, even though I think that for Kant intuition is in a sense the source of syntheticity in mathematics, I do not think that we have to argue, to justify Kant's views, that mathematical proofs or arguments are full of "intuitive" (that is non-deductive, or even "non-logical", or "constructive") steps, as some say.

III.1 Judgments and Propositions

Let me begin with a remark on *Jäsche Logic* (Kant, JL). Here, Kant does not distinguish between analytic and synthetic judgments, but only between analytic and synthetic propositions. According to him, every proposition is a judgment, but not every judgment is a proposition: a proposition is an assertoric [*assertorisch*] judgment (*ibid.*, § 30, note 3), that is a judgment "accompanied with the consciousness [*Bewusstsein*]" of "the reality[3] [*Wirklichkeit*] of the judging" (*ibid.*, §30). Such a definition is not so different from that of the first *Critique*—according to which an assertoric judgment is that in which "affirmation or negation is viewed as real [*wirklich*] (true [*wahr*])" (Kant A, 74; B, 100)—but the reference to the idea of consciousness makes my point clearer.

That between problematic [*problematisch*], assertoric and apodeictic judgment is, according to Kant, a distinction of judgments on the base of their modality, that is "the way in which something is maintained [*behauptet*] or denied [*verneint*] in the judgment" (Kant, JL, § 30, note 1). What is important here is the modality of maintaining or denial and not what it is maintained or denied: a problematic judgment maintains or denies something possibly (*möglicherweise*); an assertoric judgment maintains or denies something really (*wirklich*); an apodeictic judgment maintains or denies something necessarily (*notwendigerweise*). The distinction does not concern the modal form of the judgment itself, but the modality of the act of formulating such a judgment. As Kant writes in the first Critique:

> "The modality [*Modalität*] of judgments is a quite peculiar function. Its distinguishing characteristic is that it contributes nothing to the content [*Inhalt*] of the judgment [...], but concerns only the value of the copula in the relation to thought [*Denken*] in general." (Kant A, 74; B, 99-100)

But, what does it mean that something is maintained or denied possibly, really or necessarily?

If we try to understand such a distinction using the usual conception of modality in terms of truth in a given collection of worlds, we are not able to do it without a criterion founded on the modal form of appropriate statements. It is possible to imagine different ways to construct sets of worlds and to associate any judgment with appropriate statements to be evaluated with regard to these worlds, in order to say if such a judgment is problematic, assertoric or apodeictic. In this way we can justify, for example, that a judgment as < It is possible that A > is not problematic, since it maintains something: particularly, it maintains that A is possible.

However, in that way we reduce any judgment to usual modal statements and we decide about its nature on the basis of the modal form of the associated statements.

I think that Kant's distinction should not be understood this way. In my interpretation, this distinction is rather a question of justification.

A problematic judgment is not a judgment expressed by an appropriate statement that is true in the worlds belonging to a proper non-empty sub-set of a given set of appropriate worlds. It is a judgment, referring to only one world, that has been formulated without any kind of justification. As Kant does not admit any sort of guess, this means that the act of formulating this judgment cannot be an act of stating anything; it is simply an act of expressing a certain connection.

But such a connection, you might notice, has to be a possible one. Hence, we have to explain what a possible connection is. There are two ways for doing that. First, we could say that the logical form of this connection has to be a possible form of a judgment, that is: it has to respect certain logical (or simply syntactical) rules of formation. Second, we could say that it is the content of the connection that has to be possible. If so, we come back to modality, intended in the usual extensional sense, but now we are considering it, not in order to know whether a judgment is problematic or not, but to know whether a certain connection can be a judgment or not. Thus, we would say that the act of formulating a problematic judgment is the act of expressing an arbitrary connection we have ascertained to be possible.

As, according to Kant, the "expression through words" is a necessary condition for the act of judging (Kant JL, § 30, note 3), the first solution leads us to conclude that problematic judgments are sentences (or are expressed by sentences), while the second solution leads us to conclude that problematic judgments are non-contradictory sentences (or are expressed by non-contradictory sentences).

What is important to me is that according to both, the first and the second interpretation, the act of formulating a problematic judgment does not require any justification of the content of the judgment itself. This is not the case for assertoric and apodeictic judgments, since, according to Kant, the act of formulating an assertoric or apodeictic judgment is an act of stating something. The difference between these two sorts of acts lies in the nature of justification. If such a justification merely depends on the "laws of understanding [*Verstand*]" (Kant A, 76; B, 101) the judgment is apodeictic, otherwise it is assertoric.

If I am right, Kant's distinction is asymmetric, since it actually distinguishes between problematic and non-problematic judgments on the one hand, and between non-problematic assertoric judgments and non-problematic apodeictic judgments on the other hand. As long as problematic judgments are sentences (or are expressed by sentences), non-problematic judgments—both assertoric and apodeictic—are statements (or are expressed by statements).

Now, if only propositions can be analytic or synthetic and propositions are assertoric judgments, it follows that Kant's distinction between analytic and synthetic does not apply to sentences, but rather to statements. However, if we would like to stay close to Kant's text, we should argue that the only statements that could really be called "analytic" or "synthetic" are assertoric statements. But if that is so, how could Kant have advanced the view that mathematical judgments (which in his views are apodeictic judgments) are synthetic? The difficulty would be a major one, if Kant had not explained once again his notion of proposition in the following terms:

"Before I have a proposition I must first judge [*urteilen*]; and I judge about much that I cannot make out [*ausmachen*][4], which I must do, however, as soon as I determine a judgment as a proposition." (Kant JL, §30, note 3)

According to such a characterization, a proposition is a judgment associated with an act of making out. Since in my interpretation this is true for any sort of non-problematic judgment, that is any sort of statements, we have to conclude that any sort of non-problematic judgment is a proposition. The point is plaintly this: Kant's distinction between analytic and synthetic judgments lies exactly in the nature of such an act of "making out" and can then be applied to any sort of statement. Thus, I propose to force Kant's text a little bit and to interpret Kant's distinction between analytic and synthetic propositions as referring to any sort of statement.

III.2 Analytic and Synthetic Propositions

According to paragraph 36 of the *Jäsche Logic*, "propositions whose certainty rest on identity [*Identität*] of concepts (of the predicate with the notion of the subject) are called analytic propositions", while "propositions whose truth [*Wahrheit*] is not grounded [*gründet*] on identity of concepts must be called *synthetic*" (*ibid.*, § 36). Even though he speaks of identity, Kant is clearly referring to the identity of the concept-predicate P with a part of the concept of the subject S. The remark 1 about the same paragraph 36 is clear:

"An example of an *analytic* proposition is [...] [:'] To everything x, to which the concept of body $(a + b)$ suits [*zukommt*], suits [*kommt*][5] also *extension* (b) ['].
An example of a *synthetic* proposition is [...] [:'] To everything x, to which the concept of body $(a + b)$ suits, suits also *attraction* (c) ['']." (*ibid.*, § 36, note 1)

These examples fit very well with (D_1), but here Kant does not seem to insist on the fact that the concepts-predicate b and c belong or do not belong to the concept of the subject $(a+b)$. What is important here is rather that the act of "making out" the content of the sentence "every body is extended" rests on ascertaining the appurtenance of the concept b to the concept $a+b$, while the act of "making out" the content of the sentence "every body attracts" does not rest and can not rest on ascertaining any logic relation between concept $a+b$ and concept c. The point I want to make is this: the content of analytical judgments "is made out" merely by analyzing concepts; to "make out" the content of a synthetic judgment, we in contrast have to go away from concepts and base ourselves on something else. The following quotation, drawn from the *Introduction* to the first edition of the *Critique of Pure Reason*, seems very clear to me:

"[...] through analytic judgments our knowledge is not in any way extended, and the concept which I already have is merely set forth and made intelligible to me; [...] in synthetic judgments I must have besides the concept of the subject something else (X), upon which the understanding may rely, if it is to know that a predicate, not contained in this concept, nevertheless belongs to it." (Kant A, 7-8)

Even though Kant eliminated this passage in the second edition, the same point is clearly expressed in the sections IV and V of the *Introduction*[6]. The main question Kant faces in these sections, after having presented (D_1), could be presented like this: on what do we ground ourselves for "making out" the content of synthetic judgments, if it is not on analysis of concepts?

For *a posteriori* judgments the answer is very simple and clear: we ground ourselves on our experience of objects, particularly of the objects that fall under the concepts occurring in the judgments themselves. For *a priori* judgments, the question is much more difficult, since here we cannot refer to any sort of experience.

"[...] in *a priori* synthetic judgments—Kant writes—this help is entirely lacking. Upon what, then, am I to rely, when I seek to go beyond the concept A, and to know that another concept B is connected with it? Through what is the synthesis made possible? since[7] I do not here have the advantage of looking around in the field of experience [*Erfahrung*][...]. What is here the unknown = X which gives support to the understanding when it believes that it can discover outside the concept A a predicate B foreign to this concept, which it yet at the same time considers to be connected with it?" (*ibid.* A, 9; B, 12-13)

Even though this is one of the most fundamental questions in the first Critique (since it is equivalent to the famous one: "how are synthetic *a priori* judgments possible?") Kant does not sketch a general answer in the *Introduction*. He prefers to consider mathematics, natural sciences and metaphysics separately (in section V), and even in these cases he does not give a direct answer to the question.

If we abstract from such an answer, we once again limit ourselves to subject-predicate judgments and we assume that, according to Kant, analytic and synthetic statements have to form two complementary classes we may provisionally formulate Kant's distinction as follows:

(D_3) A statement of the form $< \forall x[S(x) \Rightarrow P(x)] >$—where S and P are concepts and $S(x)$ and $P(x)$ mean respectively that x belongs to the domains of S and P—is analytic if and only if the act of "making out" that every x which is S is also P is grounded on nothing but the ascertainment of the logical relation 'P belongs to S' between the concepts S and P; it is synthetic if and only if this act asks for moving away from the consideration of the logical relations between the concepts S and P.

If we consider judgments as statements and assume that the appurtenance of P to S is a sufficient (and obviously necessary) condition for the act of "making out" that every x that is S is also P is grounded on the ascertainment of such a logical relation, then the first part of (D_3) (the definition of analyticity) is equivalent to the first part of (D_1). Moreover, the second part of (D_1) (the definition of syntheticity) is perfectly complementary to the first part: according to it, a judgment is synthetic if and only if it is not analytic. Thus, if we accept that the second part of (D_3) is also perfectly complementary to the first part, we have to conclude that (D_1) and (D_3) are absolutely equivalent under the previous conditions. Now, in the first *Critique*, Kant is most of all concerned with statements rather than with sentences, thus we can imagine that, for him, (D_1) really deals with statements, rather than with sentences. Moreover, he certainly accepts the appurtenance of S to P as a sufficient condition for the act of "making out" that every x that is S is also P is grounded on the ascertainment of such a logical relation. So, if I am right in asserting that (D_3) is a Kantian distinction, and if we assume that, according to Kant, the second part of such a distinction is purely complementary to the first, we should conclude that in the *Introduction* to the first *Critique*, Kant advanced (D_1) as a simplified version of (D_3). This is just my thesis.

However, (D_3) is a provisional distinction, for at least two reasons. First, it does not specify what enables us to formulate synthetic statements; second, it is restricted to subject-predicate statements. Moreover, it is also not totally satisfactory, since it is grounded on the interpretation of S and P as concepts, while, strictly speaking, they are predicates.

The latter difficulty is obviously connected with my shifting from judgments to statements and can only be solved by presenting an appropriate theory of concepts. Such a theory is also necessary for generalizing (D_3) to any sort of statements and making its second part explicit. Furthermore, these two latter tasks also need an appropriate theory of logical counter-parts of concepts. I doubt that two appropriate theories of this sort are really available in Kant's philosophy. In the next paragraph, I will try to expound Kant's theory of concept and its logical counter-part (that is intuition or object) as briefly as I can, and as I am able to understand it, in order to make Kant's own distinction clearer—even though not

satisfactory yet—and to understand Kant's reasons for claiming that "mathematical knowledge" is synthetic.

IV Concept, Object and Intuition: the Final Version of Kant's Distinction

In the *Critique of Pure Reason*, Kant is concerned with conditions of knowledge. For him, judgments are forms or moments of knowledge. Thus, if we intend assertoric and apodeictic judgments as statements, we have to consider statements both as logical (or linguistic) forms and as cognitive acts: intended in the first way, a statement is the logical form of the same statement, intended in the second way. But forms can be classified and so, in the first sense, statements both are forms and have forms of a higher level. These forms of higher level can be expressed by logical formulas as $< \forall x[S(x) \Rightarrow P(x)] >$, so that these formulas express forms of forms of cognitive acts. The elements occurring in these formulas then have to express elements of a cognitive act, that is elements of an act of knowing.

Now, for Kant, knowledge can be either *a priori* or *a posteriori*. The *a posteriori* knowledge is nothing but experience and generally consists in appropriate representations and judgments connected to the occurrence of a sensation. In this sense, it is knowledge of objects. On the other side, *a priori* knowledge is independent from the occurrence of sensations, but it is neither knowledge of something different than objects, nor is it a form of knowledge alternative to experience. Rather, it consists of representations and judgments that make *a posteriori* knowledge or experience possible. In this sense, it is a condition of *a posteriori* knowledge and it is justified as such. The possibility of *a priori* knowledge is thus the possibility of the conditions of possibility of *a posteriori* knowledge or experience. Hence, as for Kant *a posteriori* knowledge is a fact, *a priori* knowledge is a fact too and the form and nature of the latter depends on the form and nature of the former. To understand the first (*a priori* knowledge), we then have to understand the second (*a posteriori* knowledge).

IV.1 *A Posteriori* Knowledge

A posteriori knowledge is for Kant either "objective perception [*objektive Perzeption*]" or judgment. An objective perception is for Kant a species of the genus of representation:

> "The genus is *representation* in general (*representatio*). Subordinate to it stands representation with consciousness (*perceptio*). A *perception* which relates solely to the subject as the modification of its state is sensation [*Empfindung*] (*sensatio*), an objective perception is *knowledge* (*cognitio*)." (*ibid.* A, 320; B, 376)

In other words: an objective perception is a perception intended as an act of perceiving something, rather than as the event of the subject's modification, which is sensation. The difference between objective perception and sensation does not concern the nature or the direction of consciousness; in the former as well as in the latter, consciousness is directed toward what is perceived. Objective perception and sensation are just two different (and complementary) aspects of perception, that is the act in which a subject represents something to himself, as the cause of modification of its own internal status. Hence, there is no objective perception without sensation, and the reason for this is not merely that objective perception and sensation are necessarily connected facts. They are simply not different facts, but different aspects of the same fact, namely perception. Thus, there is also no perception without objective perception or sensation:

> "Perception [*Wahrnehmung*][8] is empirical [*empirische*] consciousness, that is a consciousness in which sensation is to be found." (*ibid.* B, 207)

Kant's distinction between sensation and objective perception can be expressed in the following terms: if we analyze perception without considering the specificity of consciousness, only insisting on its presence, we speak about sensation; in contrast, if we analyze perception regarding the specific nature of consciousness occurring in it, we speak about objective perception. To analyze objective perception is then the same as analyzing consciousness occurring in perception.

However, Kant's aim is not that of analyzing *a posteriori* knowledge as such, but it is rather that of looking for its conditions. Concerning objective perception, Kant's problem is the following: how is it possible for a subject to represent something to himself as the cause of a certain sensation (the modification of his internal status)?

According to Kant, the cause of a sensation, as the subject of such a sensation represents it to himself, is an object (*Gegenstand, Object*)[9]. If the term "representation" means what is represented, rather than the act of representing it, an object is a conscious representation:

> "Everything, every representation even, in so far as we are conscious of it, may be entitled object." (*ibid.* A, 189; B, 234)

Even though elementary knowledge is always a representation of something, such a something is an object only as long as it is represented in an act of objective perception. Thus, we cannot intend objective perception as a representation of something that is given as an object before the representation itself. Obviously, a subject can represent something to himself that has been already given as an object, but this is not an act of objective perception. It is rather a judgment that makes the subject able to classify objects (which are already given as such), according to their particular characters. Properly speaking, such a representation

does not produce objects, but classes of objects, aspects of objects, functions of objects, etc. According to Kant, an object is properly a representation of something that is not an object, and *a posteriori* knowledge just begins when an object is given in this proper sense. Hence, objective perception is representation of something that cannot be known, but only thought as a something that is represented by a certain object (*ibid.* B, xxv)[11].

The problem of the conditions of objective perception is then the problem of the possibility of the represention of something, which is not known as such, but only thought as the cause of a sensation, as a certain object. This way, we have arrived at the crucial point: such a representation is possible for Kant only if the object arises from a "subjective constitution [*subjektive Beschaffenheit*]" (*ibid.* A, 44; B, 62), which is necessarily *a priori*. Thus, even though objective perception, as a perception of a certain object, is a sort of *a posteriori* knowledge, it is possible only as the result of an *a priori* act of subjective constitution of the object itself and, as such, it can be genetically analyzed in two different (and even opposite) aspects:

> "This [Knowledge or objective perception] is either *intuition* [*Anschauung*] or *concept* (*intuitus vel conceptus*). The former relates immediately to the object and is singular[10] [*einzeln*], the latter refers to it mediately by means of a feature which several things may have in common." (*ibid.* A, 320; B, 376-377)

As long as they occur in objective perception, intuition and concept are thus two different aspects of our representation of the cause of a sensation as an object for Kant, that is two aspects of subjective constitution of such an object. These aspects have not to be confounded with two complementary and alternative forms of subjective constitution of an object. The opposition between intuition as "singular representation" and concept as "universal representation" (cf. also Kant JL, §1) is founded for Kant in another and deeper opposition: that between intuition as the aspect of objective perception for which the object is given as such, and concept as the aspect of objective perception for which the same object is thought as such (Kant A, 19; B, 33)[11]. And for Kant an object cannot be given as such if it is not thought, and it cannot be thought as such if it is not given. Thus, regarding objective perception, a singular representation is not a sort of representation different from universal representation: singular and universal representation, *i. e.* intuition and concept, are two aspects of the same representation, that is perception taken as knowledge. They are then two aspects of the same act, the act of subjective constitution of an object.

In their primitive and more fundamental sense, intuition and concept have thus to be intended, with respect to *a posteriori* knowledge, as two opposite cognitive functions we distinguish by analysis in only one cognitive act, the act of subjective constitution of objects. Since this act is nothing but objective perception, an object is by definition an empirical entity. However such an entity has not an external existence, but results from an act of representation that has been analyzed as an act of constitution.

Let us continue. According to Kant, an object is given as such when the subject connects his sensations (the modifications of his status) to a spatio-temporal unity, and it is thought as such when he recognizes such an entity as an example of a certain class of spatio-temporal unities. The former condition is certainly necessary for the latter to take place, but, according to Kant, the latter condition too is, a necessary condition for the former to take place, since spatio-temporal bounds of a certain unity do not depend on the unity itself, but have been imposed on it by the subjective constitution, according to a certain reason that could not be anywhere else but in the concept.

This remark leads us to the final step of Kant's analysis of objective perception: the act of subjective constitution of an object is possible only if we dispose of two connected faculties, or better of one faculty that can be analyzed into two different aspects. According to one of its aspects, this faculty enables us to connect our sensations to certain positions in an order—the spatio-temporal order—that is already given (and can be analyzed in two different aspects, the internal order, or sense—which is time—and the external order or sense—which is space). According to the other of its aspects, this faculty enables us to recognize these positions as particular representations of certain forms that are already given too.

Here a first important shift occurs: these two different aspects are generally taken as opposite faculties (of pure reason) and the first is confounded with intuition itself, the second one being the faculty of understanding. As a faculty, intuition is not opposed by Kant to concept anymore, it is rather opposed to a faculty—namely understanding—that is taken as the faculty of producing and even composing concepts. Thus, while intuition becomes a faculty, the place of concept is taken by a plurality of concepts intended as a sort of entities produced and manipulated by the subject.

If I am correct, Kant's analysis of conditions of objective perception could be summarized as follows: the act of representing as an object which is thought as the cause of our sensation is an act of subjective constitution. In such a constitution, two faculties concur: intuition and understanding. Intuition connects our sensations to positions in spatio-temporal order, that is an order that is already given as such; understanding recognizes these positions as particular representations of certain forms that also are already given. These faculties are necessarily connected, since forms are already given as possibilities of positioning in spatio-temporal order, and intuition realizes such a connection under the guidance of the capacity of recognizing positions in spatio-temporal order as particular representations of these forms.

These forms are not objects, since the subject does not represent them to himself as the cause of his sensations; they are thought as concepts, but they are just given as forms in "pure intuition", or "pure forms". Furthermore, the objects thus constituted are not examples of the concepts of these forms. Instead, these concepts are necessarily *a priori* and universal, while objects are empirical entities connected to particular sensations. The connection between objects and the concepts of pure forms is governed by schematism. I will not discuss this question here. What interests me is rather that the objects which are constituted in objective perception (intended as an act of constitution) are not instances of the concepts of the pure forms which occur in such an act. Properly speaking, there is no object that is an instantiation of these concepts; they are just concepts of pure forms. Thus intuition and understanding, as long as they are intended as faculties which concur in the act of *a priori* constitution of objects, are both certainly *a priori* faculties. But they are not properly pure, since they apply to particular sensations. However, such an application is possible only if the subject disposes of pure forms. In other words: the subjective constitution of an object depends not only on two *a priori* faculties, but also on the disposability of *a priori* entities as pure forms. The first task then of *a priori* knowledge is to provide these entities.

Once objects have been constituted, they are given as particular spatio-temporal unities and they are thought as particular representations of concepts of pure forms, as examples of empirical and individual concepts. Still, this is not the final step of our *a posteriori* knowledge. It is only the final step of the act of exhibiting these objects as such, namely objective perception. To know these objects in their respective relations, we have to be able to pass to judgments. Nevertheless, not every judgment is an act of *a posteriori* knowledge, since what is essential in *a posteriori* knowledge is not the logical form of judgment, but the occurrence of an experience. When it is not merely an act of objective perception, an act of *a posteriori* knowledge is necessarily a judgment only according to its form, or, if you prefer: the logical form of judgment is only a formal or external—even though necessary—condition of experience. We cannot have experience of anything else but objects; moreover a simple succession of acts of objective perception is not an experience yet, it is nothing but a "rhapsody of perceptions" (*ibid.* A, 156; B, 195), that is a rhapsody of different and isolated acts of elementary experience. In his genuine sense, experience asks for a connection between these acts, and judgment is just a logical form of this connection. Still, the occurrence of a connection of this form is only a necessary condition for knowledge, since in order to have knowledge, such a connection must not only be a judgment; it must also be an objective judgment.

But what makes a judgment "objective"? Certainly, a judgment is not objective when it connects objects, since, according to Kant, a judgment always connects concepts. Rather, we should say that a judgment is objective if it connects concepts according to the respective objects. However, in face of such an answer, we could insist: what does it mean exactly, in any particular case, that a judgment connects concepts according to the respective objects? This is a very difficult problem, but in a sense, this is not our problem now. At the present stage of the analysis, what is important is this: whatever such a condition would be, it is certainly impossible to satisfy it, if the concepts we are connecting were all concepts of particular objects. Thus, in order to make *a posteriori* judgments possible, a first condition has to be satisfied: the subject has to dispose of non-elementary empirical concepts, that is of empirical concepts different from distinct concepts of individual and particular objects. These concepts are concepts of forms of particular objects. Hence, when it is not merely an act of objective perception, an act of *a posteriori* knowledge is just a judgment connecting these concepts to each other, or to concepts of particular objects. It is only by the mediation of these concepts that a judgment can (indirectly) connect particular acts of objective perception. As these concepts have to be empirical, they cannot come from any other source than objective perception itself. But since they are non-elementary, they cannot result from a simple succession of acts of objective perception. They have to be produced by a different sort of connection of objective perceptions. Still, this is not the end of the story, since once these non-elementary concepts have been produced, in order to have a judgment, they have to be connected to one another, or to elementary concepts. And this is certainly not possible if they are produced in different and isolated acts.

Furthermore, in order to be an act of knowledge, a judgment must not merely be problematic; it not only must connect concepts, but it must state the content of such a connection. Thus, the possibility of non-elementary *a posteriori* knowledge depends on the possibility of producing non-problematic *a posteriori* judgments.

IV.2 *A Priori* Knowledge

The analysis of *a posteriori* knowledge has led us to distinguish three tasks for *a priori* knowledge: *i)* to provide pure forms and to permit both *ii)* the production of non-elementary concepts and *iii)* the connection of non-elementary or elementary concepts in a judgment that is not merely problematic.

Let us begin with the second task. For Kant, the production of non-elementary concepts is the result of a synthesis of understanding. It is an act of understanding, but it is not as such an *a priori* act, since it operates on empirical concepts given as forms of real (and not only possible) experience. However, this act would be impossible without the unity of internal sense that makes the different acts of objective perception different elements of only one unity of knowledge, and such a unity is for Kant assured by "pure [*reine*] intuition". Here, intuition is no longer a faculty occurring in the act of constitution of objects or an aspect of objective perception, it becomes a guarantee of the possibility of the synthesis of understanding. Thus, the passage from sensible intuition to pure intuition produces a new important shift in Kant's conception of intuition.

Nevertheless unity of internal sense is only a formal condition for the synthesis of understanding applied to elementary empirical concepts. It guarantees the possibility of such a synthesis, but it does not guarantee that something like a genuine concept is produced. In other words: it does not guarantee that the result of the synthesis of understanding is, as such, a component of an act of knowledge. Even though the results of such a synthesis could certainly not be concepts of objects, they have to be able to refer to objects as concepts of objects do, that is, they have to be exemplified by aspects, functions, relations etc., or, in general, forms of possible objects. These results have to be "really possible concepts", they have to "agree with the formal conditions of an experience in general" (*ibid.* A, 220; B, 267). Of course, this could not mean that any result of a synthesis of understanding applied to elementary empirical concepts had to be exemplified in such an indirect way by an actual object. The problem then is this: what is the result of a synthesis of understanding applied to elementary empirical concepts?

The first part of the answer is trivial: the result has at least to be a "logically possible concept". Whatever the synthesis of the understanding is, it must respect the condition of logical possibility, that is the principle of non-contradiction. However, this is not a sufficient condition, yet. Another condition is needed, but it is not so easy to formulate. As I have just said, for Kant any concept has also to be indirectly exemplified by something that we could represent as a possible object. But clearly this is not a criterion, since the subject does not dispose of possible objects as such, and then he cannot classify logically possible concepts by comparing them to possible objects. Thus, if a demarcation is possible between logically possible concepts that are really possible, and logically possible concepts that are not really possible, its criterion can not be based on appealing to a comparison to possible objects. In other words: possible objects should be, by definition, nothing but the objects of really possible concepts, and not *vice versa*. If we want to distinguish between logically possible concepts that are also really possible and logically possible concepts that are not really possible, we have to refer directly to the synthesis of intuition as such. Now, according to Kant, such a discrimination does not depend on a criterion, rather it depends on a faculty. Such a faculty is again pure intuition. In this case, pure intuition is not directly applied to objective perceptions as a condition of unity, it directly applies to the concepts of empirical objects, as a condition of compatibility. For producing new empirical concepts, understanding realises a synthesis by starting from elementary empirical concepts, that are concepts of objects which manifest concepts of pure forms in particular; the result of such a synthesis is a really possible concept if it is produced in a way that is compatible with the conditions of compositions of concepts of particular objects and pure forms.

Let us consider now the third task of *a priori* knowledge: that of making non-problematic *a posteriori* judgments possible. Let us imagine that a subject has non-elementary empirical concepts at his disposal. In order to connect them to another, or with elementary empirical concepts, in a problematic judgment, he has to be able to consider his own concepts together, as part of a unity of consciousness. Such a unity is assured by pure intuition that is now applied to elementary or non-elementary concepts as a condition of unity. Still, this is only the beginning of the story. To obtain *a posteriori* knowledge, the problematic judgment has to be justified and transformed into an assertoric judgment (since it is clear that no *a posteriori* judgment can be apodeictic). To make it, we have to come back to the objects themselves—the objects that directly or indirectly exemplify the concepts occurring in the judgment—and to consider the distinct acts of objective perception corresponding to them as only one experience. Thus, pure intuition has to occur once again as a guarantee of such a unity. In this new role, pure intuition does not work simply as a deaf guarantee for the act of synthesis; according to Kant, it is also the base of a class of synthetic *a priori* judgments that express the conditions of *a posteriori* knowledge discursively. These judgments are the dynamic principle of pure understanding, "analogies of experience", and "postulates of empirical thought in general" which are rules "according to which a unity of experience may arise from perception" (*ibid.* A, 180; B, 222).

Even though we could go on by analyzing the justifications (or deductions) and the function of these synthetic *a priori* judgments (which are obviously apodeictic judgments), I stop here, since I am not directly concerned with this sort of judgments. It is sufficient to have stated that they are grounded on pure intuition as a guarantee of the formal possibility of non-elementary experience, that is the possibility of the necessary form of *a posteriori* judgments.

However, the possibility of the form of *a posteriori* judgments is not yet the possibility of these judgments as such. For this possibility to be insured, we still have to guarantee that these forms can be filled up by connections that express something as a "possible experience" (*ibid.* A, 160; B, 199). This is guaranteed by the fact that objects are necessarily "extensive magnitudes" characterized by "intensive magnitudes". This is the content of the principles of "axioms of intuition" and "anticipations of perception". Such a fact is expressed by these judgments—that are synthetic *a priori* judgments and are, as such, produced by pure understanding—but it depends on the nature of intuition—as it occurs in the subjective constitution of objects—and it is present to the subject thanks to pure intuition.

Thus both axioms of intuition and anticipations of perception are justified, according to Kant, by a sort of application of pure intuition to intuition itself, or at least to the form of intuition. This is a new essential function of pure intuition.

Still, this is not the end of the story, since, up to now, we have only justified the possibility of the realization of a necessary form of experience, and not the possibility of assigning a real content to such a form. Now, in order to realize the latter possibility, we need both pure forms and real judgments connecting the concepts of these forms. Thus, we have arrived at the first task of *a priori* knowledge.

Let us begin with the first point. The act of subjective constitution of objects, as it was just described, asks for pure forms, but it does not give any guarantee that they are possible. Again, such a guarantee is provided by pure intuition that seems to guarantee both the availability of elementary pure forms (as straight lines and circles) and the possibility of composing them in order to produce other forms (as triangles or squares). On the first point, Kant is not really explicit. He seems to reason as if these forms were given as such to pure intuition. In contrast, he does not leave any doubt as to the second point (cf. for example, *ibid.* A, 220-226; B, 267-274). The synthesis of understanding produces new concepts of pure forms that have to be not only logically possible, but really possible too: they have to be forms that can be manifested in particular by possible objects. The problem is thus analogous to the one we just discussed with respect to non-elementary concepts: how can really possible concepts of pure forms be distinguished from really impossible ones? Of course, such a distinction cannot be made *a posteriori* and has to rely on an *a priori* capacity of the subject. Thus, the guarantee of this capacity is once again pure intuition as a guarantee of the possibility of certain sorts of objects. This is, I believe, responsible for Kant's monolithic conception of mathematics, and particularly of geometry[12].

Moreover, even if we accept that really possible concepts of pure forms are given (and distinguished by really impossible ones), we do not have any guarantee of the possibility of judgments connecting them. These judgments do not provide, as such, a condition of possibility of *a posteriori* knowledge in general, but they make possible particular experiences and contribute, in this way, to our knowledge. According to their forms, these judgments are submitted to the conditions of possibility of any sort of judgment. Nevertheless, the question here is not that of the unity of different acts of subjective constitution of objects, it rather refers to the subject's own consciousness. The judgment has to connect concepts of pure forms here, concepts that occur in the act of subjective constitution of objects as something that is already given. Thus, the unity that has to be guaranteed is the unity both of the field of giveness of elementary pure forms—that is also a condition of possibility of their composition—and of the different acts of their composition. Thus, if such a unity is guaranteed by pure intuition as well, a new shift occurs.

But the question is not only that of the formal possibility of judgments connecting concepts of pure forms. These judgments provide a condition of possibility for experience only if they are not merely problematic. Thus, the main question is that of their justification.

Of course, if these judgments connect non elementary concepts of pure forms, they can be logical consequences of the way in which the concepts that occur in them have been produced. They are then analytic judgments. However, according to Kant, this is not the only way by which a judgment connecting concepts of pure forms can be justified.

Even though these judgments speak about pure forms, it is possible to justify them by considering objects (as physical figures or collections of physical objects) which are constructed—according to a fixed procedure (as the constructive procedure given in the Euclidean postulates[13])—in order to be particular representations of these forms. Since such objects are only considered for the forms they represent, the conclusions we draw from considering them necessarily apply to these forms. As pure intuition makes the subject certain of the real possibility of the concepts of pure forms, it simultaneously makes him certain of the possibility of constructing appropriate empirical objects for this task. But, how can the subject be certain that the objects he effectively constructs have the form he wants them to have, that they are particular representations of pure forms? Moreover: how can the subject be sure that, in considering them, he refers only to the properties that make them particular representations of pure forms? For Kant, the guarantee of all that is again pure intuition[14]. Thus, a new shift occurs: intuition now becomes a guarantee of the correspondence of certain objects and procedures to the concepts of pure forms.

If we accept that pure intuition provides the previous guarantees, we have to conclude that it enables a subject to justify judgments about pure concepts by leaving these concepts, but without referring to anything as pure objects. These judgments are then synthetic, but since they concern the concepts of pure forms and use objects only as they are constructed according to the concepts of these forms, they are *a priori* and apodeictic too. Finally, since the pure forms are nothing but possibilities of positioning in spatio-temporal order, they are part of the pure science of space and time, namely mathematics. Hence, they are mathematical judgments[15].

IV.3 Kant's Distinction

If I am not mistaken, Kant's distinction between analytic and synthetic judgments refers only to non-problematic judgments, that is to statements, it concerns judgments as forms of knowledge, and it is, as such, independent of the particular

logical form of these judgments. Thus it can finally be formulated in the following way:

$(D_3)^*$ As long as an act of knowledge consists in the act of "making out" a statement of the form $< A(P, Q, ..., S) >$—where the concepts $P, Q, ..., S$ occur—it is analytic if and only if at least one of these concepts is non-elementary and the act itself is founded on nothing but the ascertainment of the logical relations occurring between the concepts $P, Q, ..., S$, according to the way in which the non-elementary concepts which take place among them has been produced by a synthesis of understanding; it is synthetic, if and only if it asks for an appeal either to the experience of some objects—according to the way in which these objects fall under the concepts $P, Q, .., S$—or to some guarantee provided by pure intuition.

V Kant's Ontologism

According to Kant, an act of knowledge, consisting in an act of "making out" a statement, can only be grounded on: *i*) the ascertainment of the logical relations that take place between certain concepts, according to the way in which the non-elementary ones are produced by a synthesis of understanding, *ii*) the experience of some objects, *iii*) an appeal to some guarantee provided by pure intuition. Hence, every act of knowledge of this sort is either analytic or synthetic, according to $(D_3)^*$.

However, this is no complementarity between analytic and synthetic statements yet. For the domains of analytic and synthetic statements to be complementary, it is also necessary that no statement is analytic and synthetic at the same time. Certainly $(D_3)^*$ only satisfies such a condition if we accept that no concept can be considered as an object. This is definitely Kant's idea, since for Kant, concepts and objects are essentially different entities or forms of representation. For him, the distinction between objects and concepts is not a logical one; it does not concern logical roles, but the intrinsic characters of these entities: a concept could never be intended as the cause of a certain sensation, as the subject represents it to himself. However, it seems that Kant always wants to maintain something as a correlation between concepts and objects: even though understanding is able to realize any sort of synthesis of concepts already given, we cannot say that the result of this synthesis is a genuine concept if we cannot say that it refers in same way to one or more objects.

In order to satisfy both conditions, Kant should provide a non-relational characterization of two sorts of entities (concepts and objects) that he wants to intend as essentially correlative. This is the root of many difficulties in Kant's philosophy, I think. The first condition seems to be satisfied if we understand the object as a sort of specification, of a "reality" that is already given in confused terms, realized by means of concepts. Such a reality—which we have to take as absolute first—provides the object with its intrinsic and irreducible nature (contrary to the mental or, if you like, discursive nature of the concept), without denying its correlative character with respect to the concept. Thus it seems that the second condition also can be satisfied regarding the dependence of the object on the concept. But, what about the dependence of the concept on the object? To guarantee it, we have to assume that a concept is one only if it provides a characterization of an object—intended, as I just said, as something that participates in the primitive reality. If this is not the case, we do not really have a concept, but only an arbitrary synthesis of understanding: understanding—by means of imagination[16]—puts together different concepts without really producing a new concept. But this solution is very weak if we do not think that the primitive reality can act, as such, on imagination, while the latter produces its synthesis. Now, even though we could find the means for expressing such a condition, without denying the apriority of the formal conditions of knowledge, we again have the problem of distinguishing guided imagination, which produces real concepts, from completely arbitrary imagination, which produces nothing but an empty synthesis of concepts. Certainly, we cannot do it by referring to primitive reality itself, since it is inaccessible. Thus the problem arises: how can we do it?

Still, if an object is nothing but the cause of a certain sensation, and knowledge is always concerned with objects (even though it is not necessarily an experience with certain objects), as Kant believes, then knowledge is always concerned with the subject's representation of the causes of his sensations: either knowledge is directly such a representation or a judgment connecting in some direct or indirect way different representations of this sort, or it is something like an expression of the conditions of possibility of these representations or connections. But, if a knowledge of the latter sort is not directly about objects, about what is it directly? To give but one example: about what is, in Kant's views, a judgment concerning triangles as such? In a sense, it is about the objects that are particular manifestations of triangles, but certainly it is not directly about them. The correct answer is certainly not that such a judgment is about pure forms, since then the question arises: what is a pure form, if it is neither an object nor a concept? and, if it is a concept, of which object is it the concept?

I am not able to find any satisfactory (that is not merely metaphoric) answer to these questions in Kant's philosophy.

The difficulty even grows if we consider it given the background of the leading principle of Kant's theory of knowledge, which is not only the (Platonic) idea that there is no knowledge without justification, but the stronger precept, according to which any theory of knowledge is a theory of justification, as a guarantee of the

validity of the knowledge itself. On this background, the previous questions become really essential: what is a possible justification (or what is the form of a possible justification) of *a priori* knowledge? Such a question is so fundamental in Kant's framework that it cannot be evaded. Nevertheless Kant's answer is really *ad hoc*: a possible justification of such a sort of knowledge consists in an appeal to pure intuition. The problem of this answer is not simply that it is not really an answer to the question—being rather an answer to another question, namely: where could a possible justification of *a priori* knowledge be found?—but that it is either circular or metaphoric. In Kant's philosophy, pure intuition is nothing but the guarantee of the possibility of *a priori* knowledge (Frege 1884, § 12)—or even the genus under which any guarantee of this sort falls down.

Now, it seems to me that such a difficulty does not depend on a limit in Kant's elaboration of transcendental philosophy of knowledge. Rather, I think that it depends on the premises of this philosophy themselves: *i*) the idea that knowledge is always concerned with the subject's representations of the cause of his own sensations; *ii*) the conception of a theory of knowledge as a theory of justification, as a guarantee of validity of the knowledge itself.

I suspect that no satisfactory theory of knowledge is possible on grounds of these premises. Moreover, I think that Kant inherited these premises from the ontological tradition of empiricism. According to the first, knowledge is something like a human interpretation and connection of a number of original facts that are sensations, while, according to the second, a theory of knowledge is something like a general scheme of reduction of any act we want to intend as an act of knowledge either to these facts as such, or to the original conditions of possibility of their interpretation or connection.

VI Analytic and Synthetic Acts of Reasoning

In contrast to the above, I think that from a philosophical point of view a certain act is an act of knowledge if and only if it has a certain logical form, and a theory of knowledge is nothing but a theory of this form as such. Briefly speaking, I think that an act of knowledge is either an act of exhibition of an object or an act of attribution of properties or relations to objects. However, I think that not every act of attributing properties or relations to objects is an act of knowledge. In order to be an act of knowledge, such an act has to satisfy two conditions: first, the objects to which properties are attributed must be already exhibited as such (and be present as such to the subject that attributes properties or relations to them); second, such an attribution has to be a consequence of an analysis of the objects themselves. Generally, the act of attributing properties or relations to objects by grounding on an analysis either of the objects themselves or of the concepts of these objects, property or relation is, in my views, an act of reasoning, and an act of reasoning is an act of knowledge if and only if this attribution depends on (is locally justified by) the analysis of the objects themselves. An act of reasoning of this sort is synthetic[17], while an act of reasoning is analytic when the attribution solely depends on the analysis of the concepts of these objects, properties or relations . A non-elementary act of knowledge is then a synthetic act of reasoning.

As long as we accept the idea that any act of attribution of properties or relations to objects is expressed (or could be expressed) by a statement, such a distinction can be formulated as follows:

(D_4) An act of reasoning expressed by the statement $A(P, Q, ..., S, a, b, ..., d)$ —where the (monadic or polyadic) predicates P, Q, ..., S and the names of objects a, b, ..., d occur—is analytic if and only if it is grounded on nothing but the analysis of the concepts of these predicates or objects; it is synthetic, if and only if it is grounded on the analysis of at least one of the objects that fall under these concepts.

Since for me an act of reasoning is an act of attributing properties or relations to objects, by analyzing either these objects themselves or the concepts of these objects, properties or relations, any act of reasoning is then either analytic or synthetic, and cannot be both. However, this is not merely a question of definining the term "act of reasoning". I think that a subject can only analyze objects or concepts. Thus an act of attributing properties or relations to objects is either an act of reasoning, or it is not grounded on an act of analysis, or it is finally grounded on the analysis of other objects or concepts. And it seems to me that, if we are speaking about science, we are interested only in the first sort of acts of attributing properties or relations to objects.

Moreover, even though the terms "reasoning" or, *a fortiori*, "act of reasoning" are not, as such, Kantian ones, and must not be confused with the term "inference" used by Kant as referring to logical forms of acts of reasoning in my sense (Kant JL, part I, ch. 3)[18], it seems to me that what is interesting in Kant's distinction between analytic and synthetic judgments or statements is that such a distinction does not deal with statements merely intended as linguistic objects, but with the acts of formulating judgments. In this sense, my distinction between analytic and synthetic acts of reasoning fits perfectly with the spirit of Kant's own distinction.

However, it essentially differs from this distinction for a number of other reasons, the main ones of which are concerned with the notion of object. Since for Kant an object is nothing but the cause of a sensation, as the subject represents it to himself, Kant cannot generally intend a judgment as an act of attributing properties to objects. Moreover, he cannot accept the idea that a synthetic judgment is an act of attributing properties to an object by grounding on the analysis of the object itself, since, if he accepted this criterion, he would deny the possibility of any sort of synthetic *a priori* judgment.

It is clear that, according to (D_4), an act of reasoning can be both synthetic and *a priori* only if we accept the idea that there are pure objects. In the following parts of my paper, I will try to justify this possibility by basing myself on a radically non-Kantian notion of object, in order to defend the following thesis:

(T_2) What makes an act of reasoning a mathematical one is that it is grounded on the analysis of mathematical objects; thus mathematical acts of reasoning are synthetic in the sense of (D_4). Moreover, as mathematical objects are pure objects, mathematical acts of reasoning are not only synthetic, but they are also *a priori*.

VI.1 Objects

According to Kant, an object enters the subject's horizon when it is properly constituted by the subject himself. Hence, there is no object for Kant which has not entered the horizon of a subject. Furthermore, an object can enter the horizon of a subject only if a sensation occurs. The act of constitution of an object is then an act of interpretation of a fact that necessarily has to be intended as preceding the appearance of such an object within the subject's horizon. However, this fact cannot be described, in Kant's framework, without referring to such an appearance, and it is even thought only as its source. Thus, in order to say what an object is, Kant has to refer to something that he can think and represent to himself only as the original source of the object itself.

In my opinion, such a situation is unsatisfactory. If we want to avoid such a difficulty (without returning to the idea that objects subsist as such, independent of any act of the subject), we have to give up the idea that an object is the result of an act of interpretation of a fact. Elsewhere, I defined an object as the meaning of the argument of a predication, intended as an intentional act (Panza 1995*b*, 116). I believe of course that this is a good definition, but it is in a sense *a posteriori* with respect to the advent of the object itself in the horizon of the subject. If we imagine that the object is already there, we can use such a definition for characterizing it. Here, I would like to advance a genetic definition: an object is the intentional content of an act of exhibition; an object is exactly what a subject is exhibiting when he wants to exhibit something, he does it, and he recognizes his act just as the act of exhibiting this something. An act of predication (intended as an intentional act) is either addressed to an object that has already been exhibited, or it is part of the act of the exhibition itself. An object that has already been exhibited is not continuously present to the consciousness of the subject. Rather, I would say that an object has already been exhibited when the subject is able to represent it to his consciousness—to evoke it—by using any conventional symbol that is generally intended as the name of the object itself. If this is the case, the object is also able to attribute any property to it, without coming back to the act of exhibition as such. Therefore I say that the object is already present in the subject's horizon.

If we accept such a point of view, the particular nature of an object depends on the particular nature of the act of exhibition itself. Moreover, only logical differences are important here. We cannot distinguish empirical from pure objects by saying that the act of exhibiting the first ones is connected in some way with the occurrence of a sensation. If we do that, we fall back on the previous difficulty. If we want to avoid it, and also avoid any heritage of classical ontological empiricism, we have to use the notion of empirical object to explain what a sensation is, and not *vice versa*.

Perhaps we could do that, by referring to the ostensive character of certain acts of exhibition, but then we have to know what an ostensive act is, before knowing what an empirical object—and *a fortiori* a sensation—is, and I am not sure that this is really possible. Thus, I prefer to pursue a different strategy. In order to present it, I have to introduce the notion of concept.

VI.2 Concepts

First of all, we could intend a concept as the subjective function that enables the subject to identify an object as such, to exhibit it to himself. This characterization does not apply to any sort of concept: the concepts to which it applies are concepts of objects, rather than concepts of properties or relations. From a logical point of view, we could say that a subject possesses such a capacity if and only if he disposes of a criterion of identity and he is able to apply it both to the contents of different acts of exhibition and to the objects evoked by a certain name (or in another way). Hence, we could say that a subject possesses a concept of an object if and only if he disposes of such a criterion and he is able to apply it.

A concept of a (monadic) property is the subjective function that makes the subject able to assign an object that has already been exhibited to a certain class of objects. From a logical point of view, we say that a subject possesses such a capacity—and then the corresponding concept—if and only if he disposes of a criterion for that, and is able to apply it to any sort of objects, by concluding either that they have or do not have to be assigned to that class, or that their modalities of exhibition do not enable him to decide if they have or do not have to be assigned to that class. If a subject possesses such a capacity, he possesses the concept.

Finally, a concept of a relation is the subjective function that makes the subject able to assign a certain class of objects that have been already exhibited and assigned to such a class, to a certain class of classes of objects. In this case too, a subject possesses such a capacity—and the corresponding concept—if he disposes of a criterion for that, and is able to apply it in order to obtain one of the three issues I have just indicated for concepts of properties.

Let us first consider concepts of objects. According to the previous characterizations, a subject cannot exhibit an object to himself, without possessing the concept of this object, since a subject realizes an act of exhibiting something to himself only if he recognizes one of his acts as an act of exhibiting something to himself and he is able to do it, only if he is able to distinguish his act of exhibition from any other act. Moreover, as no object can be exhibited to any subject if this subject does not ultimately exhibit it to himself, we can conclude that no object can be exhibited to any subject if this subject does not possess the concept of this object. Analogously, a subject cannot possess a concept of a certain object without exhibiting such an object to himself, since he cannot possess a criterion of identity if he does not represent such a criterion to himself, and he cannot represent it to himself without representing to himself the content of an act of exhibition that satisfies this criterion itself. The object of this concept then is present in the horizon of the subject himself, that is: it has been exhibited to him. Thus, the important difference is not between empty and full concepts, but between objects that are preceded by their concepts and objects that precede their concepts. In my view, the objects of the first kind are pure, while those of the second kind are empirical. In the first case—the case of pure objects—the act of exhibition consists in the presentation of the concept itself, the effort of formulating the corresponding criterion. In the second case—the case of empirical objects—the corresponding criterion acts before having been formulated; its formulation is only a *post festum* description of a capacity we have already applied. Of course, we can try to transmit to someone—a child, for example—the concept of Venus by showing a certain star to him and ask him to recognize Venus. But the child really exhibits Venus to himself only when he changes his concepts: Venus is not a star such and such; it is exactly the star the child has finally exhibited to himself. Starting from this moment, the name "Venus" has a new meaning for him, it does not evoke a star such and such (what is, according to me, a pure object), it evokes just the star that he has exhibited to himself at such an occasion.

Consider the concepts of properties or relations. Even though these concepts are not concepts of objects, but ask for a previous exhibition of certain objects, we can put them together in order to produce concepts of objects (which are certainly pure). Moreover, according to the previous definition, any concept of a property or a relation corresponds to the class of objects or classes that are formed by using the corresponding criteria. These classes are certainly objects, but their concepts are not the concepts of the property or the relation to which they correspond, according to the previous definition. The concepts of these objects are just the concepts of these classes taken as objects. Thus, it is perfectly possible to possess the concept of a property or a relation without exhibiting the corresponding classes as such, which are generally open classes and could even be empty.

Let us now come back to the concepts of objects. According to my definitions, concepts of object and objects are two logical categories correlated to one another: just as much as an object could be intended as the correlate of a concept of an object, a concept of an object could be intended as the correlate of an object. Now, if we intend objects and concepts of objects in such a way, we must also intend an act of exhibiting an object as a public act, when it consists in a certain subject's effort to transmit the possession of a concept of a certain object to other subjects. Thus, an object has been exhibited publicly when the capacity of identifying it has been transmitted to a number of subjects. Hence, according to the previous definition, an object that has publicly been exhibited cannot be an empirical object.

Of course, not only the possession of concepts of objects can be transmitted in such a way; the same is true for concepts of properties and concepts of relations. However an effort to transmit these concepts is not a public act of exhibition of the classes (of objects or classes) corresponding to these concepts.

To transmit a concept we use language. Thus, when we study public phenomena, like science, we can intend a concept, by extension, as a linguistic characterization of an object, a property or a relation (that is the aspect of an object or a class of objects that make this object or class the members of a certain class). From my point of view, such a characterization has not to be intended as the discursive transposition of the properties of the object, or the conditions of the property or the relation—which is close to the Leibnitzian notion of complete concept. As a matter of fact, complete concept is only an abstract notion, grounded on ontological presuppositions, and it cannot be used as such in a theory of human knowledge. A linguistic characterization of an object, a property or a relation rather has to be intended as a way to fix these entities in our discourse, and in this sense it is only a sort of "concrete" representation of the concept (as a subjective function), an "exposition" or "explication" of it. This representation cannot in general be a complete characterization of the object, the property or the relation, but it is only a means for transmitting certain capacities in a community of subjects.

As a representation, such a characterization is not a representation of an object, a property or a relation, it is just a representation of their concepts. But, if these concepts are linguistically represented, they are *ipso facto* exhibited and thus, they are objects, even though they are certainly not the objects of themselves or the classes corresponding to themselves. So, when it has been presented as such, any concept is an object, even it is certainly a pure object.

VI.3 Analysis of Concepts, Analysis of Objects

I am now able to explain what I mean when speaking of an act of reasoning grounded on the analysis of the concepts of the objects to which such an act is attributing properties or relations, that is an analytic act of reasoning. This is an act of reasoning where such an attribution is a consequence—according to certain rules of inference—of nothing but the concepts of the objects, the properties or the relations, intended as linguistic characterizations of them.

The acts of reasoning expressed respectively by the statement $< P(a) >$ and $< R(b, c) >$ are for example analytic if and only if these statements are consequences of nothing but the concepts of the property P and of the object a and of the relation R and of the objects b and c, respectively.

The situation is a bit more complicated for an act of reasoning expressed by a statement like $< \forall x[S(x) \Rightarrow P(x)] >$[19]. In order to consider such a statement as an expression of an act of reasoning, we have to intend it as the attribution of the property P to some objects. I have said that it is possible to possess the concept of the property S without having exhibited the class of the objects which are S. Thus, either this statement—as long as it is intended as an expression of an act of reasoning—attributes the property P to all the objects which have been already exhibited and are S, or the concept S in it is taken as a concept of an object rather than a property, or finally it attributes properties to potential objects, that is the objects which could be elements of the class connected to the concept S. In the first case, such a statement is only an abbreviation of a conjunction of statements of the form $< P(a) >$, and the corresponding act of reasoning is analytic if and only if all the acts of reasoning corresponding to these statements are analytic. In the second case the statement $< \forall x[S(x) \Rightarrow P(x)] >$ is not universal and is rather equivalent to only one statement of the form $< P(a) >$ and the corresponding act of reasoning is analytic under the conditions of analyticity of the act of reasoning corresponding to such a statement. Finally in the third case, the act of reasoning corresponding to the statement $< \forall x[S(x) \Rightarrow P(x)] >$ is analytic if the attribution of the property P to all the objects, which could be elements of the class connected to the concept S, is a consequence of nothing but the concepts of the properties S and P.

Still, a statement like $< \exists x\ [P(x)] >$ expresses an act of reasoning in my view, only if it is a logical consequence of a statement of the form $< P(a) >$, and such an act is analytic if and only if the act of reasoning expressed by this statement is analytic.

Here the term "consequence" has to be conceived in its general sense: the analyticity of an act of reasoning depends on the rules of inference that characterize what a consequence of a certain linguistic characterization is. What is important obviously is that these rules do not depend on the exhibition of the objects connected to the considered concepts.

An act of reasoning is grounded on an analysis of the objects to which it attributes properties or relations, and it is then synthetic, if such an attribution depends on the exhibition of these objects as such (that is: if it cannot be realized if such an exhibition is not realized). As any exhibition of an object asks for the possession of its concept, a subject which does not possess the concept of certain objects can certainly not realize a synthetic act of reasoning by attributing properties or relations to these objects themselves. However, even though such a condition is necessary, it is not sufficient.

Thus, the acts of reasoning expressed, respectively, by the statements $< P(a) >$ and $< R(b, c) >$ are synthetic if and only if the attribution of the property P and the relation R respectively to the object a and the objects b and c depends on the exhibition of these objects.

An act of reasoning expressed by a statement like $< \forall x[S(x) \Rightarrow P(x)] >$ could then be synthetic only if such a statement is intended in one of the first two ways I just mentioned. In the first case it is synthetic if and only if one of the conjuncts of the statement of which it is an abbreviation is synthetic. In the second case it is synthetic if and only if the equivalent statement of the form $< P(a) >$ is synthetic.

Finally, an act of reasoning expressed by a statement like $< \exists x\ [P(x)] >$ is synthetic if and only if such a statement is a logical consequence of a statement of the form $< P(a) >$, which expresses a synthetic act of reasoning.

In order to apply the previous definitions to particular acts of reasoning, we have to understand both what an act of exhibition of an object occurring in these acts could be, and how an attribution of properties or relations to such an object could depend on the exhibition of this object itself. As my sole aim is to justify thesis (T_2), I may limit myself to the case of mathematical objects. Therefore, I have to explain what a mathematical object is; why it is pure; and how is it possible to formulate a (synthetic) act of reasoning about it, by grounding on its exhibition. This is now my task.

VII Naive Formalism and Conceptualism

There is a first and very radical objection to (T_2) that we can formulate without having any idea of what a mathematical object could be. One could argue that the object-concept dichotomy does not provide suitable categories for speaking about a formal system, as modern mathematics ultimately is: in a formal system there are no concepts at all and if there are objects, they are only symbols (that is terms of a net of rules). Thus, even though (T_2) is possibly right with respect to pre-modern mathematics, it is certainly wrong for modern mathematics.

I think there is a deep misunderstanding in such an objection. Such a misunderstanding has been denounced a number of times and it is not necessary here to insist on it. In my opinion, it consists in confusing (modern) mathematics as such with a certain collection of axiomatic systems intended as systems of sentences deduced by means of a number of explicit rules of inference starting from a number of axioms, which are in their turn simply intended as the starting point of deduction. I believe this is just a confusion. Even if we accepted the idea that without axiomatic systems intended in this sense, there is no mathematics (which compels us to conclude that a great part of what has historically been and is nowadays indented as mathematics, is not mathematics), we should add that an axiomatic system, intended in such a way, is only a tool for mathematicians. Not only such a tool has to be constructed, and its construction has to be intended as an important part of the work of mathematicians (that is mathematics as an activity), but even when a mathematician disposes of it, he looks for deductions in it only in order to produce proofs of theorems. And theorems as such are not part of this system.

What is then a theorem? Let us take a very simple example. Even though we can exhibit a deductive path that starts from Peano's axioms and usual definitions of Peano's arithmetic and arrives at the sentence $< 7 + 5 = 12 >$, still the theorem a mathematician wants to prove by exhibiting such a path does not consist in such a sentence. It is rather expressed by the statement < the number '12' is the result of addition of the number '7' and the number '5' >: the symbols "7", "5" and "12" are not simply symbols introduced by the usual definitions; for a mathematician they are names of numbers.

Nevertheless, even if I was right on this point, (T_2) could still be wrong. The statement < the number '12' is the result of addition of the number '7' and the number '5' > could be interpreted as a statement of the form $< \forall x[\{7{+}5\}(x) \Rightarrow 12(x)] >$ or $< \forall x \forall y[(\{7{+}5\}(x) \wedge 12(y) \Rightarrow =(x, y)] >$, attributing the property '(to be) 12' to potential objects which could be elements of the class connected to the concept '7+5' or the relation of equality to the potential objects which could be elements of the classes connected to the concepts '7+5' and '12', respectively. If this has been a correct analysis, and we intended such a statement as the expression of an act of reasoning in my sense, we should conclude, according to my definitions, that such an act is analytic. Thus one could accept all my definitions, agree with me that a mathematical statement is the expression of an act of reasoning and still argue that (T_2) is wrong.

A suitable framework for such a position is implicit in those philosophical conceptions with regard to mathematics that are generally arranged under the term "conceptualism". According to such a point of view, a mathematical theory is nothing but a relational structure of concepts.

A new version of conceptualism has recently been proposed by L. Tharp (Tharp 1989-1991). Tharp wants to avoid all problems of the referential view of mathematics from the very beginning, by putting forward the idea that mathematical assertions should be regarded as expressing relations among concepts instead of among objects. Tharp explains the claims of his conceptualist position by a comparison to fiction. As an illustration of the role of fiction in his argument, Tharp presents a very short story.

> "The only people in our story are Gertrude and Hamlet. Gertrude is a queen. Hamlet is a prince, and Gertrude is Hamlet's mother. [...] Given these two stipulations which constitute our story, various consequences follow from the meanings of the concepts 'prince', 'queen' and 'mother', and are evidently true-in-the-story: for example no princes are queens; Gertrude and Hamlet are distinct; Hamlet is not Gertrude's mother. None of these conclusions follow logically from the given story, however. Also, the first conclusion doesn't even use our stipulations. Although the stipulations are largely arbitrary, once they are fixed, the consequences of those stipulations are thereby fixed." (*ibid.* part I, 168-169)

Now, according to the conceptualist point of view, the previous conclusions can be intended as expressing relations between concepts. These concepts do not subsume objects or relations between objects under them and thus it is not possible to go away from them in order to get out any conclusion. So, the questions to which we can found an answer in the story are previously delimited. We cannot ask for instance, what color Hamlet's eyes have or what his mother Gertrude weighs: Hamlet and Gertrude are not objects to which we have access.

This is Tharp's position. But, if we want to refute (T_2), after having accepted all my definitions, we are not compelled to accept this position. We could deny that there is something like a concept without objects and argue that any concept, even if it is taken as a concept of a property or a relation, corresponds to actual or potential objects. We should simply state that all the concepts that occur in mathematical acts of reasoning are concepts of properties or relations that do not correspond to objects that have already been exhibited, but only to potential objects that will never be exhibited in mathematics. I am not sure whereas this position is tenable, but since we can always retreat to Tharp's more solid position, there is no need for me to criticize it as such.

In order to make my point clear, I will consider an example different from the "discursive" example of Tharp. I am taking it from an excellent forthcoming paper by R. Casati (Casati fc) where the author tries to "capture some of the intuitions regarding absolute rest and motion", as they work in Newton's conception of space, by presenting an axiomatic system founded on two axioms:

(A_1) If x and y are at absolute rest, then x is at rest relatively to y $(\forall x, y[(R(x) \wedge R(y)) \Rightarrow \mathcal{R}(x, y)])$.

(A_2) If x is at absolute rest and y moves, then x moves relatively to y $(\forall x, y[(R(x) \wedge \neg R(y)) \Rightarrow \neg\, \mathcal{R}(x, y)])$.

From these two very simple axioms and using usual rules of first order predicate calculus, Casati draws a number of "theorems" that describe Newton's point of view.

Here, the variables x and y stand for bodies, but clearly this is not essential for the success of the deduction. In fact x and y could simply stand for any sort of potential objects that are or not R and are or are not between them in the relation $\mathcal{R}$. Moreover, the deduction is also independent of the particular interpretation which Casati advances for the predicates R and $\mathcal{R}$. This interpretation is only responsible for the fact that Casati's axiomatic system captures or does not capture the Newtonian conceptions. Really, (A_1) and (A_2) work simply as Carnap's meaning postulates, fixing non-logical relations between certain predicates (Carnap 1952).

Of course my point is not concerned with the analyticity or syntheticity of (A_1) and (A_2), since I am interested in the analyticity or syntheticity of acts of reasoning and, if they are intended as meaning postulates, these statements do not express acts of reasoning. They are merely stipulations. Moreover, if these stipulations are intended as explicit expressions of the relations that take place between the concepts of the property R and the relation $\mathcal{R}$ the acts of reasoning expressed by the theorems deduced from (A_1) and (A_2) are clearly analytic. If these stipulations are not intended in such a way, they have to be taken as arbitrary stipulations and so the theorems deduced from them are consequences of the new Casatian concepts of the property R and the relation $\mathcal{R}$. Once again the acts of reasoning expressed by these theorems are then analytic.

My point should be clear now: if a mathematical theory is expressed by an axiomatic system like the previous one, no mathematical theorem can ever express a synthetic act of reasoning and thus (T_2) is certainly wrong. Hence, in order to justify (T_2), I have to argue that a mathematical theory is not expressed by a system of axioms like the previous one. This is my thesis: in my view, a mathematical theory deals with actual pure objects and not only with concepts of properties or relations.

VIII Madame Bovary as a Pure Object

According to my definitions, the notion of pure object is not problematic as such. We can exhibit a number of pure objects, such as Madame Bovary, the first mover, the concept of analyticity, the property '(to be) red', or the number '3'. Each of them corresponds to a particular way of exhibition, which characterizes its specific nature and which is different in each case. Of course I am not concerned here with these ways as such. My question is a different one: is it possible to ground ourself on a pure object as such (rather than on its concept) for realizing an act of reasoning?

Let us first consider the example of a pure object, which is certainly not a mathematical object, like Madame Bovary. As the corresponding concept (intended as a discursive characterization of such an object) let me take everything Flaubert says about Madame Bovary in his novel. Are we able to derive from it, and from our background knowledge, some other knowledge about Madame Bovary? There are good reasons for answering "yes", as well as "no". I do not intend, here, to discuss these reasons. For my task, it is sufficient to remark that, even if we answer "yes"—by asserting, for example, according to Tharp, that in our background knowledge the concepts occurring in the complex concept of Madame Bovary are connected to other concepts that do not occur directly in it (in such a way that we can surely conclude, for example, that Madame Bovary was not able to write with "Word 5" on a Macintosh PowerBook 145)—we have to maintain that there is no way for grounding our act of reasoning on Madame Bovary as an object. We can derive new knowledge only by considering the concept of Madame Bovary. As an object, Madame Bovary is a purely semantic entity: not only is it the correlate of its concept, but there is no way for having access to it that is not a way for having access to its concept.

You might think that this is the situation with every pure object: the act of exhibiting a pure object really consists in presenting its concept, and you might think that this makes it impossible to have a way of having access to it that is not a way for having access to its concept.

Let us consider this point. According to my definition, a pure object is a semantic entity. In a sense, such an object is only logically different from the corresponding concept: it is only an entity that has a different logical role than the concept. As the act of exhibiting it consists in presenting the concept, and as this presentation is nothing but a linguistic performance, a pure object could be intended as the reference of a linguistic term. The act of exhibition of the concept itself fixes such a reference, and a number of linguistic (and pragmatic) conventions makes a community of subjects able to recognize it as the meaning of the same term in different linguistic contexts. Thus, we have finally to conceive a pure object as the reference of this term, when it is used in certain contexts that a community of subjects is able to recognize.

Marco Santambrogio recently clarified the idea of a non-Fregean (that is non-distributive) notion of reference, and attached a very powerful theory of abstract objects to it (Santambrogio 1992). According to him, we can intend the reference of "certain parts of discourse" as "their contribution to the truth or falseness of the statements in which they appear" (*ibid.*, 144). In this sense, the reference of the terms "Madame Bovary" or "triangle" in the statements < Madame Bovary killed herself by ingesting arsenic > or < the sum of the internal angles of a triangle is π > is that which makes these statements true.

Santambrogio's idea enable us to formulate the problem in a new way: what makes the statements < Madame Bovary killed herself by ingesting arsenic > or < the sum of the internal angles of a the triangle is π > true? Certainly this is a pure object, but: how does this object make these two different statements true? My thesis is that the answer is different in the two cases.

When claiming, in the first case, that the statement < Madame Bovary killed herself by ingesting arsenic > is made true because of the object 'Madame Bovary' rather than because of its concept, we are saying merely that this statement is literally true only if it refers to the object 'Madame Bovary', rather than to its concept: it is really not the concept of Madame Bovary that killed itself in this or in another way; concepts do not kill themselves. However, it is true that Madame Bovary killed herself by ingesting arsenic only because this is said in the presentation of the concept of Madame Bovary and we know it only because we have read this presentation. In other words: here the object 'Madame Bovary', even though it is evoked by its proper name, works ultimately as an object that satisfies the property '(to be) Madame Bovary', when such a property occurs in a meaning postulate like this:

(A3) $\forall x[\text{Madame-Bovary}(x) \Rightarrow \text{killed-herself-by-ingesting-arsenic}(x)]$

Thus, there is no way to realize a synthetic act of reasoning about Madame Bovary.

IX Euclidean Geometry

IX.1 Kant, Once Again

Let us now consider the case of the triangle. Here is what Kant says about the proof of the Euclidean theorem on the internal angles of a triangle:

> "Suppose a philosopher be given the concept of a triangle and he be left to find out, in his own way, what relation the sum of its angles bears to a right angle. He has nothing but the concept of a figure enclosed by three straight lines, and possessing three angles. However long he meditates on this concept, he will never produce anything new. He can analyze and clarify the concept of a straight line or of an angle or of the number three, but he can never arrive at any properties not already lied on[20] these concepts. Now let the geometrician take up these questions. He at once begins by constructing a triangle. Since he knows that the sum of two right angles is exactly equal to the sum of all the adjacent angles which can be constructed from a single point on a straight line, he prolongs one side of his triangle and obtains two adjacent angles, which together are equal to two right angles. He then divides the external angle by drawing a line parallel to the opposite side of the triangle, and observes that he has thus obtained an external adjacent angle which is equal to the internal angle—and so on. In this fashion, through a chain of inferences guided throughout by intuition, he arrives at a fully evident and universally valid solution of the problem." (Kant A, 716-717; B, 744-745)

If we read such a passage in the light of the previous non-Kantian notion of object, it seems to suggest that the Euclidean proof does not deal with the concept of triangle, but with the triangle as an object. Two reactions are quite natural in face of such a thesis. The first is typically philosophical and consists in asking, in a skeptic voice: "what is it, the triangle as an object?" The second consists in recognizing that classical procedures in elementary geometry, as that described by Kant, seem to be concerned with something essentially different from the logical rules of inference we use in performing analytic acts of reasoning started by meaning postulates.

Kant's idea seems to be made clear in the following parts of the first section of chapter one of the *Transcendental Doctrine of Method* (from which I have taken the previous quotation), where the notions of definition, axiom and proof are discussed (*ibid.* A, 727-738; B, 755-766)). According to Kant, definitions are possible only in mathematics:

> "To *define*, as the word itself indicates, really only means to present the detailedly complete [*ausführlichen*], concept of a thing [*Ding*] originally [*ursprünglich*][21] within its limits [*Grenzen*]." (*ibid.* A, 727; B, 755)

The definition is then made precise in a footnote:

> "Detailed completeness[22] means clarity [*Klarheit*] and sufficiency of characteristics [*Zulänglichkeit der Merkmale*]; by limits is meant the precision [*Präzision*] shown in there not being more of these characteristics than belong to the detailed complete concept; by *original* is meant that this determination of these limits is not derived from anything else, and therefore does not require any proof [*Beweis*] [...]." (*ibid.*)

Now, all this is possible only if the concept is "arbitrarily thought [*willkürlich gedacht*]" (*ibid.* A, 729; B, 757)[23], but the presentation of a concept that is "arbitrarily thought" is the definition of a "true object", only if such a concept "contains an arbitrary synthesis that admits of *a priori* construction [*welche a priori konstruiert werden kann*]" (*ibid.* A, 729; B, 758), and this is possible only in mathematics.

Here Kant seems to come very close to the idea that mathematics deals with pure objects. This is what he writes next:

> "[...] mathematics is the only science that has definitions. For the object which it thinks it exhibits [*stellt*] *a priori* in intuition, and this object certainly cannot contain either more or less than the concept, since it is through the definition that the concept of the object is given—and given originally, that is, without its being necessary to derive the definition from any other source." (*ibid.* A, 729-730; B, 757-758)

This passage seems to be quite unambiguous: if the object that "mathematics thinks" and "exhibits in intuition" was the particular empirical figure we trace on a sheet of paper or a blackboard when we repeat the Euclidean proof, Kant should

certainly not say about it that it "cannot contain either more or less than the concept".

Nevertheless, after this passage, Kant insists generically on the "construction of the concept", rather than on the presence of a pure (mathematical) object. He distinguishes philosophy from mathematics by saying that the former is only able to "expose" or "explain" given concepts—by performing their definitions, analytically—while the latter "constructs" concepts "originally framed"—and performs their definitions synthetically (*ibid.* A, 730; B, 758).

Of course, what Kant means by "exposition" is not a kind of description of something that is already given as such, and what he means by "construction" is not the act of providing it originally. If it was so, it would be very easy to reply that philosophy constructs its concepts too (where could it take them from, otherwise?) and even advances by constructing further and furher concepts, while mathematics accepts its definitions and merely deduces theorems from them. However, Kant's point is not to deny it. The "exposition of concepts" in Kant's sense is perfectly compatible with their construction in the previous sense, just like their "construction" in Kant's sense is compatible with their exposition in the previous sense. What is important for Kant is that no philosophical construction (in the previous non-Kantian sense) can produce anything but concepts: it is, and it cannot be anything but an exposition of concepts (even though these concepts are new). What is exhibited in such a construction is nothing but concepts, and thus such a construction is nothing but an "exposition of concepts". Mathematical definitions in contrast exhibit objects, and not merely concepts. Thus, for Kant, the construction of concept is just the access to the object, as in the seventeenth century the construction of equations was just the exhibition of the mathematical object expressed by its roots (Bos 1984). This point seems to be made clearly by Kant, not only in the previous passage, but also in his discussion of axioms and proofs in mathematics:

> "Mathematics [...] can have axioms, since by means of the construction [*Konstruktion*] of concepts in the intuition of the object it can combine the predicates of the object both *a priori* and immediately [...]." (*ibid.* A, 732; B, 760)
>
> "[...] mathematics can consider the universal *in concreto* [*das Allgemeine in concreto*] (in the singular[24] intuition) and yet at the same time through pure *a priori* representation [...][it realizes] *demonstrations* [*Demonstrationen*], which, as the term itself indicates, proceed in and through the intuition of the object." (*ibid.* A, 734-735; B, 762-763)

However, there is no way, in Kant's framework, for making the idea of an object that could be just the object of a mathematical concept (as the concept of triangle) clear and acceptable. This object should be pure, and there is no room for pure objects in Kant's framework. Thus, Kant alternates passages like the the previous one and others much more ambiguous, such as this one:

> "Mathematics alone, therefore, contains demonstrations, since it derives its knowledge not from concepts but from the construction of them, that is, from intuition, which can be given *a priori* in accordance with the concepts." (*ibid.* A, 734; B, 762)

Therefore, the only interpretation of the remarks contained in the first section of chapter one of the *Transcendental Doctrine of Method* which seems to be consistent with Kant's philosophy, is the one I have already given in the previous paragraph III.2.: intuition is pure, but objects are not. Pure intuition assures us that usual empirical objects which are manifest in particular pure forms are constructible and that we are able to operate on them in such a way that all the conclusions we draw from such an operation are also true for pure forms. Thus the triangle that the mathematician constructs is a particular empirical figure, but the conclusions he draws by operating on it, as in the Euclidean proof, are about the triangle as pure form, which is not really an object.

The distinction between pure forms and objects is quite impossible to clarify. However, the problem is not solved simply by eliminating such a distinction. Even though we consider pure forms as genuine objects, the situation does not change essentially: if the Euclidean proof deals with a particular empirical object—as in the previous reconstruction of Kant's argument—it can be a proof of a geometrical theorem only if it stays constantly under the control of the concept. But if this is so, the guarantee of the theorem comes just from the concept, and thus such a theorem expresses an analytical act of reasoning, in my sense.

IX.2 Euclid's Proof of The Theorem on Internal Angles of a Triangle

My point should now be clear: in my view, the Euclidean proof uses an empirical figure, but does not deal essentially with it. Such a figure is nothing but a particular notation (an icon, as Peirce says (Peirce 1885, 163)[25]) for the real object of such a proof, that is just the triangle. Like any object, the triangle is particular, but it can be represented by an infinite class of empirical figures. Even though these figures work in any reformulation of the Euclidean proof as a very particular notation, which expresses directly some properties of the triangle itself, this proof runs by applying a number of constructive procedures chosen in a certain domain of permitted procedures to such a notation.

To understand this point, let us reconstruct the Euclidean proof from the very beginning.

Euclid imagines we know what a (finite) straight line is and takes straight lines as elementary objects. He represents them by empirical lines that have two essential properties: they are continuous lines (property of continuity) and they are open lines that separate a region of the surface on which they are traced into two parts we can distinguish (property of separation). These properties are not expounded or defined by Euclid: they are simply two manifest properties of empirical lines we use as notations of straight lines. But they are also the only two properties of these lines that occur as such in the proof of any theorem of Euclid's geometry.

In order to assure such a starting point of his geometry, Euclid certainly has to make an appeal to a certain capacity of his readers: this capacity could be described in my terms as possessing a certain concept. This concept has a very particular nature: it is the concept of a pure object—that is just a straight line—rather than the concept of a property, but it can be used for introducing an open domain of pure objects. These objects are not—as it is the case with every concept of a property—different objects corresponding to different concepts; they are different objects corresponding to the same concept, which is the concept of an object. Simply, these objects are introduced and considered, one after the other, in different positions: a straight line differs from another only by its position. But position is a relative property and it is not possible to characterize the positions of two different straight lines, if we do not intend them as different straight lines beforehand.

Thus, two straight lines are not the objects of two different concepts we arrange in the same class, according to a concept of a property. They are two different objects corresponding to the same concept of an object. But, as these objects are treated in geometry exactly in the same way, they also can be intended as two different manifestations of the same object, too. They merely differ according to an original subjective capacity of differentiation, the capacity which enables us to distinguish different positions in spatio-temporal order. If we generally consider the modalities according to which we can operate on it, we have to speak about a straight line as only one object; if we pass to another level and we consider different applications of certain procedures consistent which these modalities, we have to speak about straight lines as different objects.

If I am right, a straight line is an object exhibited according to a quite complex strategy, appealing to different subjective capacities. However, such an exhibition is not completed since the operative procedures according to which we can operate on a straight line (or on straight lines) are not fixed. This is the task of the Euclidean postulates. These postulates are certainly not simple sentences working as starting points of a deductive game. They are constructive clauses (cf. the paper of Mäenpää in the present volume, who particularly insists on this point) that teach how to compose straight lines, in order to construct non-elementary objects starting from these. First, these objects are constructed, and then they are analyzed just like the objects which are constructed as they are, starting by straight lines.

Now, imagine that three straight lines are given. This means that three empirical lines are traced on a certain surface. If the third of these lines is long enough, relatively to the others, by applying the theorems I.2 of the *Elements*, we can construct the triangle that has these straight lines as its sides, and trace a corresponding figure on the same surface. Then, by applying the theorem I.27, we can construct a straight line parallel to one of the sides of the triangle, passing through the opposite vertex. Furthermore, by applying the second postulate, we can prolong all the sides. If we trace on our surface the lines corresponding to these constructions, we have a new figure. According to the property of separation for empirical straight lines, we can now recognize three angles on the same straight line. By applying the theorem I.29 to the angles formed by a transversal on two parallel straight lines, we can finally prove the theorem.

It is clear that the triangle and the angles we have considered here are the triangle and the angles that have been constructed, according to the previous procedure, starting with the three given straight lines. Thus, they are, like straight lines, pure objects.

As an object, the triangle is exhibited in Euclidean geometry when their modalities of construction starting with straight lines are given, according to the possibilities admitted by the postulates and the properties of continuity and separation of empirical lines. Such an exhibition is in a sense a presentation of a complex concept—that is not the naive and original concept of triangle, as a typical form of empirical objects, but a "mathematical" translation of it. But once this concept has been exhibited in such a way, it does not operate as such in the Euclidean proof; it does not control anything. The proof is properly the result of an analysis of the triangle as an object, that is an account of the properties of it, according to: its particular way of construction; the constructive clauses expressed by the postulates; the properties of continuity and separation of empirical lines; the subjective capacity of multiplication of pure objects in space and time.

X Arithmetical Proofs

If my analysis of the Euclidean proof of the theorem on the internal angles of a triangle is correct, such a theorem expresses, in the Euclidean framework, a synthetic act of reasoning, according to (D_4). The specification "in the Euclidean framework" is essential, since the same theorem could be stated as a consequence of a suitable class of meaning postulates. In this case, it would express an analytical act of reasoning. Thus, in order to justify (T_2) I now have to argue that the Euclidean framework is a typical framework of mathematical acts of reasoning.

The first step in the argument should obviously consist in stating that the situation of the previous theorem is common to every theorem of Euclidean geometry. Since I cannot present a general account of Euclidean geometry here, I am compelled to take for granted that this is the case. I will simply try to argue that the situation I have just described is not typical—with respect to its structural

characters—of such a mathematical theory, but it is general for mathematics as such, that is for classical, as well as for modern mathematics.

With the term "structural characters", I refer of course to the relations between concepts and objects, independent of their particular nature. My point is this: what you make when you conduct a geometrical proof, as the one we have just considered, is not to compare at every step the empirical figure in front of you—the notations of geometrical objects you are dealing with—to your concepts of these objects; simply, you apply with respect to certain figures—which you know to be good notations for these objects—certain standard procedures, you know as being permitted in the context of your theory. Thus, if the concepts of the geometrical objects occur, they occur not in the proof as such, but in an original stage, when the question is that of fixing notations (and identity criteria for them) and legitimate procedures. But if you know that the notations you are using are good notations, and the procedures are accepted, the concept does not occur as such.

Thus, if I am right, the structure of the a Euclidean proof could be described as follows. First, we have a certain number of original and naive concepts of properties, the concepts of spatial forms of extended objects. We associate such concepts with certain empirical figures we learn to reproduce according to certain relations of equivalence. Then, we introduce a number of procedures to transform our figures, and we fix certain rules that allow us to draw certain conclusions from certain figures (by considering the path we have pursued to attain them). Finally we apply these procedures and rules to our figures and we draw our theorems.

Of course, this is not, as such, the structure of every mathematical proof. There is something here that is typical of classical geometry—that is geometry in its original and proper sense. I obviously refer to the empirical figures or notations, which are not merely conventional or uninvolved symbols, but occur as such in the proof itself as bearers of certain properties—the properties of continuity and separation—that are also essential properties of the mathematical objects. Even though such a circumstance seems to entail a more natural development of mathematical acts of reasoning, it obscures its essential character. Empirical figures are essential tools of a geometrical proof, since a geometrical object is essentially a pure object represented by them (certainly it is quite possible to translate a geometrical proof into a purely linguistic deduction, but the result of this translation is not really a geometrical theory, but only a representation of it), but they are not essential tools of a mathematical proof as such.

X.1 $< 7 + 5 = 12 >$

However, the essential occurrence of empirical figures or notations within a mathematical proof, as bearers of certain properties of mathematical objects is not, as such, proper only to classical geometry. Let us consider another example drawn from classical or constructive arithmetic, which once again is a Kantian example. Here is what Kant writes in section V of the introduction to the second edition of the *Critique of Pure Reason.*

> "We might, indeed, at first suppose that the proposition 7+5 = 12 is a merely analytic proposition, and follows by the principle of contradiction from the concept of a sum of 7 and 5. But if we look more closely we find that the concept of the sum of 7 and 5 contains nothing save the union of the two numbers into one, and in this no thought is being taken as to what that single number may be which combines both. The concept of 12 is by no means already thought in merely thinking the union of 7 and 5, and I may analyze my concept of such a possible sum as long as I please, still I shall never find the 12 in it. We have to go outside these concepts, and call in the aid of the intuition which corresponds to one of them, our five fingers, for instance, or, as Segner does in his *Arithmetic*, five points, adding to the concept of 7, unit by unit, the five given in intuition. For starting with the number 7, and for the concept of 5 calling in the aid of the fingers of my hand as intuition, I now add one by one to the number 7 the units which I previously took together to form the number 5, and with the aid of that figure [the hand] see the number 12 come into being. That 5 should be added to 7, I have indeed already thought in the concept of a sum = 7+5, but not that thus sum is equivalent to the number 12. Arithmetical propositions are therefore always synthetic. This is still more evident if we take larger numbers. For it is then obvious that, however we might turn and twist our concepts, we could never, by the mere analysis of them, and without the aid of intuition, discover what is the sum." (Kant B, 15)

If we analyze the proof described by Kant, as we have done in the case of the Euclidean proof of the theorem on the sum of internal angles of a triangle, we find the following structure. First we have an original and naive concept of number, as a property of any collection of distinct objects: two collections have the same number, if and only if, we can alternatively eliminate or mark their objects one after the other, and finish our work at the same time (or stage). By using this concept, we arrange all the collections of objects we are considering into different classes in such a way that all collections which belong to the same class have the same number. Then, we associate each class of collections we have just formed with a collection of conventional symbols that has the same number as all the collections which belong to such a class. Finally, we fix some procedures for operating on the collections of symbols that have been formed by respecting this condition: all we could do with these collections of symbols, according to these procedures, has to be repeatable when any collection of symbols has been replaced by any other collection with the same number. In other words: we determine these procedures so that they are completely independent of the choice of symbols. Particularly: *i*) we order our collections of symbols in such a way that we can move from each of them to the following one by adding only one symbol, we associate to any collection a conventional name and we order all the names, according to the order of the respective collections; *ii*) we define an operation of composition of two collections of symbols, so that the result of such a composition is exactly the collection of symbols that is formed by putting together the two collections we are composing, and we extend such an operation to the names of

our collections. In this way the names '7' and '5' are associated with two collections of symbols, the composition of which gives just the collection of symbols associated to the name '12'.

We can decide that these names are the names of the collections of symbols themselves, of the classes of collections with which these collections of symbols are associated, or of the forms of these collections. This is not important. What is important is that in this way we have proven the statement $< 7 + 5 = 12 >$, by operating—as in the case of a geometrical Euclidean proof—on suitable empirical figures, or notations (the collections of symbols), according to certain procedures. The numbers, intended as objects, are just the objects that are represented by these notations. They are exhibited when the modalities of construction of the corresponding collections are given and the procedures for operating on these collections are fixed. Once again, such an exhibition is, in a sense, a presentation of the concepts of numbers, but these concepts do not occur as such in the proof of our theorem. This proof has the same structure as every proof in Euclidean classical geometry: according to (D_4), the theorem $< 7 + 5 = 12 >$ expresses a synthetic act of reasoning.

X.2 Peano's Arithmetic

If I am right, I have given two arguments for the claim that the two classical Kantian examples of mathematical synthetic *a priori* judgments express, in their natural mathematical framework, synthetic acts of reasoning. However, these are not arguments in favor of (T_2) yet. The objection one could advance is very traditional: even though the previous arguments are correct, they prove nothing but the syntheticity of acts of reasoning proper to classical mathematics, that is Euclidean classical geometry and elementary constructive arithmetic; but these theories are essentially pre-modern mathematical theories and their structural characteristics—particularly the ones I have considered in the previous arguments—are not structural characteristics of modern mathematical theories. Obviously, I think this is wrong. I now have to justify my view.

I have spoken about the mathematical concepts of triangle and of different numbers, but a doubt could arise. We can provide many different characterizations both of the triangle and of number 3. We can say, for example, that a triangle is a region of the plane confined by three non-parallel straight lines, or that it is the region of space that is common to three angles placed in such a manner that every side of one of them is also a side of one (and only one) of the two others. In analogy, we may say that the number 3 is the first odd prime number (if 1 is not prime or not odd), or the only divisor of 9 other than 1 and 9 itself, or that it is the result of the addition of 2 to 1. All of these definitions characterize the triangle and the number 3 as the only objects that satisfy certain conditions, that is: as the only members of the classes associated to certain concepts of properties. But, how can we say that these classes contain the same objects?

Let us imagine that a new novelist writes a modern version of *Madame Bovary*, where Madame Bovary does not kill herself by ingesting arsenic, but by eating poisonous mushrooms. It seems very natural to me to think that we are faced with a new concept of Madame Bovary, and consequently that the new novelist has exhibited a new object, which bears the same name as Flaubert's personage, but is not the same person. Yet, this is clearly not the case with mathematical objects. Even though they are exhibited by means of presenting different concepts, they are not different objects. But, how is this possible if a mathematical object is a pure object?

The answer is not simply that the different concepts of properties we might use for characterizing a mathematical object are equivalent, since this is exactly what we have to explain: how can they be equivalent? It is neither that we dispose of a suitable class of meaning postulates, since these postulates do not take part, as such, in a mathematical theory, and are, at most, a way of expressing the equivalence of different concepts, rather than to guarantee this equivalence.

The different concepts of properties to be used for characterizing a mathematical object are equivalent because their corresponding classes contain only one object that is always the same. And this object is not the object of these concepts, since these concepts are just concepts of properties, rather than concepts of objects. This object corresponds to another concept: the triangle is the geometrical object that is constructed in a certain way starting with straight lines, according to Euclidean constructive clauses; the number 3 is the number represented by the collection of symbols that is constructed in a certain way, starting with only one symbol. Thus, the other concepts I have just presented are equivalent because we can prove in Euclidean classical geometry and in elementary constructive arithmetic that the classes corresponding to these concepts are just composed by the triangle and the number three.

This simple remark clarifies what the essentially structural character of a mathematical theory is: it is just the disposability of a suitable class of concepts of objects that works essentially as such, rather than as concepts of properties. The examples of classical Euclidean geometry and elementary constructive arithmetic show two different modalities for satisfying such a condition. These modalities have an important aspect in common: they are constructive ways grounded on an original cognitive capacity, that is the capacity to fix the elementary objects of constructions—straight line and unity—and to multiply them in space and time. But other, non-constructive modalities are possible.

Using Salanskis' terminology (Salanskis 1995), I oppose these constructive modalities to "correlative" ways. According to a constructive modality, a mathematical object is exhibited when the way for constructing it is exhibited and the

procedures for operating on it are fixed. According to a correlative modality, a domain—generally a set—of mathematical objects is exhibited when the conditions that such a class has to satisfy as such are expounded and the criterion for distinguishing—if necessary—the different objects of such a domain have been given. For example, this is the case of Peano's arithmetic.

Let us also consider such an example. In Peano's arithmetic we assume that we know what a class is and—by means of Peano's axioms—we fix the conditions that a class has to respect in order to be a progression. These conditions refer to the members of the class itself, so that we have to assume too that we can consider these members separately, as different objects, even though we characterize all of them simply as members of a certain class. Thus, we make an appeal, once again, to our original capacity to multiply a certain object—the member of a certain class—in space and time.

The first axiom tells us that it has to be possible to take one element of the class, to nominate it—let us say by the name 'α'—and to evoke it, and only it, by means of this name, in any circumstance. Thus a class is a progression only if it has at least a member. The second axiom tell us that any member x of the class is associated with another (and only an other) one x_Γ by means of a certain monadic operator Γ, that need not to be characterized ulteriorely, even though we have to assume that, for every member x of the class, we can individuate the member x_Γ associated to it by Γ. As this axiom does not specify that x_Γ and x are distinct objects, every singleton could satisfy the first two Peano's axioms. The third axiom tells us that the member α of the class is associated to no other member by the operator Γ. Thus our class could not be a singleton, but it could be, for example, a couple $\{\alpha, \alpha_\Gamma\}$, if $(\alpha_\Gamma)_\Gamma$ is α_Γ itself. The fourth axiom tells us that the member of the class which is associated by Γ with a certain member x cannot be associated with another member y of the class, distinct from x. Hence, the class must be infinite and must be almost a starting point with respect to Γ. But it is possible that it was composed by different Γ-chains (one of which starts with α) independent of each other. Finally the fifth axiom tells us that this cannot be the case, since any property of α (for example the property of participating in the Γ-chain starting with α)—that, if it is a property of a member x of the class, then is also a property of x_Γ—is a property of every member of the class.

Once these axioms are given, we can assume that a class N is a progression, that is: *i*) we use the concept of property '(to be) a progression', or 'to respect the Peano's axioms for exhibiting an open class of classes (the class of progressions), by assuming that classes are, as such, already given objects; *ii*) we assume that such a class is not empty (for example, by asserting that we are able to exhibit a progression, as we have just done for the domain of numbers in elementary constructive arithmetic); *iii*) we assume we are able to choose a member of this class, to give a name to it and and to evoke it, and only it, by means of this name, in any circumstance. If N is a progression, we can pick out one of its member, which is α. We take this member and we rename it "zero" ("0") then we rename the element α_{T} of N "one" ("1"), and so on.

Up to now, we have used a concept of property and applied it to the class of classes in order to pick out the progressions. Then we have assumed that we can take one and only one progression, that is just N. The concept of N is thus a concept of an object, since, in order to be N, a class has not only to be a progression, but also has to be the progression we have chosen. It is not important that we are able to distinguish N from any other progression (certainly, we are not able do to it). What is important is that we take the concept of N as a concept of object: the progression we have chosen as the progression of natural numbers. Once we have done it, the concepts of the different members of N work also as concepts of objects (rather than as concepts of properties), because the concept of the member α of a progression is used as a concept of an object (rather than a concept of a property). Thus, we are in front of an infinite set of objects, which are just Peano's numbers.

To prove the theorem $< 7 + 5 = 12 >$, we now have to introduce the operation of sum to the members of the progression N. For that we state that for every three members x, y and z of N: *i*) $+(x, 0) =_{\mathrm{df}} x$; *ii*) $+(x, 1) =_{\mathrm{df}} x_{\mathrm{T}}$; *iii*) $+[x, +(y, z)] =_{\mathrm{df}} +[+(x, y), z)]$; *iv*) $+(x, y) =_{\mathrm{df}} +(y, x)$; where "$\nu = \mu$" means: "$\nu$ and μ are two notations or names for the same member of N". Once we have done this, we can prove the theorem in the classical Leibnitzian way. However, if I am right, the conducting of such a proof, is an argument for the syntheticity of the act of reasoning expressed by this theorem, rather than from its analyticity. If the mathematical concepts of the numbers 5, 7 and 12 are certainly responsible for the exhibition of these objects, they do not work as such in the proof; rather this proof deals with their objects.

XI Concepts of Objects, Concepts of Properties: the Essential Character of Mathematics

The previous three examples should clarify the essential character of mathematical acts of reasoning, which turns them into synthetic acts of reasoning, according to (D_4). Mathematical objects are not only pure, but they are also exhibited by a very complex act of presenting their concepts. These concepts are generally constructed with the aim of providing a suitable translation of other concepts. Such a translation is successful when we are able to imagine a deductive structure applying to the names or notations of different objects we have introduced, first by multiplying a pure object in space and time, and second by individualizing some of the distinct objects we have created in such a way by a simple act of nomination. Because of the deductive structure and the particular nature of the act of

multiplication, all the objects arising from this sort of act are submitted to the same procedures and correspond to the same concept. Thus, such a concept is in a sense the concept of only one object, let us say *a*. It is only by a new act of individualization that we can change our level of analysis and pass to consider distinct *a*'s, each of them now being characterized in a particular and suitable way, and submitted as such to the fixed procedures. As the concepts of these objects are not only presentations of the distinctive character of such objects, but integrate both, the act of multiplication in space and time and the act of fixing the possible procedures that can be applied to the objects themselves, they give, in a sense, an autonomous life to their objects, by enabling us to consider them as such. It is just this act of consideration of mathematical objects as such that is typical of mathematical acts of reasoning and makes them synthetic acts of reasoning.

Thus, a mathematical act of reasoning is only possible according to an intentional act which consists in treating a concept that fixes a certain character as a concept of an object, rather than as a concept of a property. Usually, the distinction between a concept of an object and a concept of a property is conceived as the logic correlate of a metaphysical difference between individual substances and their attributes. In contrast, I think that such a difference lies merely in the intentional use of concepts. If our concept of chair is such that to be a chair (or better the chair) means to be a particular object and not to enjoy a particular property referred to a certain class of specified objects, then we have to accept the idea that the chair (and not this or that chair) is an object, a pure object, of course.

Some argue that an object of this sort—like the triangle, or the natural number—is a universal object. However, I cannot understand what a universal object could be, since for me an object is essentially an individual entity. Nevertheless, this does not mean that for me an object is a certain determined substance, but merely that its exhibition (or evocation) exhausts certain exigencies of individuation that a subject could advance: it is possible to treat the pseudo-properties '(to be) *a*' as a "final characterization" with respect to a certain domain of other properties. Thus a concept of an object is nothing but a final characterization, working with respect to certain exigencies of individuation[26].

As the exigencies of individuation could be very different from one another, the same characterization could work in different context either as a concept of an object (that is a final characterization), or as a concept of a property. This is true for any sort of pure objects. The chair, as an object, is nothing but a particular kind of drawing-room suite: here, the concept of a drawing-room suite is a concept of a property, while the concept of the chair is a concept of an object; likewise the triangle, as an object, is nothing but a particular geometrical figure, a particular polygon: here the concept of triangle is a concept of an object, while the concept of polygon is a concept of a property. However, the same characterization that provides the concept of the chair can be taken to express a property, and, in such a sense, it can be specified: we could have, for example, the Louis XIV chair, or the Louis XIV chair conserved in Versailles, and so on. Analogously for triangles: we could have the isosceles triangle, the isosceles triangle associated with a certain construction, and so on.

Thus, in order to have a domain of objects, we need not individuate a particular substance or a particular content of thought that is intrinsically individual. We simply have to fix the final stage of an exigency of individuation. This is exactly what we do when we expound a mathematical theory as a theory of a certain domain of objects. Hence, that a certain concept is a concept of an object a does not mean that we cannot imagine, and even exhibit a number of different a's. If we do that, we are simply changing our exigencies of individuation, and we are passing to a theory of a strictly different domain of objects.

Since the different exigencies of individuation can often be hierarchically ordered, it is then possible to organize the respective theories, with originally strictly different domains of objects, such that they form only one general theory, the objectual domains of which is hierarchically structured. This is the case with classical Euclidean geometry. Thus, certain concepts of mathematical objects of such a general theory can be specified ulteriorly, when a new exigency of individuation is advanced. However, as these concepts are just concepts of objects, they characterize individual entities on which it is possible to operate according to fixed procedures. I think that it is just this essential character of a mathematical theory that makes it possible, in mathematics, to operate—as Kant said—on the universal *in concreto.*

Moreover, this is also the condition of possibility of analysis as a mathematical method.

Imagine that a mathematical problem asks for the individuation of one or more a's which satisfy certain conditions. If these conditions characterize one or more a's which are still unknown (they do not provide a presentation of the concepts of these objects, but only a presentation of the concept of a property or a relation that they have to satisfy), we can use a suitable notation for expressing these objects and operate on it with respect both to the fixed procedures that apply on the object a and to the conditions which the objects we are looking for have to satisfy.

A very simple case is the following. We are looking for two complex numbers the sum and the product of which are φ and ψ, respectively (where "φ" and "ψ" are names or notations of two objects given as such, two natural numbers). Thus we can express these two numbers by the symbols "x" and "y" and operate on them as if they were common complex numbers. Here, "x" and "y" are the names of two potential objects that satisfy two different, even though reciprocal, properties: for x, the property 'to produce φ and ψ, respectively, when it is added and multiplied to y' and, for y, the property 'to produce φ and ψ, respectively, when it is added and multiplied to x'. Thus the concepts of x and y work as concepts of properties here. However, as the objects that satisfy these properties are certainly complex numbers, we can treat x and y as names of specified complex numbers, operate on them according to the algebraic procedures and solve the problem by exhibiting two couples of complex numbers, let us say, c_1 and c_2 and d_1 and d_2 (which are the objects of suitable concepts of objects) which satisfy the condition of the problem.

In my terminology (Panza fc), x and y are "conditional objects", while c_1, c_2, d_1 and d_2 are "proper objects". This terminology allows us to reformulate the classical Pappus' distinction between analysis and synthesis (as mathematical methods or procedures) in the following terms: analysis consists in operating on conditional objects as if they were proper objects, in order to determinate the proper objects that satisfy a given condition; synthesis is just the act of exhibiting or determinating these objects.

XII Concluding Remarks

If I am right, my notion of mathematical objects as pure objects not only provides a reformulation of the Kantian distinction between analytic and synthetic judgments, as a distinction between analytic and synthetic acts of reasoning, according to which mathematical acts of reasoning are just synthetic, but it also provides a reformulation of Pappus's distinction between analysis and synthesis and makes these two classical distinctions not so extraneous to each other, as it has been usually argued: while both "analysis" and "analytic" refer to our activity on concepts, "synthesis" and "synthetic" refers to our activity on objects. In such a way, the connections between the general question of analysis and synthesis in mathematical knowledge and the classical controversy on Platonism (Panza and Salanskis 1995) also become clear.

It seems to me that such a result is important from a historical point of view, as well. Even though my starting point is essentially a non-Kantian one, my interpretation of Kant's distinction fits very well with some crucial aspects of Kant's interpretation of mathematics.

Firstly, the distinction between "analytic" and "synthetic" bears, in my view, neither on the logical internal form of statements nor on their relations to other statements, nor does it apply to statements as such. Rather, it refers to the logical nature of the act that a statement expresses. It seems to me that, despite the criterion presented by Kant in his *Introduction* to the second edition of the *Critique of Pure Reason*, this is also the case with Kant's distinction itself.

Secondly, in my understanding, an act of reasoning is synthetic because it is grounded on the analysis of the objects to which it is attributing properties or relations, rather than on their concepts. Even though the Kantian thesis asserting the syntheticity of mathematical judgments has frequently been defended by referring to a mentalist conception of intuition—a sort of intellectual light that should originate mathematical principles, axioms or proofs—it seems to me that for Kant a judgment (or better a statement) is synthetic if intuition occurs in its justification, as a modality or even a guarantee of the actual or possible presentation of an object. According to Kant, work on concepts is, in fact, a mark of analyticity, rather than syntheticity. It is only by going away from our concepts, and further away from our mental contents, that we can formulate a synthetic judgment.

Thirdly, my argument for the syntheticity of mathematical acts of reasoning links such a thesis to Kant's thesis, according to which "mathematics can consider the universal *in concreto*". Both this thesis and the other one, which asserts the syntheticity of mathematical knowledge, or judgments, are parts of the hard core of Kant's philosophy of mathematics. Nevertheless, Kant's interpreters frequently fail in showing the link between them. If the second of these theses is reformulated in the manner I have suggested, this link becomes evident.

Still, these three remarks do not eliminate the major differences between my conceptions and those of Kant. I think that these differences are reducible to a fundamental one that I would like to expound, as clearly as possible, at the end of my paper. According to Kant, the distinction between empirical and pure concepts is a primitive one, and it is not ulteriorly explicable. Nevertheless, Kant seems to reason as if empirical intuition could be "prolonged" in the pure one, by providing a guarantee of *a priori* knowledge as a sort of "potentially empirical" or "pre-empirical" knowledge. In such a way, pure intuition has a task to fulfill: it has to found the possibility of *a posteriori* knowledge, by guaranteeing the empirical content of *a priori* knowledge. Particularly, mathematical knowledge is for Kant about the general forms of (empirical) objects, such forms being just the forms that these objects have as such, the forms in which they present themselves to the empirical intuition. A subject, according to him, has intuition of objects only as contents that fill up general pure forms, and the possibility of prolonging empirical intuition in the pure one is nothing but a way to come back to the transcendental origins of empirical intuition itself. Thus, pure intuition often has to work as a criterion of constructibility of mathematical concepts, or of the real possibility of them (Kant A, 220-221; B, 267-268, for example), and it can realize its task only by imposing on these concepts the limits characterizing our empirical intuition. In such a way, mathematics has to respect, as such, certain conditions, or it has to be kept within certain limits, which cannot merely be the limits of thought, and cannot be found other than in the characters of subjective evidence[27].

Such a war-machine is founded on a deep and essential confusion. If objects are filling up general pure forms, it is not possible to refer to them, or their form, in order to distinguish constructible from not constructible concepts. I do not know if this confusion (which is nothing but a circularity) can be avoided when we want to realize the double program of Kant: to found the possibility of *a posteriori* knowledge on the availability of mathematical knowledge and to found mathematical knowledge as such on the conditions of the possibility of *a posteriori* knowledge. I do not know it and I am not interested in it. In my view a philosophy of knowledge has to found nothing: neither the possibility of empirical knowledge, nor mathematics as such. It only has to provide the hermeneutic tools for understanding knowledge as it is, as it has been historically realized by individuals. When I speak about subject, I do not refer, as Kant does, to a transcendental, universal or typical subject. I refer to individual subjects, just like us, I look for a characterization of our cognitive acts, I try to distinguish them among the totality of our acts according to a formal criterion, and finally I aim to describe and understand what sorts of subjective abilities are employed in our cognitive activity.

Thus the task of a philosophy of mathematics is, for me, that of providing valuable categories for characterizing and understanding mathematics, as a typical human activity, and not that of founding its legitimacy on an irrefutable guarantee—even though to understand a mathematical theory is also to come back to its origins and to make clear (and eventually discuss) its reasons. Here, I have suggested that mathematics is both the activity of constituting pure objects on which synthetic acts of reasoning are possible and to realize these acts. Even though it is not only a formal deductive game, it is both the activity of constructing pure objects, according to a certain aim—so that a (quasi-) formal game could be applied to them, for discovering their properties or relations—and the activity of applying this game and realizing this task.

Centre F. Viète of the History and Philosophy of Sciences,
University of Nantes

Notes

* I thank Andreas Etges, Agnese Grieco, Claudio La Rocca, Jean Petitot, Jean-Michel Salanskis, and specially Michael Otte for their linguistic and philosophical suggestions and commentaries on a number of previous versions of my paper.

1 In order to avoid any possible misunderstanding due to the English translation, I add the German original term in brackets after the first occurrence of every English term translating a Kantian key term. If no particular reference is made when the expression is used again, it always refers to the same original German term. A glossary is given at the end of the paper. If not stated otherwise, English translations of Kant's statements are quoted from (CS) and (LY).

2 For a recent version of the logicist program, cf. Wright (1983).

3 For reasons of uniformity with the text of the first *Critique*, I here change the Young's translation and translate "*Wirklichkeit*" with "reality" instead of "actuality".

4 Young translates here "to decide" for "*ausmachen*", but this seems to be a misleading translation, since a decision is a choice between different possibilities that are already given as such, which is strange to the meaning of the German term "*ausmachen*".

5 Once again I changed Young's translation, translating "*kommen*" and "*zukommen*" with "to suit"—instead of with "to belong" as Young does—in order to avoid any possible confusion between the relation of object and concept—which is the case here—and the relation between two concepts, which Kant indicates with the verb "*gehören*", above translated with "to belong".

6 Here is an other excerpt both from the first and the second edition, that is not less clear (cf. *ibid.* A, 154-155; B, 193-194):

> "In the analytic judgment we keep to the given concept, and seek to extract something from it. [...] But in synthetic judgments I have to advance beyond the given concept, viewing as in relation with the concept something altogether different from what was thought in it. This relation is consequently never a relation either of identity or of contradiction; and from the judgment, taken in and by itself, the truth or falsity of the relation can never be discovered."

7 I changed the order of propositions of Smith's translation, in order to stay closer to the original.

8 Following Smith's translation, I translate both the German "*Perzeption*" (which very rarely appears in Kant's texts) and "*Wahrnehmung*" with "perception", by considering these terms equivalent in meaning.

9 According to Eisler's classical *Kant Lexicon* [cf. Eisler [1930], p. 391], there is no appreciable difference between Kant's use of the terms "*Gegenstand*" and "*Objekt*" in the first *Critique*, even if these terms, taking together, cover a large spectrum of different meanings. Here I consider only one of these meanings (actually covered in the first *Critique* by both German terms), namely the one according to which an object is that of which elementary knowledge is just knowledge. What is important to me is not that the same terms could or could not have other meaning in Kant's works (certainly they do), but that for Kant there is no room for something like a pure object (either *Gegenstand* or *Objekt*), intended as that of which *a priori* knowledge could just be knowledge.

10 I prefer the term "singular" to Smith's term "single".

11 The passage, at the very beginning of the *Aesthetics*, is so well known that it is not necessary to quote it here.

12 Cf on this point Panza (1995*a*), where I have tried to justify such a thesis.

13 For Kant's interpretation of postulates as clauses for constructing objects , cf. Kant [JL], § 32 and 38:

> "[...] *practical* propositions [...] are those that state the action whereby, as its necessary condition, an object becomes possible."
> "A *postulate* is a practical, immediately certain proposition, or a principle that determines a possible action, in the case of which it is presupposed that the way of executing it is immediately certain."

14 A. Ferrarin (1995, 137) writes:

> "[According to Kant] the synthesis [...] involves the necessity to go beyond the concept and show its pure a priori determination of spatio-temporal intuition: the guidance for the construction of the object. And a synthetic judgment is not a formal, discursive relation between the subject and its predicate, but the activity of exhibiting in intuition the real belonging of a property of its object."

I completely agree with that.

15 Notice that, if I am correct, a mathematical judgment is for Kant a judgment about (the concepts of) pure forms, but it is justified by means of procedures referred to empirical entities. Thus, truth, or even necessity, of mathematical judgments must as such be independent of their justification or proof. Of course, we can insist on the possibility of imagining the objects occurring in such a justification, but this does not change the situation, since imagination has just to be imagination of objects.

16 On the role of imagination and its relations with understanding in Kant's theory of knowledge cf. Palumbo (1984).

17 It might appear paradoxical that a synthetical act of reasoning depends on an analysis, but I believe it is not. Cf. on this point the concluding chapter of the present book (particularly § VI).

18 In my view a reasoning is a specific and particular event that cannot be repeated as such, even if it can take a general form that logic can study (that is the "inference" in Kant's sense).

19 Analogous considerations could be advanced for universal statements of a conditional form where relational predicates occur.

20 Here Kant uses the verb "*liegen*" which I translate literally as "to lie on"—instead of "to contain", as Smith does—in order to mark the difference with the verb "*enthalten*", just translated "to contain".

21 Smith simply translates "*ausführlichen*" as "complete". Instead I follow the suggestion of Giorgio Colli who in his Italian translation [Einaudi, Torino, 1957] just translates it as "*dettagliatamente completo*". Moreover, he translates "*ursprünglich*" as an adjective referring to "concept", while it works in the German text as an adverb referring to "*Grenzen*". This is the reason for a second change of Smith's translation.

22 Cf. the previous note (21).

23 Smith translates "arbitrarily invented".

24 Cf. the previous footnote (10).

25 Not any notation is for Peirce an icon:

> "I call a sign—he writes (1886, 163)—which stands for something merely because it resembles it, an icon. Icons are so completely substituted for their objects as hardly to be distinguished from them. Such are the diagrams of geometry."

In my (1985*a*), I called this sort of notations "transparent" and I considered their role in classical Euclidean geometry (cf. *ibid.*, § 5, pp. 78-84); I will come back later to some of the arguments I have presented there. In the paragraph X, I will argue that this sort of notation essentially occurs also in classical or constructive arithmetic. Moreover, Peirce (*ibid.*, 165) believes that icons play an essential role in algebraic (or formal) deduction too. I will not discuss this last point here, even though I think that Peirce's argument is far from being completely wrong. On the role of diagrammatic thinking, as founded on iconic notations, in mathematics, according to Peirce, cf. the paper by M. Otte in the present volume.

26 A similar idea has been advanced by M. Otte, in his discussion of the Locke-Berkeley-Kant controversy on the "general triangle" (Otte 1994*b*, 276-284 and 1995, 102). It is evident that a pure object is indeterminate with respect to a very large range of properties. This is the case both with the triangle and with Madame Bovary. To the question "how tall is Madame Bovary?", we have no answer. The reason is not that we do not know how tall Madame Bovary is. The reason is that, for every property like "to be *x* tall" Madame Bovary does not have such a property without having its negation. Simply, Madame Bovary is a woman, but she does not have a tallness, even if this does not mean that Madame Bovary is a "universal woman". On the consequences of such a character of a pure, and particularly a mathematical object, cf. my (1995*b*), § 6., pp. 122-128.

27 It seems to me that such a question is connected with the one advanced by Parrini, by referring to Herbart, concerning the conditions of possibility of "determinate knowledge" (1990, 60-62).

Glossary

Analytic: *analytisch.*
Apodeictic: *apodiktisch.*
Assertoric: *assertorisch.*
Belong (to): *gehören.*
Clarity: *Klarheit.*
Concept: *Begriff.*
Consciousness: *Bewußtsein.*
Construct (to): *konstruieren.*
Contain (to): *enthalten.*
Content: *Inhalt.*
Construction: *Konstruktion.*
Demonstration: *Demonstration.*
Denied: *verneint.*
Detailedly complete: *ausführlich.*
Empirical: *empirisch.*
Exhibit (to): *vorstellen.*
Experience: *Erfahrung.*
Grounded: *gründet.*
Identity: *Identität.*
Inferences: *Schlüsse.*
Intuition: *Anschauung.*
Judge (to): *urteilen.*
Judgments: *Urteile.*
Knowledge: *Erkennntnis.*
Limit: *Grenze.*
Maintained: *behauptet.*
Make out (to): *ausmachen.*
Mathematical: *mathematisch.*
Modality: *Modalität.*
Object: *Gegenstand* or *Objekt.*
Objective perception: *objektive Perzeption.*
Original: *ursprünglich.*
Perception: *Perzeption* or *Wahrnehmung.*
Precision: *Präzision.*
Predicate: *Prädikat.*
Problematic: *problematisch.*
Propositions: *Sätze.*
Pure: *rein.*
Real: *wirklich.*
Reality: *Wirklichkeit.*
Representation: *Vorstellung.*
Sensation: *Empfindung.*
Singular: *einzeln.*
Subject: *Subjekt.*
Subjective constitution: *subjektive Beschaffenheit.*
Sufficiency of characteristics: *Zulänglichkeit der Merkmale.*
Suit (to): *kommen, zukommen.*
Synthetic: *synthetisch.*
Thing: *Ding.*
Thought: *Denken.*
True: *wahr.*
Truth: *Wahrheit.*
Understanding: *Verstand.*
Universal in concreto: *Allgemeines in concreto.*

MICHAEL OTTE

ANALYSIS AND SYNTHESIS IN MATHEMATICS FROM THE PERSPECTIVE OF CHARLES S. PEIRCE'S PHILOSOPHY*

I Introduction

This paper is particularly concerned with Peirce's conception of mathematics. Taking into account that there exists a great deal of scholarly insight into his philosophy of science, one is surprised to notice how indefinite, uneven and varied opinions are regarding Peirce's conception of mathematics. Peirce has declared mathematics to be paradigmatic for philosophy (CP, 7.80) which leads us to investigate the relationship of Peirce's epistemology to classical German philosophy, to the conceptions of Leibniz, Hegel and above all of Kant. Kantian thought is not only crucial for Peirce's early period but is indispensable to any understanding of Peirce's philosophy and his conception of mathematics. It is true that certain beliefs, common to Kant and Peirce take on a different importance and meaning when passing from one to the other. Intuition, for instance, was an important term for both. Peirce called "intuition" "the one sole method of valuable thought" (*ibid.*, 1.383). Another common idea refers to the linkage between generality and continuity. Continuity, writes Peirce, for instance, is "nothing but a higher type of that which we know as generality. It is relational generality" (*ibid.*, 6.190; with respect to Kant cf., for instance, B 206). As, however, the architecture of Kant's *Kritik* rests exclusively on the idea of the *a priori*, and as Peirce, on the other hand, does not share Kantian aprioricism, these, as well as various other ideas, change in meaning and acquire new roles. For instance, time is such, says Kant, "that every part of it has similar parts,—a proposition very different from merely saying that Time is infinitely divisible, though Kant himself did not perceive the distinction" (*ibid.*, 8.114). However, because of his aprioricism, Kant had no need for that distinction. For Peirce, on the contrary, it was of crucial importance. Continuity, or similarity of parts, making time (as well as space) an individual whole, was absolutely essential, because continuity in this manner serves to introduce a new type of metaphysics, the universe being conceived of as a system of interrelated systems rather than as a set of isolated things.

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 327–362.

> "Nominalism—or at least, modern Nominalisms—is precisely the doctrine that the Universe is a heap of sand whose grains have nothing to do with one another, and to recognize concatenation is to recognize that there is something that is not Individual and has another mode of Real Being than that of an Individual Existent." (Ms 641)

The *a priori* is nothing but that which is universally valid and "whatever is universally true is involved in the conditions of experience" (CP, 2.690). In contrast to his predecessors, Kant considered these general conditions to be subjective rather than objective. "It was the essence of his philosophy to regard [...] the reality as the normal product of mental action, and not as the incognizable cause of it" (*ibid.*, 8.15). Peirce now claims that his new philosophy of synechism (synechism is a regulative principle of logic based on the idea of continuity) allows these general conditions or the *a priori* to be understood as being both subjective and objective by relating them to an evolutionary process which is at the same time constrained and yet not absolutely determined. The resulting relativity of the distinction between the subjective and the objective gives the principle of continuity its prominent place in methodology, because in order to reconcile relativity and objectivity of knowledge, one has to accept that the distinction between the assertion that $A = B$ and its negation cannot be absolute, since "absolute discontinuity cannot be proved to be real" (*ibid.*, 8, 278). "When Synechism has united the two worlds of the subjective and the objective; the belief in the relativity of the subjective and the objective gains new life" (*ibid.*, 6.590).

Kathleen Hull (Hull 1994) claims that Peirce closely follows Kant in his understanding of mathematics and of mathematical reasoning. It therefore seems justified to approach the matter historically and to begin with Kant, with a consideration of Kant's conception of mathematics. As is well-known, Kant characterizes mathematics in terms of the analytic-synthetic distinction, claiming that mathematical propositions, or the judgments represented by them, are synthetic *a priori.* As Peirce abandoned aprioricism, his specific answer to the continuum problem and his conception of an evolutionary realism derived from that answer, made his views essentially different from Kant's, rendering the analytic-synthetic distinction somewhat relative, because the distinction between the subjective and the objective became a relative one (with respect to the connection between these two distinctions see part V of this paper, and, *ex negativo*, also Grice and Strawson 1971). Thus, I agree with Kathleen Hull, this being my first premise, that "mathematics, not logic, is the cornerstone of Peirce's architectonic" (Hull 1994, 273). And this is so exactly because of the importance of generality as based on the idea of continuity to which the law of contradiction cannot be applied, and Peirce, as well as Hilbert, therefore had to look for a different logic of reasoning.

My second premise then is that we should take Peirce's views with respect to mathematics and to science in general as being based on one and the same philosophical conception. This implies for instance that mathematical axioms and natural laws are to be classified as ontologically of the same nature, they both employ free variables and are generals in the sense of Peirce's philosophical realism as based on the reality of the continuum. Knowledge of any sort is formal and to take on meaning it has to be applied. As it cannot be itself a theory of its own application, we enter into an infinite regress of meta-levels. Aprioricism (any kind of foundationalism, in fact), on the one side and philosophies of practice (like pragmatism) on the other side meet the challenge of infinite regress differently. With respect to Peirce, one notices that ideas like evolution and continuity become important as substitutes for the idea of foundationalism. In evolution the infinite regress is interrupted as certain possibilities are in fact realized and others not and the continuum represents all that is possible.

This brings me to the third premise of my argument. We should take seriously Peirce's approach to the question of philosophical realism when trying to understand his views regarding mathematics. In contrast to this requirement, investigations of Peirce's realist approach to mathematics sometimes start by asking "can one be a realist without being a platonist" (Engel-Tiercelin 1993), while for Peirce realism and platonism really had nothing in common. In exploring the connections between "Peirce and logicism", Susan Haack places everything into a Fregean framework from the very beginning by asking whether Peirce would ascribe to two theses she presents characterizing logicism in the sense of Frege. Then the impression is conveyed that Peirce was not really consistent in his views, as he seems to accept one of the theses but not the other. He held, it is claimed, that mathematics is reducible to logic and yet staunchly denied another logicist thesis, that the epistemic foundations of mathematics lie in logic, whereas "Frege took it for granted that both theses stand or fall together" (Haack 1993, 36). At this point I do not want to discuss the content of these claims (see however Houser 1993), but would rather oppose the justification of the approach as such. Peirce starts by observing how mathematicians really practice their business and how they accomplish their results, rather than, like Frege, with an idea of how they should perform their activities.

Finally in his excellent and influential book on the development of Peirce's philosophy, M. G. Murphey claims that "the creative or dynamic agent" in this development is Peirce's logic (Murphey 1961, 3), as well as that in spirit "Peirce has more in common with the logicist school than with intuitionism" (*ibid.*, 288). It is correct that Peirce did not accept that "mathematics limits itself to the range of objects it can construct" (*ibid.*). But taking into account the Kantian roots of his philosophy, it is equally correct to say, that he would never believe that the construction and the presentation of mathematical objects could be completely separated. Just this particular problem, how to conceive of the link between the development and substantiation of mathematical knowledge, might already suggest that a framework different from the traditional philosophies of mathematics, formalism, platonism, intuitionism or logicism has to be found in order to understand what Peirce had in mind.

II Analysis and Synthesis from Leibniz to Kant

With respect to the analytic-synthetic distinction Kant states:

> "In all judgments wherein the relation of the subject to the predicate is thought this relation is possible in two different ways. Either the predicate B belongs to the subject A, as something which is contained (covertly) in this concept A; or B lies completely outside of the concept A, although it stands in connection with it. In the first instance, I term the judgment analytical, in the second synthetical." (B, 10)

Synthesis is the opposite of analysis. Now, during the age of classical rationalism the term "analysis" is used in two applications:

1. Empirical theories are analytical, as far as they claim to speak about the essence of reality as such, as far as they seek to find out what is the core of a thing. This is because analysis proceeds from a given unknown which it seeks to investigate. Substances or essences are real and are the real subjects of predication. Kant's reformulation of the analyticity of judgments stays in line with this as long as one assumes that conceptualization captures the essence of some real being or some existing substance. But how is this to be guaranteed? By the structure of the epistemic subject, says Kant. "It was the essence of his philosophy to regard the real object as determined by the mind" (Peirce CP, 8.15).

2. Logical theories are analytical, as far as they deal with the way something which has been said can be said in another way as well, and this is how the law of contradiction, that is the claim that something cannot be simultaneously named p and non-p, obtains its significance. It becomes the basis for the analyticity of formal theories. Algebra, and mediated by it also geometry, are called "analytic" as soon as the unknown variable "x" is introduced into their activities. Equations, taken as $S = P$ expressions, represent not only a method but rather a way of securing true knowledge.

To classical ontologism all true propositions had been analytically true. Classical thought had as its ultimate goal, which was in general only to be accomplished by God because it required an infinite analysis, the determination of individual substances. This view is an outgrowth of the static world view of the classical age but also a result of its optimism that the world is knowable. And the existence of God is a basis of this optimism. In this manner the law of contradiction used formally serves to give proofs of existence. This later became a fundamental idea of Cantorian pure mathematics. Kant does not accept that a non-contradictory is also real (Kant B, 629).

Leibniz has created our modern concept of mathematical proof by understanding that a proof is valid by virtue of its form, not by virtue of its content. This does not imply, however, contrary to a claim made by Russell (Russell 1903*b*, 178) that Leibniz's philosophy rests solely on his logic, because Leibniz assumed a one-one correspondence between concepts and objects. Symbols represent thoughts and collections of thoughts determine or represent objects. As in his view all things in this world are constituted by the concepts corresponding to them in God's mind, proof amounts to an infinite analysis of the respective concept, and all cognition becomes analytical cognition. "Leibniz making proof a matter of ontology not methodology, asserts that all true propositions have an *a priori* proof, although in general human beings cannot make those proofs", because of the infinity of the analysis required. (Hacking 1984, 221). Thus it is due to our limitations that some truths appear to be contingent and not necessary.

Everybody knows analytically that Hamlet's mother cannot have been a man, but nobody can know *a priori* and analytically what was the color of her eyes. Leibniz would consider this due to the fact that we, the human beings, unlike God, do not have the complete concept of "Hamlet's mother" at our disposal. We do not know all the details of her existence, nor the complete story of her life. In mathematics we do, because mathematical concepts are simpler, and thus mathematical truth is based on proof and mathematics is analytical (Hacking 1984). In mathematics the intensions of concepts are just definitions and mathematical concepts can be analyzed. It is therefore easy to see whether a proposition is analytic or synthetic, because we stay completely inside a language system as soon as we reason in mathematics. This does not apply to Kant's views.

The law of contradiction may, according to the above distinction, be interpreted in various ways. Let us consider the example "gold is a yellow metal". According to Kant the law of contradiction comes in because, according to the usual definition of terms, it would be a self-contradiction to say: "gold is not a yellow metal or bodies are not extended". Kant believed that to any substance some predicates inherently and essentially belong while others depend on experience, but the distinctions he draws in some cases of empirical concepts are rather arbitrary.

Now Kant takes great pains to distinguish analytic and synthetic propositions, because his view of the analytic-synthetic distinction depends on the invalidation of the ontological proof of God's existence and represents his own Copernican step. Classical thought rested in the idea of God. The proof of the existence of God warrants Leibniz' foundation of truth on proof as well as the Cartesian *cogito ergo sum*, this final truth which constitutes the foundations of the entire structure of Cartesian rationality. Accordingly a schism was caused in the heritage of the classical age, hence also in the foundations of modern science, by the invalidation of the proofs of God's existence, for God guaranteed a strict correspondence between clear and distinct thought on the one hand and external reality on the other.

Kant claims that "God exists", can never be analytic, as Leibniz believes, because "being is not a real predicate; that is, it is not a concept of something which could be added to the concept of a thing" (Kant B, 626). Thus the proposition "God exists" is not real knowledge. Kant realized "that no general description of existence is possible, which is perhaps the most valuable proposition that the Critic contains" (Peirce CP, 1.35). Kant changes orientation from substances and essential properties to concepts and objects, or functions and arguments as we have already seen when observing his definition of analytic and synthetic. God exists means that the extension of the concept God is not empty. God exists and God, on such a presupposition signify the extension and the intension of one and the same concept, or the factuality and the possibility of the being of God respectively (*ibid.*, 4.583). Extension and intension of concepts appear to be relatively independent of one another and the transition from the possible to the factual cannot be accomplished by means of logic or language and pure thinking. All judgments are conditional. The proposition that a triangle necessarily has three angles does not say "that three angles are absolutely necessary, but that, under the condition that there is a triangle, three angles will necessarily be found in it" (Kant B, 622). Kant, contrary to Cantor or Leibniz, did not consider consistency sufficient for existence even in mathematics.

And Kant shared with Leibniz the foundational concern expressed by aprioricism. Kant's transcendental subject, which takes the role of Leibniz's God, at first sight, shares the problematic nature of the latter, such that not much seems to be gained by this kind of reorientation. On the one hand all knowledge is based on the structure of the transcendental subject. But on the other hand, the transcendental subject is not directly accessible to the individual subject because the proposition "I think", which according to Kant is the supreme foundation of all knowledge (*ibid.*, 132), in itself does not express any knowledge. The proposition "I think" does not imply my existence, although it contains the proposition "I exist". Thus the transcendental subject is in a sense declared to be a thing in itself, "a kind of otherworldly entity" (Lektorsky 1980, 84-86). It guarantees however the constitution of the subject and object of knowledge by means of the process of Synthesis.

"A mind, in which all the manifold should also be given by self-consciousness would be intuitive; our mind can only think, and must look for its intuition to sense" (Kant B, 135). Thus the transcendental subject could become known only after having been externalized by constructive activity, and mathematics plays a special role in this process, as by means of mathematical reasoning we become aware of the forms of all intuition, which mathematics presents as original intuitions themselves. Mathematics therefore presents the general conditions of all knowledge *in concreto* (see, for instance Panza's paper, in this volume).

> "The constructivist project, rooted in Descartes' geometry and exfoliated in Kant's critical enterprise, took its bearings from the desire to master and possess nature, where nature was understood as the locus of apparently ineliminable or intractable otherness. Mind could aspire to master its other [...] by externalizing itself in a construction carrying the clear marks of its inward and deliberate origin." (Lachterman 1989, 23)

However, the idea of the human self producing itself, as well as knowledge as part of it, represents an essential step, because after taking it, the growth and the justification of knowledge become interrelated and intertwined. To understand the reality of knowledge one has to understand the reality of understanding. And in order to accomplish this one has to find a point where understanding is construction, conceived of as a unity of process and result. This can already be guessed from the very special role mathematics seems to play in Kantian epistemology. Mathematics gives the best example of knowledge as active creation. "Approximation to the ideal of a thoroughly free divine or archetypical intellect yields at one and the same time the basic sense of our active existence and the limits or mitigations to which this active existence is inevitably subject" (*ibid.*, 11). Kant, in fact, warns philosophers against trying to imitate mathematical procedures and methods, because in philosophy this unfolding identity of concept and object does not exist.

This warning is based on a very problematic distinction within the area of synthetic judgments *a priori*, classifying them into intuitive ones and discursive ones. The latter refer primarily to the ordering function of general concepts whereas the former are related to the structure of perception. The judgments of pure mathematics belong to the class of intuitive judgments. Kant himself describes the intended distinction as follows:

> "An *a priori* conception contains either a pure intuition, and in this case it can be constructed; or it contains nothing but the synthesis of possible intuitions which are not given *a priori*. In this latter case the concept may help us to form synthetical a priori judgments, but only discursively by means of concepts and never intuitively, by means of the construction of concepts." (Kant B, 749)

And he generally classifies axioms as intuitive principles, adding that philosophy does not possess any axioms and "has no right to impose her a priori principles upon thought, until it has established their authority and validity by a thorough deduction" (*ibid.*, 762; we should take into account at this point that deduction means legitimation rather than logical or mathematical deduction). Thus Kant introduces a separation between intuitive and discursive knowledge, which seems to exclude mathematics from conceptual thinking. Mathematics "does not only construct magnitudes, as in geometry; it also constructs magnitude as such, as in algebra" (*ibid.*, 745). Synthetic *a priori* knowledge in the sense of Kant is most importantly characterized by its constructivity.

Kant believes that mathematics rests on concepts that are given by definitions and that mathematical cognition originates from the construction of the concepts.

> "To construct a concept, however, means: to present the intuition corresponding to it *a priori* [...]. Thus, I construct a triangle by the presentation of the object corresponding to this concept either by mere imagination in pure intuition, or after the latter also on paper, in empirical intuition, in both cases completely *a priori* [...]." (*ibid.*, 741-742)

This distinction depends on the fact that in the world of phenomena "there are two elements—the form of intuition (space and time) [...] and the matter or content, that which is presented in space and time, [...]" (*ibid.*, 751). We are able to construct mathematical concepts *a priori* "in as much as we are ourselves the creators of the objects of the concepts in space and time" (*ibid.*, 752). Mathematical concepts were constituted by definitions (*ibid.*, 756) and had to be reified or applied in intuition according to space and time as the forms of pure intuition. In as much as mathematical reasoning operates on these reifications it is synthetical, otherwise analytical. But no mathematical truth can be acquired by analytical reasoning only, because we have to apply a concept to gain knowledge. We cannot cogitate a straight line without drawing it, Kant says, (*ibid.*, 154). The line drawn is empirical and is therefore no mathematical object, but it is the construction of a mathematical concept. To know means to observe one's own constructive activity and its results.

Kant held a much narrower view with respect to the subject matter of mathematics than say Leibniz, as his contemporaries already noticed. Why is "the form of mathematical knowledge the cause that it is limited exclusively to quantities" (*ibid.*, 743)? Because the construction refers to either geometrical or algebraic algorithms or functions taken in intension, construction of the pair $\{x,f(x)\}$. Thus a judgment is to be presented by a pair or a relation $\{x,f(x)\}$. It is sometimes said that Kant gained his vision of mathematical cognition from the problems of analytical geometry. And his concepts indeed depend on functions insofar as Kant defines functions as the "unity of the act of arranging various representations (*Vorstellungen*) under one common representation" (*ibid.*, 93). The pair or relation $\{x,f(x)\}$ represents a reified function or a judgment. All judgments are functions. Kant took the idea of function from algebraic analysis in the sense of Euler and Lagrange, identifying function with algorithm or formula (Cassirer 1910).

A completed or reified function may be understood as a representation of a representation of an object. This Kant also calls "mediate knowledge of an object". Lachterman claims that Kant took his understanding of the technique of construction from algebra and not from geometry. "Kant's phrase 'construction of a concept' is derived from the expression 'construction of an algebraic equation'". This latter expression refers to "the interpretation of the terms of the equation in ways that lead to the actual exhibition of a particular geometric formation satisfying the general equation" (Lachterman 1989, 11). The construction was meant to yield line segments that corresponded to the roots of the equation together with its application in a particular case. This technique does nothing but exhibit the algorithm by which we arrive at the roots of the equation. The technique gradually disappears around the middle of the eighteenth century. But between the publication of Descartes' *Geometry* and about 1770 it was considered crucial within algebra and analytic geometry and was developed by first-ranking mathematicians (Bos 1984).

Euclidean geometry itself is thoroughly algorithmic. Euclid had founded a geometry that allowed constructions by straight lines and circles. "Descartes had extended this geometry by allowing in principle all algebraic curves as means of construction" (Bos 1984, 360). Newton wanted algebra to be more subservient to geometry and wished "to work out a truly geometrical approach to the construction of problems and equations. Geometrical simplicity, namely the simplicity of tracing, should be the criterion, not algebraic simplicity" (*ibid.*, 362-363). In 1835-1844 a similar motivation led Grassmann, to introduce the direct methods of vector algebra into the geometrical sciences in order to mathematize projective geometry. The point of reference of the construction should be immanent rather then external, was the demand. Grassmann, like Leibniz or Poncelet, wanted to operate on geometrical entities rather than on functions (coordinates) in order to realize a synthesis brought about by the intrinsic properties of space itself. The following statement from Peirce's Cambridge Conference of March 1898 sounds very much like Grassmann indeed:

> "That which already had been called the Elements of geometry long before the day of Euclid is a collection of convenient propositions concerning relations between the lengths of line, the area of surfaces, the volumes of solids and the measures of angles. It concerns itself only incidentally with the intrinsic properties of space." (Peirce CCL, 242-243)

But it is projective geometry or topology (geometrical topic as Peirce called it) "what the philosopher must study who seeks to learn anything about continuity from geometry" (*ibid.*, 246). And continuity is essential to understand synthesis as soon as Aprioricism has been abandoned, as has already been mentioned in the introduction above.

The Greeks, Peirce believes, were acquainted with projective geometry and had already perceived "that it was more fundamental,—more intimately concerned with the intrinsic nature of space,—than metric is" (*ibid.*, 244). Principles of continuity are indispensable when reasoning about infinity, as in calculus or the theory of irrationals, for instance. By means of the notion of similarity or self-similarity Greek diagrams demonstrate the irrationality of the measure of the ratio of the side to the diagonal of certain regular polygons, like, for example, the regular pentagon. One may in fact consider these diagrams in different ways.

Concentrating on the exhibition of self-similarity one might obtain as a result, on the basis of the visual representation, insight into the recursive structure of the Euclidean algorithm for determining a common unit or the greatest common divisor. The true application of the notion of similarity requires us to disregard scales, that means accepting geometric space as a continuum and as an individual whole, in the sense of Peirce. The invariance under geometric similarity then directly demonstrates that the algorithm does not lead to the desired goal, the algorithm itself having been transformed into an object of thought. Side and diagonal are thus incommensurable, *i.e.* $\sqrt{5}$ is an irrational number. Note that I have not proposed to replace the geometrical quantities involved by their numerical measures with respect to a certain unit and therefore I have not obtained the result by an indirect method, based on the law of contradiction, but have "seen" it directly because of the recursive structure of the algorithm.

It is by geometric construction that we notice the concept of say $\sqrt{5}$ not being empty, but such a root is not a number, says Kant, "but only the rule of approximating it" (letter by Kant to Rehberg, quoted after Parsons 1983, 111). But the law of approximating it or the rule can replace the series of values in that approximation, it is the intensional side of this concept of $\sqrt{5}$. Besides Kant, such a view was also held by intuitionists like Kronecker. Kronecker argued that if you have a rule which effectively determines every term of an infinite sequence, then the law itself can replace the sequence. It is obvious that one can only represent the class of computable numbers (in the sense of Church or Turing) in this manner, and therefore the idea of infinity involved here means the countable infinite only.

To the challenge, put forward by Rehberg, that the application of arithmetical truth to sensible items may well be subject to the conditions of time, but not arithmetic as such, Kant replies by letter:

> "As soon as instead of x, the number of which it is the sign $\sqrt{5}$ is given, in order not merely to designate its root as in algebra, but to find it, as in arithmetic, the condition of all generation of numbers, namely time, is unavoidably presupposed." (*ibid.*, 117)

Again we might conclude from this that the procedure or rather its concept, the concept of a particular algorithm is to be exhibited in space or time as the forms of pure intuition. Once again we may observe that it is mathematical constructivity, that means the exhibition of the sense of mathematical concepts which Kant had in mind, when terming mathematics synthetic *a priori* knowledge. Existence is made equal to exhibition in space and time. We construct problems which did not exist prior to our definition of them. And by constructing them and presenting their properties, we construct the construction itself, exhibiting it in the forms of pure intuition.

III Kant and Forster

Peirce agrees with Kant in that it is the idea of the (epistemic) subject on which any conception of knowing is founded. Peirce's concern, however, is not with the unity of ideas (*Vorstellungen*) in a self-consciousness, but rather with the socially effective unity represented by signs, like works of art or of science. "Consciousness is used to denote the I think, the unity of thought; but the unity of thought is nothing but the unity of symbolization", Peirce says (CP 7.585).

Kant assumes that all our knowledge extending cognitions are synthetical. For him, however, this synthesis does not lie in the matter of experience as such, but springs from the function of cognizant consciousness itself which this way becomes aware of itself. The synthetic unity of consciousness, according to Kant, is "an objective condition of all knowledge. [...] For in the absence of this synthesis, the manifold would not be united in one consciousness" (Kant B, 138). Peirce now stresses that this very unity is based on the reality of the continuum. The continuum being that on which the unity of symbolization is based. This unity is not just an *ex post* fact. Representations or interpretations are not arbitrary or just contingent.

> "Thus, the question of nominalism and realism has taken this shape: Are any continua real? Now Kant, like the faithful nominalist [...], says: 'no'. The continuity of Time and Space are merely subjective. There is nothing of the sort in the real thing in itself." (Peirce Ms, 439 and NEM, IV, 343)

That Kant had given epistemology too much of a "subjectivist" turn emerges therefore, first of all, in his conception of the (epistemic) subject, which he conceives primarily in terms of activity, or according to Peircean terminology, as Secondness.

> "Secondness is that in each of two absolutely severed and remote subjects, which pairs it with the other not for my mind nor for, or by, any mediating subject or circumstance whatsoever, but in those two subjects alone. [...] But this pairedness [...] is not mediated or brought about; and consequently it is not of a comprehensible nature, but is absolutely blind. [...] In their essence the two subjects are not paired." (Peirce CCL, 147-148)

Kant had learnt from Hume that relations are "external", that they represent nothing of the essence of the relata, that they are arbitrary. What in the nature of Paul should cause his being taller than Peter? All subjects are isolated like Leibnizian monads. Continuity we find, according to Kant, only in the realm of phenomena as they are synthezised by activity.

Peirce, in contrast, repeatedly emphasized (for instance in his various criticisms of William James, who held views of the continuum similar to Kant's) that action is not the ultimate aim and end of humans (CP, 2.151; 2.763; 5.3; 5.429; 8.115; 8.212). The highest kind of synthesis according to Peirce is represented by

Thirdness. Thirdness replaces Kant's so-called "highest point", that is, synthetic unity of consciousness. Thirdness is what makes representation real. Under the perspective of Thirdness the human subject is to be characterized primarily by its capacity to grow, or to learn and evolve.

Hegel had already put forward a similar criticism of Kantian dualism (cf. Hegel, "Glaube und Wissen", W, I, 1-154). But Hegel neglected the importance of Secondness altogether. Hegel regards the Third as the only true one Category. For in the Hegelian system the other two are only introduced in order to be *aufgehoben*" (Peirce CP, 5.79). Hegel,

> "seeing that the *Begriff* in a sense implies Secondness and Firstness, failed to see that nevertheless they are elements of the phenomenon not to be *aufgehoben*, but as real and able to stand their ground as the *Begriff* itself. The third element of the phenomenon is that we perceive it to be intelligible, that is, to be subject to law, or capable of being represented by a general sign or Symbol." (*ibid.*, 8.268)

Peirce's own position is reflected very clearly in some passages taken from a manuscript written in 1890 under the title *A Guess at the Riddle*:

> "The highest kind of synthesis is what the mind is compelled to make neither by the inward attractions of the feelings or representations themselves, nor by a transcendental force of necessity, but in the interest of intelligibility, that is, in the interest of the synthesizing 'I think' itself; and this it does by introducing an idea not contained in the data, which gives connections which they would not otherwise have had. [...] Kant gives the erroneous view that ideas are presented separated and then thought together by the mind. This is his doctrine that a mental synthesis precedes every analysis. What really happens is that something is presented which in itself has no parts, but which nevertheless is analyzed by the mind, that is to say, its having parts consists in this, that the mind afterward recognizes those parts in it. Those partial ideas are really not in the first idea, in itself, though they are separated out from it. It is a case of destructive distillation. When, having thus separated them, we think over them, we are carried in spite of ourselves from one thought to another, and therein lies the first real synthesis. An earlier synthesis than that is a fiction." (*ibid.*, 1.383-384; this resembles closely Marx's characterization of the dialectical method)

The problematic nature of Kant's conception of the subject, and of his entire epistemology, is nicely reflected in a controversy between Kant and Forster, which took place in 1785. As a boy joining his father, Georg Forster (1754-1794), Alexander von Humboldt's teacher, accompanied James Cook on the latter's second sailing around the world. This voyage took almost three years, and Forster became famous in Europe, still a young man, for his report of it. In an article entitled "Noch etwas über die Menschenrassen [Some Additional Remarks on Human Races]", in which he opposed Kant's considerations concerning "Die Bestimmung des Begriffs einer Menschenrasse und mutmaßlicher Anfang der Menschengeschichte [Determining the Concept of a Human Race and presumptive Beginnings of Human History]", Forster wrote:

> "A large part of the merit Linné earned in botany was incontestably in the precise definitions. [...] After certain assumptions which he abstracted from his own experience, he designed his structure and fitted the creatures of Nature into it. As long as our insight remains limited, however, we would seem far from an infallibility of principles. Will categorizations which are based on limited experience, while possibly useful within these limits, not appear one-sided and half-true once the horizon is expanded, the point of view displaced? [...] Perhaps our present scheme of the sciences will become obsolete and deficient half a century from now, just like the previous ones. Even speculative philosophy would seem to be prone to this fate. Who does not immediately think of the *Critique of Pure Reason* in this context? Even if the theorem that one can only find in experience what one needs if one knows beforehand what to look for, were undisputedly correct [as Kant had written in the *Berliner Monatsschrift* of November 1785 (Kant SA, VII, 107)], a certain care would nevertheless be in order when applying this theorem, to avoid the most common of illusions, namely that in looking for what one needs, one presumes to have found the same even in places where it is really not present." (Forster W, I, 5-6)

Forster's point here is that there can be no transcendental and absolute insight. Otherwise, the new and unexpected would be nothing but a passive case of application of the preestablished categorical frame and the established prejudices. The new would be reduced to things already well familiar, and new insights could never emerge.

In content, the polemic between Forster and Kant is about determining the concept of Human Race and about the question whether Europeans and Africans belong to different genera, or whether they should not better be considered, because of a presumptive common origin, as species of one and the same genus. Both authors depart from their own concept of Nature. For Forster, who traveled the world already as a boy, Nature is the whole, is reality as a continuum, in which all differences and connections can be found.

Forster always points out the systemic character of reality and of Nature in particular.

> "A Negro"—Forster says for instance, is properly speaking—"a true Negro only in his own fatherland. Any creature of Nature is what it should be only in the locality for which it has been created; a truth which is seen confirmed every day in menageries and botanical gardens. A Negro born in Europe is like a greenhouse plant, a modified creature, in all properties subject to change more or less unlike that which would have become of him in his own fatherland." (*ibid.*, 13)

Forster was very familiar with the principle of continuity as it was used by eighteenth-century French authors, like Buffon or Robinet for instance, to emphasize the "Great Chain of Being" (Lovejoy 1936). On the other hand, Forster says, all our categorizations are necessarily arbitrary, a situation which already results from the fact that we are only able to think within fixed differences while the distances between the various genera in Nature fill an entire continuum.

> "The order of Nature does not follow our categorizations, and as soon as one tries to impose them on it, one falls into inconsistencies. Each and every system is meant only to be a guideline for memory by giving sections as Nature itself seems to make them." (Forster W, 22)

Hence, and in contrast to Kantian epistemology, any constructive synthesis is preceded or accompanied by analysis.

What can be said in view of this situation with regard to the question whether "Negro" is a genus or a species of humankind?

Forster, on the basis of his own systemic reasoning, assumes that Nature, like any continuum, forms a complete whole in every locality of the earth and in every climatic zone and that man represents no exception.

> "If every region produced the creatures which were appropriate to it, and moreover in precisely those relations which were indispensable for their safety and upkeep: how is it that the fragile human being should be an exception here? Rather, Nature has given its own character, as Herr Kant himself professes, its special organization, an original relationship to a climate and suitability to the latter to each and every stock and race. Indisputably, this precise relationship between the land and its inhabitants can be most easily and briefly explained by the local emergence of the latter." (*ibid.*, 28)

Kant, in contrast, had claimed that all human races stemmed from only one and the same root.

Forster hesitates to answer the question "whether there are several original races" with certainty, but considers this hypothesis no less plausible than the Kantian one. And to Kant's teleological or functionalist reasoning that in case of bigger differences human beings need to wage war on one another and that it is thus not in the interest of Nature to create such differentiation, Forster objects as follows:

> "In a world where nothing is superfluous, where everything is linked by the finest nuances, where the concept of perfection finally consists in the aggregate and in the harmonic cooperation of all individual parts, the idea of a second genus of humans would be for the supreme mind a forceful means to develop ideas and feelings which are worthy of an earthly creature endowed with reason, thus interweaving this creature himself much more firmly with the plan of the whole."

And he observes that one need only look at the slave trade to see how idealistic and abstract Kant's considerations are. Slavery has not been prevented at all by the belief that all human beings are of one kind only.

Kant published a reply to Forster's objections, "Über den Gebrauch teleologischer Prinzipien in der Philosophie" (Kant SA, VIII, 157-184). In his retort Kant wishes to do more than just maintain his position on the necessity of *a priori* principles: "It is indubitably certain that by mere empirical stumbling about without a guiding principle defining that which is sought after, nothing useful would ever be found" (*ibid.*, 161). Kant accordingly begins with a quite different concept of Nature:

> "If, by Nature, we understand the embodiment of everything which exists determined by laws [...] research into Nature can pursue two paths, either the merely theoretical or the teleological one, while using, however, [...] only such purposes which can become known to us by experience [...] for its intention. [...] Rightfully, reason calls first for theory in every study of Nature, and only later for teleology." (*ibid.*, 159)

Nature is no more than order and uniformity of appearances. We prescribe *a priori* rules to which all possible experiential reality must conform.

For Kant it seems indisputable that "where theoretical sources of knowledge do not suffice, we may make use of the teleological principles, but with such limitations of its use that theoretical-speculative research will always be assured precedence in order to try its best effort on the question at hand" (*ibid.*, 164). From the necessity of this principle, Kant now derives an essential distinction between natural history and a mere description of Nature. Natural history, according to Kant, is exclusively concerned with "pursuing back, only as far as analogy permits, the connection between certain present features of natural things and their causes in former times according to the laws of cause and effect which we do not invent but derive from forces of Nature as they present themselves to us" (*ibid.*, 162). It is evident here that Kant is not concerned with the objects, but with the laws, and further with getting "to know more precisely the limits of these laws lying in reason itself, together with the principles according to which they could best be extended" (*ibid.*, 165).

Kant's intention is to determine

> "how the greatest variety in genesis can be reconciled by reason with the strictest unity of origin [...]. And one sees clearly here that one must be guided by a certain principle to even observe, that is to pay attention to what could give indication of the origin not only of similarity of appearance, because we are concerned here with a task of natural history, not of the description of nature." (*ibid.*, 164)

Kant then introduces such a principle which is intended to demonstrate a difference of origin, that is "the impossibility of obtaining fertile descendants by mixing two genetically different species of humanity" (*ibid.*, 164-165).

According to this concept, Kant writes, "all men on the wide Earth belong to one and the same genus of nature, because they can consistently sire fertile children with one another, no matter how large the differences in their appearances encountered" (Kant SA, II, 430). Kant says that to assume a variety of "local creations" is an opinion "which multiplies the number of causes without necessity" (*ibid.*, 431). "It is the appropriateness in an organization which is the general reason from which we conclude that there is a design originally placed in the nature of a creature" (Kant SA, VII, 103).

Against this criterion, Forster again raises systemic objections by arguing that things in Nature are quite different from those in an experimental situation brought about arbitrarily. Artificial experiments, like breeding experiments "conducted with animals under the constraints of captivity" must not be quoted as genuine scientific explanations of cause. But he does not see this as an absolute counter argument to Kant concerning the hypothesis on the origin of Man, or a counter-argument only insofar as he qualifies Kant's criterion as totally arbitrary, as a matter of mere definition. This resembles Hegel's charge that Kantian reason furnishes only postulates and not knowledge of reality (cf. Hegel, "Glauben und Wissen", *cit.*).

For Kant reasoning is founded on certain teleological principles. Thus, he says:

> "In view of the varieties [*i.e.* of species], nature seems to prevent a fusion, because it runs counter to its purpose, namely manifolds of characters, while it at least permits this (fusion) in case of different races [...] because this makes the creature adapted to several climates while not making it suitable for any of them to the same degree found in the original adaptation to it." (*ibid.*, VIII, 166-167)

That the latter also leads to disadvantages is proved, for Kant, by "the inferior quality" of (American) Indians who exist both in the northern and in the tropical climates. Kant argues against Forster who assumes that every region created its own human race by saying that:

> "If one does not want to add a second to the special creation of the Negro already suggested by Herr Forster, namely that of the American (Indian), no other answer remains but that America is too cold or too new to ever produce the degeneration of the Negroes or of the yellow Indians, or to have produced them in the short period it has been inhabited." (*ibid.*, 176)

Kant thus assumes that men are, on the one had, of one common origin and that on the other hand, a cause lying in themselves, "and not merely in the climate", must have led to the differences between them. For Kant, as is well known, the transcendental principles of the use of reason must serve as a basis to derive everything else in a way coordinated with observation. Laws are verified ex post, since "by mere empirical stumbling around without guiding principles as to what should be sought", nothing useful will be ever found, "for to have experience methodically means solely to observe" (*ibid.*, 161). For Forster, conversely, the principles themselves must also result from observation, even if this cannot be imagined to come by itself and without activity from the cognizant subject.

The excessive mixture of speculations and principles ranging from phlogiston theory to medicine which he draws upon to explain differences in skin color and the like is very remarkable in Kant's argumentation. His contributions are entirely unreadable, while Forster's are still informative today. For instance, Kant takes external features like skin color for mere body paint "which is added to the skin by the sun and which will be taken away again by colder air" (*ibid.*, 105). Everything which cannot be brought in agreement with any kind of experience is mere speculation. In any case, Kant gives the element of the epistemic subject's activity priority over the material element and this is how the principle of synthetic unity of apperception really works. The contrast between Kant and Forster seems essentially to correspond to the two poles in the system-subsystem paradox. This is sometimes presented as follows: "Any given system can be adequately described provided it is regarded as an element of a larger system. The problem of presenting a given system as an element of a larger system can only be solved if this system is described as a system" (Blauberg, Sadovsky and Yudin 1977, 270). It seems obvious that the system paradoxes enforce an evolutionary perspective for their resolution. Kant starts from the necessity of characterizing his own subsystem, Man, as a system before all else, because the (epistemic) subject guarantees the possibility of knowledge, whereas Forster characterizes Man primarily as a subsystem of a more comprehensive system, namely Nature.

One cannot err in assuming that Kantian reasoning is rather more determined by the inner regularities and forces of the mind, that is by mental motive forces, and less by intuition and experience. It seems to be a reasoning based, as Peirce said, on the relation of similarity, for "of the two generally recognized principles of association, contiguity and similarity, the former is a connection due to a power without, the latter a connection due to a power within" (Peirce CP, 6.105). Now Peirce has pointed out that it is exactly analytical reasoning which "depends upon associations of similarity, synthetical reasoning upon associations of contiguity"(*ibid.*, 6.595).

IV Some Issues where Peirce and Kant differ

Peirce writes:

> "Kant divided propositions into Analytic, or Explicatory, and Synthetic, or Ampliative. He defined an analytic proposition as one whose predicate was implied in its subject. This was an objectionable definition due to Kant's total ignorance of the logic of relatives. The distinction is generally condemned by modern writers; and what they have in mind (almost always most confusedly) is just. The only fault that Kant's distinction has is that it is ambiguous, owing to his ignorance of the logic of relatives and consequently of the real nature of mathematical proof. He had his choice of making either one of two distinctions. Let definitions everywhere be substituted for definite in the proposition. Then it was open to him to say that if the proposition could be reduced to an identical one by merely attaching aggregates to its subjects and components to its predicate it was an analytic proposition; but otherwise was synthetic. Or he might have said that if the proposition could be proved to be true by logical necessity without further hypothesis it was an analytic one; but otherwise, was synthetic. These two statements Kant would have supposed to be equivalent. But they are not so." (NEM IV, 58)

The difference Peirce has in mind, I believe is this: Any subject-predicate expression can be transformed by means of a hypostatic abstraction into a logically and empirically equivalent relational statement (CP, 1.551; with respect to the fundamentally important notion of hypostatic abstraction see also: *ibid.*, 4.234, 4.235, 4.463, 4.549, 5.447, 5.534 and NEM IV, 49). Now if the original statement has not just been a logical truth it exhibits its hypothetical character, because the reality of hypostatic abstractions and of relations in general remains a hypothetical one. We have to construct hypostatic abstractions to make possible what Peirce calls theorematic reasoning.

Since Kant's abstract definition is ambiguous, Peirce continues:

> "We naturally look to his examples, in order to determine what he means. Now turning to Rosenkranz and Schubert's edition of his works, Vol. II [the *Kritik*, Kant B, 14] p. 702 we read, *Mathematische Urtheile sind insgesammt synthetisch*. That certainly indicates the former of the two meanings, which in my opinion gives, too, the more important division. The statement, however, is unusually extravagant, to come from Kant. Thus, the 'Urtheile' of Euclid's Elements must be regarded as mathematical; and no less than 132 of them are definitions, which are certainly analytical. Kant maintains, too, that 7+5 = 12 is a synthetical judgment, which he could not have done if he had been acquainted with the logic of relatives. For if we write G for "next greater than," the definition of 7 is 7 = G6 and that of 12 is 12 = G11. Now it is part of the definition of plus, that $Gx+y = G(x+y)$. That is, that G6+5 = G11 is implied in 6+5 = 11. But the definition of 6 is 6 = G5, and that of 11 is 11 = G10; so that G5+5 = G10 is implied in 5+5 = 10, and so on down to 0+5 = 5. But further it is a part of the definition of plus that $x+Gy = G(x+y)$ and the definition of 5 is 5 = G4, so that 0+G4 = G4 is implied in 0+4 = 4, and so on down to 0+0 = 0. But this last is part of the definition of plus. There is, in short, no theorematic reasoning required to prove from the definitions that 7+5 = 12. It is not even necessary to take account of the general definition of an integer number. But Kant was quite unaware that there was such a thing as theorematic reasoning, because he had not studied the logic of relatives. Consequently, not being able to account for the richness of mathematics and the mysterious or occult character of its principal theorems by corollarial reasoning, he was led to believe that all mathematical propositions are synthetic." (NEM IV, 58)

Now theorematic reasoning, according to Peirce, essentially depends on hypostatic abstraction. I am able to prove, he writes, "that the most practically important results of mathematics could not in any way be attained without this operation of abstraction" (*ibid.*, 49). We depend on hypostatic abstractions to make relations visible that would otherwise remain hidden.

Kant says that we do not have axioms in arithmetic, because statements like "7+5 = 12" have nothing general to themselves (Kant B, 206). Number symbols seem to be proper names of concepts that have to be applied to gain objectivity. This implies the syntheticity of the statement. But Kant wants it to be *a priori* also. The whole matter, as presented above, therefore rests on a sharp distinction between intuitive and discursive conceptions and procedures.

Peirce ascribes to Kant the merit of having given for the first time in history the distinction between the intuitive and discursive processes of the mind its proper weight. If mathematics is not merely tautological it must contain an intuitive element. But the line between intuition and logic being drawn too firmly, the greatest merit of Kant's doctrine turns itself at the same time into its greatest fault (Peirce CP, 1.35). Kant misses the importance of relations, and "wholly fails to see that even the simplest syllogistic conclusion can only be drawn by observing the relation of the terms in the premises and conclusion" (Peirce W, 5, 258). This is done by means of appropriately constructed diagrams. Peirce believes that mathematics proceeds by diagrammatic reasoning and that a diagram is characterized by the fact that one is able to find out more than was necessary to construct it. Mathematical reasoning is diagrammatic. But diagrams may nowadays contain highly complex conceptual structures. Recall for example the exact sequence defining the notion of a group extension, or the diagrams of homological algebra in general. In any case they do not contain names of definite objects. They are icons and deal with generals only, with hypostatic abstractions, and any individual, "whatever is determinate in every respect must be banished from the logic of mathematics" (Peirce NEM IV, XIII). An icon, like a free variable, does not "profess to represent anything; for if it did, that would be a manner of signifying its object, not consisting in merely resembling it" (Peirce CP, 8.119).

According to Kant, a theorem like "7+5 = 12" is not purely analytical, because

> "the conception of a sum of 7 and 5 contains nothing but the uniting of the two numbers into one, whereby it cannot at all be cogitated what this single number is which embraces both. The conception of twelve is by no means already obtained by merely cogitating the union of 7 and 5; [...] One must go beyond these concepts, and have recourse to an intuition [...]." (Kant B, 15-16)

An intuition of what, the reader might ask. And he might think, what is clearly needed is an intuition or a concept of the relations and algorithms involved, the relation of recurrence, for instance (such were already the views of Bolzano in 1810 and later again Poincaré). Kant continues by saying: "[...] an intuition, which corresponds to one of the two—our five fingers, for example, [...] and so by degrees add the units contained in the five given in the intuition to the conception of seven" (*ibid.*, B 16). Thus it is obvious that the syntheticity derives from my faculty of coping with the algorithm and that this in turn relies on the fact that it is applied onto particular cases. Kant's distinction between purely conceptual argument or deduction on one side and the application of concepts on intuitions (concepts according to Kant can only be applied on *Vorstellungen* of things rather than things themselves (*ibid.* B, 94)) remains artificial, because even in formal deduction a meta-cognitive element is always present. To state that in Peircean terminology: deduction involves Thirdness and is not confined to Firstness and Secondness.

This results in the first point of difference. Peirce even says that the entire Kantian philosophy must fall to the ground, as his logical system of distinctions of propositions is artificial, resting on mere accidents of language. As soon as one formulates the concept of arithmetical sum, for instance, in terms of the cardinality of sets, the concept is obtained as a law, and the arithmetical theorems in question thus become synthetical. As soon as the whole numbers, however, are constructed completely from the concept of ordinal numbers, introducing the concept of sum axiomatically and recursively on the basis of the successor operation of the ordinal numbers, the arithmetical theorems become analytical (Otte 1992, part IV). This situation leaves us with the choice of either negating the analytic-synthetic distinction any objective meaning or claiming that the operations of mathematical deduction and of concrete observation are not as distinct as it might appear. Peirce takes, as has been shown above, the second route. What is missing in Kant, he says, is the logic of relatives as it is developed from an analysis of diagrams, and as it is involved even in a perceptual judgment.

Now the logic of relatives shows

> "that observation and ingenuity are involved in the reasoning process. For it leads us to perceive that purely deductive reasonings involve discovery as truly as does the experimentation of the chemist; only the discovery here is of the secrets of the mind within, instead of those of Nature's mind. Now the distinction between the Inward and the Outward, great and decisive as it is, is, after all, only a matter of degree." (Peirce NEM, IV, 355)

Thus the analytic-synthetic distinction also is only a matter of degree (see also part V of this paper).

The usefulness of mathematics is due to the fact that mathematical relations are to be interpreted and applied in an indeterminate multitude of constellations. They relate possibilities not facts. Kant already had seen that things necessarily remain isolated. Thus laws or axioms do relate generals rather than things. They are conditional counterfactuals. Sets of possibilities is what physicists speak about: the configuration space of a system is the set of its possible instantaneous states. Natural laws and mathematical axioms or propositions thus establish relations between possibilities, which means between free variables or continua. "A true general is a whatever-should-be which will impart its generality to the following would be", as Peirce says (Ms, 641). Peirce thus assumes that a characteristic of mathematical thought is, "that it can have no success where it cannot generalize". Mathematicians strive for the greatest possible generality, often "exchanging a smaller problem that involves exceptions for a larger one free from them" (Peirce CP, 6.236). But generalization in respect to its widest possible scope is continuity or refers to the continuum, because "the continuum is all that is possible" (Peirce CCL, 160). In contrast to Kant Peirce believes that continuity is real and that possibility is not just our present possibility. The idea of possibility is not constrained by the idea of a (transcendental) subject. The human subject is a potentially unlimited being and growth or evolution marks its essence, rather than activity (see part III).

Thus we may understand his second point of disagreement with Kant, which is to be seen in the characterization of continuity. We know about the importance of the principle of continuity from the history of mathematics. To Peirce's realism it is however essential to conceive of the continuum, not as a collective entity, but as strictly general. Peirce uses the idea of continuity to introduce the reality of generality. But the reality of a general is the reality of the possible. Thus continuity is possibility. The possible is however not determined and fixed in every respect. Therefore Peirce refuses the continuum being constructed and built up from particulars, as in Cantorian set theory or arithmetized analysis after the fashion of Cauchy. Possibility is essential to Peirce because to really conceive of epistemology in evolutionary terms, the indeterminate or less determinate and possible must to a certain degree govern evolution. Only the past is factual, whereas thought is directed also to the future and therefore to the possible, rather than factual. Otherwise one could not understand how new objects, new laws and new knowledge in general can arise.

With Peirce's abandonment of aprioricism the relation between generality and continuity becomes prominent. Free variables such as in axiomatic statements or statements like "a triangle has ..." do not imply a definite ontological commitment. A free variable or a "general triangle" does not represent a general that is predicative. It refers to a mere possibility. Therefore the term "general" is used by Peirce to designate a regularity or a law open to an indefinite number of instantiations, which means to something beyond all definite cardinality and this something therefore represents a continuum. Were it a set of distinct individuals and not a continuum, then Cantor's powerset axiom would show that it cannot be beyond all multitude. Is there any sense, asks Peirce, "in saying that something that is not a multitude of distinct individuals is more than every multitude of distinct individuals". Yes, he answers, there is in the following way.

> "That which is possible is in so far general, and as general, it ceases to be individual. Hence, remembering that the word 'potential' means indeterminate yet capable of determination in any special case, there may be a potential aggregate of all possibilities that are consistent with certain general conditions; and this may be such that given any collection of distinct individuals whatsoever, out of that potential aggregate there may be actualized a more multitudinous collection than the given collection." (Peirce CCL, 247)

The particular is at the same time general, and the concept of the general must be related to continuity, because a general relationship is a relationship that is stable under small perturbances. Such a variation does not concern a set of facts but a set of possibilities or hypotheses such that a general is a relation between possibilities, which are dependent on continuity and which have no isolated individual existence. The continuum thereby gains an ontological status independent of synthesizing activity and this certainly implies that any mathematical reasoning contains an analytical element, because of the fact that the continuum is not, as Kant believed, subordinate or secondary to a preceding mental synthesis. This idea thus involves that of a continuum. This new idea of general was expressed by Poncelet and by Peirce in nearly the same words.

We know that an algebraic or a complex analytic function f, such that $f(x) = g(x)$ holds for as small a variation of the argument as you please, is identical with g. Poncelet, on the basis of such observations, and taking into account that analytical geometry consists in coordinating continua, understood that the principle of continuity is at the heart of operating with equations like $x = 5$; and that it is the secret of algebraic generality. We can accept $x = 5$ and operate with it although a variable x and a particular value of that variable are of different logical type. The particular, an ellipse for instance, represents in a certain sense, which cannot universally be specified, the general, the conic; as long as it represents certain essential properties pertinent to that purpose, which are stable under continuous variation. Poncelet aimed at a method that was based on the interaction of general and particular, concept and representation. As in geometry, one can always only represent the general by a particular, the genus by a species, or the category by a prototype, as with the idea of "general triangle", for instance, or like a conic section by a particular exemplar, like a circle or an ellipse, one has to employ the principle of continuity to state in full generality relationships that have been verified for a particular diagram. Poncelet himself described the procedure as follows:

> "Let us consider some geometrical diagram, its actual position being arbitrary and in a way indeterminate with respect to all the possible positions it could assume without violating the conditions which are supposed to hold between its different parts. Suppose now that we discover a property of this figure, whether it be metrical or descriptive, by means of ordinary explicit reasoning—that is, by methods alone regarded as rigorous in certain cases. Is it not clear that if, observing the given conditions, we gradually alter the original diagram by imposing a continuous but arbitrary motion on some of its parts, the discovered properties of the original diagram will still hold throughout the successive stages of the system, always provided that we note certain alterations, such as that certain quantities vanish, etc.—alterations, however, which can easily be recognized a priori and by reliable rules?" (Poncelet AAG, II, 531)

Thus the permanence of relationships rather than the empirical and isolated existence or non-existence of the relata validates the argument. The general is of the character of a relationship or connection, like an idea that spreads among minds. Peirce makes this comparison between natural laws and the effect of words. "It is proper to say that a general principle that is operative in the real world is of the essential nature of a representation and of a symbol because its *modus operandi* is the same as that by which words produce physical effects" (Peirce CP, 5.105).

Third, the belief that mathematics represents absolute and apodictic true knowledge, may be questioned on grounds of two types of arguments, doubting that there is indubitable knowledge at all or questioning that mathematics represents factual knowledge. Peirce voices both kinds of disbelief. We have already dealt with one of them above in the first point of divergence. With respect to the second Peirce writes:

> "Kant regarded mathematical propositions as synthetical judgments *a priori*; wherein there is this much truth, that they are not, for the most part, what he called analytical judgments; that is, the predicate is not, in the sense he intended, contained in the definition of the subject. But if the propositions of arithmetic for example are true cognitions, or even forms of cognitions, this circumstance is quite aside from their mathematical truth." (*ibid.*, 4.232)

Mathematics is not at all concerned with meanings (*ibid.*, 5.567), but rather, as Peirce writes, with the substance of hypotheses. "Mathematics is purely hypothetical: it produces nothing but conditional propositions" (*ibid.*, 4.240). And what is more important: mathematics cannot be applied to reality by first identifying premises in every detail. Observable details do not at all guarantee any real connection and "synthetic inference is founded upon a classification of facts, not according to their characters, but according to the manner of obtaining them" (*ibid.*, 2.692).

This, however, implies that all knowledge is fallible and subject to possible revision. We have seen how Peirce's conception of the subject matter of mathematics is connected with his conception of the continuum and that this conception in turn implied to treat the problem of the evolution of knowledge in mathematics and in the natural sciences on *a par*. It also follows that theories become realities *sui generis* in relation to concrete reality. This means, that they cannot simultaneously be theories of their own application. Interpretation is a meta-operation that leads to a new representation. But theories being also signs (besides being entities in their own right) take part in a continuum of signs. This continuum, again, is not just a collection of particulars, because it incorporates all the meta-meta ...-levels of interpretation.

How then does Peirce the Pragmatist conceive of the interaction of general and particular? This is what Doctor Z, a character in one of Peirce' dialogues, asked the Pragmatist:

> "You say that no collection of individuals could ever be adequate to the extension of a concept in general, [...]. But really I do not quite see how you propose to reconcile that to the proposition that the meaning extends no further than to future embodiments of it." (*ibid.*, 5.526)

The Pragmatist in answering this question illustrates his views "by the consideration of the continuity of space". I shall, he says,

> "adopt the Leibnizian conception of space in place of the Newtonian. In that Leibnizian view, Space is merely a possibility [...] of no matter what affections of bodies (determining their relative positions), together with the impossibility of those affections being actualized otherwise than under certain limitations, expressed in the postulates of topical, graphical and metrical geometry. No collection of points [...] could fill a line so that there would be room for no more points, and in that respect the line is truly general, [...] and yet it is so to say nothing but the way in which actual bodies conduct themselves." (*ibid.*, 5.530)

Fourth, Peirce, as opposed to Kant, does not see the problem in the question: "How are synthetical judgments *a priori* possible ?", but rather in the more general question: "How are any synthetical judgments at all possible? How is it that a man can observe one fact and straightway pronounce judgment concerning another different fact not involved in the first?" (*ibid.*, 2.690). An answer is given which reminds us of the principle of continuity, which is fundamental to Peirce's philosophy. The answer is this: "whatever is universally true is involved in the conditions of experience" (*ibid.*, 2.691); and further: "experiences whose conditions are the same will have the same general character" (*ibid.*, 2.692). The principle of continuity referring to generals cannot be based on a concept of uniformity of Nature (Mill).

> "Mill never made up his mind in what sense he took the phrase uniformity of Nature when he spoke of it as the basis of induction. In some passages [...] Mill holds that it is not the knowledge of the uniformity, but the uniformity itself that supports induction, and furthermore that it is no special uniformity but a general uniformity in nature. Mill's mind was certainly acute and vigorous, but it was not mathematically accurate; and it is by that trait that I am forced to explain his not seeing that this general uniformity could not be so defined as not on the one hand to appear manifestly false or on the other hand to render no support to induction, or both. He says it means that under similar circumstances similar events will occur. But this is vague. Does he mean that objects alike in all respects but one are alike in that one? But plainly no two different real objects are alike in all respects but one. Does he mean that objects sufficiently alike in other respects are alike in any given respect? But that would be but another way of saying that no two different objects are alike in all respects but one. It is obviously true; but it has no bearing on induction, where we deal with objects which we well know are, like all existing things, alike in numberless respects and unlike in numberless other respects." (*ibid.*, 1.92)

The principle of continuity applies here because "whatever is universally true is involved in the conditions of experience" (*ibid.*, 2.691), that is, belongs to the general aspects of that particular event in question, to its law like character. The principle of continuity, according to Peirce is a methodological principle regulating the interaction between general and particular and it is the only such fundamental principle, lending support also to induction.

If we understand Kant in the sense that synthetical judgments a priori just signify conditions of experience (see the introduction), then the difference between Kant and Peirce amounts essentially to the question of the nature and ontological status of generals (or continua) or laws.

> "While uniformity is a character which might be realized, in all its fullness, in a short series of past events, law, on the other hand, is essentially a character of an indefinite future; and while uniformity involves a regularity exact and exceptionless, law only requires an approach to uniformity in a decided majority of cases. [...] The law should be a truth expressible as a conditional proposition whose antecedent and consequent express experiences in a future tense, and further, that, as long as the law retains the character of a law, there should be possible occasions in an indefinite future when events of the kind described in the antecedent may come to pass. Such, then, ought to be our conception of law, whether it has been so or not." (*ibid.*, 8.192)

For Peirce, the reality of the "general" becomes clear from the way we deal with natural laws: natural laws are general because they permit predictions, and not only because they are stated with regard "to many things", as the traditional definitions of the general say. In other words: the Aristotelian concept of the general as something predicative is replaced here by another concept of the general, a cognition or a situation being designated as "general" which permits predictions to a certain degree. If these predictions, however, are not to be held to be acciden-

tally true, the general must be assumed to be an active connection in reality, be it in nature or in history (see for example *ibid.*, 5.103). If the possibility of predictions with regard to future events is given (a stone raised will fall down), this possibility must find a basis in the reality of the connection between things suggested here. And if relations (the laws of falling bodies) and relata (the series of falling stones) thus have the same ontological status, then there exists a genuine, that is in Peirce's sense an inexhaustible continuum between these two entities—between the general law and the particular case—both whose existence is assumed here.

Let us come back once more to the analogy between mathematical axioms and natural laws to illustrate how Peirce's ideas about continuity are linked to his philosophical realism. To explain a statement like $2+2=4$ (*ibid.*, 4.91), or $7+5=12$ if you like, one first argues, as in discourse on ordinary knowledge, that this proposition expresses a simple matter of fact, to be easily verified by means of a calculation (which however is in itself independent of such verification as it seems present in intuition). After a while one goes on, completely as in the case of science, to try and give an explanation of this fact. This endeavor implies a change of perspective, a jump to a level of different logical or categorical type. The law gives a unified account of what is otherwise a mere series (Armstrong 1983). In this endeavor one uses the general and abstract to explain the particular and concrete, or seemingly concrete, in exactly the same manner in which Newton's laws are used to explain simple mechanical phenomena, or Ohm's law is used to explain the facts of electricity. The general, as used in scientific explanations of such kind, in our case for instance the associative law of algebra, is less sure from a concrete empirical point of view and less positive than the individual facts to be founded on it. The less certain is used to explain the more certain, because what could be more certain because the effects of a law can never be certain. Such a strategy makes sense if it is employed exploratively and predictively, even though the predictions made can never be absolutely sure.

Nominalism, denying the existence of universals outside the mind, has no use for the idea that laws are relations between universals and therefore cannot explain the power of prediction inherent in them. Nominalism, or empiricism, perhaps, would speak of an inductive establishment of regularities, in which theoretical concepts and scientific laws lose their independent meaning. The great difference between induction and what is involved here is "that the former infers the existence of phenomena such as we have observed in cases which are similar, while hypothesis supposes something of a different kind from what we have directly observed, and frequently something which it would be impossible for us to observe directly" (Peirce W, III, 335-336).

The nominalists would say that a natural law is a mere representation, "the word mere meaning that to be represented and really to be are two very different things" (Peirce CP, 5.96). Now natural laws, as was said earlier, have some importance to us because of their predictive power. Exactly because of their prognostic function they cannot just be established by empirical verification. The nominalist would say they are free creations of the human mind, making their effectualness a miracle or a matter of pure chance "in order to escape the conclusion that general principles are really operative in nature" (*ibid.*, 5.101). It makes no difference that the laws of nature do produce their effects with a certain probability only. On the contrary probability judgments exhibit much more clearly the general character of synthetic reasoning (*ibid.*, 2.692 and Ms, 107).

For Peirce, the general is thus necessarily of a hypothetical character, as it is seen from the very outset in its potential for development. And this holds in the same vein for the natural laws which in Peirce's view are subject to evolution in the same way as the physical phenomena determined by them. On this basis, the paradigmatic role of mathematics can be seen in the very fact that for Peirce it always had to do with hypotheses alone, so that the mode "in which mathematicians generalize" (CP, 6.26) can be used to study the process of increasing generalization within a "true" continuum of applications. Again this continuum, not being collective, just forms a space of possibilities.

The process of applying a theorem is thus a generalization. Firstly, because collective experience accumulates and is embodied within the system of symbolic means and every application of that means fosters this process, being at the same time dependent on it. Therefore generalization takes place, because the embodiment of experience in the construction of signs suggests new analogies, and generalized hypotheses. Generalization thus is both a social process and an object-related one. Two continua, one linking the sign with its object, and the other established by the successive series of interpretants, appear as if fused into one, because interpretants depend on the relation between sign and signified object.

The idea of sign brings us to the fifth divergence between Kant and Peirce. Kant's refutation of the ontological proof of God's existence, which formed the basis of Leibnizianism, confines us in philosophy to a construction of concepts, without providing the certainty that these concepts are not empty. Hence, the question arises as to how these concepts can be applied to objects. Kant's answer consists in pointing out the role of intuition. In mathematics these objects are only variables, such that mathematical reasoning becomes hypothetical. Peirce introduces the following changes: on the one hand, he eliminates the difference between concept and representation (*Vorstellung*) by means of the notion of "sign". On the other hand, he has a quite different idea of what reasoning or inferring is. Peirce always stressed that the insufficiencies of Kant's epistemology were due to the latter's insufficient logic, to a mere subject-predicate logic, and that this logic, in order to remedy the defect, must be extended to a logic of relations. It is in this

very aspect, that diagrammatic reasoning becomes indispensable. Diagrammatic thinking is essentially established by the principle of continuity (*ibid.*, 5.162) and it shows that deduction and induction or analysis and synthesis are not so thoroughly unlike as might be thought (*ibid.*, 5.579).

A sixth aspect is also closely linked with the role of signs and means of representation, namely that mathematics is essentially a kind of social cognition. Mathematical cognition is the art of bridging gaps by inventing analogies and generalizations. Pure mathematics is the child of an explosive growth of mathematical activity that occurred around 1800 and that, in its sources, may be summarily characterized by stating that for the first time in the history of mathematics a great number of connections between apparently very different results and problems was discovered (Scharlau 1979, 277). A complementary presupposition is hidden here, namely that plurality and difference played a fundamental epistemological role. The world was seen as ruled by difference rather than by similarity or equality. In view of the fact that equality and difference are the fundamental subject matter of mathematics, it seems plausible to claim that

> "the chief characteristic of mathematical propositions is the wide variety of equivalent formulations that they possess. [...] In mathematics the number of ways of expressing what is in some sense the same fact while apparently not talking about the same objects is especially striking." (Putnam 1975, 45)

It seems obvious then that mathematics cannot be analytic, as otherwise there should be a universal mechanism that decides for any *A* whether one should be allowed to call it *B* thus deciding whether *A* could also be called *B*. It seems not surprising at all that Quine in "Two Dogmas" (1953) was not able to define synonymy in logical terms.

V The Analytic-Synthetic Distinction according to Peirce is only relative

Kant's definition of analytic judgments expresses a whole or partial identity between concepts serving as subject and predicate. The predicate essentially belongs to the subject and the subject is presented in its essential properties or relations. What is new about this situation in Kantian philosophy is only the fact that the essence of an object is not given but is constructed. Knowledge, says Kant,

> "consists in the determinate relation of given representations (*Vorstellungen*) to an object; and an object is that in the concept of which the manifold of a given intuition is united. Now, all unification of representations demand unity of consciousness in the synthesis of them. Consequently it is the unity of consciousness that alone constitutes the relation of representations to an object, and therefore their objective validity, [...] and upon it therefore rests the very possibility of the understanding." (Kant B, 137)

Accordingly knowledge and understanding depend on consciousness and the (epistemic) subject becomes the pivotal and crucial point of epistemology. Peirce substitutes the subject's consciousness for the sign. In a sign, like in a work of art for instance, the synthesis of representations is realized in a way similar to the way the very essence of Monet's garden at Giverny has been realized in his paintings.

> "The work of the poet or novelist is not so utterly different from that of the scientific man. The artist introduces a fiction; but it is not an arbitrary one; it exhibits affinities to which the mind accords a certain approval in pronouncing them beautiful, which if it is not exactly the same as saying that the synthesis is true, is something of the same general kind. The geometer draws a diagram, which if not exactly a fiction, is at least a creation, and by means of observation of that diagram he is able to synthesize and show relations between elements which before seemed to have no necessary connection. The realities compel us to put some things into very close relation and others less so, in a highly complicated, and in a sense itself unintelligible manner; but it is the genius of the mind, that takes up all these hints of sense, adds immensely to them, makes them precise, and shows them in intelligible form in the intuitions of space and time." (Peirce CP, 1.383)

The objectivity of a piece of art or of a theory which "compels us to put some things into very close relation and others less so" is due to the fact that works of art or theories, besides being signs, became recognized as realities *sui generis*. They are, in Peirce's words, distinct *quales* or *qualia*.

> "In so far as qualia can be said to have anything in common, that which belongs to one and all is unity; and the various synthetical unities which Kant attributes to the different operations of the mind, as well as the unity of logical consistency, or specific unity, and also the unity of the individual object, all these unities originate, not in the operations of the intellect, but in the quale-consciousness upon which the intellect operates." (*ibid.*, 6.225)

By his "semiotic transformation" of critical philosophy, Peirce was able to take into account that looking from different perspectives on one and the same thing and viewing different objects from one and the same point of view become indistinguishable approaches, as in the fusion of analytical geometry and linear algebra. The semiotic theory attempts to explain cognitive growth as a process in which the stages are indifferently members of a social community or sequential states of a single person. Knowledge and cognition are relative only in that they have to grow and to be generalized. That is their essential nature. Man is a sign himself and the processes of objective and of communicative generalization become unified into one process. Peirce semiotic theory now relies essentially on the logic of continuity and on the reality of the continuum. I cannot extensively deal with this thesis here but take it into account only as far as it concerns my topic.

With respect to this "semiotic transformation" of critical philosophy the reformulation of the definition of analytical judgments—in the sense of Kant as given by Quine in his "Two Dogmas of Empiricism"—seems justified. Quine writes:

> "Kant conceived of an analytic statement as one that attributes to its subject no more than is already conceptually contained in the subject. This formulation has two shortcomings: it limits itself to statements of subject-predicate form, and it appeals to a notion of containment which is left at a metaphorical level. But Kant's intent, evident more from the use he makes of the notion of analyticity than from his definition of it, can be restated thus: a statement is analytic when it is true by virtue of meanings and independently of fact." (Quine 1953, 20-21)

Meanings are generals, they are instances of Thirdness, and that implies that an investigation into meaning relations is a meta-knowledge activity. Any mental activity, in fact, involves the idea of context and this means meta-cognitive elements. For instance, the form which a simple distinction commonly takes is "All things of sort S are either A or B". A simple distinction thus already involves generality (hinted at by the term: " ... of sort S"). Every cognitive activity involves a meta-cognitive element. To give but one more example: human rote learning is an example of a very rudimentary form of cognitive activity. But normally it is accompanied by a second-order phenomenon which we may call "learning to rote learn". For any given subject, there is an improvement in rote learning with successive sessions asymptotically approaching a degree of skill which varies from subject to subject. Meta-cognitive activity making that one has thought about any subject itself a subject of thought creates what Peirce has termed "hypostatic abstractions".

> "In order to get an inkling—though a very slight one—of the importance of this operation in mathematics, it will suffice to remember that a collection is an hypostatic abstraction, or *ens rationis*, that multitude is the hypostatic abstraction derived from a predicate of a collection, and that a cardinal number is an abstraction attached to a multitude." (Peirce CP, 5.534)

Now hypostatic abstractions like the essence of "Two" or like "Blue-ness" are indeterminate in many respects and to varying degrees, they are continua and they are real. Thus they represent Thirdness.

The analytic-synthetic distinction must therefore be liberated from questions about objectivity and objective truth. It is a methodological question. One has, with respect to the purpose at hand, to choose the appropriate level of generality. And taking into account the identity between generality and continuity any investigation into meaning relations should be governed by the principle of continuity rather than the principle of identity of indiscernibles. One would ask then how meanings become connected, that is become species of one kind or type, rather than whether different meanings refer to the same thing or are identical. Now

> "the meanings of words ordinarily depend upon our tendencies to weld together qualities and our aptitudes to see resemblance, or, to use the received phrase, upon associations by similarity; while experience is bound together, and only recognizable, by forces acting upon us, or, to use an even worse chosen technical term, by means of associations by contiguity." (*ibid.*, 3.419)

And

> "analytical reasoning depends upon associations of similarity, synthetical reasoning upon associations of contiguity. The logic of relatives, which justifies these assertions, shows accordingly that deductive reasoning is really quite different from what it was supposed by Kant to be; and this explains how it is that he and others have taken various mathematical propositions to be synthetical which in their ideal sense, as propositions of pure mathematics, are in truth only analytical." (*ibid.*, 6.595)

This error with respect to the character of deduction in pure mathematics is due to the sharp discrimination Kant has drawn between deductive inference and observation or between discursive and intuitive knowledge. Kant

> "saw far more clearly than any predecessor had done the whole philosophical import of this distinction. This was what emancipated him from Leibnizianism, and at the same time turned him against sensationalism. [...] But he drew too hard a line between the operations of observation and of ratiocination." (*ibid.*, 1.35)

Kant shared with Leibniz a foundationalist attitude with respect to knowledge. He, however, conceived of the foundations differently from the God's eyes perspective of Leibnizianism. This different orientation made him emphasize the distinction between discursive and intuitive knowledge, because only God's mind is intuitive, whereas ours is necessarily discursive (Kant B, 135). Peirce does not accept Kantian foundationalism and the sharp separation between the subjective and the objective in Kantian thought and this makes the analytic-synthetic distinction a relative one too.

> "The truth is our ideas about the distinction between analytical and synthetical judgments is much modified by the logic of relatives [...]. Deduction, or analytical reasoning, is [...] a reasoning in which the conclusion follows (necessarily, or probably) from the state of things expressed in the premises, in contradistinction to scientific or synthetical reasoning, which is a reasoning in which the conclusion follows probably and approximately from the premises, owing to the conditions under which the latter have been observed [...]. The two classes of reasoning present, besides, some other contrasts [...] some significant resemblances. Deduction is really a matter of perception and of experimentation, just as induction and hypothetical inference are; only, the perception and experimentation are concerned with imaginary objects instead of with real ones." (Peirce CP, 6.595)

Mathematics, being based on experimentation with diagrams, has deduction as its main method of reasoning (Peirce knows of two other methods, namely induction and abduction) and Thirdness as its main category.

Another explanation of the connection between the analytic-synthetic distinction and the subject-object relation can be furnished via a discussion about the character of relations and in particular via the question whether relations are internal or external (Peirce uses the attributes "relation of reason" *vs.* "real relation" and he parallels analytic knowledge with the former (*ibid.*, 1.365)). The attempts by Russell and Moore to understand relations and to see the implications of the distinction between internal and external relations led to the establishment of analytic philosophy around the turn of the century (Moore 1922, 276-309).

Kant believed that relations are "external" and real knowledge must therefore be synthetical. All objects (substances) are isolated like Leibnizian monads. Continuity we find only in the realm of phenomena as they are synthesized by activity. Kant accordingly based synthesis and continuity on activity. But contrary to Hume he believed in the objective character of the synthesis and the resulting knowledge because the subject's activity is framed by conditions that are *a priori*. Therefrom comes his project of understanding how synthetic knowledge *a priori* is possible.

An analytic proposition implies $S = P$ and this means $S < P$ and $S > P$. $S < P$, in words: the predicate is to be applied to the subject; whereas $S > P$ means the predicate inherently belongs to the subject. This last expression is normally used when explaining what analyticity of judgments means. $S < P$ and $S > P$, however, are equivalent, as we have just seen from the rephrasing (a more formal statement of this equivalence can be found in Peirce (Peirce CCL, 131 ff.). We see from this that if all relations are internal all propositions are analytical. The externality of relations by contrast, leads to synthetic propositions. Instead of using *a priori* intuition to secure the objectivity of synthetic knowledge Peirce uses a theory of the continuum. Objectivity of knowledge namely is an ontological question, according to Peirce. It is the question of the reality and generality (which is the same) of relations, and the latter question depends on continuity (as we have seen when discussing Poncelet's views). On these grounds, the analytic-synthetic distinction becomes relative. We have, in fact, shown that analysis and synthesis are complementary elements in every mental activity (even in formal deduction).

Quine in "Two Dogmas of Empiricism" claims (1953, 37) that Peirce adhered to the verification theory of meaning and held a limit theory of truth. This, however, is a too narrow interpretation of the pragmatic maxim and of Peirce's frequent endorsement that the truth of any proposition is a function of whether or not its being accepted by the epistemic community in the idealized long run. In a controversy with William James and the latter's views on pragmatism, Peirce denied the existence of absolute individuals and stressed the importance of the general, which is a continuum that is not collective. The continuum of space, we recall, served Peirce as an illustration of such a potential aggregate that contains only general conditions "which permit the determination of individuals". The pragmatic maxim in a narrow sense implies a God's eye perspective, as Peirce had explained in a review of Royce's philosophy because the thing which God imagines, and the opinion to which investigation would ultimately lead, in point of fact, coincide. (Peirce CP, 8.41). Thus to hold a verification theory of meaning would amount to falling back on Leibnizianism, which certainly was not on Peirce's mind.

Quine, in "Two Dogmas of Empiricism", linked the analytic-synthetic distinction to the classical view of scientific knowledge, namely to the belief that each meaningful statement is equivalent to some logical construct upon statements that express direct matters of fact. Quine, in fact, defines this type of reductionism more narrowly, but we want here to stick to the classical Aristotelian scheme of a science. Such a science is a system of sentences which satisfies the following postulates: there is a finite number of terms and a finite number of sentences such that the meaning of the terms and the truth of the sentences are so obvious as to require no further explanation and proof. The meaning of any other term as well as the truth of any other sentence is definable or logical inferable starting from the original collections of terms and sentences, which are given by means of intuition and experience.

Kant adhered to this Aristotelian model of rationality, but radicalized it by amplifying the part of the rational mind as a standard, since he learned from Hume that all knowledge presupposes a synthetic constructive element. His views are best illustrated by quoting his characterization of the term "Nature" (Kant A, 125-128). The unity of apperception is the basis of any order and uniformity of Nature (cf. part III of this paper).

Holism in the sense of Kuhn or Feyerabend followed the Kantian route a little further still. On this account theory as a whole or the paradigm becomes the standard which determines fact and rationality. But this standard becomes thoroughly relative. Kant sacrificed truth for objectivity. Now even objectivity is to be understood relative to the theory in question. Kuhn or Feyerabend believe that theory as a whole determines the intensions of its terms and that intensions determine extensions. The theory or the paradigm becomes a way of seeing the world, which is completely incommensurable with other ways. It is clear then that the analytic-synthetic distinction loses all objective meaning because of the thoroughgoing relativism involved. Where do the scientific revolutions and the new rationality standards come from? To answer questions like this we would have to engage in an understanding of the objectivity of the subjective outside apriorieism. The task then is to see how in the evolution of knowledge social and objective factors interact. Quine finally believes that the theoretical system as a whole must be squared with experience but is as such hopelessly underdetermined by experiential fact. Quine's solution of the dilemma of relativism is that "in practice we end the regress of background languages, in discussions on reference, by acquiescing in our mother tongue and taking its words at face value" (Quine 1968, 201). This means we understand scientific objectivity as resting on common sense. But this is, says Chomsky, "no help at all, since every question he had raised can be raised about the mother tongue and the face value of its words" (Chomsky 1976, 186). Common sense convictions themselves have to be taken as variables and have to be related to scientific expertise and inquiry. It is the relationship between science and commonsense knowledge which determines our cultural evolution.

Nevertheless it is common sense where our most stable convictions are borrowed from, even in science. Meaning essentially depends on the fact that all humans ultimately live in a common world, irrespective of the fact that pluralism and diversity are very essential to human life. Peirce always stresses that purposes of "a general description" are intended in the pragmatic maxim, and that

> "upon innumerable questions, we have already reached the final opinion. How do we know that? Do we fancy ourselves infallible? Not at all; but throwing off as probably erroneous a thousandth or even a hundredth of all the beliefs established beyond present doubt, there must remain a vast multitude in which the final opinion has been reached. Every directory, guide-book, dictionary, history, and work of science is crammed with such facts. In the history of science, it has sometimes occurred that a really wise man has said concerning one question or another that there was reason to believe it never would be answered. The proportion of these which have in point of fact been conclusively settled very soon after the prediction has been surprisingly large. Our experience in this direction warrants us in saying with the highest degree of empirical confidence that questions that are either practical or could conceivably become so are susceptible of receiving final solutions provided the existence of the human race be indefinitely prolonged and the particular question excite sufficient interest." (Peirce CP, 8.43)

VI Pure and Applied Mathematics: Some Examples of Non-Kantian Applications of Mathematics

What concerns us here is the complementarity of means and problems, or of methods and objects, which became prominent. This complementarity becomes essentially Thirdness, if one takes into account that activity has to enter as a third into the relation. The fundamental ideas of science or mathematics are of a methodological character, rather then of an objective one. Objects and relations become means and means become objects of scientific activity. Means and objects are fully differentiable by their respective moments on individual cognitive activity, but they play a completely symmetric part in the development of cognition. This complementarity (difference and unity) of objects and means accounts for the emergence and dynamism of pure mathematics in the nineteenth century. It follows from this that there are no absolute foundations nor universal justification processes for mathematics. Looking from different perspectives on one and the same thing and viewing different objects from one and the same point of view become methodologically indistinguishable approaches, as in the fusion of analytical geometry and linear algebra. This equivalence or complementarity is represented in the idea of sign, when taken in the sense of Peirce. Linear algebra or synthetic projective geometry were meant by its inventors as new and more fundamental approaches to geometry, in comparison to the ones espoused by Euclid or Descartes. Still they did not lead as was hoped to the final determination of mathematics. They were in fact first steps towards what later on became called a de-ontologization of mathematics.

Mathematical ontology nowadays can only be conceived of as Peirce's inexhaustible continuum of real possibilities of relations. And which of these possibilities become actualized in a certain context and at a certain point in time depends on our goals and means of knowing. The future determines the past, which is the universe of the factual. The world contains only signs and the continuum of possibilities ahead in the future. A theory of meaning based on concepts abstracted from substances does not permit us to distinguish between analytical or synthetical judgments of cognition. The continuum's meaning serves only as a philosophical hypothesis which enables us to tie meanings to whether a practice of cognition has been verified and to justify generalizations by their ability to predict. The foundation of mathematics cannot be separated from its application. This is the conclusion we draw from what has been said above.

The dialectic of means and objects may briefly be summarized as follows:

A) As in any other cognitive activity, object and means of cognition are linked in mathematical activity as well. Mathematics cannot proceed in an exclusive orientation towards universal, formal methods. This would in the last instance amount to mathematical activity itself being suited to mechanization and formalization. Mathematics, too, forms specific concepts intended to serve in the grasping of mathematical facts.

B) Object and means are not only linked, but also stand in opposition to one another. Objects or facts are resistant to cognition. They represent Secondness, as Peirce says. And problems do not produce the means to their solutions out of themselves. Modern mathematics even obtains its own dynamics in no small part from applying theorems and methods which at first glance have nothing to do with the problems at hand.

In this, we understand by "object" any problem or any kind of resistance of reality against the subject's activity, and by "means" anything which seems appropriate to achieve mediation between the subject and the object of cognition. In this sense not only sign systems but also theories—knowledge of any kind and also intuitions in the Kantian sense—are means of the subject's activity.

This double problematic of means and objects as outlined under (A) and (B) also determines the relationship between analysis and synthesis and the quite controversial evaluations of the latter.

With regard to (A), for instance, the advantage of synthesis is presented as concreteness and genuine objectivity in mathematics (AS), whereas under (B) synthesis is presented as a method too much dependent on the particulars of the situation under consideration that proceeds timidly, conservatively and tentatively, and by chance and error. Synthesis is a method which becomes mired in the particular and is unable to attain genuine generalization (BS).

Under (A), conversely, the restricted character of logic or of algebraic analysis is salient.

> "The objects considered which are mere compositions or compounds of elements do not contain more or less than the elements themselves; as a result, the goal pursued will always be determined by the means applied [...]. The problem is from the very outset cast into the mold of algebraic composition." (Boutroux 1920, 193-194)

In these words Boutroux criticizes Cartesian algebraic science. The means themselves, dominating thought too much, become the only objects considered. Or, to put it differently, knowledge becomes abstract and formal (AA). From perspective (B), on the other hand, algebraic generalization appears as an opportunity for symbolic generality which detaches itself from links too close to referential meanings, and in which true generalization is attained by introducing hypostatic abstractions, whereas in synthetic mathematics the general is always only presented by a particular (BA).

For purposes of illustration, let us consider two examples of mathematical application. The first concerns the so-called "theory of cellular automatons" and the possibilities of using them to describe the developmental dynamics of processes. In an application developed by Bielefeld mathematicians, the matter at hand was to investigate heterogeneous-catalytic reactions on metal surfaces. These are chemical reactions occurring in many processes of detoxification of exhaust fumes, and in particular in exhaust catalyzers for car engines. Complicated oscillation patterns were formed in these reactions, and it was possible to simulate these by cellular automatons. These simulations, however, were not hit upon by analyses of the chemical processes at hand, but rather by observing that a certain function of number theory shows a quite similar oscillatory behavior. And this function in turn was easily represented by a cellular automaton. Only afterwards did it become possible to give a chemically plausible interpretation for this behavior (Jahnke 1992). These relations of similarity led to a computer simulation of the relevant processes, and this in turn led to deeper study and interpretation. Mathematics and computer simulation just furnished a reservoir of forms.

A second, similar example comes from research into the brain and into cognition. First, by investigating the brain, the computer was used to try and find out what thinking really is. This mechanistic or reductionist approach, however, did not bring theory close to the "essence" of cognitions. Later, computers were variously used in trying to identify certain brain activities within an electrical thunderstorm which can be measured on the scalp. The results obtained were then used to build apparatuses which transform certain brain activities into material processes such as controlling an airplane. For this, it is necessary that the individual whose brain is the source of the signals learns to repeatedly produce certain impulses at will, just as I do involuntarily when I raise my right arm. How a certain effect can be produced must be found out by every individual for himself. There is, so to say, no clear-cut material basis for that. "In principle, it doesn't matter what signal is measured as long as one is able to influence it somehow with one's brain", as A. Junka, one of the pioneers in the analysis of biosignals, describes his own working philosophy (*Focus*, No. 28 July 1994, 104 ff.). Together with some friends, he set up a company which developed a so-called biolink system. Three forehead electrodes do not only record electric currents in the brain, but also signals from facial muscles. A calculating method is used which analyzes the signals in real time in ten different frequency ranges. According to the strength of the signal received, the computer can be made to carry out certain actions. "This must not be seen too analytically, the main thing is that it feels good" (*ibid.*), Junka points out to those who want to decide and find out for themselves how they want to coordinate their own will and their brain activity. Particularly for wheelchair patients with severed spinal cords opportunities hitherto unheard of are provided.

The researchers do not approach the matter analytically, but rather play around with various types of brain control devices. What matters therefore is not the question what thinking really is, here and at this point in time, but rather how thinking can influence reality. The computer thus is a machine which establishes relations between the brain and some other entity and confers a certain reality on them. Similarly, the diagram in mathematics is a machine which permits us to confer reality to certain relations. The process is always the same. From a continuum of real possibilities, some of these are being actualized by means of distinctions. In this sense, Peirce guessed

> "that the laws of nature are ideas or resolutions in the mind of some vast consciousness, who, whether supreme or subordinate, is a Deity relative to us. I do not approve of mixing Religion and Philosophy; but as a purely philosophical hypothesis, that has the advantage of being supported by analogy." (Peirce CP, 5.107)

Institute for Didactics of Mathematics,
University of Bielefeld

Note

* I would like to thank Michael Hoffmann, Marco Panza, Andreas Etges, Richard Steigmann and Günter Seib for helpful remarks and linguistic advice on earlier versions of this paper.

III. History and Philosophy

MARCO PANZA

CLASSICAL SOURCES FOR THE CONCEPTS OF ANALYSIS AND SYNTHESIS*

In the introduction to the present book, different meanings of the terms "analysis", "synthesis" and their cognates, variously related to mathematics are taken into account. It appears to me that the papers composing the present volume, exhibit a great variety of meanings of these terms as they occurred in the history of philosophy of mathematics. In such a situation, it is quite natural to wonder if, when speaking of analysis and synthesis in mathematics, we are really speaking of a unitary and well-defined question, or if the title of the present book merely refers to a number of different and unconnected questions. At first glance, one might believe that this is the case; that what is common to the different meanings of "analysis" and "synthesis" consists just in the fact that people happen to use these same words. But if a term is used to refer to different meanings, it is plausible that there is a reason for that. Even though these meanings are really different, it is nevertheless possible, for example, that they are linked by a causal chain which is so long that the ends of it have actually nothing to do with beginnings. If this were the case, our book would finally be concerned with a succession of semantic shifts or stretches rather than with a historical and philosophical question. I do not believe that this is so. The different topics discussed in the various contributions are, I believe, intrinsically connected to each other; besides, I argue that all of them are parts of only one question, and that this question can be addressed both as a historical and as a philosophical one.

I should like to provide two distinct arguments: the first is based on my understanding of the relations between history and philosophy of mathematics, the second one is concerned with my understanding of the different meanings of "analysis" and "synthesis" and their cognates. The main objective of the present essay is to state and unfold the second of these arguments. Thus I will consider the first one only very briefly.

Mathematics is a human activity (here, ch. 11), as is philosophy. Mathematics is concerned with the creation and study of mathematical objects (here, ch. 12, par. IV), while philosophy creates and studies philosophical objects. A philosophical object is nothing but a concept. It is a general category we use in our explanation of certain phenomena, for example, the phenomenon of knowledge. Thus, philosophy takes part in any explanatory activity. Thus, as long as mathematics is

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 365–414.

an explanatory activity, it contains philosophy as a part of it. But, as long as it is a human activity, mathematics is also a phenomenon that we would possibly want to explain. Such an explanation is exactly the goal of a different sort of activity which is generally either called "history" (or "historiography") or "philosophy" of mathematics. The use of one or the other of these two distinct names depends on the particular aspect of explanation on which we want to insist. By using the first name, we insist on a local explanation, that is the explanation of a fragment of mathematics, as it has been performed (and according to the results it has produced). By using the second name, we insist on the search for and discussion of the general categories we use in such an explanation. This does not mean, of course, that I intend history (or historiography) of mathematics as a particular application of philosophy of mathematics. As an activity, mathematics is a single and individual phenomenon and it seems to me that it is not possible to intend it as a succession of repetitions of certain patterns or models. Thus, philosophy of mathematics is not the activity of describing patterns or models of mathematics. By speaking of general categories, I do not refer to general patterns for mathematical activity, but to general concepts we use in order to speak about such an activity and to explain it.

From such a point of view, the question of analysis and synthesis in mathematics is the question of legitimacy, nature and use of the general categories of analysis and synthesis for the explanation of (certain fragments of) mathematics, and it is really a unitary question if the terms "analysis" and "synthesis" refer, or could refer, to two general concepts used to speak of mathematics and explain it. It is a matter of fact that these terms have been used both to explain and do mathematics. A number of papers of our book aim to understand and discuss some of these uses. If their conclusions were intended as an evidence for a radical difference between these uses, it would not be possible to assert that they are parts of an answer (or even different partial answers) to only one historical and philosophical question. We would be justified in speaking about the philosophical and historical question of analysis and synthesis in mathematics only if we accepted to specify a particular meaning in which we use the terms "analysis" and "synthesis". This is not my wish, since I do not think that the conclusions of the previous papers are evidence for a radical difference between the admitted uses of these terms. I think, quite to the contrary, that the different concepts of analysis and synthesis discussed in the previous essays are intended as different elements of two classes of equivalence which constitute as such two general concepts; that is, they are different forms of exposition of these concepts. The aim of the present essay is to expound some important aspects of these concepts by discussing some classical source.

I Philology and Literature

Both the terms "analysis" and "synthesis" stem from the Greek. As they are composed of more primitive terms, they could, in a sense, be understood as sorts of descriptions. Thus, at a first glance, we can consider their etymology as a source of suggestions.

The Greek term for "analysis" is "ἀνάλυσις" that is composed by the prefix "ἀνά" and the substantive "λύσις". The prefix "ἀνά" was generally used in Greek to indicate the idea of motion upwards, and could accordingly be translated by expressions like: "upwards", "above", "towards", or even "near" or "close to". However, in composed words it is also used sometimes in the sense of "back" or "backwards". The substantive "λύσις" is used in different senses too, like "solution" or "conclusion", but—as it is derived in turn from the verb "λύω", that means "to free", "to liberate", "to loose", "to unknot", "to dissolve", or even "to break" or "to destroy"—it is also used to indicate the ideas of liberation, loosening, dissolution or even destruction. Thus, tentative transitions of "ἀνάλυσις" could be: "back from solution"—or, as it was common for Latin translations of Greek texts, "resolution [*resolutio*]"—or "back from conclusion", but also "toward the solution", "close to the conclusion" or again "what brings to the solution (or dissolution or even destruction)", "what makes it possible to unknot something", etc.

The situation is simpler for the term "synthesis", that is the English version of "σύνθεσις" or (more seldom) "ξύνθεσις". This is composed by the prefix "σύν" (or "ξύν")—which means "with" or "together"—and the verb "τίθημι"—which means "to put", "to lay (down)", "to set" or even "to state". Thus a synthesis could be etymologically intended as the act of putting (something) together or the act of stating (something) with an accord.

These swift etymological considerations suggest a starting point for our search: etymologically, the Greek terms for "analysis" and "synthesis" do not oppose each other in a direct way. Whatever semantic opposition there is, it is that between the verb "λύω", which vehicles an idea of separation and the prefix "σύν" which transports an idea of composition. However, though the term "σύνθεσις" directly refers to the action of composing, the term "ἀνάλυσις" refers to the action of separating only in a more indirect way, by means of the prefix "ἀνά" and according to the complex idea of "λύσις".

This is confirmed by the occurrence of the terms "ἀνάλυσις", "σύνθεσις" and their cognates in the Greek *corpus*, where they are not generally used to express two opposite ideas. Even though the first one is often used to express an idea close to that of separation, such an idea is generally more complex, and it is not in direct contrast to an idea of composition as transported by the term "σύνθεσις".[1] In the *Odyssey*, Penelope, waiting for Ulysses to return, "analysed [ἀλλύεσκεν]" her web during the night, but she did not synthesize it during the day; she "weaved [ὑφαίνεσκεν]" it (*Odyssey*, β, 104-105 and τ, 149-150). In the tragedy by Sophocles, the chorus snubs Electra because of her inability to "analyze" herself from her males (*Electra*, 142), but Electra never synthesizes herself with them. Again, for the author of *On the Universe* (which was during a long time ascribed to Aristotle), some winds can be formed by "analysis" of clouds' thickness (*On the Universe*, 394*b*, 17), but no cloud is formed by synthesis. In these three examples, "analysis" and its cognates carry respectively the ideas of unraveling, liberation and dissolution, three ideas expressing separation that are not opposed to compositions by "synthesis". A similar exercise is possible starting from the term "synthesis". According to Pindar (*The Pythian Odes*, IV, 168) the agreement between Pelias and Jason, after which the latter leaves for Colchis to seek the Golden Fleece, is just a "synthesis". You can find the same idea of synthesis, as an agreement in Plutarch (*Live of Sulla*, 35, 10), who uses the verb "to synthesize [συντίθημι]" to indicate the act of bargaining over a marriage (namely the marriage of Sulla and Valeria at the end of Sulla's life). Following Isocrates (X. *Helen*, 11), a "synthesis" is then the act of drafting an oration—five centuries later, it will be for Plutarch (*Moralia*, 747*d*) the act of composing a poem—, while for Aeschylus (*Prometheus Bound*, 460) it was, in the same vein but more fundamentally, the science of writing, that is the art of arranging letters in order to form a word. In such a sense, it is one of the gifts from Prometheus to human beings, which make them able to reason and think. Six centuries later, Plutarch associates the idea of synthesis to a different art, namely the art of counting or even to the science of numbers. In his treatise *The Obsolescence of Oracles*, he generalizes an old definition of (natural) numbers as "synthesis of unities", already quoted by Aristotle in *Metaphysics* as a customary one (1039*a* 12), and uses the term "σύνθεσις" to refer both to the composition of (natural) numbers by smaller numbers (*Moralia*, 429*b*, cf. also 744*b*) and to their addition (416*b*). Cognates of the verb "συντίθημι" were besides used in the *Elements* (for example in the definitions VII, 13-14) in a similar sense, to indicate composition of numbers or magnitudes. In these five examples, "synthesis" means something close to composition, but it does not appeal to any sort of analysis, before it, or after it.

Of course these examples have not to be taken too seriously, in particular when two common verbs like "ἀναλύω" and "συντίθημι" are involved. They confirm however that the opposition between analysis and synthesis was not as natural in Greek culture as it is for us. Moreover as long as, in all of these previous examples, analysis and synthesis are particular sorts of separation and composition, they seem to operate on certain objects to change their relational status or obtain other sort of objects of the same logical nature. Neither synthesis, nor analysis entails a passage from the particular to the universal, or from the universal to the particular, or from objects to concepts or *vice versa*.

II Plato

The same seems to be true of the idea of synthesis as it occurs in Plato's dialogues. In the *Cratylus* (431*c*), Plato comes back to the idea of Aeschylus and generalizes it with respect to the structure of language, by saying a proposition is a "synthesis" of verbs and nouns (cf. also *Sophist*, 263*d* and Plutarch, *Moralia*, 1011*e*, which just assigns such a definition to Plato). In the *Republic*, he speaks of "synthesis" as referring to the combination of parts in a certain system. You have not to believe—he argues (611*b*)—that soul consists of distinct parts, since it is difficult that a being is immortal if is composed (σύνθετον) from a number of parts, except when the "synthesis" is perfect. Elsewhere, in the same treatise (533*b*), he speaks about "synthesis" of manufactures (συντιθεμένα) as one of the concerns of τέχναι. And in the *Phaedo* (92*e* - 93*a*) he treats harmony (ἁρμονία) as something produced by an act of synthesis. In these examples, synthesis is something like the process of composing or arranging objects into a structure or system, and it is not, as such, opposed to any sort of analysis. Moreover, in contrast to the term "synthesis", the term "analysis" is not part of Plato's lexicon.

This does not prevent Plato from contrasting the ideas of composition and separation in the core of his philosophy, namely in his presentation of dialectics. In the *Phaedrus* (265*c* - 266*c*) he calls "dialecticians" those, who are able to operate with "division" and "gathering" (διαίρεσις καὶ συναγωγή). By the second of these conducts, scattered ideas are grouped together, while, by the first, one idea is presented according to its natural joints. Plato's choice to use the term "συναγωγή" rather than "σύνθεσις" to indicate the first of these operations could be understood as a symptom of his will to distinguish between two different sorts of compositions: the assemblage of distinct objects in order to form a certain system (we could call "σύνθεσις") and the subsuming of distinct ideas under one of a higher type (we could call "συναγωγή"). As, for Plato, ideas are contrasted to appearances in terms of an opposition between real objects and fictitious ones, this distinction does neither correspond to the distinction between composition of objects and composition of concepts nor does it refer to subsumption of objects under concepts. As long as Plato does not dispose of concepts, both synthesis and συναγωγή operate on objects ("ideas"), but while the result of synthesis is a new object, which operates as such in a certain realm, the result of συναγωγή is the acknowledgment of a certain relation linking different ideas, which produces, as Plato says, "clearness and consistency" of discourse (*ibid.* 265*d*). According to such a conduct, we can say, for example, as Plato says, what is love, but we do not necessarily recognize the different sorts of loves, that is the different ideas which are submitted to the idea of love (but which do not compose it). This is the concern of διαίρεσις, which operates on an idea that has been made clear by συναγωγή and which recognizes its different species.

III Aristotle

III.1 Synthesis

When Aristotle in *Politics* (1294*a*, 30 - 1294*b*, 1) speaks jointly of διαίρεσις and σύνθεσις, he seems not to understand them very differently from Plato's. The διαίρεσις is the distinctive character of certain forms (namely democracy and oligarchy as forms of government), while σύνθεσις is just the composition of these forms by resulting in a new form (of government). Similarily in the *Metaphysics*, where Aristotle contrasts σύνθεσις with διαίρεσις (1027*b*, 19; and 1067*b*, 26), by respectively referring to the composition and separation of subject and predicate, or (1042*b*, 12-18) observing that differences (διαφοραί) between subjects may depend on the manner in which they are "synthesized". Aristotle in these arguments associates the notion of synthesis with an idea of separation, but he does not express the latter by the term "analysis", using respectively the Platonic term "διαίρεσις" and the term "διαφορά". Here, a synthesis is a way to produce objects (either subjects or forms), which can be distinguished (or separated) from one another in terms of the particular character of synthesis itself. Elsewhere, in *Metaphysics*, the term "synthesis" is used to indicate a particular mode of composition—which Aristotle explicitly distinguishes both from mixture (μῖξις; 1043*a*, 13; and 1092*a*, 26) and from communion (συνουσία; 1045*b*, 12)—or composition in general (1113*b*, 22; and 1114*b*, 37).

III.2 Analysis: *Analytics Prior* and *Posterior*

Thus, taken as such, the idea of synthesis seem not to suffer very deep modifications, when passing from Plato to Aristotle: both authors use it to express the composition of objects in order to form new objects. What is new in Aristotle is rather the conception of objects upon which a synthesis may operate. Like Plato, Aristotle believes that knowledge entails, as a necessary condition of it, a fundamental duality. But he substitutes for Plato's duality of real objects (that is ideas) and fictitious objects (that is appearances) the duality of matter and form, or subject and predicate, and finally, object and concept (here, ch. 12, § IV.1 and IV.2). Thus Aristotelian objects are objects of certain concepts, subjects of certain predicates, or pieces of matter with a certain form.

Therefore, in contrast to Plato, a proposition like "Socrates is mortal", for Aristotle, does not mean to say that the idea of Socrates is subsumed, in the hierarchy of ideas, under the idea of mortality, but is to say that the predicate '(to be) mortal' applies to the subject 'Socrates', or that the object 'Socrates' belongs to the extension of the concept 'mortal'. "Socrates" is here the name of an object (which functions as the subject of a predication). However, according to Aristotle,

an object is not merely a piece of matter, rather it is a substance; it is a piece of matter with a certain form. And it is this form, which makes this substance just what it is. Thus, the term "Socrates", properly speaking, refers to this form, that is to a predicate or, even to a concept (here, ch. 11, § II). The question thus is the following: to what piece of matter does the form 'Socrates' apply? In different terms: what is the subject of the predicate '(to be) Socrates' or the object which belongs to the concept '(that which is) Socrates'. In answering that this object (subject or piece of matter) is just Socrates, we accept to use a concept (a form or a predicate) to indicate a piece of matter, a subject or an object. No knowledge would be possible if we were not able to do it. But no knowledge would be possible yet, if all forms, predicates or concepts were treated as pieces of matter, subjects or objects. Thus knowledge asks for a distinction between forms, predicate or concepts, which indicate pieces of matter, subjects or objects, and forms, predicate or concepts which do not. Of course, such a distinction is relative to specific acts of knowledge, since we can utter both the sentence "Socrates is mortal" and the other "this man is Socrates". Therefore, new and essentially non-Platonic problems arise at the core of the Aristotelian theory of knowledge: is it possible—in a certain epistemological context—to treat a certain form, predicate or concept as a piece of matter, a subject or an object, or is this impossible? Under which conditions is such a thing possible? What piece of matter, subject or object, is the content of this form, predicate or concept, when it is treated in such a way? In other and simpler terms: is a certain concept able to indicate an object, or a plurality of objects, or it is not?

This is not the same question as asking if one or more objects fall under a certain concept, since the latter may be possible, even though the concept is not able to indicate any objects as such. Take the example of the concept '(to be) red'. Its extension is certainly not empty in the context of our empirical knowledge. Nevertheless, it fails to indicate any empirical object as such. Nor is it the question whether a certain predicate is essential to a certain subject, or not, since it is possible that we agree in considering a certain predicate as essential to a certain subject (for example the predicate '(to be) human' for Socrates), even if we maintain that it does not indicate an objects as such.

Even though he seems to accept the intensional distinction between predicates which can indicate a subject and predicates which are essential for a certain subject, Aristotle seems to believe that no predicate can be essential for a certain subject, if it is not able to indicate an object. By essential predicates of a certain subject *P* Aristotle means (*Posterior Analytics* 73*a* 34 - 73*b* 3) both the predicates which belong to the essence of this subject (as the predicate '(to be a) man' belongs to the essence of Socrates). And the predicates such that if they are taken as indicating a subject, let us say *Q*, then the predicate which indicates the subject *P* belongs to the essence of this subject Q. Thus a predicate Q is an essential predicate for a subject P if and only if either the predication "P is Q" or the predication "Q is P" are essential predications, that is: they assign to their respective subjects a predicate which belongs to the essence of them[2]. On the base of such a definition, Aristotle argues[3] in chapters I, 19 - I, 22 of *Posterior Analytics* in favor of the following thesis:

If "P is Q" is an essential predication, and $\{P_j\}$, $\{Q_j\}$ and $\{S_j\}$ are three series of predicates respectively occurring in the series of predications:

(*a*) $\{$"P_1 is P", "P_2 is P_1", "P_3 is P_2", ...$\}$

(*b*) $\{$"Q is Q_1", "Q_1 is Q_2", "Q_2 is Q_3", ...$\}$

(*c*) $\{$"P is S_1", "S_1 is S_2", ..., "S_{w-1} is S_w", "S_w is Q"$\}$

then:

(*i*) if the predications of the series (*a*) and (*b*) are all essential, then the series $\{P_j\}$, and $\{Q_j\}$ are finite;

(*ii*) if the predications of the series (*c*) are all essential, then the series $\{S_j\}$ is finite;

(*iii*) if the negations of the predications of the series (*a*), (*b*) and (*c*) are all essential, then the series $\{P_j\}$, $\{Q_j\}$ and $\{S_j\}$ are finite.

As, according to him, a proof can only contain essential predications, this means both that no proof goes on *ad infinitum*, and that there is no proof of everything (*ibid.*, 82*a*, 6-8). In the chapters I, 20 - I, 21, he argues that if (*i*) is true, then (*ii*) and (*iii*) are also true. Finally in the chapter I, 22 he argues that (*i*) is true.

At the beginning of this chapter, Aristotle states that no subject can be defined and known, if its essential predicates are infinite in number (82*b*, 37 - 83*a*, 1)[4] and that no predicate can be an essential predicate of a certain subject, if it is not able to indicate a subject, namely either the same subject to which it applies or a certain species of it (*ibid*, 83*a*, 24-25). According to the literal reading of the second of these theses, it is not possible that a predication "P is Q" is essential, if the predicate Q does not indicate a subject that is just P (since, if Q is a species of P, it is certainly not essential). However Aristotle seems to think that this predication could also be essential if the predicate Q indicates a subject of which the subject P is just a species. In any case, Aristotle is arguing that if there is no white which is just white, without besides being also something else (*ibid*, 83*a*, 30-32), then the predicate '(to be) white' cannot be essential of any subject. This means, according to Aristotle, that Platonic ideas have to be rejected, or, at least, that they can not occur in a proof (*ibid*, 83*a*, 32-33).

After this, Aristotle advances three different arguments in favor of (*i*), the third of which (*ibid.*, 84*a* 17-28) is called "analytic" (*ibid.*, 84*a* 8), and contrasted to the other two, which are said to be "logical [λόγικος]" or—as someone translate, according to Gerard of Cremona— "dialectic".

Let us look how such an argument runs. If the downward series of predicates P_i is infinite, there will be for every (natural) number j a predicate P_j, such that "P_j, is P_{j-1}" is an essential predication, thus, reascending the series, we should conclude that for every (natural) number j there is a predicate P_j such that "P_j is P" is an essential predication. But this is impossible, because it is not possible that infinitely many things belong to only one thing. Thus the conclusion is proved for the first series. The same argument works for the second series too, because if this series were infinite, there would be for every (natural) number j a predicate Q_j, such that "P is Q_j" is an essential predication, what makes definition impossible.

It is not important here whether this argument is correct or not[5]. What is important for us is that Aristotle calls this argument "analytic". What does he mean by that? Which character of this argument does he want to underline by choosing such a qualification? If we consider two further passages of the *Posterior Analytics*, where the term "analysis" occurs with a clearer meaning, two distinct answers are possible[6]. The first answer appeals to a passage of chapter I, 32 (88*b*, 15-20), where Aristotle argues that, from the obvious premise that every (right) conclusion can be proved starting from all principles, it does not follow that the principles are the same for every science. And, as a counter-example, he mentions the cases of mathematics and analysis. Clearly, the term "analysis" here refers to the science of syllogisms or, generally, the science of proof, in harmony with the title itself of Aristotle's treatises on this topic (cf. also *Metaphysics*, 1005*b*, 4). If we accept such a notion of analysis, we may assert that Aristotle's argument is analytic, because it proceeds by (implicit) syllogisms. The second answer appeals to a passage of chapter I, 12 (78*a* 6-8). There Aristotle says that if it were impossible to derive truth from falsehood, "analysis" would be easy, because it would there be necessarily convertibility (ἀντέστρεφε). Here, the term "analysis" seems to refer to deduction of knowns from unknowns, or (accepted or acceptable) premises from conclusions we are trying to prove (cf. Barnes 1975, p. 147). If we assume this is the meaning of the term "analysis", we can assert that Aristotle's argument is analytic, because it assumes that conclusions are true and deduces something that is known to be false (or accepted as false) that is: it is a *reductio ad absurdum*.

Taken separately, these two answers might be convincing. However, when compared with each other, the problem arises of how to understand their compatibility. Why is the science of proof called "analysis", if analysis is, in a different sense, regressive deduction? We can find an answer to such a question in the first lecture (*Proemium*) of Saint Thomas's commentary on the *Posterior Analytics*. Thomas's argument is the following. At the beginning of the *Metaphysics*, Aristotle says that man lives thanks to art and reason. Art is a certain order of reason, according to which human acts attain certain ends. Reason not only directs the acts of inferior parts of man, but it is an act too. Thus, there is an art of reason which enables us to order the acts of reason without mistake. This art is logic, that is thus both rational (as every art) and is about reason. Therefore, logic is divided into different parts, according to the differences of the acts of reason. There are three kinds of acts of reason. The first one is understanding of the indivisible and simple; this is the matter of Aristotle's *Categories*. The second is the act of composition or division, which produces respectively affirmative and negative judgments; this is the matter of Aristotle's *De interpretatione*. The third finally is "concerned with what is proper to reason [*secundum id quod est proprium rationis*]" and it is just the act of inference (as Thomas says: it is "*discurrere ab uno in aliud, ut per id quod est notum deveniat in cognitionem ignoti*"). Such an act in turn can be performed according to three different modalities, since reason can act with or without necessity (or certainty), and if it acts without necessity, it may attain truth or falsehood. The part of logic which treats the first kind of these modalities of reason is called "*iudicativa*" and it produces judgments which have certainty of science. Now, such a certainty is only possible if these judgments are "resolved" into the first principles (they are brought back to the certainty of the first act of reason, that is the understanding of indivisible and simple). Because of that, this part of logic is called "analysis" and is the matter of Aristotle's *Analytics*.

Generally kept back by this splendid argument is the fact that, according to Aristotle, analysis is concerned with certainty and demonstration (rather than with probability and discovery—which is, according to Thomas, the matter of Aristotle's *Topics* —or false arguments—which is the matter of Aristotle's *Sophistici elenchi*[7]). This is certainly the case: according to Aristotle, analysis is concerned with certainty and demonstration. But, if Thomas is right, as I believe he is, it is not because analysis is demonstrative, but because demonstration is necessarily analytic, that is: it guarantees the truth of the conclusions by reducing them to first principles. This does not mean that a proof of T is necessarily a deduction of (some) first principles from T, since Aristotle knows perfectly well that truth can be deduced from falsehood. The point is different and may be stated as follows. If a proposition T is given and has to be proven (or refuted), the only thing we can do is just look for first principles from which T can be deduced. Thus, if we consider a proof from the point of view of its conclusions, rather than of its principles, it is necessarily preceded by a regressive conduct that reduces these conclusions to their principles. By calling "analysis" the science of proof, Aristotle seems to insist on this aspect of proof (Ross 1949, 400), that is really the most important one, if we are concerned—as Aristotle was—with the truth of conclusions and the conditions of such a truth. Of course, if the regressive conduct consists in deducing the negation of one first principle from T, it is *ipso facto* (at least from classical,or Aristotelian point of view) a proof of $\neg T$. This is exactly the case with the previous argument, but it does not represent the general case.

Thus, when Aristotle states that his previous argument is analytic, he is referring to analysis as a regressive conduct, which brings us from certain statements

to the principles making them true (or provable)[8]. A similar idea is evoked in a short passage of the *Metaphysics* (1063*b*, 15-19), where Aristotle argues that contrary statements cannot both be true. The reason of that, he says, is evident "by analyzing the definitions of contraries into [its] principle [ἐπ' ἀρχὴν τοὺς λόγους ἀναλύουσι τοὺς τῶν ἐναντίων]". Here, analysis is a regressive conduct, which brings us from a definition to the principle that explains it, assigns to it a certain meaning.

This seems to be quite clear, but it is not yet the end of the story, since in the *Prior Analytics* Aristotle often uses (cf., as only an example, 51*a* 18-19) the term "analysis" with in a strictly different meaning (Hintikka and Remes 1974, 31). According to this meaning, analysis is reduction, or more precisely, breaking up of a certain figure of syllogism into another figure (cf. Smith 1983, 161), which enables us to know whether the syllogisms of the former figure are valid or invalid. Thus, the science of proof is concerned with regressive reduction in a twofold way. First, because proof asks for regressive reductions of conclusions to first principles, and second because a necessary condition for the correctness of a certain proof is its reducibility to the accepted figures of syllogism. It is just because the act of this double regressive reduction is an analysis, that proof is concerned with analysis: it is not analysis that is demonstrative for Aristotle—as Timmermans (1995) says, for example—but proof that is necessarily analytic.

Once again, this is not the end of the story. Before leaving the *Analytics*, let us briefly come back to the previous analytic argument. What Aristotle asserts by such an argument[9] is that no proof is possible about a certain subject, indicated by a predicate P, if the regressive series of predicates P_j which specify P, does not terminate in a predicate $P_n = A$ which can not be ulteriorly specified. Aristotle speaks of proof, but he seems to refer to knowledge in general. In our terms, he is thus asserting that no knowledge is possible if there are no concepts which are, as such, concepts of objects, rather than concepts of properties or relations; in different and more Aristotelian terms: no knowledge is possible if there are no forms which are intrinsically substances.

III.3 Analysis: *Nicomachean Ethics*

Let us keep this result in mind, and consider now the famous argument of the chapters III, 3 - III, 5 of Aristotle's *Nicomachean Ethics* (1111*a*, 21 - 1113*a*, 12; cf. here, ch. 9, par. II). Here Aristotle is discussing the difference between a voluntary act (ἑκούσιον) and a choice (προαίρεσις). While a voluntary act is the act of which the moving principle is in the agent itself (111*a* 22-23), a choice is certainly a voluntary act, but it is not any kind of voluntary act. First of all, choice is neither appetite nor anger, nor wish (βούλησις). Moreover, it is neither opinion (δόξα) in general, nor a particular kind of opinion. There are different reasons for that. Two of them are: first, opinion may concern any kind of object, while choice can only be exercised on things which are in our own power; second, opinion is either true or false, whereas choice is either good or bad. Thus opinion, as such, is different from choice, even though the former either precedes or accompanies the latter. Namely (1112*a*, 15) choice is a voluntary act which has been the object of deliberation (προβεβουλευμένον[10]), the voluntary act which follows (and depend on) an act of deliberation (βούλευσις). By referring to the act of the βουλή[11], Aristotle seems to assert that choice is an act resulting from a plural, or even public or political consideration, aiming at determination of a certain action, that is the choice itself.

Now, according to the previous characterization of a voluntary act, the agent of such a deliberation can be nothing else but the subject, who operates the choice himself. But what is the object of such a deliberation, about what is it? This is the topic of chapter 5. Implicitly, the answer has already been given, since Aristotle has said above that choice can only be exercised on things that are in our own power. However, he tries now to make such an answer explicit, by extending it to any sort of deliberation, and by saying what sorts of things these things are, or are not. First of all, according to Aristotle, eternal (that is necessary[12]) things—like those which mathematics treats—are not objects of deliberation. The same is true for that which changes if it changes always in the same way—like the subject of natural motions—or without any regularity—like rain—or still according to chance—like finding a treasure. This is quite clear, since no human (or political) subject—that is the agent of a deliberation—can intervene on these things. According to Aristotle, the range of deliberation is however even narrower, since each subject only deliberates on things which he is able to modify. For example, Aristotle says, no Lacedæmonian deliberates on the Scythian government. Thus, if we are referring to deliberation, human power has to be intended as practical and political power, that is power fixed by accidental constraints and even social conventions. Moreover, deliberation does not concern ends, but only means, which are necessary to reach already fixed ends. Namely, the objects of deliberation are just two. First, if the same end can be reached by a number of distinct means, deliberation establishes, which of them entails the easier and better realization of this end. Second, if the end can be reached only in one way, it establishes the chain of means which produces this way, by descending from it, up to the actual situation of the subject.

Aristotle continues (1112*b* 20-21) "who is deliberating seems to research and analyze the way described as [it happens with] a (geometrical) figure [ὁ [...] βουλευόμενος ἔοικε ζητεῖν καὶ ἀναλύειν τὸν εἰρημένον τρόπον ὥσπερ διάγραμμα]". Here our translation is literal, but we could interpret the previous passage in this way: "who is deliberating seems to research in the way described like he were analyzing [a] (geometrical) figure". Thus Aristotle seems to intend that what has

been described is just the path of analysis. Deliberation is thus a sort of analysis, or better, analysis is the form of deliberation; it is a form of thinking, namely the form or thinking which deliberation satisfies. But how can this form be characterized in general; what is proper to it, rather than to the particular nature of deliberation? It seems that Aristotle would like to answer such a question, since he immediately remarks (1112*b*, 21-23) that, though every deliberation is a research, not every research is a deliberation—"as [it is the case of] mathematical ones [οἷον αἱ μαθηματικαί]"—and asserts (1112*b*, 23-24) that what is last in the analysis is the first "in generation [ἐν τῇ γενέσει]". The meaning of Aristotle's comparison is not completely clear. Different translations understand it in quite different ways. It seems to me, however, that Aristotle is comparing respectively deliberation with the path that brings us from the definition of a certain figure to the elements from which the construction (or generation) of this figure starts (and, implicitly, choice of the construction itself), and he is asserting that both deliberation and this path are examples of analysis. However, comparison is not identification, since the path that goes from the definition of a certain figure to the elements from which the construction of this figure begins is a mathematical research and mathematical researches are not deliberations (even though every deliberation is a research).

If this is correct, Aristotle thinks that, as long as it is a regressive reduction, analysis can be both the reduction of the definition of a geometrical figure to the elements starting from which the (geometrical) construction of this figure is possible (which I shall call a "geometrical reduction"), and the reduction of a certain end to the actually available means, from which a chain of means, bringing us to such an end, could start. In the first case, analysis brings us from a certain condition (that is not still an actual object, but only a character that a certain object should be eventually satisfy) to the actual objects starting from which another actual object satisfying the given condition will certainly (and always) be produced. In the second case, analysis brings us from the determination of a certain end (that has not been actually reached), to the means that possibly may produce such an end. While in this second sense analysis is deliberation, in the first sense it is not.

As we have just seen, a deliberation, according to Aristotle, is never about eternal (that is necessary) things and therefore, it is not accompanied by the guarantee that the end will be reached by following the chain of means that it is actually indicating. Here, Aristotle seems very close to a Platonic conception, since he seems to argue that deliberation is just a matter of opinion and not of knowledge. According to such a point of view, analysis does necessarily accompany the demonstrative necessity of mathematics. Thus, while the agent of the first sort of analysis is nothing but the mathematician, who actually knows that a certain construction is possible and that it certainly produces an object satisfying certain conditions, the agent of the second sort of analysis is the βουλή, or generally the political community that has to evaluate the risks and chances of a certain choice. I do not say that, as long as analysis is a regressive reduction, it is not necessarily a regressive deduction; still, neither do I say that analysis is necessarily neither a regressive deduction nor a regressive reduction preparing a possible deduction. What I am saying is, that analysis is not necessarily neither a regressive deduction, nor a regressive reduction preparing a successful enterprise (and thus, *a fortiori*, a demonstrative performance, as a geometrical construction is).

This is only one aspect of the question, however, since there is a further important, and, as I believe, deeper aspect both of deliberation (in Aristotle's sense) and of geometrical reduction, according to which they appear logically similar, despite the radical difference between practical reason (to which deliberation seems to belong) and purely speculative reason (to which geometrical construction seems to belong, instead). A deliberation starts with the fixation of an end and is concerned with considering suitable means to reach this end. Now, to fix an end means to present both the concept of a state of things and to state the will to realize it. Thus, in the case of deliberation, analysis terminates with the determination of a possible action that has to be performed in order to produce a certain state of things. Similarly, a geometrical reduction does not start by merely stating a definition, but only when the aim is stated to exhibit an actual object satisfying such a definition. In the case of geometrical construction, the conclusion of analysis is therefore also the determination of a possible action which has to be performed, the difference being that in the first case, the action produces, or should produce, a new state of things, while in the second case, the action permits one to exhibit a geometrical object. In the first, as in the second case, however, the result of such an action is just something which falls under the concept presented in the first stage; it is the object of this concept. Thus in both the cases, analysis is reduction of a certain concept, which is given as such (independently from the corresponding object), to the conditions of actual realization of the corresponding object, that is the conditions that make this realization actually possible (for the agent of the analysis himself).

IV Aristotelian Forms of Analysis

Following Aristotle in his arguments of *Analytics* and *Nicomachean Ethics*, we have thus encountered four examples of what he calls "analysis": the regressive conduct connected to a proof of a given statement T (that is its reduction to accepted principles or their negation) or to an explication of a certain definition—which we could call "reduction to principles"—, the reduction of a certain figure of syllogism to a different figure—which we could call "syllogistic reduction"—,

the geometrical reduction, and the deliberation. What do these four examples have in common?

A first answer is already implicitly contained in what I have said: they are all examples of regressive reduction[13]. Differently from the first two, the third and the fourth examples, however, are not examples of regressive reduction, because in them something is reduced to something different which is already given or known as being true or false. These are examples of regressive reduction, because they reduce a concept to certain conditions that can be satisfied in the actual situation of the subject. This observation suggests a possible generalization of the idea of regressive reduction: a reduction is regressive when: *i*) it is finite; *ii*) it is such that its last stage is a conclusive stage, a stage that could not support any further reduction (as long as analysis is always research, as Aristotle says, it is finished only when its last stage does not ask for any other research of the same kind); *iii*) the reason for it is that such a stage is the stage of the actual knowledge, disposability or possibility of the subject. Such a generalization enables us to say that every analysis is, according to Aristotle, a regressive reduction.

Another common aspect of the four previous examples is that they refer to analysis as a form of inferential thinking, rather than merely as a form of a system of sentences or statements. Even though Aristotle directly presents the third argument of chapter I, 22 of the *Posterior Analytics* as "analytic", it seems quite obvious that he means that the conduct of reasoning that follows such an argument is analytic. This is quite evident in the case of deliberation. Thus, we might say that, for Aristotle, analysis is a form of inferential thinking, that is a system, or even a chain, of (intentionally) connected acts which brings us from a certain stage to another, essentially different one. These are acts of representation and assertion of certain contents. Moreover, the representation of these contents may be meant as a certain sentence in an available language. If this is the case, their assertion is a statement in this language. By using—as I have already done above, at a number of occasions—the same term to indicate both the form and the substance of which this form is just the form, we might then say that an analysis is a system of acts of thinking, expressed by a system of statements.

For Aristotle, an analysis, following the two previous remarks, is a system of acts of thinking realizing a regressive reduction. This means that, in order to be an analysis, a system of acts of thinking has to carry one from a certain stage to an essentially different, stage. We could call the first stage, the "initial stage of analysis", and the second the "final stage of analysis". Our previous characterization of the notion of regressive reduction specifies the nature of the final stage. As long as the notion of reduction is taken for granted however, this characterization specifies neither the nature of the initial stage nor the relations between the initial and the final stage.

What seems clear from the previous examples, is that the initial stage has to include the stating of a certain aim, and the final stage has not only to be a conclusive stage, according to the previous conditions (*i*) and (*ii*), but has also to be conclusive with respect to the possibility of realization of such an aim. However the four examples differ on this point. While in deliberation and geometrical reduction a concept is given in the initial stage, in reduction to principles and syllogistic reduction, that which is given in the initial stage is an object. Thus, we have to conclude that, according to Aristotle, there are two kinds of analysis: those which start with an object (we might call them "analysis of objects") and those which start with a concept (and might be called "analysis of concepts").

Moreover, in the reduction to principles the aim is just to prove the given statement (or the classification of the given definition), in syllogistic reduction is the validation of the given inference, in deliberation it is the realization of the end characterized by the given concept, and finally in the geometrical reduction it is the exhibition of one or more objects, which satisfy the given concept. It is then clear that the aim is neither the same for all types of analysis, neither is it the same respectively for all the types of analysis of objects, nor for all the types of analysis of concepts.

Still, while in deliberation, in geometrical reduction, and in reduction to principles, as well—when this does not consist in deducing the negation of one principle starting from the given statement—analysis does not realize the aim, but merely indicates the conditions of its realization, in syllogistic reduction, and in reduction to principles—when this consists in deducing the negation of one principle starting from the given statement—analysis does realize the aim (or at least it provides all the material allowing us to say that the aim has been realized). The latter cases both are examples of analysis of objects. In them the givens are objects that have actually been exhibited to the subject. However these objects are so given that the subject ignores something about them, namely he ignores whether these objects enjoy or do not enjoy certain properties. The aim just specifies which properties they have and further states the will to know whether these objects satisfy these properties or not. Thus, by saying that in these cases an object is given, we are stressing that what is given will be considered as an object in the act of thinking (or, if you prefer, in the statement) that finally states that the aim has been reached. It seems, according to the previous examples, that, when this is the case, analysis can realize the aim alone. Now, in the case of analysis of concepts as well, the givens might be intended, in a sense, as objects, since every concept can be treated as an object and a subject just treats it in this way when taking it as being given. Nevertheless these concepts will not occur as objects in the act of thinking (or, if you prefer, in the statement) that states that the aim has been reached finally; they just occur in it as concepts. This remark should render the previous distinction between analysis of concepts and analysis of objects. Besides, it should also justify the following general conclusion: no analysis of concept can produce as such the realization of the aim occurring in its initial stage. Thus, we can refine our previous distinctions by distinguishing three different genera of analysis: analysis of concepts; analysis of objects which does not realize alone the aim occurring in its initial stage (or "non conclusive analysis of objects"); and analysis of objects which does realize alone the aim occurring in its initial stage (or "conclusive analysis of objects").

Consider first the previous examples of analysis of concepts, that is deliberation and geometrical reduction. It is obvious that the conditions of realizing the aim are not the same in the two cases. The following are two obvious necessary conditions. In the case of geometrical construction, the subject has to operate on the given object which analysis has indicated and realize the construction according to the accepted clauses. If we assume that these clauses are just the Euclidean axioms, such a construction may be intended as a synthesis, in the usual meaning of this term (cf. the next paragraph V.4): it is a construction of a new object starting from given objects. Thus, we could say that in this case, the aim occurring in the initial stage of analysis is not realized as long as no synthesis follows the analysis. In the case of deliberation the subject has to act, he must pass from deliberation to choice. In this case no one of the previous senses of the term "synthesis" seems to entitle us to say that the aim occurring in the initial stage of analysis is not realized as long as no synthesis follows the analysis. Are these two necessary conditions also sufficient? At first glance, we might say that this is not the case, since neither synthesis nor choice produces the realization of the aim, if the analysis has not indicated the correct starting point for them. Such an answer is certainly correct, but it also trivial. And triviality cannot simply be avoided by considering nothing but the case of correct analysis, since we have no general means to distinguish *a priori* between correct analysis and false analysis. The situation is quite different in the two cases. This is clear if we consider examples of geometrical construction taken from Euclid's geometry: while for deliberation we certainly do not dispose of these means, for geometrical reduction we possibly dispose of them. This remark elucidates the essential difference between deliberation and "mathematical analysis" stated by Aristotle. Besides, it makes this distinction independent of the Platonic attitude inherent in the argument of *Nicomachean Ethics*. From an intensional point of view, the correct distinction thus is the one between analysis of concepts regulated by a criterion of correctness (relatively to the aim) which operates *a priori* from the actual application of its indications (or "regulated analysis of concepts") and analysis of concepts which is not regulated by a criterion of this sort (or "non regulated analysis of concepts"). The only example of a regulated analysis of concepts Aristotle presents is an example where the aim is reached if and only if a synthesis follows the analysis.

Consider now the previous example of a non-conclusive analysis of objects. It is a reduction to principles which does not consist in deducing the negation of one principle starting from the given statement. In this example a necessary condition for the aim to be realized is that a deduction of the given statement from first principles is conducted. In this case analysis has two distinct tasks: to indicate which first principles have to be taken as starting points of this deduction, and to suggest the path of this deduction. Clearly, to do it is not to conduct the proof of the given statement. This proof demands that deduction is conducted. If analysis is nothing but a regressive deduction, the indication of the first principles which have to be taken as the starting points of the proof is obvious. In this case, the only criterion for the correctness of the analysis (relatively to the aim) is convertibility of deduction. Now, this criterion is *a priori*, in the previous sense, only if it operates on the analysis itself. Hence, it is *a priori* only if it states that analysis has to contain only inferences by equivalence. This is in general a too restrictive criterion, however, since T might be deductible from certain first principles, even if it is not equivalent to them. Nevertheless, no other *a priori* criterion for the correctness of the analysis seems to be available in this case. As long as it is a non-conclusive analysis (of objects), a reduction to principles is thus either regulated or not regulated; if it is regulated it fails, in general, to exhibit all the sufficient conditions of deduction of the given statement.

In the first as well in the second case, the realization of the aim demands that the analysis is followed by a deduction, which, according to the previous senses of this term, is not a synthesis. There is an aspect of non-conclusive reduction of principles (as it is intended by Aristotle) however, which makes it similar to geometrical construction and even suggests a generalization of the idea of synthesis which includes such a deduction. To understand this point let us come back to the very last remark of the previous paragraph III.2, where I have argued that for Aristotle no proof is possible about something that is P if there is no predicate $P_n = A$, intrinsically indicating a subject. This means that the first principles of any proof are just statements which refer to an object just given as such, rather than to an object which merely satisfies a certain concept of property (ch. 12, par. VI.2). This is to say that no proof is possible if an object is not exhibited as such. As one of the tasks of non-conclusive reduction to principles is to indicate the first principles from which the proof can start, this means that, in this case, one of the tasks of analysis is just to indicate some objects which are given as such, serving as the starting points of the proof (these objects are clearly not first principles, they are rather that about which first principles speak, since no first principle is an object given as such, being rather an object satisfying the concept 'to be known as true'). This is also true of geometrical construction: one of its tasks is to indicate a given object given as such, serving as the starting point of construction. Thus, as long as they follow an analysis, both, proof and geometrical construction, start from objects which have to be given as such, rather than as objects which satisfy certain concepts of property.

Even though there is no evidence to ascribe such a generalization to Aristotle, we might call "synthesis" any conduct of thinking that follows a non-conclusive analysis, realizing the aim occurring in the initial stage of this analysis, and starts from an object that is given as such (rather than as the object which satisfies a certain concept of property). This meaning of the term "synthesis" has become common during the modern age, but it seems to us that there is no room for it in the Greek culture of the classical age. While the notion of analysis, because of Aristotle, grows into gnoseological complexity which enables it to describe a fundamental conduct of knowledge, the notion of synthesis does not seem to suffer a similar evolution and always refers, in the Greek culture of the classical age, to the composition of given objects in order to obtain new objects, or, more in general, to the construction of new objects, starting from given objects. Moreover, when, probably in the first half of the fourth-century of the Christian era, Pappus explicitely contrasts synthesis with analysis, describing them as successive stages of a geometrical method, he does not take into account the notion of analysis in all its Aristotelian complexity. Rather, it seems that the generalization of the notion of synthesis will only occur later, when the Pappusian opposition of it to analysis will be considered in the framework of the general Aristotelian conception of the latter.

V Analysis and Synthesis According to Pappus

V.1 Pappus's Definition

At the beginning of the 7th book of *Mathematical Collection* (VII, 1-2), when Pappus expounds the method of analysis and synthesis, he seems to advance a rational reconstruction of an important fragment of Greek mathematics (here, ch. 6, par. I). He does not say merely that the "domain [or treasury] of analysis [ἀναλυόμενος; literally: being analyzed]"[14] is a certain matter (namely a matter prepared for those who, after having got usual elements, wish to gain "in the (geometrical) figures [ἐν γραμμαῖς]" the power of solving the problems which are proposed to them—and the only matter useful for that). His proposition is more complex: "Ὁ καλούμενος ἀναλυόμενος, my son Hermodorus, κατὰ σύλληψιν ἰδία τίς ἐστιν ὕλη ...". The problem is with the expressions "καλούμενος [being called]" and "κατὰ σύλληψιν [according to the comprehension]". Hintikka and Remes, following Heath, translate the first expression by "so-called" and substitute for the second the adverbial form "in short" ("The so-called Treasury of Analysis, my dear Hermodorus is, in short,..."; Heath's translation is: "The so-called ἀναλυόμενος ('Treasury of Analysis') is, to put it shortly, ..."). Jones also agrees with

Heath about the first expression, but renders the second in the verbal impersonal form "taken as a whole" ("That which is called the Domain of Analysis, my son Hermodorus, is, taken as a whole,..."). The same idea of using a verbal form to translate "κατὰ σύλληψιν" was already used by Hultsch (Pappus CH, 635). Hultsch used however a personal form for that and even the first person singular: "ut paucis comprehendam"[15]. This is also the solution advanced by Ver Eecke who translates the whole expression "ὁ καλούμενος ἀναλυόμενος" by "le champ de l'analyse" and renders "κατὰ σύλληψιν" as an auto-reference: "Le champ de l'analyse, tel que je le conçois, mon fils Hermodore, est...".[16]

From a philological point of view, Ver Eecke's solution is probably too extreme. Nevertheless it at least suggests that Pappus is here interpreting the work of Greek mathematicans of the classical age (here, ch. 6, 170, note 2), rather than expounding a method largely and explicitly employed in Greek geometry. According to such an interpretation (that is also that of Hultsch) we could even guess that, even if they applied conducts of thinking or arguments that could be intended as examples of analysis and synthesis in Pappusian sense, these mathemacians did not conceptualize them as Pappus does.

Pappus' exposition of the method of analysis and synthesis is well known (here ch. 8, par. II), and I may limit myself to some remarks (cf. also here, ch. 12, 320-321). As we have just seen, the domain of analysis is presented first as concerned with non-elementary geometrical problems. According to Pappus, this matter was treated by Euclid, Apollonius and Aristaeus the Elder, by using the method of analysis and synthesis. This method then is applied to the realization of a certain aim; and this is perfectly consistent with the Aristotelian conception of analysis. Pappus' description of the first stage of this method, that is just analysis, is also consistent with Aristotle's views[17]: analysis is presented as a way, or a path (ὁδός; ἔφοδος), which leads from the assumption of what is sought, as if it were admitted, to something that is already admitted, that is a first principle. It is thus an inverted (ἀνάπαλιν) way and, namely, it is an inverted solution or conclusion (ἀνάπαλιν λύσις). The final stage of analysis for Pappus is the initial stage of synthesis. The latter follows after the former and just considers what is given as given. It is also a way, and it is namely the inverted way of analysis. Since Pappus says: "in the synthesis, on the other hand, by inverting the way [ἐξ ὑποστροφῆς], that which has been grasped last in analysis [τὸ ἐν τῇ ἀναλύσει καταληφθὲν ὕστατον] is supposed [to be] already gotten and [its] consequences [ἑπόμενα] and prolegomena [προηγούμενα] [are] ordered according to their nature [κατὰ φύσιν τάξαντες] and [are] linked with one another to arrive, at the end [εἰς τέλος], at the construction of what is sought [τῆς τοῦ ζητουμένου κατασκευῆς]". The Greek term for "construction" is thus a cognate of the verb "κατασκευάζω", which has really a more general sense and also means "to organize", "to set out", or "to prepare", and we could generally intend—coupling it with the term "τέλος"—as referring to the realization of the aim.

Thus Pappus uses the term "synthesis" to refer generally to the argument, which follows a non-conclusive analysis and leads from the final stage of it onto the realization of the aim. Hence, the woolliness of his text has an obvious justification: he is trying to provide a general description of different sorts of processes. However, Pappus's generality goes not as far as Aristotle's. According to him, there are two types of analysis. One of them enables us to research that which is true (ζητητικὸν τἀληθοῦς) and is called "theoretical [θεωρητικόν]", while the other is able to get what was proposed (ποριστικὸν τοῦ προταθέντος) and is called "problematical [προβληματικόν]". In the first one—Pappus says—what is sought is supposed to be true, while in the second what is proposed is supposed to be known. Starting from these suppositions, the theoretical analysis brings us to something which is admitted as being true or false, while the problematical analysis brings us to something that is admitted as being possible (realizable or given) or impossible. Even though Pappus's language is very general (and also quite ambiguous and inaccurate), it seems clear that he is only concerned with geometry and believes that as long as it provides a geometrical argument, analysis is either a regression to principles or a geometrical reduction. Moreover, he seems to restrict his description to convertible analysis, since he argues that both, truth and falsehood, or possibility and impossibility, occurring respectively in the final stage of theoretical or problematical analysis, entail respectively truth and falsehood, or possibility and impossibility of the thing that is sought or proposed. The proof and the construction then are nothing but the reversal of analysis. If this is the case, synthesis only needs to exhibit proof or construction, since analysis is able to conclude, both whether the given sentence is true or false and whether the proposed definition can be satisfied or not, and to indicate the whole conduct of proof and construction. Such a strong (logical) restriction however does not appears to be consistent with Pappus's mathematical practice, nor even with the (historical and mathematical) extension he ascribes to the method of analysis and synthesis[18]. Nevertheless, Pappus's presentation makes his attitude manifest. Even though in a sense Pappus is generalizing the classical notion of synthesis as simple composition, he is, in a different sense, restricting it. Not only does he make of synthesis nothing but the prolongation of analysis, but he also considers both, analysis and synthesis, as quite codified procedures belonging to a technical domain.

V.2 Heron and/or a *Scholium* to Euclid's *Elements*

Pappus's presentation of the method of analysis and synthesis is probably not the very first one, even though it is certainly the most extensive and explicit. We can refer to two pieces of evidence to support this thesis. The first one is al-Nayrīzī's

Arabic account of certain passages from Heron's commentary on book II of the *Elements* (al-Nayrīzī ECC, 89) and the second one is an interpolation introduced at different places in the beginning of the 13th book of Euclid's *Elements* (Euclid OO, IV, 364-381). Because of the similarity between these two expositions, Heiberg (Heiberg 1903, 58) ascribed the second one also to Heron, who lived in Alexandria during the Christian era: during the first century, according to Neugebauer (1938), or during the third-century, a little earlier than Pappus (Heath 1921, II, 298-306). Knorr (1986, 355) guesses that it is successive to Pappus and merely depends on Heron's (and Pappus's) exposition, instead.

According to Gerard's translation from the Arabic[19], Heron describes analysis (*dissolutio*) as a way to answer a question: "first we set that which is in the order of thing sought [*primo ponamus illud in ordinem rei quesite*]" (al-Nayrīzī ECC, 89, 14-15), then we "reduce [it] <to that> of which the proof has already preceded [*reducemus <ad illam>, cuius probatio iam precessit*]" (*ibid.*, 89, 15-16). The synthesis (*compositio*) is then nothing but a composition: "we begin from a thing known, then we compose until the thing sought is come upon [*incipiamus a re nota, deinde componemus, donec res quesita inveniantur*]" (*ibid.*, 89, 18-19).

Heron[20] seems, like Pappus, to include in his general presentation both problematic and theoretical analysis (that is geometrical reduction and reduction to principles). But he presents, differently from Pappus, synthesis as a simple process of composition of objects, which is only consistent with the first sort of analysis. This does not prevent him from exemplifying the method by proving theorems with it, namely by applying it to the demonstration of the first thirteen theorems of book II of the *Elements* (*ibid*, 89-110).

The application of the method of analysis and synthesis to the proof of theorems is however much more clear in the interpolation to book XIII of the *Elements*. Here a proof, different from Euclid's one, is provided for each one of the propositions XIII, 1 - XIII, 5. These proofs consist of two distinct parts, the first of them being called "analysis" and the second "synthesis". Moreover, a general definition is advanced. According to this definition, "analysis, on the one hand, is the assumption of that which is sought as [if it were] admitted up [to arrive], by means of [its] consequences, to something [which is] admitted [as] true [ἀνάλυσις μὲν οὖν ἐστι λῆψις τοῦ ζητουμένον ὡς ὁμολογουμένον διὰ τῶν ἀκολούθων ἐπί τι ἀληθές ὁμολογούμενον]" (Euclid OO, IV, 364, 18-20); while "synthesis [is], on the other hand, the assumption of that which is admitted up [to arrive], by means of [its] consequences, to something [which is] admitted [as] true [σύνθεσις δὲ λῆψις τοῦ ὁμολογουμένον διὰ τῶν ἀκολούθων ἐπί τι ἀληθές ὁμολογούμενον]" (*ibid.* 366, 1-2) or, in the Theonine version, "the assumption of that which is admitted and then, the attainment (or the ending [?]), by means of [its] consequences of what is sought [λῆψις τοῦ ὁμολογουμένον διὰ τῶν ἀκολούθων ἐπί τὴν τοῦ ζητουμένου κατάληξιν ἤτοι κατάληψιν]" (*ibid.* n. 2).

Consider as an example the alternative proof of proposition XIII, 1 (*ibid*, IV, 366-369): if a segment AB (fig. 1) is cut in C (according to the construction exposed in the proposition II, 11), in such a way that AC is the mean proportional between AB and CB, and the segment DA is equal to the half of it, then the square constructed on $AC+DA$ is five times the square constructed on DA:

$$[(AB : AC = AC : CB) \wedge (AB = 2DA)] \Rightarrow \text{Sq.}(AC + DA) = 5[\text{Sq.}(DA)]$$

In modern terms, if we put $AB = K$ and $AC = x$, the antecedent provides the equation: $x^2 + Kx - K^2 = 0$, from which we have: $\left(x+\frac{K}{2}\right)^2 = 5\left(\frac{K}{2}\right)^2$, that was to be proved.

The scholiast takes both AB and AC ($< AB$) as given on the same straight line, in such a way that $AB : AC = AC : CB$ and constructs on the same straight line, but on the opposite side than AB, a segment DA, so that $AB = 2DA$. Then he assumes that

$$\text{Sq.}(CD) = 5\text{Sq.}(DA) \qquad (a.1)$$

and proceeds according to the following deduction:

$$\text{Sq.}(CD) = \text{Sq.}(DA + AC) \qquad (a.2)$$

$$\text{Sq.}(CD) = \text{Sq.}(DA) + \text{Sq.}(AC) + 2\text{Rect.}(DA, AC) \qquad (a.3)$$

$$\text{Sq.}(AC) + 2\text{Rect.}(DA, AC) = \text{Sq.}(CD) - \text{Sq.}(DA) \qquad (a.4)$$

$$\text{Sq.}(AC) + 2\text{Rect.}(DA, AC) = 4\text{Sq}(DA) \qquad (a.5)$$

according to $(a.1)$ and $(a.4)$,

$$2\text{Rect.}(DA, AC) = \text{Rect.}(AB, AC) \qquad (a.6)$$

$$\text{Sq.}(AC) = \text{Rect.}(AB, CB) \qquad (a.7)$$

according to the proportion $AB : AC = AC : CB$,

$$\text{Rect.}(AB, AC) + \text{Rect.}(AB, CB) = 4\text{Sq.}(DA) \qquad (a.8)$$

according to $(a.5)$, $(a.6)$ and $(a.7)$,

$$AC + CB = AB \qquad (a.9)$$

$$\text{Rect.}(AB, AC) + \text{Rect.}(AB, CB) = \text{Sq.}(AB) \qquad (a.10)$$

$$\text{Sq.}(AB) = 4\text{Sq.}(DA) \qquad (a.11)$$

according to $(a.8)$ and $(a.10)$.

As $(a.11)$ follows from the hypothesis $AB = 2DA$, without appealing to $(a.1)$, it is true and then $(a.1)$ entails something that is true. Thus, as analysis finishes with it, synthesis has to begin with it:

$$\mathrm{Sq.}(AB) = 4\mathrm{Sq.}(DA) \qquad (s.1)$$

$$\mathrm{Sq.}(AB) = \mathrm{Rect.}(AB, AC) + \mathrm{Rect.}(AB, CB) \qquad (s.2)$$

$$4\mathrm{Sq.}(DA) = 2\mathrm{Rect.}(DA, AC) + \mathrm{Sq.}(AC) \qquad (s.3)$$

according to $(s.1)$, $(s.2)$, $(a.6)$ and $(a.7)$ which do not depend on $(a.1)$,

$$5\mathrm{Sq.}(AD) = \mathrm{Sq.}(CD) \qquad (s.4)$$

according to $(s.3)$ and the figure 1 that is a part of the figure constructed by Euclid in his proof of the same XIII,1.

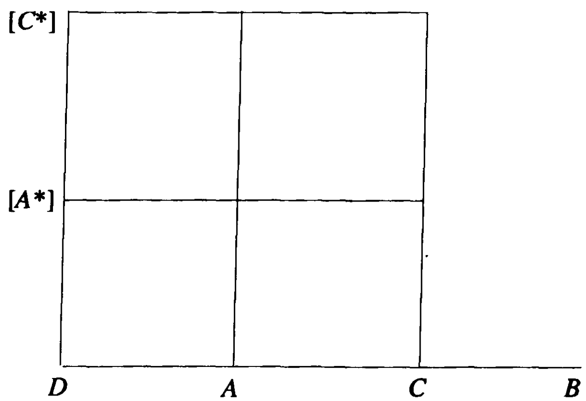


Figure 1

Clearly, the above analysis is, according to our previous terminology, an example of a non-conclusive and non-regulated[21] analysis of objects, namely, it is a non-conclusive and non-regulated reduction to principles. In its final stage, it indicates the starting point of the proof, by expressing an obvious property of an object given as such, namely the segment DB, constructed starting from AB for addition of DA, equal to a half of AB itself. Thus, taken as such, it does not include any logical novelty with respect to Aristotelian conceptions. The same is true for the proof (that is the synthesis), if it is taken as such, since it does not differ, according to its logical aspect, from common Euclidean proofs. The difference

between this proof and that proposed by Euclid for the same proposition XIII, 1 does not concern its logical character. Rather, scholiast's proof (its synthesis) is significantly simpler and wilier than Euclid's. This is clearly possible because of the indication of the analysis that suggests a good (but as such not obvious) starting point for it[22]. What the scholiast does in his interpolation thus is to apply, in a wily way, an Aristotelian indication, in order to obtain a not obvious suggestion to improve Euclid's proof. What is essentially new, with respect to Aristotelian conceptions and Euclid's mathematical practice, is both the explicit presentation of the analysis as a premise of a proof, namely as an argument suggesting the starting point of this proof; and the consequent interpretation of the proof as the second stage of a single and general method to produce (mathematical) arguments, including a heuristic as well a demonstrative aspect. Both the first and the second novelty are underlined by the use of the term "synthesis" to refer to the second stage of this method, which is nothing but what Aristotle and Euclid have called "proof".

V.3 Evidences for the application of Pappus' method in the classical age: Apollonius, Archimedes and Aristotle once again

In the 7th book of the *Collection*, Pappus argues that the method of analysis and synthesis, as he describes it, was actually working in Greek mathematics of the classical age, and namely in a large *corpus* of texts that, as a whole forms the "καλούμενος ἀναλυόμενος": Euclid's *Data*, *Porisms* and *Surface-Loci*, Apollonius's *Conics*, *Plane Loci*, *Cutting-off of a Ratio*, *Cutting-off of an Area*, *Determinate Section*, *Contacts* and *Vergings*, Aristaeus's *Solid Loci* and finally Eratosthenes' *On Means*. The aim of the 7th book of the *Collection* is to exhibit some results or lemmas (λήμματα) which should be useful to get the main results contained in them.

Unfortunately, among the treatises that Pappus mentions as part of the domain of analysis, only Euclid's *Data* has reached us in an integral Greek version. Besides, we dispose of the Greek text of the first four books of Apollonius's *Conics*, and of Arabic versions both of the books V-VII of the same treatise (the book VIII being lost) and of Apollonius' *Cutting-off of a Ratio*. All the other treatises are lost (except for few fragments).

Euclid's *Data* is concerned with the problem of determining that which can be given (constructed) if certain geometrical objects are taken as given (in magnitude, species, or position) and, according to Pappus's terminology, all its arguments seem to be typically synthetic[23].

Even though it exposes the theory of the conics "in a synthetic mode" (Knorr, 1986, 293), Apollonius's *Conics* in contrast presents many examples of conclusive reduction to principles and we can even find in this treatise some arguments like the following, which aims to prove that if from a point D (fig. 2), external to a conic section we draw both a tangent DB and a chord DEC of this conic section, and from the point B we draw an other straight line BZ that cut DEC in a point Z in such a way that $ZC : EZ = DC : DE$, then this straight line cuts the conic section in a point A such that the straight line DA is the second tangent to it, passing from D (prop. IV, 1). In order to proof this proposition—that clearly teaches as to draw the second tangent to a conic section when a tangent has been already drawn—Apollonius assumes that the tangent DA is already drawn and the straight line BA cuts the chord DEC in a point H, different from the point Z, which satisfies the previous proportion. Then, appealing to proposition III, 37—which is just the reciprocal of the proposition that he is proving—he concludes that this is absurd. Hence, he derives that the straight line BA cuts DEC in a point Z which satisfies the previous proportion and here terminates his proof.

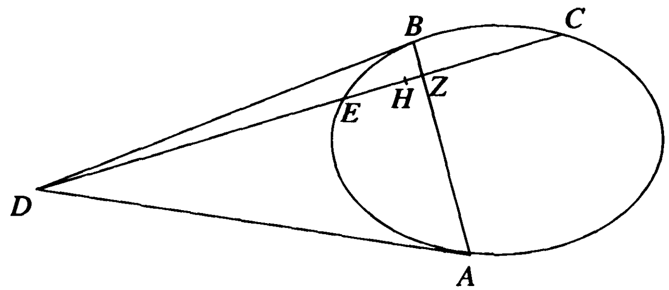


Figure 2

The logical schema of the argument is the following:

$$[\mathrm{Tg}(DB) \wedge \mathrm{Tg}(DA)] \Rightarrow (BA \text{ cuts } DEC \text{ in } Z) \qquad (1)$$

according to III, 37,

$$\mathrm{Tg}(DB) \qquad (2)$$

$$\mathrm{Tg}\,(DA) \wedge \neg(BA \text{ cuts } DEC \text{ in } Z) \qquad (3)$$

by assumption,

$$\neg(1) \qquad (4)$$

by *modus tollens*,

$$\neg(3) \qquad (5)$$

by *reductio ad absurdum*.

It is clear that (5) is not equivalent to Tg(DA) and it thus does not accomplish the proof. To prove the proposition, we still have to appeal, both to the existence and uniqueness of a second tangent and of the fourth proportional. Thus the argument (1)-(4) is not a conclusive analysis. But, according to the Aristotelian conceptions, this is no more a non-conclusive reduction to principles, except if we take it as a suggestion of starting the proof from the contemporary (hypothetical) negation of both the conjuncts of (3). In such a case we face to a non-conclusive reduction to principles, preparing a conclusive reduction to principles. This example could be taken as a symptom of a liberal use of regressive reduction as a heuristic tool in Greek geometry, but not yet as a symptom of the general application of Pappus's method to the proof of theorems.

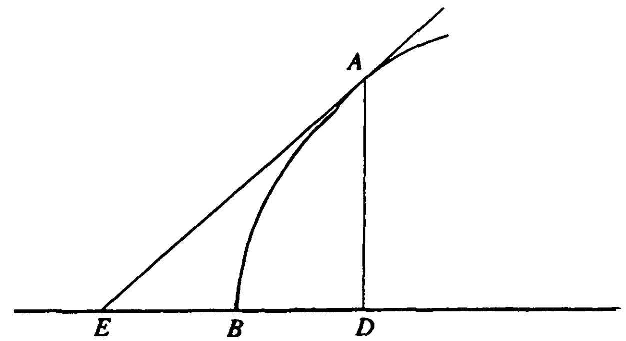


Figure 3

A number of cases of analysis and synthesis occur, in contrast, in book II (propositions II, 49-51: Apollonius GE, I, 274-305), applied to the solution of problems[24], *i. e.* to the construction of geometrical objects satisfying certain conditions. Let us consider, as a simple example, the first part of proposition II, 49, where the problem is to draw a tangent to a parabola in a certain point. Apollonius's argument runs as follows. Let AB (fig. 3) be the parabola and let the point A be given on it. Let us also assume that the tangent is traced, and let it be AE, the point E lying on the straight line prolonging the diameter of the parabola. From the point A let us draw the perpendicular AD to the diameter. As both the point A and the (diameter of the) parabola are given, the segment AD is also given in position. Beside, according to proposition I, 35 of the *Conics* themselves, EB is equal to BD. Thus as BD is given, EB is also given and, as B is given, the point E is given too. Thus the tangent is given in position. This is the first part of the argument. The second one is introduced by the phrases: "it will be synthesized in this way [Συντεϑήσεται δὴ οὕτως]" *(ibid*, 274, 21)[25], and it consists of course in the presention of the obvious construction of the tangent. Let the perpendicular *AD* to the diameter *DB* be drawn and the point *E* taken on the straight line prolonging such a diameter in a way such that $EB = BD$. The straight line *EA* passing through the given points *E* and *A* will be the tangent sought.

Even though arguments of this sort are rather exceptional in the *Conics*, they are common in Apollonius' *Cutting-off of a Ratio*, as it is presented to us in the Latin translation from Arabic by E. Halley (Apollonius SRH).

Consider as an example the first problem of such a treatise (*ibid.*, 1-3). Two parallel straight lines *AB* and *CD* (fig. 4) are given in position and three points *E*, *Z* et *T* are given as well, the first on *AB*, the second on *CD* and the third not on these straight lines, being rather inside the angle *DZH* (where *H* is any point on the straigt line *EZ* after *Z* itself). Apollonius is searching for the position of a straight line passing from *T* and cutting *AB* and *CD* respectively in two points determining together with points *E* and *Z* two segments which are between them in a given ratio. He imagines first that this straight line cuts *AB* between *E* and *B* and *CD* between *Z* and *D* and calls *K* and *L* the points where it does it. He assumes these points as given and draws the right *TLK*. Then he draws the straight line *ET* which is obviously given, as both the points *E* and *T* are given. The point *M* of intersection of this straight line with *CD* is given too. Thus also the ratio Rat.(*ET*, *MT*) is given. But (for the VI, 2 of the *Elements*) this ratio is clearly equal to the ratio Rat.(*EK*, *ML*) and then this latter ratio is given. Thus, as the ratio Rat.(*EK*, *ZL*) is given, the ratio Rat.(*ZL*, *ML*) is given for composition and therefore the ratio Rat.(*ZM*, *ML*) is also given for substraction. Now, as *ZM* is given, this means that *ML* is given and thus the point *L* and the searched straight line *TLK* are given too.

After this argument is been presented, Apollonius's treatise continues with a new paragraph which is opened by the phrase: "Componetur autem Problema hoc modo" (*ibid.*, 2), and presents an actually construction of the straight line *TLK*, starting from two segments *N* and *XO* that are between them in the same ratio than the two segments that are determined respectivelly on *AB* and *CD* by the line sought.

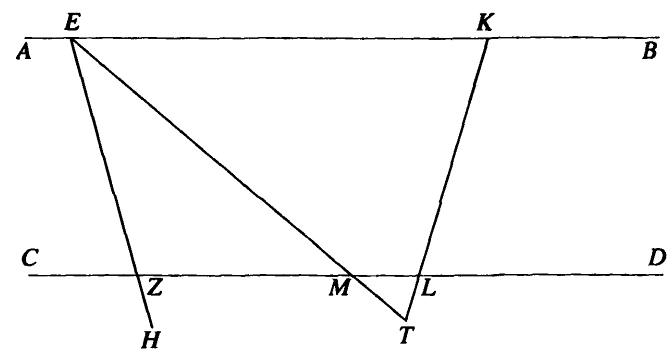


Figure 4

Even though Apollonius does not say so explicitly, the two previous constructions are then preceded by an analysis, and they are thus synthesis in Pappus's sense. Both in the first and in the second case, analysis is clearly problematical, or, if you prefer, it is just a geometrical reduction. If we consider Halley's translation from the Arabic as faithful to Apollonius's treatise, we thus have to conclude that Apollonius not only proceeded as in the Pappus's method in a short fragment of his *Conics*, but he also composed a genuine analytical treatise (in Pappus's sense). This justifies the belief that other treatises of the same Apollonius actually proceed in the same style.

Still, we can find other, similar evidences apart from Pappus' analytical corpus, in the book II of Archimedes's treatise *On the Sphere and Cylinder* (Archimedes OO, I, 168-229; cf. Knorr 1986, 170-174), for example. This is composed of nine propositions: three theorems (2, 8 and 9) and six problems (1 and 3-7). The solution of all the problems runs in two stages: the first is a classical geometrical reduction (or, in Pappus's terms, problematic analysis), while the second is a geometrical construction, explicitly presented by Archimedes himself as a synthesis[26].

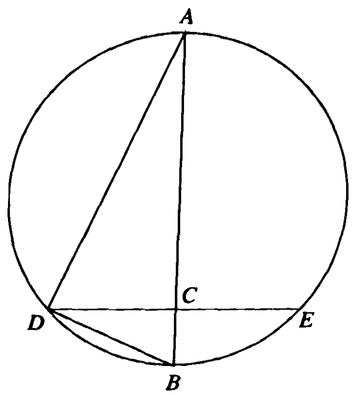


Figure 5

Let us consider as a very simple example problem 3 (*ibid*, 184-187): to cut a sphere by a plane into two segments, in such a way that the ratio between these segments is equal to a given ratio. Archimedes assumes that the plane sought cuts the great circle *ADBE* (fig. 5) of the given sphere in the points *D* and *E*, being perpendicular in *C* to the diameter *AB* of this circle, and he draws the chords *AD* and *DB*. Then he remarks that the surfaces of the segments *ADE* and *ABE* are respectively equal to the surfaces of the circles of radius *AD* and *DB* (for the propositions I, 42 and I, 43 of the same treatise), which are between them as Sq.(*AD*) and Sq.(*DB*), *i. e.*—because of the Pythagorean theorem—as *AC* and *CB*. Hence he concludes that, as the ratio between the surfaces of the segments *ADE* and *ABE* is given, the ratio between *AC* and *CB* is also given and thus the plane sought is given too. The synthesis is then obvious: it is a question of dividing the diameter *AB* by a point *C* such that the ratio between *AC* and *CB* is equal to the given ratio—which is made by a simple application of the proposition VI, 10 of the *Elements*—and of proving that this point satisfies the conditions of the original problem.

The evidence for Archimedes' application of the method of analysis and synthesis becomes even stronger, if we observe that when, in the course of the solution of problem 4, he assumes that a certain problem is solved—namely the problem of dividing a given segment so that one of its parts is to another given segment as a given surface is to the square constructed on the other part of it—, he announces that it will both be analyzed and synthesized at the and of the treatise: "ἐπὶ τέλει ἀναλυθήσεταί τε καὶ συντεθήσεται" (*ibid*, I, 192, 5-6; cf. Dijksterhuis [1956, 195]). Neither the analysis nor the synthesis are actually given by Archimedes in

his treatise, but they are reconstructed by Eutocius in his commentary, and attributed by him to Archimedes himself (Archimedes OO, III, 132-149). Besides, Eutocius also attributes in his commentary three other explicit applications of Pappus's method to mathematicians of the classical age: two to Menaechmus (*ibid.* III, 78-85) and one to Diocles (*ibid.*, III, 160-177)[27]. In all these cases Eutocius introduces the second stage of the solution by the same formula, that we can also find in Apollonius's and Archimedes' treatises: "Συντεθήσεται δὴ οὕτως" (*ibid.*, III, 136, 14; 80, 4; 82, 18; and 168, 26 respectively).

An extrinsic, but relevant argument to accept previous examples as evidence for an explicit application of the method of analysis and synthesis by Greek mathematicians of the classical era could finally come from a short passage taken from chapter 16 of Aristotle's *Sophistici Elenchi*. Here Aristotle insists on the difference between our capacity of seeing and solving the faults of an argument when we consider it and our ability in meeting it quickly in discussion. He argues, both that we often do not know at certain occasions things we know in other circumstances, and that speed and slowness in argumentation depend on training. Thus, he concludes, "sometimes it happens as with (geometrical) figures [καθάπερ ἐν τοῖς διαγράμμασιν], for there sometimes [after] having analyzed, on the other hand, we are not able to synthesize [ἀναλύσαντες ἐνίοτε συνθεῖναι πάλιν ἀδυνατοῦμεν]" (175*a*, 26-28)[28]. The verb "συντίθημι" seems to refer here to the actual construction of the figure after analysis has shown the starting point of it. Thus we could imagine that it occurs here in its common sense in Greek common language and merely indicates a composition of objects in order to produce an object. But it is also possible that Aristotle actually refers to a common procedure in geometry, namely the procedure of analysis and synthesis (Hintikka and Remes, 1974, 87).

V.4 Coming Back to Pappus

The previous examples should be sufficient evidence to support a historical hypothesis: Greek mathematicians of the classical age actually applied a two-stage method to solve problems[29], coupling the construction of mathematical objects which satisfy certain conditions, with a previous geometrical reduction, which indicated to them both a starting point and a plan for construction. This thesis is perfectly consistent with our previous interpretation of Aristotle's comparison of analysis and deliberation in chapter III, 5 of the *Nicomachean Ethics*. However, this comparison disagrees with previous examples of the use of the terms "analysis" and "synthesis". While Aristotle uses only the first, the second occurs very prominently both in Apollonius's and in Archimedes' arguments. Such a prominent occurrence of the second term might perhaps not be very significant, since this term here has a meaning that is very close to the common meaning. Even though here synthesis is not strictly a composition of given objects which form—because of this composition itself—a new object, it is nothing but a construction of a new object, which starts from given objects and follows accepted constructive clauses. The almost complete absence of the term "analysis" in Apollonius's and Archimedes' arguments might in contrast indicate a deep difference between Aristotelian conception of analysis as form of thinking and the conceptualization of a geometrical procedure, consisting in the investigation of that which is given when the objects sought are taken as given[30] and aiming at the individuation, both of a starting point and of a plan for construction. It could be the case that the term "analysis" was used in the classical age to refer to the Aristotelian notion but not, or not frequently, to this geometrical procedure.

If this were the case, the two previously mentioned passages from Aristotle's *Nicomachean Ethics* and *Sophistici Elenchi* would contain, as a philosophical judgment, the acknowledgment of the analytical nature of this geometrical procedure. From such a point of view, Pappus's general description of the method of analysis and synthesis seems to occupy a middle position between Aristotle's conceptions and mathematical practice[31]. Even though Pappus uses the term 'analysis' to refer to this geometrical procedure (that is just a geometrical reduction), he assignes to it a very specific and technical meaning. However this meaning is wide enough such that the term "analysis" also refers to Aristotelian reduction to principles. Moreover Pappus's description associates—as Aristotle did—under the same term of "theoretical analysis", both reduction *ad absurdum* (or conclusive reduction to principles) and non-conclusive reduction to principles. Thus it actually unifies three procedures. The mathematical relevance of such an unification is understandable when we observe that the third of these procedures (namely non-conclusive reduction to principles) is almost absent from the geometrical practice of the classical age. Besides, the previous example, taken from the *scholium* to book XIII of the *Elememts* makes manifest the technical gain of applying non-conclusive reduction to principles to the proof of geometrical theorems. Even though al-Nayrīzī's commentary seems to show that this is not an original idea of Pappus, the available evidence seems to confirm that it is nevertheless an acquisition of Pappus' time, at least if we assume that his *scholium* goes back to that time (like it should be the case if Heiberg and Heath are respectively right in ascribing it to Heron and in guessing that Heron lived in the third century). The passage from the idea of synthesis, as simple composition or construction, to the idea of synthesis, as an inferential procedure following an analysis, seems to be joined by such a later acquisition.

Nevertheless, the interest of Pappus's description is not exausted by that. It is also concerned with the idea of synthesis as reconstruction of the natural order. As we have just seen, Pappus uses the properly Aristotelian term "φύσις", saying that

synthesis orders the consequences and the prolegomena of the givens "κατὰ φύσιν". He not merely refers to a logically correct order here, but appeals to the reality of nature. A comparison with the very beginning of Aristotle's *Physics* is thus unavoidable. There (184*a*, 16 - 184*b*, 14) Aristotle argues (184*a*, 16-18) that the way (ὁδός) of knowledge goes from that whch is more knowable and clear to us, up to that which is more knowable and clearer by nature (τῇ φύσει) and specifies (184*a* 21-26) that what is manifest and clearer to us is what is more confused (τὰ συγκεχυμένα μᾶλλον), or the whole (ὅλον). It is only afterwards, he adds, that, starting from it, the elements and principles (τὰ στοιχεῖα αἱ καὶ ἀρχαὶ) become known, by division. Finally he concludes that (in knowledge) we have to proceed "from the general to the particular [ἐκ τῶν καθόλου ἐπὶ τὰ καθ' ἕκαστα]", since the general is a sort of whole, because it contains a plurality of things (πολλὰ) as (its) parts. Aristotle's term for "division" is not "ἀνάλυσις", but "διαίρεσις", and there is a reason for that. In fact, as long as it is the way of knowledge in the Aristotelian sense, division goes from what is given for us to what we seek, from the object that is given as such, to the conditions of its realization. This proceeding is exactly the opposite of a regressive reduction. Nevertheless, Aristotle's description has been understood during the Latin and modern ages as a typical characterization of analysis (and the term "διαίρεσις" has often been translated by "analysis" or "resolutio").

Pappus's reference to the notion of nature provides a key to understand such a shifting. It seems just to result from an inversion of the Aristotelian point of view, according to which what is given as such is not that which is given to us as such. Rather, it is that which is given as such in itself (or in the eternity of truth). Thus the problem becomes one of understanding what is given to us as such, according to the eternal truth of what is given as such in itself, that means to represent it to ourselves as a system or even a collection of parts or properties; these parts or properties being intended as first elements, which are given as such in themselves. I am not arguing that Pappus actually realizes such an inversion (that is quite natural from a Platonic point of view, and particularly with respect to mathematical matters). I am merely observing that Pappus's argument seems to suggest such a possibility or may even be suggested by it[32]. In this non-Aristotelian sense, analysis and synthesis of course come together, since the "resolution" of an object into its elements or parts asks for its reconstitution, according to the nature (or even its nature). However, such a reconstitution (that is just a synthesis in the original sense of this term) is not necessary for the realization of the aim, because the problem was that of understanding the object, not that of reconstructing it as such. As long as we realize the synthesis, it is nothing but a repetition of a process that has to have occurred already in nature. Thus, a new sort of conclusive analysis of objects arises. And, even though its notion is definitely not Aristotelian, it may be characterized in terms that refer to Aristotle's *Physics*. We might call it "reduction to elements"[33].

VI Thomas

Aristotle's exposition of the way of knowledge in the beginning of the *Physics* was one of the major references for medieval conceptions of "*resolutio*" (that is "*re-solutio*": "ἀνα-λύσις") and "*compositio*" (that is "*cum-positio*":"συν-θέσις"). According to B. Gerceau[34] (1968, 217) it is, for example, just on the background of this text that Albertus Magnus, the master of Thomas, read Chalcidius's commentary to Plato's *Timaeus*, where these notions are discussed. This means both that he understands them as referring to the process of knowledge—rather than to the order of cosmological reality—and that he considers that *resolutio* brings us from what is first in our knowledge to what is first as such. Hence, the latter is (from the point of view of the cognitive subject) an upward conduct bringing us from the complex in itself, but first for us, to the simple in itself[35], but last for us. In different terms, it is just a reduction to elements. However, what is complex in itself (and first for us), is the individual as such, while what is simple in itself (and the last for us) is that which makes the individual belong to a certain species; thus *resolutio* brings us from the individual to the species. Still, the individual is a whole, while its elements are parts of it, hence *resolutio* goes also from the whole to its parts. Finally, if the reference is not to a single act of knowledge, but to human knowledge as such, the individual is part of multiple and the species is unity, thus, *resolutio* goes from multiple to unity, as Thomas says in *De Trinitate* (qu. 6, a.1, c.). The *compositio* is then (still for the point of view of the cognitive subject) a downward conduct, bringing from the simple in itself to the complex in itself, from the universal as a principle, to the individual, from the parts to the whole, from the one to the multiple.

Even though this conception inverts the extensional order of Aristotelian analysis, it does not invert its logic (or one of the intensional orders that characterizes Aristotelian analysis): analysis always proceeds regressively from the last to the first, from the not given to the given, from the problem to its solution, or to the conditions of the solution. Moreover, it is a conduct of thinking, a way of knowledge.

This is however not the only sense ascribed to the pair "*resolutio-compositio*" in the 13th-century philosophy. An further sense comes up with Peter of Spain, from the eclectic views exposed by Boethius in his *Commentary* on Porphyry's *Isagoge*, where Platonic and Aristotelian conceptions are applied together to provide a complex representation of logic (Garceau 1968, 210-213). According to Boethius, there are two different but complementary ways of distinguishing the different parts of logic: either these parts are *definitio*, *partitio* and *collectio*, or

they are *inventio* and *judicium*. While the second distinction comes from Aristotle, passing through Cicero's *Topics*, the first refers to *Phaedrus's* distinction between διαίρεσις and συναγωγή. The complementarity of these distinctions appears when Boethius argues that *inventio* provides material for *definitio*, *partitio* and *collectio*—which includes, in turn, *demonstratio*, *dialectica* and *sophistica*, dealing respectively with necessary, probable or false arguments—while *judicium* determines whether we are well defined and divided, whether our arguments are necessary, probable or false and whether they are linked by inferential relations, or not. In this way, Plato's distinction between διαίρεσις and συναγωγή is grafted onto an Aristotelian schema. It is hence not surprising that Peter of Spain, more than seven centuries later, in his commentary on *De anima* (*Quæst. Præemb.*) interpreted the ideas of *resolutio* and *compositio* as referring to Plato's dialectic—by effacing the essential distinction between συναγωγή and σύνθεσις. *Resolutio* becomes, in this frame, a downward path bringing us from the genus to the species, from the one to the multiple, while *compositio* becomes an upward path bringing us from the species to the genus, from the multiple to the one.

Even though, in this way, Peter of Spain agrees with Aristotle on the regressive nature of analysis, he seems to change the point of view from which analysis is considered. Analysis is not regressive because it brings us from the last to the first, from the not given to the given. It is regressive because it goes from the higher to the lower. It is not a way of knowledge, but a sense in the disposition of being.

In *Quæstio* 14 of *Summa, prima secundæ* (a. 5) Thomas treats the following question: "does deliberation [*consilum*] proceed by *resolutorio* order?" In the first objection, he argues that this cannot always be the case, since deliberation "is concerned with that which is done by us [*est de his quæ a nobis aguntur*]" and our operations (*operationes*) proceed more *modo compositivo*, than *modo resolutorio*; that is, according to Albertus's views: they go *de simplicibus ad composita*. Still, in the second objection, he adds that deliberation is an *inquisitio rationis*, and, according to the most convenient order, reason "begins with that which is prior and goes to that which is posterior [*a prioribus incipit, et ad posteriora devenit*]", such that deliberation has to go from the present (that is prior), to the future (that is posterior), and not *viceversa*. As Thomas refers just to chapter III.5 of the *Nicomachean Ethics*, his answer is obviously positive: deliberation does proceed according to the *resolutorio* order. The argument implicitly refers to the beginning of the *Physics*. We can consider prior and posterior—he argues—either with respect the order of knowledge (*cognitione*) or to the order of being (*esse*). If what is anterior in the first order were also anterior in the second, deliberation would be *compositiva*. But it is not always so, and it is particularly not so in the case of deliberation, where the end (*finis*) is prior in intention (*intentio*), but posterior in being. Thus, deliberation is *resolutiva*. The solutions of the previous objections are not essentially different: deliberation deals with operations and "order of reasoning about operations is contrary to the order of operating [*ordo ratiocinandi de operationibus, est contrarius ordini operandi*]"; reason starts from what is prior for reason (*secundum rationis*), but not always from what is prior in time.

Six orders are mentioned in this argument: the order of knowledge, the order of reason, the order of being, the order of time, the order of intention and the order of (human) operations (or acts). Deliberation—says Thomas—proceeds analytically, since it goes from what is the last in the order of acts to that which is the first in the same order. As deliberation is an *inquisitio* of reason, it has to go from what is first for reason to that which is the last for reason. But when reason applies to action, what is first for reason, is the last in the order of acts. This is just our end. Certainly, it is also the last in the order of time, while it is the first in the order of intention. Moreover, it seems to be, according to Thomas's argument, the first in the order of knowledge, and, if it is so, it is then the last in the order of being too. Therefore for Thomas, deliberation is an example of analysis, since it brings us from the last in the order of being (acts and time) to the first in the order of knowledge (intention) and reason, whereas, for Aristotle, it was an example of analysis, since it brought us from what is given to us as the object of a certain concept (that is just the end), to what is given to us as such (the act we can perform here and now). Thus if Thomas' conclusion is the same as Aristotle's, it is because of the fact that in deliberation knowledge is nothing but a means for action, and it is not intended as such (*ibid.*, I-II, qu. 14, a. 3). Such a remark enables Thomas to accept Aristotle's thesis of chapter III.5 of *Nicomachean Ethics*, by appealing to an argument that is similar to the one Aristotle advances at the beginning of the *Physics*. However such a double agreement stands on many differences. Nevertheless, on two essential points Aristotle and Thomas agree: for both of them, analysis is a regressive conduct of thinking (or reason, according to Thomas' terminology); this conduct can be applied in order to reduce either concepts to the conditions of their satisfaction or aims to the conditions of their realization.

The same tension between the point of view of knowledge and the point of view of being appears when we consider Thomas's conception of relations between the pairs *resolutio-compositio* and *inventio-judicium* (Garceau, 1968, 218-220). As a matter of fact, Thomas sometimes identifies *resolutio* with *judicium* and *compositio* with *inventio*, and at other occasions identifies *resolutio* with *inventio* and *compositio* with *judicium*.

He states the first double identification, when he speaks from the point of view of knowledge and considers *inventio* as a research for conclusions, starting from principles, and *judicium* as an evaluation of conclusions in the light of principles[36]. This seems to be the case of the *Proemio* of the commentary to the *Posterior Analytics* I have quoted before. Now, Thomas is here properly concerned with the conduct of reason that brings us to the act of judging, rather than with this act as such. According to Garceau (*ibid.*) this is also the case of the other occurrences of the first double identification in Thomas's writings. If this is correct, Thomas asserts that the act of judging is prepared by an analysis. *Quaestiones* 13 and 14 of *Summa, prima secundæ* are even more explicit. In the latter (a. 1), Thomas argues that in doubtful and uncertain matters, reason does not pronounce a judgment (*profert iudicium*) without previous *inquistione* "concerned with [his] choice [*de eligendis*]", which is just said "deliberation". Thus, he says that the act of pronouncing a judgment is a sort of choice (*electio*), that is formally an act of reason, but being substantially an act of will, instead (qu. 13, a. 1). If we accept that synthesis is just what comes after analysis and is made possible by it, we can conclude that the act of judging is a synthesis, it is made possible by an analysis and can even express an act of will, as choice is. In this sense, synthesis is no more, strictly speaking, a conduct of thinking or a way bringing us from a certain stage to a different one. It is a singular act of reason which closes an analysis and eventually expresses a will. Whether a judgment, in turn, then is either analytic or synthetic, cannot depend on the nature of this act, but on the characters of the analysis which leads to it. This exactly seems to be the idea of Kant (here, ch. 12).

Thomas states the second double identification instead, when he speaks from the point of view of the intrinsic nature of being, which the results of *inventio* and *judicium* express or identify. From such a point of view, *inventio* assumes the character of an analysis, since it reduces what is the first for us, but the last and the most complex in itself, to the intrinsic simplicity of its principles. *Judicium*, in contrast, is a synthesis, since by it the intrinsic complexity of reality is understood, starting from the intrinsic simplicity of principles. In this case, the term "*judicium*" clearly refers to the act of judgment as such.

Thus, from the point of view of judgment, the two previous double identifications do not contradict one another: in both the cases, the act of judgment seems to be intended as an act of synthesis, preceded and prepared by an analytic conduct.

VII Viète and Descartes

Pappus' characterization of the mathematical method of analysis and synthesis and medieval doctrines of *resolutio et compositio*, in their relations with Thomas's theory of judgment, seem to be the two gateways through which the Aristotelian idea of analysis enters the modern age. By passing through both these gateways it comes to be associated to a non-Aristotelian idea of synthesis, which generalizes Plato's and even a pre-Platonic conception of synthesis as mere composition of objects (by integrating Plato's conception of συναγωγή, for example), but also restricts its range, just because of this association. Still, by passing through the first of these gateways, it is both clarified—or even codified—and restricted to the specific domain of geometry; namely it is identified with nothing but geometrical reduction and reduction to principles. Finally, by passing through the second gateway, it loses its Aristotelian purity, both being confounded with διαίρεσις (and integrating, in such a way, Plato's dialectic) and being projected on a plurality of distinct orders, often opposite each other (here, ch. 13, par. II).

In coming out from the first gateway, Aristotelian idea of analysis is met by Viète, who goes on to formulate a very ambitious program: to apply to geometry both methods and results of Diophantus' arithmetic (ch. 3, par II.1). This program is clearly expounded in the *Isagoge* (1591) and partially realized in a number of works published later.

Application to geometry of arithmetical technics was impeded in pre-modern mathematics for different reasons, the most important of which was probably the absence of a general definition of internal multiplication between geometrical magnitudes. Though for Greek mathematicians (integral positive) numbers could be multiplied with each other and into any sort of magnitude, the same was not true with respect to magnitudes in general. Construction of squares, rectangles, cubes or parallelepipeds was of course intended as particular analogues to the multiplication of numbers, when two or three segments were involved. This was not, however, a general definition of multiplication for geometrical quantities. Moreover, such a geometrical "*proto*-multiplication" was not conservative with respect to homogeneity, by producing a result that could be added neither to its factors, nor to any other magnitude of the same kind. Viète's basic idea, to pass beyond such a difficulty was to provide a *quasi*-axiomatic definition of multiplication as a general operation on quantities (both numbers and magnitudes). In this way, he enabled himself to pass from proportions between geometrical magnitudes like $a : b = A : B$, to equations like $aB = bA$, and to express different sorts of problems involving geometrical magnitudes in terms of equations. In order to accomplish that, Viète proposed to use a genuinely analytic procedure.

In the very beginning of the *Isagoge* he defines analysis as a "certain way to search for the truth in mathematics [*veritatis inquendæ via quædam in Mathematicis*]" (Viète 1591*a*, 4r). He mentions the opinion according to which Plato was the first to come upon (*invenire*) it[37]; he ascribes to Theon (who lived in Alexandria in the 4th-century A. D.) the merit of being the first to call this way "analysis", and asserts that he is just quoting Theon's definition. It is possible that Viète refers here to the *scholium* of book XIII of Euclid's *Elements* in the form it takes in the Theonine version[38]. Analysis, he says, is "the assumption of what is questioned as if it were admitted, [in order to arrive], by means of [its] consequences, to what is admitted to be true [*adsumptio quæsiti tanquam concessi per consequentia ad verum concessum*]"; in contrast (*ut contrà*), synthesis is "the assumption of what is admitted, [in order to arrive], by means of [its] consequences, to the end and comprehension of that which is questioned [*adsumptio concessi per consequentia ad quæsiti finem & comprehensionem*]" (*ibid.*). The use of the terms "*finem*" and "*comprehensionem*" is perfectly consistent with the Aristotelian conception of analysis, as I have presented it, and serves Viète's program too. In fact, though he mentions the two kinds of analysis distinguished by Pappus (by calling them "ζητητική" and "ποριστική")—saying that the previous definition is perfectly pertinent for them—and even asserts to have added a third kind to them, which he calls "ῥητιξή" (from "ῥέω": to flow; but also: to explain) or "ἰεξηγητική" (from "ἐξηγέομαι": to conduct up to the end, to explain, or to expose), he profoundly changes the intended sense of Pappus's distinction. Far from being three distinct species of the same genus, Viète's zetetics, poristics and exegetics (or rhetics) are three successive stages of the same conduct. According to Viète's general definition, in the first stage "an equation or a proportion is obtained between the magnitude which are sought and that which is given [*invenitur æqualitas proportiove magnitudinis, de quâ quæritur, cum iis quæ data sunt*]"; in the second one "the truth of the theorem concerning with the equation or proportion [*de æqualitate vel proportione ordinati Theorematis veritas examinatur*]"; and finally in the third one "the magnitude is exhibited [starting] from the equation or proportion about what is questioned [*ex ordinata æqualitate vel proportione ipsa de qua quæritur exhibetur magnitudo*]" (*ibid*). However, zetetics more properly consists in transforming the given problem in an equation, eventually by passing from one or more proportions, and in solving it; clearly it is an analytic procedure. Poristics consists in verification of the conclusions of zetetics; it can be as such—as we shall see later—either an analytic or a synthetic procedure. Finally, exegetics consists in the exhibition of the searched magnitude; it is certainly a synthetic proceduire .

In order to understand the relations among the three stages of Viète's methods, we have to investigate the nature of zetetics. As long as it is expounded in general terms, Viète's idea is quite simple. If a problem is advanced according to which certain magnitudes are sought, he proposes to assume that these magnitudes are given and to indicate them with certain letters (Viète actually uses capital vowels (here, ch. 6, notr 13), but we can use the last letters of the Latin alphabet, as we normally do). Then he proposes to work on these magnitudes as if they were actually given, in order to translate, according to the new definition of geometrical multiplication, the conditions of the problem in a certain equation that could be solved according to the usual arithmetical technics, or transformed into a new proportion. Imagine that this problem asks for the construction of two segments which should form a rectangle equal to a certain square B and should have the same ratio as two other segments S and R, which is a particular case of the zetitic II,1 (Viète 1591*b*, lib. II, z. 1). If these segments are called x and y, we have the proportion $x : y = S : R$ and thus, according to Viète (but in modern notation), $x = \frac{Sy}{R}$ and $y = \frac{Rx}{S}$ and then $B = \frac{Sy^2}{R} = \frac{Rx^2}{S}$ or $Sy^2 = RB$ and $Rx^2 = SB$. Even if these equations were solved, as if they were usual arithmetic equations, the exhibition of their roots would not yet be the construction of the segments sought. Therefore, it is not the exhibition of the solution of the problem. The situation does not change if we transform these equations into two corresponding proportions, as Viète actually does. We have then, respectively, the proportions $S : R = B : y^2$ and $R : S = B : x^2$ which do not exhibit as such the segments sought. When we face the roots of the previous equations or the proportions that correspond to the latter, in both cases two problems remain still open: first to verify, starting from the magnitudes that are actually given, whether the relations expressed by these roots or proportions are correct, and second to interpret either of these roots or these proportions as suitable suggestions to actually realize the construction we were seeking. Poristics should solve the first problem, exegetics should solve the second one.

We have just asserted that the first stage of Viète method is an example of analysis. The reason is clear: it is a conduct of thinking, responding to a certain aim, which starts from the hypothesis that a certain object, which is only presented as the object of a certain concept, is given a such, and runs by assuming that we can actually operate on and with such an object. This is also the case of Aristotelian geometrical construction. However zetetics does not bring us from this hypothesis to the exhibition of an object that is given as such, rather it terminates as soon as the object which is sought is presented as the object of a new concept: the concept of root of a certain equation, or, to be more precise, the concept of being the (geometrical) magnitude that is expressed by a root of a certain equation. Thus it is not strictly a regressive conduct, since it does not regress from that which is not given as such to that which is just given as such. Rather, it exploits the admission occurring in its first stage to exhibit a certain operational relation, that was unknown before, between the object sought and the objects given as such. Therefore, as long as it is not a regressive conduct, Viète's zetetics is a way to come upon a certain relational configuration that was unknown before. Even though it is not conclusive with respect to the aim occurring in its first stage, it is conclusive with respect to a different aim, which is just that of exhibiting such a configuration. It is then an example of a new, non-Aristotelian sort of analysis, that we might call a "configurational analysis".

Because of the particular nature of this analysis—and inspite of Viète's declaration in the chapter VI of *Isagoge* (Viète 1591*a*, 8r), where it is described as a sort of synthesis—the stage which follows zetetics can be either an analytic or a synthetic procedure. The reason is clear: its specific aim is proving a theorem—that is just the conclusion of zetetic—and it can do this either by a conclusive reduction to principles or by a synthetic proof that can eventually be preceded by a non-conclusive reduction to principles. However, both in the first and in the second case, poristics does not realize the aim occurring in the first stage of zetetics. Rather, it is conclusive only with respect to an intermediary aim, which is just the specific aim the conclusions of zetetics leave to it. The task of realizing the principal aim is thus left to the third stage, that is exegetics.

Even though, exegetics thus is a geometrical construction and is then, because of its logical form, a quite normal synthesis, its connection with the previous analysis is not the same that in Pappusian and Medieval examples. In fact it does not start from the object that analysis has indicated. As long as it starts from the final stage of analysis (that is the final stage of zetetics), it has to interpret the final stage of analysis; namely, it has to transform the expression of a certain relational configuration into a suggestion for a geometrical construction. Thus, at its very beginning, it has to proceed as analysis does, starting from the presentation of a certain concept and seeking for the first elements of construction. This is specifically difficult, because of the non-geometrical character of the configuration exhibited by zetetics. In fact, in Viète's method, zetetics realizes its specific aim and exhibit such a configuration, thanks to a *quasi*-axiomatic definition of internal multiplication. But, even though such a definition enables mathematicians to write equations where products (and ratios) of magnitudes occur and to manipulate them, it does not specify what a product (or a ratio) of magnitudes is. This is the source of one of the main difficulties of Viète's program, since, in order to assign a geometrical sense to his equations and their roots, Viète proposes to interpret them according to a generalization of the classical definition of product of segments as constructions of rectangles or parallelepipeds. The problem with this suggestion is twofold. First, such a definition does not work for any sort of geometrical magnitude. Second it forces us to distinguish magnitudes according to their order with respect to a certain base, since, according to the previous definition, internal multiplication between segments is not conservative with respect to homogeneity.

This difficulty is one of the starting points of Descartes' program in geometry. Many scholars have underlined that Cartesian geometry is nothing but a collection of methods to solve geometrical problems. I do not believe this is the case. Rather, I think that the aim motivating Descartes' *Geometry* was a new foundation of geometry as a whole. And such a foundation makes an essential appeal to the analytic way of thinking. This is the last stage in the history of the notion of analysis I shall consider here, since it is the first stage of a new era, where the original Aristotelian notion gets its modern character.

As is well known, Descartes, in his *Discours de la méthode*, contrasts the "*Analyse des anciens*" and the "*Algèbre des modernes*" (1637, 19). He refers to them as two "arts" and considers them together with a third "art", that is logic. His famous four precepts (*ibid.*, 20) are expounded by him as the only "laws" of a method that "comprenant les avantages de ces trois [arts], fust exempte de leurs defaux" (*ibid.*, 19). The first and the third precept seem to recommend a quite non-analytic conduct of thinking: never accept as true anything of which we do not have evidence; always start with the simplest and the most easily knowable objects in thinking and proceed step by step upwards to the knowledge of the most composed ones. Though such an apparent refusal of analysis seems to be balanced by the second precept, this precept does not really recommend an analytic conduct, limiting itself to suggest to always divide any difficulty in as many "particles" as possible. Such an attitude seems to be inconsistent with the equally famous precept of the *Géométrie*, which in contrast recommends a very analytic conduct:

> "Ansi, voulant resoudre quelque problesme, on doit d'abord le considerer comme desia fait, & donner des nommes a toutes les lignes qui semblent necessarires pour les construire, aussi bien à celles qui sont inconnuës qu'aux autres" (*ibid.*, 300; cf. here, ch. 1, 25-26 and ch. 8, 208).

The contrast appears to be even more evident when we observe that, just after having set forth his four precepts, Descartes presents a very short abstract of his geometry, as an example of his method.

How can these precepts for a good conduct of thinking be rendered consistent? The answer depends on Descartes' conception of his method, as a combination of the advantages of (Aristotelian) logic, classical geometry (which, by referring to Pappus' interpretation of it, he calls "analysis of the ancients") and of what he calls "the algebra of moderns". From the first of these "arts"—which he here understands as the art of conducting logical proofs—Descartes takes the progressivity of thinking and the certainty of the starting points. From the second, he takes both the modalities of giveness of objects and the conditions of their possible comparison. Finally, from the third, he takes the modalities of expressing both objects and operations and the agility of deduction that these modalities permit; in fact, when he speaks of "algebra", he seems to refer to the modern (for him) technics of transforming and solving equations. The key to understanding Descartes' point of view seems just to lie in the previous distinction between modalities of giveness and comparison and modalities of expression. This distinction is already visible in Viète's program, the aim of which is just to find a way to work with the "algebraic technics" on certain expressions of geometrical objects, in order to obtain suitable suggestions to perform classical constructions. However, in Descartes's new geometry it seems to become much more explicit.

In following Israel's suggestion (here, ch. 1), we might come back to the *Regulæ* in order to understand Descartes' views. In the *Regula* XIV (Descartes AT, X, 450-52), Descartes states that there are only two sorts of things which compare themselves to each other (the Latin verb is "*confero*": literally "to bring together", and it is used here in the passive form): multitudes and magnitudes. And he adds that we dispose of two sorts (*genara*) of figures "to conceive them [*ad illas conceptui nostro proponendas*]". The first type of these figures are diagrams (as systems of points or genealogical trees), the second are geometrical figures. By using Descartes' terms, they respectively "exhibit [or are to exhibit: *exhibenda*]" multitudes and "explicate [*explicant*]" magnitudes. Among all the possible classes of figures of these sorts, Descartes wants to choose only one and use its elements as general representations of multitudes and magnitudes. In order to justify his choice, he remarks that all the conditions (*habitudines*) which can subsist (*esse*) between entities of the same genus (that is all the relations between such entities) refer (*esse referenda*) either to order or to measure. Then he states that measure essentially differs from order because of the necessity of the consideration of a third term, when two entities are compared according to it (which is not the case of order). Finally, he argues that "as far as a unity is assumed [*beneficio unitatis assumptiæ*]" magnitudes can be reduced to multitudes, and the multitudes of unities can be disposed in such an order, that every difficulty "concerning the knowledge of measure [*quæ ad mensuræ cognitionem pertinebat*]" only depends on order. Starting from these premises, Descartes concludes that, as long as it is question of proportions between magnitudes, only segments can be considered and that the same figures can be used to exhibit both, multitudes and magnitudes.

Descartes' argument may appear rather obscure, but it becomes very clear as soon as it is considered in connection with his geometry. What Descartes says, is that if a certain magnitude is assumed as a parameter to measure all the other magnitudes of the same genus (a unity of measure), then the essential difference between comparison by order and comparison by measure—that is just the necessity of a third term—fails, since the third term is given already once for all. Thus, it is possible to intend any proportion as a relation with respect to the order and pass from it to a usual identity. Namely, as he will teach in the very beginning of *Géométrie* (Descartes 1637, 297-298) and as he anticipates in the *Reguala* XVIII (Descartes AT, X, 463), a proportion like $u : a = b : c$ (where u is just the unit) means that c is the product of a and b. Such a definition is completely independent of the nature of the measured quantities: they can be multitudes or magnitudes, and, if they are magnitudes, they can be any sort of magnitudes. Therefore, to give a sense to the product of two magnitudes a and b, it is merely necessary that the unity is chosen as homogenous either to a or to b. But, if this is the case, the comparison of distinct quantities can be expressed by a consistent formalism, which does not depend on the particular nature of these quantities, and thus, as long as we are comparing them, all quantities can be intended as being segments.

Regula XIV stops here. This is not however the end of the story, since these considerations do imply neither that each quantity can be compared to every other (since Descartes' argument refers to the modalities of comparison, but not to the possibility of it), nor that the product of two quantities can be exhibited, if these quantities are given, together with a unit homogeneous to one of them. According to the previous definition, this is only possible, if the fourth proportional between these quantities and the unit can be exhibited. If both these quantities are segments, theorem VI, 12 of Euclid's *Elements* teaches us that this is always possible. But if this is not the case, no *a priori* guarantee can be given for that. Thus, Descartes' definition of internal product for any sort of magnitudes (that can be easily applied to multitudes too) does not go together with the possibility of exhibiting this product under any circumstances. If we want this possibility to exist always, we have not only to treat or represent all quantities as segments—as long as we are measuring them—, we have also to assume that they are segments. The same argument may be applied to internal division, integral power and any sort of root (the only difference being, for the last case, that the possibility of exhibition of every root of a given segment does not depend on any theorem of Euclid's geometry, but on Descartes' enlargement of Euclid's constructive clauses).

If we want to do geometry in general, we of course, cannot restrict ourselves to the consideration of segments. However, we may assume that only segments are given as such and try to construct any geometrical entity (that is a magnitude or a form), step by step, starting by segments. This is the progressive way of (Aristotelian) logic. Nevertheless, if we want to reach non-rectilinear figures by this construction, we cannot limit ourselves to Euclid's constructive clauses. According to Descartes, there is no question of adding further postulates to Euclid's. It is even preferable to eliminate these postulates as such. We have only to be confident of our capacity to distinguish and trace segments and to perform elementary operations with them (like to construct a circle by rotating a segment) or with ideal machines composed by segments or other objects, which have already been constructed (like in the case of the construction of the ellipse by means of the gardener's method). Hence, the construction of geometrical objects, starting from segments, is not submitted to any general rule, but has simply to satisfy a condition of exactness, which Descartes actually formulates in his *Géométrie* in different and not always consistent ways. This general precept both expresses the condition of certainty of the starting points—which Descartes inherited from (Aristotelian) logic—and the modalities of giveness of geometrical objects. I have just said that Descartes inherited these modalities from classical geometry (read through the glasses of Pappus' interpretation). In fact, these modalities are formally the same which work in classical geometry: only objects that are explicitly constructed starting from elementary objects are given as such. However, the substance of this condition has changed, since such a condition is no longer expressed in terms of deductive constraints (like in the Euclidean deductive system), but it is merely satisfied by the application of a constructive capacity which looks after its own exactness. Thus the progressive order of Descartes's method is not the order of Aristotelian proof, it is rather an order of construction, or, in the original Latin sense of the term, an order of *inventio* (that is literally the act of coming in or upon)[39].

As long as geometrical objects are given as such, the modalities according to which we can operate on them and compare them, are the same as in classical geometry: two segments are added, for example, by juxtaposition (the term is explicit) and compared by referring to the conditions of their mutual inclusion. This is the second aspect of classical geometry inherited by Descartes. Nevertheless the objects, which are given as such are not the only ones we are able to consider. We can also consider objects, which are simply characterized by the conditions they have to satisfy. These objects are not given as such, but, as long it is question of their comparison with other objects (which are given, instead), we can express them by means of suitable terms and apply to them the usual rules of proportion. Moreover, if a unit is given, proportions can be expressed by equations (or, if you prefer, translated into them). Such a possibility enable us to determine the relational configuration of any domain of known or unknown quantities and to characterize them as the objects which satisfy (or, better, would satisfy) certain conditions. This is the modality for representing both quantities as well as operations on quantities. It is the consequent agility of formalism that Descartes inherits from the "algebra of moderns". This is also the analytic procedure on which Descartes' geometry is founded. However, this is not a regressive conduct, being rather, as in the case of Viète, a configurational analysis (here, ch. 8).

However, two novelties make Descartes' analysis essentially different from Viète's. First, the introduction of a unit (that is, in modern terms, the neutral element of a multiplicative group) eliminates any necessity of distinguishing quantities with respect to their (multilicative) order, as long as it is only question of expressing their mutual relations; and, if these quantities are supposed being segments, it enable us to perform a finite and regulated construction, which exhibits the object (obviously a segment) expressed by every finite algebraic composition of given quantities. This means that if analysis terminates in the exhibition of an identity like $x = f(a, b, ..., q)$—where $f(a, b, ..., q)$ is a finite algebraic composition of the given quantities $a, b, ..., q$—then the successive construction is certainly possible and is completely determined by analysis itself. Second, the introduction of the idea of coordinates, makes it possible to express geometrical loci by means of equations, independently from our capacity of solving the latter. Here to express is not the same than to give; but it is no more the same than to denominate. In fact, thanks to the expression of these loci by means of equations, we can establish a number of geometrical properties of them and even classify them. Moreover, if these equations are solvable, we can even actually construct any finite number of points belonging to these loci. Once again, this is not giveness of these objects as such, but it is a very strong and geometrically informative characterization of them as objects satisfying certain concepts.

These differences between Viète's and Descartes' analysis are responsible for the results of a new "art", namely modern analysis as a mathematical theory, the new theory of functions. I do not think this to be the effect of a simple oblivion of geometrical construction or even of the transformation of the previous conditions of characterization into conditions of giveness. Rather, it seems that it is the effect of Descartes' introduction of a new sort of constructive objects, which are not particular quantities, but are the relational expressions of quantities or—as they will become in the 18th-century—abstract quantities or functions (here, ch. 3 and 5 and Panza 1992). From here stem a number of new and more modern meanings of the terms "analysis" and "synthesis". The different chapters of the present book should make the greater part of these meanings clear and elucidate their mutual relations. My aim here was only to suggest the intrinsic dependence of these meanings on a single source: the Aristotelian notion of analysis as a regressive conduct of thinking performed in order to make the realization of a given aim possible.

Centre F. Viète of the History and Philosophy of Sciences,
University of Nantes

Notes

* I thank Clotilde Calabi, Jean Dhombres, Agnese Grieco, Michael Hoffmann, Francois Loget, Michael Otte, Jackie Pigeaud, Bernard Vitrac for their suggestions and linguistic and philosophical advices.

1 We owe a number of our examples of occurrences of the term "analysis", both in the Greek *corpus* and somewhere, to Timmermans (1995).

2 True to say, Aristotle's definition is not so clear. The passage I have mentioned belongs to a larger argument, where Aristotle states four different meanings for the expression "(to be) in itself". According to the third of these meanings (*Posterior Analytics*, 73*b* 5-10) Q is in itself if it is not said to be of a certain subject, let us say P, while, according to the fourth (*ibid.*, 73*b* 10-16) P is Q in itself, if it is Q because of it is just P (and for no other external reason). The first two meanings are those we have just exposed in the text. However, Aristotle seems to insist on the circumstance that predications "P is Q" and "Q is P" occur respectively in the definition of P and Q. Because of that, Barnes (1975, 114 and 112) argues both that the third and fourth meanings are ontological, while the first and second are logical, and that all of them are meanings of "Q holds of P in itself". Moreover, he maintains that the arguments of chapters 19-22 which will be discussed below only refer to the two first meanings. However, it seems to me that the third of these meanings is quite different from the other and specifically concerns the fact that the predicate 'Q' indicates a certain subject, while the fourth integrates the first two by making clear that they refer to essence, rather than merely to definition (or even to essential definition, rather than to purely linguistic definition).

3 Even though I come far from that in certain points, my reconstruction is largely indebted to Barnes' translation and commentaries as they appear in Barnes (1975).

4 Remark that, as such, this neither entails the main thesis of chapters I, 19 - I, 22, nor it is entailed by it, since it is possible that P is not able to be defined and known, and all the series of predications as the previous ones are finite, these series being infinitely many.

5 According to Barnes (1975, p. 180) the argument for the downward series of P_j is not correct. This is quite right if we consider, as Barnes does, this series as a series of predications where the predicate "inheres in the definition" (*ibid*, p. 112) of the subject and we directly refer Aristotle's argument to the possibility of a (finite) proof and definition. If it is so, the fact that for every (natural) number j there is a predicate P_j such that "P_j is P" is an essential predication (in the previous sense) only means that P inheres in the definition of infinitely many subjects. In order to make Aristotle's argument correct, we have to assume that P_j is a subject and namely a species of P—(essentially) defined by the genus to which it belongs—and that no subject can contain an infinite number of species (what makes clear the role of the second premise advanced by Aristotle at the beginning of chapter I, 22). In any case, if we do not refer, as Barnes does, the Aristotle's argument directly to the possibility of a (finite) proof and definition, it is the argument for the upward series of Q_j which fails, except if we accept that the predicate of an essential definition is a genus of the subject and no subject can belong to infinitely many embedded genus (cf. the previous endnotes 2 and 4).

6 The interpretation of Waitz (Aristotle AOG, II, 353-354) , according to which an "analytical" proof is rigorous, while a "logical" one is not, seem to be unacceptable.

7 This is one of the roots of the wrong idea of many amateur philosophers, who think that synthesis is nothing but invention (or even "intuition" as a creative act).

8 Cf. Proclus (PEEL, ed Friedlein , 17-19), which ascribes this sort of analysis to Eratosthenes.

9 Cf. the previous endnote (5).

10 Literally: "pre-delibered", since the verb "βουλεύω" means "to deliberate", as an act of a council, the "βουλή" being just the administrative council of a political community.

11 Cf. the previous note (10).

12 Aristotle's identification of eternity and necessity (his non-modal conception of necessity) has been discussed by a number of scholars. Cf. for example Hintikka (1975).

13 Of course regressive reduction is part of what we do when we "work backwards". Thus the Aristotelian notion of analysis is completely compatible with the general meaning that Szabó (1974) has ascribed to the term "ἀνάλυσις" as referring to a "working backwards". It appears to me, however, that the Aristotelian notion of analysis is more profound. It is not at all restricted to the level of methodology, but is related to foundamental questions of epistemology and metaphyics. We can even regard it as the source of modern epistemological conceptions which are not merely concerned with the examples that Szabó discusses, that are, Pólya's heuristic and Lakatos' "proof-analysis" or "method of proof(s) and refutations" (cf. Pólya 1945, particularly 141, and Lakatos 1976 and PP, II, ch. 5: "The Method of Analysis-Synthesis", 70-103).

14 "Treasury of Analysis" is Heath's and Hintikka-Remes's translation (Euclid EH, I, 138; and Hintikka and Remes 1974, 8). Jones and Ver Eecke translate the same Greek expression respectively with "Domain of analysis" (Pappus CJ, 82; cf. here, ch. 8, par. II) and "champ de l'analyse" (Pappus CVE, 477).

15 Hultsch was here following Halley's translation in the preface to Apollonius *Cutting-off of a Ratio* (SRH, XXVIII), which translated "κατὰ σύλληψιν" with "ut paucis dicam".

[16] For the references, cf. note (14).

[17] On the correspondence between Pappus's definition and Aristotle's argument of the chapters III, 3-5 of *Nicomachean Ethics* cf. Hintikka and Remes (1974, 86-87) and Knorr (1986, 356-357) that even guesses that Pappus "may present not a distillation of [..] ancient tradition , but rather a rephrasing of standard philosophical views" (*ibid.*, 357).

[18] On all the question cf. Hintikka and Remes (1974, 11-19).

[19] For a tentative literally translation of the Arabic text cf. Knorr (1986, 376).

[20] Other examples of analysis in Heron's works are listed by Hintikka and Remes (1974, 19-20, n. 2) and Knorr (1986, 376-377, n. 87). You can also consider the paragraph 136,7 of the [pseudo-] Heron's *Definitiones*, which (as it mention Porphyry) can not be antecedent to the 3rd century A.D.

[21] It is clear that, even though it is possible to intend all the identities (1)-(10) as logical equivalencies, the inferential chain (1)-(10) is not convertible as such, because of the essential occurrence of (1) in the passage from (4) to (5).

[22] The particular aim of analysis, here, seems just to provide such a suggestion. Thus it does not seem to us be "completely artificial" as Knorr says (1986, 358).

[23] A reason justifying Pappus' inclusion of Euclid's *Data* in the corpus of analysis is advanced in the note (30) above.

[24] On the classical distinction between theorems and problems in Greek mathematics cf., for example, Caveing (1990, 133-37).

[25] The same formula appears, sometimes without the particle "δή" also in: 276, 3 and 18; 278, 13 and 24; 280, 15; 282, 8; 284, 8; 286, 5; 298, 20; and 300, 22; while in: 288, 15; 290, 24; and 297, 7 we found the more explicit formula "the problem will be synthesized in this way [Συντεθήσεται δὴ τὸ πρόβλημα οὕτως]". Beside, after having presented the last analysis in prop. II. 49, Apollonius shortly concludes by observing that "the synthesis [is] like [that] of the previous [problem] [ἡ δὲ σύνθεσις ἡ αὐτὴ τῇ πρὸ αὐτοῦ]"

[26] The second stage in the solution of the problems 1, 4, 5, and 7 is introduced by the formula. "Συντετήσεται δὴ τὸ πρόβλημα οὕτως" (Archimedes OO, 172, 7; 192, 7; 198, 13; and 205, 15 respectively), while the second stage in the solution of the problems 3 and 6 is introduced by the formula. "Συντετήσεται δὴ οὕτως" (*ibid.*, 184, 21 and 204, 11).

[27] The arguments of Menaechmus aim to solve the same problem, namely, that of finding two segments which are medium proportional, according to a continuous proportion, between two given segments. Consider as an example the first of these arguments. A and E being the given segments, let us call B and C the searched ones. Imagine that these latter are taken on two straight lines perpendicular each other, in such a way that they have a common extreme. As Rect.(A, C) = Sq.(B) it is clear that the other extreme of B belongs to a given parabola passing for the other extreme of C. But as Rect.(C, B) is given—being equal to Rect.(A, E)—this point also belongs to an hyperbola that is given too. Thus it is given as well, by intersection of two conics. This makes clear that this point can be easily constructed by constructing two suitable conics.

[28] Remark that Aristotle is clearly not concerned here with a possible convertibility of analysis.

[29] The problematic character of geometrical analyses of the classical age is stressed by Knorr (1986). Cf. also Hintikka and Remes (1974, 84).

[30] As a matter of fact this is the structure of all the previous problematic analyses, which are, because of that, very similar to many arguments found in the 7th book of Pappus *Collection*. Cf. as an example the proposition 155 (Pappus CH, 905-907), quoted and discussed as paradigmatic by Hintikka and Remes (1974, 52-53). A similar argument is also in Aristotle's *Meteorologics*, 375*b*, 30-376*a*, 9. The fact that analysis is concerned here with what is given when the problem is assumed to be solved might explain Pappus' inclusion of Euclid's *Data* in the corpus of treatise belonging to the domain of analysis (Heath 1921, 422 and Knorr, 1986, 109-110).

[31] Cf. the previous note (17).

[32] Hintikka and Remes (1974, 91) observe that "analysis as a philosophical method was in vogue in the centuries before Pappus", when "widely different methods were called 'analysis'" and (*ibid.* 89-91) evoke the compound influences of Platonic and Stoic traditions on the these conceptions. Knorr (1986, 357) even argues that "Pappus could pick up [...][his] general views through te medium of commentators like Geminus and others, conversant with a syncretizing form of Platonism".

[33] We have not to confound reduction to elements in the previous sense, with a natural process of decomposition as that which Aristotle evokes in the chapter 4 of book H of *Metaphysics* (1044*a*, 15-25). Here Aristotle is opposing two (natural) processes according to which a thing comes from an other. The first one goes from the matter to the substance and is exemplified by the passage from the sweet to the fat and from the fat to the phlegm. At the opposite, the second one goes from the substance to the matter and is exemplified by the passage from the bile to the phlegm. Aristotle describes this process in general, by saying that a thing comes from another "as being analyzed in (its) principles [ὅτι ἀναλυθέντος εἰς τὴν ἀρχήν]" (1044a, 24-25) and says that the phlegm comes from the bile "by analyzing [τῷ ἀναλύεσθαι]" the latter "in [his] first matter [εἰς τὴν πρώτην ὕλην]" (1044a, 23). Clearly, analysis is not here a conduct of thinking, it is rather a natural process of decomposition of objects, the verb "ἀναλύω" being used with a meaning close to the one we have evoked in the previous paragraph I. The fact that this meaning occurs sometimes in Aristotle's writings does not entail that Aristotle does not refer in general to analysis as to a (regressive) conduct of thinking. It is just in this sense that Aristotelian notion of analysis interests us here.

[34] The following remarks on Thomas's conception of analysis and synthesis and its sources rest largely on Garceau's book (1968, specially 209-220).

[35] The idea that (geometrical) analysis brings us "from a complex to the simple" was advanced in the 6th-century by John Philoponus in his commentary to Aristotle's *Prior Analytics* (*Comm. Ar. Gr.*, XIII-2, 2, 16-17: cf. Hintikka and Remes 1974, 94). It is not clear however wheter the starting point of analysis is the complex in itself or the complex for us; as far as geometrical analysis is concerning, it is probably both.

[36] We could maintain that this is due to Albertus's views, since research necessarily goes from the simple to the complex, while the evaluation of the results of a certain research goes from the complex to the simple. However, it seems that here we are not speaking of simple and complex in themselves, but of simple and complex for us, which is not necessarily the same.

[37] This is what Proclus says (PEEL, ed Friedlein 211). It is remarkable that Proclus opposes here the method of analysis both to the method for separation (ἡ διαιρητική)—that he equally ascribes to Plato and considers as proper to every science—and to the reduction to the absurd—that, he says, does not show what is sought and only refutes its contrary (cf. also *ibid.*, 225, 8-12).

[38] The terms "*finem*" and "*comprehensionem*" (cf. below) could in fact translate the terms "κατάληξιν" and "κατάληψιν", which appears there.

[39] Of course, geometrical construction is not blind, it does not work without aims and it does not provide objects merely by chance. Rather, it is guided by the aim of constructing objects which satisfy certain conditions which are given *a priori* with respect to it. Thus, either it is preceded (both for Euclid and Descartes) by a geometrical reduction (that is just an analysis) or it consists in this reduction itself (this

is obviously possible as far as all its steps are trivially convertible). As a matter of fact, Descartes*Géométrie* is especially rich in examples where construction is exposed as a progressive conduct. However, the essential difference between Euclid's progressivity (that is and was intended as a synthesis by Pappus) and Descartes' progressivity could have suggested the latter is less far from analysis that the former is, or is even easily convertible into it. This could explain the famous remarks on analysis and synthesis advanced by Descartes in the "second answers" following his *Meditationes*, quoted by Israel (here, ch. 1, 5-6), where analysis is both considered as a conduct of proof and *inventio*. The essential difference between Descartes views—as expounded here—and Aristotelian ones does not lies, as many scholars have observed (for example, Timmermans, who construct his book (1995) on this opposition), in Descartes' identification of analysis with a conduct of invention. Foremost, the modern meaning of the term "invention" (both in English or French) is strictly different from the meaning of the Latin *"inventio"* (which in 17th-century is simply transferred to the French *"invention"*), being closer to the original idea expressed by the verb *"invenire"* (literally "to come in, or upon"), which is more like "to found"; to obtain, or even "to reach" than "to invent". And, if we speak of *inventio* in this sense, it is very easy to observe that for Aristotle too, analysis was a conduct of *inventio*. The problem rather is that for Aristotle analysis (as long as it is not conclusive) does not reach a theorem, or generally the realization of the aim, but reaches the first principles of the proof, or generally the conditions of realization of the aim. Thus, it is just "inventive" as long as it is not, as such, demonstrative (or at least conclusively demonstrative). For Descartes, in contrast, it seems to be "inventive" and "demonstrative" at the same time. As we have just said, we can eliminate such a difficulty in the interpretation of Descartes' text by referring to the difference between Descartes' proofs and usual deductions. But we can also remark that the difficulty is a very local one, since a few lines after, when he speaks of the application of analysis and synthesis to metaphysics, Descartes comes back to a very classical point of view, speaking about the "first notions *[primæ notiones]*" of geometry (here, ch. 1, 6) and remarking (AT, VII, 157) that analytic conduct is the most suitable one in metaphysics, since here that which is really important is "to perceive the first notions clearly and distinctly *[de primis notionibus clare & distincte percipiendis]*". Thus the difference with Aristotle reduces to one we have extensively discussed above: Descartes is simply referring to ontological (rather than epistemological) notions of clearness, evidence and firstness.

BIBLIOGRAPHY

Alembert, (d') J. B. le R. (1746) "Recherches sur le calcul intégral", *Hist. Acad. Roy. Sci. et Bell. Lettr. [Berlin]*, **2**, 1746 (publ. 1748), 182-224.

Apollonius (CH) *Treatise on conic sections* (ed. by T. L. Heath), Cambridge Univ. Press, Cambridge, 1896.

Apollonius (GE) *Apollonii Pergaei quae Graece exstant cum comentariis antiquis, edidit et latine interpretatus est I. L. Heiberg*, Teubner, Leizig 1891-1893 (2 vols.).

Apollonius (SRH) *De sections rationis libri duo. Ex Arabico MS^{to}. Latine Versi*, Opera & studio E. Halley, E Theatro Sheldoniano, Oxonii, 1706.

Arbuthnot, J. (1692) *Of the Laws of Chance, or, a Method of Calculation of the Hazards of Game*, Benj. Motte, London, 1692.

Archimedes (BL) *[Œuvres d'] Archimède*, texte établi et traduit par C. Mugler, Les Belles Lettres, Paris, 1970-1971 (4 vols.).

Archimedes (OO) *Archmedis Opera Omnia, iterum editit I. L. Heiberg*, Teubner, Leipzig, 1910-1915. (3, vols.).

Aristotle (AOG) *Aristotelis Organon Graece, Novis codicum auxilii aditus recognovit scholiis ineditis et commentario instruxit T. Waitz*, Neudruck der Ausgabe, Leipzig 1844-1846 (2 vols.).

Aristotle (WMC) *The Basic Works of Aristotle*, ed. with an introduction by R. McKeon, Random House, New York, 1941.

Armstrong, D.M. (1983) *What is a Law of Nature?*, Cambridge Univ. Press, Cambridge, 1983.

Arnauld, A. (1667) *Nouveaux elemens de geometrie [...]*, C. Savreux, Paris, 1667.

Artin, E. (1925-1926) "Theorie der Zöpfe", *Abh. Math. Sem. Univ. Hamburg*, **4**, 1925-1926, 47-72.

Artin, E. (1947) "Theory of braids", *Ann. Math.*, **48**, 1947, 101-126.

Artin, E. (1950) "The theory of braids", *American Scientist*, **38**, 1950, 112-119.

Bachelard, G. (1928) *Etude sur l'évolution d'un problème de physique. La propagation thermique dans les solides*, Vrin, Paris, 1928; new ed., 1973.

Barnes, J. (1975) *Aristote's Posterior Analytics*, translated with notes by J. Barnes, Clarendon Press, Oxford, 1975 (2nd ed., 1993).

Barrow, I. (1670) *Lectiones Geometricae: In quibus (praesertim) Generalia Curvarum Linearum Symptomata declarantur*, G. Godbid, Londini, 1670; 2nd ed. in I. Barrow, *Lectiones Opticae et Geometricae: In quibus Phaenomenon Opticorum Genuinae Rationes investigantur, ac exponuntur: et Generalia Curvarum Linearum Symptomata declarantur*, G. Godbid, Londini, 1674

Benacerraf, P. and Putnam, H. (1983) *Philosophy of mathematics. Selected Readings*, 2nd. ed., Cambridge Univ. Press, Cambridge, 1983.

Bernoulli, Jacob (1686) *Theses Logicae de Conversione et Oppositione Enunciationum. [...] cum Adnexis Miscellaneis*, Typis Bertschianis, Basileæ, 1686; ed. quot.: in Jacob Bernoulli (O), I, 225-238.

Bernoulli, Jacob (1691) "Specimen alterum calculi differentialis in dimetienda spirali logarithmica, loxodromiis nautarum et areis triangulorum sphaericorum; una cum additamento quodam ad problema funicularium, aliisque", *Acta Eruditorum*, Junii 1691, 282-290; in Jacob Bernoulli (O), 442-453.

Bernoulli, Jacob (1713) *Ars Conjectandi. Opus posthumum*, impensis Thurnisiorum fratum, Basileæ, 1713.

Bernoulli, Jacob (O) *Opera*, Cramer & Philibert, Genevæ, 1744 (2 voll.).

Bernoulli, Jacob (W) *Die Werke von Jacob Bernoulli*, Birkhäuser, Basel, 1969-1993 (4 voll.).

Biot, J. B. (1803) *Essai de géométrie analytique*, Duprat, Paris, 1803.

Blauberg, I.V., Sadovsky, V.N. and Yudin, E.G. (1977) *Systems Theory. Philosophical and Methodological Problems*, Progress Publ., Moscow, 1977.

Blay, M. (1992) *La naissance de la mécanique analytique*, P. U. F., Paris, 1992.

Bloch, M. (1964) *Apologie pour l'histoire ou métier d'historien*, A. Colin, Paris, 1964.

Blondel, M. (1893) *L'Action. Essai d'une critique de la vie et d'une science de la pratique*, Alcan, Paris, 1893; new ed. quoted: Paris, P.U.F., 1990.

Bloor, D. (1983) *Wittgenstein: A Social Theory of Knowledge*, Macmillan Press, London, Basingstoke, 1983.

Bochner, S. (1974) "Mathematical Reflections, Part II: Charles Sanders Peirce", *American Mathematical Monthly*, **81**, 1974, 838-852.

Bolzano, B. (1816) *Der Binomische Lehrsatz und als Folgerung aus ihm der polynomische, und die Reihen, die zur Berechnung der Logarithmen und Exponentialgrössen dienen, genauer als bisher erwiesen*, C. W. Enders, Praga, 1816; reprint with English transaltion in S. B. Russ, *The mathematical work of Bernard Bolzano published between 1804 and 1817*, Ph. D. dissertation. Open University, Great Britain, 1980, vol. III.

Bolzano B. (1817) *Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die entgegengesetztes Resultat gewaren, wenigstens eine reelle Wurzel der Gleichung liege*, Kummer, Prag, 1817; English translation quoted, by S. B. Russ: in *Historia Mathematica*, 7, 1980, 156-185.

Bolzano, B. (W) *Wissenschaftslehre*, herausg von A. Höfler und W. Schultz, Meiner, Leipzig, 1914-1931 (4 vols.).

Bos, H. J. M. (1981) "On the Representation of Curves in Descartes' Géométrie", *Archive for History of Exact Sciences*, **24**, 1981, 295-338.

Bos, H. S. M. (1984) "Arguments on Motivation in the Rise and Decline of a Mathematical Theory: the 'Construction of Equations', 1637-ca.1750", *Archives for History of Exact Sciences*, **30**, 1984, 331-380.

Bottazzini, U. (1986) *The higher calculus: A history of real and complex analysis from Euler to Weierstrass*, Springer Verlag, Heidelberg, New York, 1986.

Bourbaki, N. (1974) *Éléments d'histoire des mathématiques*, nouvelle édition, revue, corrigée et augmentée, Hermann, Paris, 1974.

Boutroux, P. (1920) *L'ideal scientifique des mathematiciens*, Alcan, Paris, 1920.

Boyer, C. B. (1939) *The Concepts of the Calculus: A Critical and Historical Discussion of the Derivative and the Integral*, Columbia Univ. Press, New York, 1939; quot. ed.: *The History of the Calculus and Its Historical Developpement*, Dover, New York, 1959.

Boyer, C. B. (1956) *History of Analytic Geometry*, Scripta Mathematica (*Scripta Mathematica studies*, 6 and 7), New York, 1956.

Boyer, C. B. (1968) *A History of Mathematics*, John Wiley and Sons, New York, London, Sydney, 1968.

Busard, H. L. L. (1976) "François Viète", *Dictionary of Scientific Biography*, XIV, 1976, 18-25.

Carathéodory, C. (1952) "Einführung in Eulers Arbeiten über Variationsrechnung", in Euler (OO), XXIV, viii-li.

Cardano G. (1545) *Artis Magnæ, sive de Regulis algbraicis liber unus*, J. Petreium, Norimbergæ, 1545.

Carnap, R. (1952) "Meaning Postulates", *Philosophical Studies*, **3**, 1952, 65-73.

Casati, R. (fc) "Rest and motion: A Newtonian Framework", forthcoming.

Cassirer, E. (1907) "Kant und die moderne Mathematik", *Kantstudien*, **12**, 1907, 1-49.

Cassirer, E. (1910) *Substanzbegriff und Funktionsbegriff. Untersuchungen über die Grundfragen der Erkenntniskritik*, B. Cassirer, Berlin, 1910

Cassirer, E. (1923-1929) *Philosophie der symbolischen Formen*, B. Cassirer, Berlin 1923-1929 (3 vols).

Cauchy, A. (1817) "Sur une loi de réciprocité qui existe entre certaines fonctions", *Bulletin des sciences par la societé Philomatique de Paris*, **2**, 1817, 121-124.

Cauchy, A. L. (1821) *Cours d'Analyse de l'École polythechnique. Première partie, Analyse Algébrique*, Debure frères, Paris, 1821; in Cauchy (OC), 2, III.

Cauchy, A. L. (1823) *Résumé des leçons données à l'École polytechnique sur le calcul infinitésimal*, tome I, Impr. Roy. Debure, Paris, 1823; in Cauchy (OC), 2, IV.

Cauchy, A. L. (OC) *Œuvres Complètes*, Gauthier-Villars, Paris, 1882-1970 (27 vols. in two series).

Caveing, M. (1990) "Introduction générale", in Euclide (EV), I, 13-148.

Chandler, B. and Magnus, W. (1982) *The History of Combinatoal Group Theory: a case study in the history of ideas*, Springer, New York, Berlin, 1982.

Chasles, M. (1875) *Aperçu historique sur l'origine et le développement des méthodes en Géométrie*, Gauthier-Villars, Paris, 1875.

Chomsky, N. (1976) *Reflections on Language*, Temple Smith, London, 1976.

Chuquet, N. (G) *La Géométrie. Première géométrie algébrique en langue française (1484)*, Introduction, texte et notes par Hervé l'Huillier, Vrin, Paris, 1979.

Commandino (1588), *Pappi Alexandrini Mathematicæ collectiones a Federico Commandino [...] in latinum conversæ*, H. Concordiam, Pisauri, 1588.

Comte, A. (1830-1842) *Cours de philosophie positive*, Bachelier, Paris, 1830-1842 (6 vols.).

Comte, A. (SDS) *Philosophie première. Cours de philosophie positive, Leçons 1-45*, ed. by M. Serres, F. Dagognet, H. Sinaceur, Hermann, Paris, 1975.

Coolidge, J. L. (1940) *A History of Geometrical Methods*, Clarendon Press, Oxford, 1940.

Coolidge, J. L. (1945) *A History of the Conic Sections and Quadric Surfaces*, Clarendon Press, Oxford, 1945; quot. ed.: Dover, New York, 1968.

Couturat, L. (1893) "L'année philosophique" de F. Pillon", *Revue de Métaphysique et de Morale*, **1**, 1893, 63-85.

Dahan, A. (1992) "Lagrange: la méthode critique du mathématicien philosophe", in Dhombres (1992*a*), 171-192.

Dahan, A. (1993) *Mathématisations. Augustin-Louis Cauchy et l'Ecole Française*, Editions du Choix and Blanchard, Paris, 1993.

Dascal, M. (1988) "On Knowing Truths of Reason", *Studia Leibnitiana*, Sonder. **15**, 1988, pp. 27-37.

Daston, L. (1988) *Classical Probability in the Enlightenment*, Princeton Univ. Press, Princeton (N. J.), 1988.

Daston, L. (1991) "History of science in an elegiac mode. E. A. Burtt's *Metaphysical foundations of modern physical science* revisited", *Isis*, **82**, 1991, 522-531.

Daston, L. (1992) "The Doctrine of Chances without Chance: Determinism, Mathematical Probability, and Quantification in the Seventeenth Century", in M. J. Nye, J. Richards, and R. Stuewer (ed. by), *The Invention of Physical Science*, Kluwer, Dordrecht, Boston, London, 1992, 27-50.

De Gand, F. (1986) "Le style mathématique des *Principia* de Newton" *Revue d'histoire des sciences*, **39**, 1986, 195-222.

De Moivre, A. (1711) "De mensura sortis seu de probabilitate eventuum in ludis a casu fortuito pendentibus", *Philosophical Transactions*, **27**, 1711, 213-264.

De Moivre, A. (1718) *The Doctrine of Chances: or A Method of Calculating the Probability of Events in Play*, W. Pearson, London, 1718; 2nd ed. W. Woodfall, London, 1738; 3rd ed. quot., A. Millar, London, 1756.

De Moivre, A. (1730) *Miscellanea Analytica de Seriebus et Quadraturis*, excudebant J. Tonson & J. Watts, Londini, 1730.

Delambre, J.-B. (1810) *Rapport historique sur les progrès des sciences mathématiques depuis 1789, et sur leur état actuel*, Impr. Imper. Paris, 1810; critical edition with presentation and notes by J. Dhombres: *Rapport à l'Empereur sur le progrès des sciences, des lettres et des arts depuis 1789. I. Sciences Mathématiques*, par Jean-Baptiste Delambre, Belin, Paris, 1989.

Demidov, S. S. (1982) "Création et développement de la théorie des équations différentielles aux dérivées partielles dans les travaux de J. d'Alembert", *Revue d'histoire des sciences*, **35**, 1982, 3-42.

Dennett, D. (1991) *Consciousness Explained*, Little, Brown and Company, Boston, 1991.

Descartes, R. (1637) *La Géométrie*, *in* R. Descartes, *Discours de la Méthode [...]. Plus la Dioptrique. Les Météores. Et la Géométrie qui sont des essaies de cette Méthode* (without indication of the author), I. Maire, Leyde, 1637, 295-413; A.T., VI, 368-485.

Descartes, R. (1641) *Meditationes de Prima Philosophia*, 1st. ed.: apud M. Soly, Paris, 1641; 2nd ed.: apud L. Elzeverium, Amstelodami, 1642; A.T., VII.

Descartes, R. (1647) *Les Méditations Métaphysiques de René Descartes*, I. Camusat et P. Le Petit, Paris, 1647; A.T. VII; quot. ed.: in Descartes (OLB), 147-409.

Descartes, R. (1659-1661) *Geometria a Renato Des Cartes [...]*, L. & D. Elzevirii, Amstelædami, 1659-1661 (2 vols.).

Descartes, R. (AT) *Oeuvres de Descartes* (publiées par Ch. Adam e P. Tannery), Vrin, Paris, 1897-1910 (12 voll.).

Descartes, R. (GSL) *Geometry*, English translation by D. Smith and M. Latham with a facsimile of the first edition, Dover, New York, 1954.

Descartes, R. (LR) *Regulæ ad directionem ingenii. Regles par la direction de l'esprit*, texte revu et trad. par G. Le Roy, Boivin, Paris, 1933.

Descartes, R. (OLB) *Œuvres et lettres*, textes présentés par A. Bridoux, Gallimard, Bibliothéque de la Pléiade, Paris, 1949.

Descartes, R. (PW) *The Philosophical Writings of Descartes*, translated by J. Cottingham, R. Stoothoff, and D. Murdoch, Cambridge Univ. Press, Cambridge, 1985.

Descartes, R. (ROP) "Regulæ ad directionem ingenii", in R. Descartes, *Opuscula Posthuma, physica et mathematica*, ex typ. P. et J. Blaeu, Amstelodami, 1701, separated pagination: 1-66; AT, X, 349-348.

Dhombres, J. (1978) *Nombre, mesure et continu, épistémologie et histoire*, Cedic/F. Nathan, Paris, 1978.

Dhombres, J. (1982-1983) "La langue des Calculs de Condillac (ou comment propager les Lumières?)" *Sciences et Techniques en Perspective*, **2**, 1982-1983, 197-230.

Dhombres, J. (1986) "Mathématisation et communauté scientifique française (1755-1825)", *Archives internationales d'histoire des sciences*, **36**, 1986, 249-293.

Dhombres, J. (1987) "Les présuposés d'Euler dans l'emploi de la méthóde fonctionelle", *Revue d'histoire des sciences*, **40**, 1987, 179-202.

Dhombres, J. (1992*a*) (sous la direction de), *L'école normale de l'an III. Leçons de Mathématiques. Laplace-Lagrange-Monge*, Dunod, Paris, 1992.

Dhombres, J. (1992*b*) "L'Affirmation du Primat de la Démarche Analytique" in Dhombres (1992*a*), 11-43.

Dhombres, J. (1995) "Adégaliser en Occitanie", in J. Cassinet (èd.), *Chemins mathématiques occitans de Gerbert à Fermat*, cherat de Troie, Toulouse, 1995,. 161-200.

Dhombres, J. (fc*a*) "Euclide revisité par Port-Royal, la révolution naturelle", Colloque Abraham Bosse, Tours, forthcoming.

Dhombres, J. (fc*b*) "L'histoire selon Bourbaki", in *Revolutions mathématiques, le sens de l'histoire*, forthcoming.

Dhombres, J. (fc*c*) "Une mathématique baroque en Europe, réseaux, ambitions et acteurs", in C. Goldstein, J. Gray, J. Ritter (éd.), *Une Europe mathématique, mythes et réalités historiques*, forthcoming.

Dhombres, J. and Robert, J.-B. (1996) Et ignem regunt numeri, *Fourier ou la chaleur mathématisèe*, Belin, Paris, 1995.

Dieudonné, J. (1939) "Les méthodes axiomatiques modernes et les fondements des mathématiques", *Revue scientifique*, **77**, 1939, 224-232.

Dieudonné, J. (1963-1982) *Élements d'analyse*, Gauthier-Villars, Paris, 1963-1982 (9 vols.).

Dieudonné, J. (1968) *Algèbre linéaire et géométrie élémentaire*, Hermann, Paris, 1968.

Dieudonné, J. (1969) *Calcul infinitésimal*, Hermann, Paris, 1991.

Dieudonné, J. (1970) "The work of Nicolas Bourbaki", *American Mathematical Monthly*, **77**, 1970, pp. 134-145.

Dieudonné, J. (1978) (dirigé par) *Abrégé d'histoire des mathématiques 1700-1900*, Hermann, Paris, 1978 (2 vols.).

Dijksterhuis, E. J. (1956) *Archimedes*, Ejnar Munksgaard, Copenhagen, 1956.

Dijksterhuis, E. J. (1961) *The Mechanization of the World Picture*, Oxford University Press, Oxford, 1961.

Dockès, P. (1969) *L'espace dans la pensée économique du XVIe au XVIIIe siècle*, Flammarion, Paris, 1969.

Duchesneau, F. (1993) *Leibniz et la méthode de la science*, Paris, P.U.F., 1993.

Duhamel, J. M. C. (1865) *Des Méthodes dans les Sciences de Raisonnement*, Gauthier-Villars, Paris, 1865; 2ed. quot.: 1875.

Echeverria, J. (1992) "Observations, Problems and Conjectures in Number Theory", *in* J. Echeverria, A. Ibarra and D. Mormann, (ed. by), *The Space of Mathematics*, Walter de Gruyter, Berlin, New York, 1992, 230-252.

Eisler, R. (1930) *Kant Lexikon*, E. S. Mittler, Berlin, 1930.

Engel-Tiercelin, C. (1993) "Peirce's Realistic Approach to Mathematics: Or, Can One Be a Realist without Being a Platonist?", in E. C. Moore, (ed. by), *Charles S. Peirce and the Philosophy of Science*, Papers from the Harvard Sesquicentennial Congress, The University of Alabama Press, Tuscaloosa and London, 1993, 30-48.

Engelsman, S. B. (1984) *Families of Curves and the Origins of Partial Differentiation*, North Holland P. C., Amsterdam, 1984.

Epple, M. (1994) "Das bunte Geflecht der mathematischen Spiele", *Math. Semesterberichte*, **41**, 1994, 113-133.

Euclid (EH) *Euclid's Elements* (trans. with introduction and commentary by Sir Thomas Heath), Cambridge Univ. Press, Cambridge, 2th edition, 1926.

Euclid (EV) *Les Éléments*, traduction et commentaires par B. Vitrac, P.U.F., Paris, 1990 (2 vols. appeared)

Euclid (OO) *Euclidis Opera Omnia*, ediderunt I. L. Heibrg et H. Menge, Teubner, Leipzig, 1983-1899 (8 vols. + 1 vol. supl.)

Euler, L. (1734-1735) "De infinitis curvis eiusdem generis seu methodus inveniendi aequationes pro infinitis curvis eiusdem generis" *Commentarii academiae scientiarum Petropolitanae*, **7**, 1734-1735 (publ. 1740), 174-189 (pp. 180-189 are incorrectly numbered as 190-199); in Euler (OO), ser. 1, XXII, 36-56.

Euler, L. (1736) *Mechanica, sive motus scientia analytice exposita*, ex typ. Acad. sci. imp., Petropoli, 1736 (2 vols.).

Euler, L. (1744) *Methodus inveniendi lineas curvas maximi minimive proprietate gaudentes sive solutio problematis isoperimetrici lattisimo sensu accepti,*, M.-M. Bousquet & soc. Lausannæ et Genevæ, 1744; in Euler (OO), ser. 1, XXIV.

Euler, L. (1748) *Introductio in Analysin Infinitorum*, M.-M. Bousquet & soc. Lausannæ, 1748 (2 vols.).

Euler, L. (1749) "Recherches sur les racines imaginaires des équations", *Hist. Acad. Roy. Sci. et Bell. Lettr. [Berlin]*, **5**, 1749 (publ. 1751), 222-288.

Euler, L. (1756) "Exposition de quelques paradoxes dans le calcul intégral", *Hist. Acad. Roy. Sci. et Bell. Lettr. [Berlin]*, **12**, 1756, (publ. 1758), 300-321; in Euler (OO), ser 1, XXII, 214-236.

Euler, L. (OO) *Opera Omnia*, ed. by the Soc. Sci. Nat. Helveticæ, Teubner, Leipzig, Berlin, Zürich, Basel, 1911 ff.

Fermat, P. de (TH) *Œuvres de Fermat*, éditées par P. Tannerry et C. Henry, Gauthier-Villars, Paris, 1891-1923 (5 vols.).

Ferrarin, A. (1995) "Construction and Mathematical Schematism. Kant on the Exhibition of a Concept in Intuition", *Kantstudien*, **86**, 1995, 131-174.

Fleck, L. (1980) *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*, Suhrkamp, Frankfurt, 1980.

Flegg, G., Hay, C. and Moss, B. (1985) *Nicolas Chuquet, Renaissance mathematician, A study with extensive translation of Chuquet's mathematical manuscript completed in 1484*, D. Reidel P. C., Dordrecht, Boston, Lancaster, 1985.

Forster, G. (W) *Works in 2 volumes*, Aufbau-Verlag, Berlin and Weimar, 1983.

Fourier, J. (1807) "Théorie de la propagation de la chaleur dans les solides", Ms. of 1807, 1-st ed. in I. Grattan-Guinness (in collaboration with J. R. Ravetz), *Fourier 1768-1830*, M.I.T. press, Cambridge (Mass.), 1972, 31-440.

Fourier, J. (1819-1820) "Théorie du mouvement de la chaleur dans les corps solides", *Mém. Acad. Roy. Sc. de l'Institut de France*, **4**, 1819-1820 (publ. 1824), 185-556.

Fourier, J. (1822) *Théorie analytique de la chaleur*, F. Didot, Paris, 1822.

Fourier, J. (OD) *Œuvres de Fourier*, publiées par les soins de M., G. Darboux, Gauthier-Villars, 1888-1890 (2 vols.).

Fraser, C. G. (1985) "J.L. Lagrange's changing approach to the foundations of the calculus of variations", *Archive for History of Exact Sciences,*, **32**, 1985, 151-191.

Fraser, C. G. (1987) "Joseph Louis Lagrange's algebraic vision of the calculus", *Historia Mathematica*, **14**, 1987, 38-53.

Fraser, C. G. (1989) "The Calculus as Algebraic Analysis: Some Observations on Mathematical Analysis in the 18th Century", *Archives for the History of Exact Sciences*, **39**, 1989, 317-335.

Fraser, C. G. (1990) "Lagrange's analytical mathematics, Its Cartesian origins and reception in Comte's positive philosophy", *Studies in the History and Philosophy of Science*, **21**, 1990, 243-256.

Fraser, C. G. (1992) "Isoperimetric problems in the variational calculus of Euler and Lagrange", *Historia Mathematica*, **19**, 1992, 4-23.

Fraser, C. G. (1994) "The origins of Euler's variational calculus", *Archive for History of Exact Sciences*, **47**, 1994, 103-141.

Frege, G. (1884) *Die Grundlagen der Arithmetik*, W. Köbner, Breslau, 1884; English translation quot., by J. L. Austin: *The foundations of Arithmetic*, Basil Blackwell, Oxford, 1951[1], 1953[2].

Frege, G. (1893-1903) *Grundgesetze der Arithmetik*, H. Pohle, Jena, 1893-1903 (2 vols.).

Fricke, R.and Klein, F. (1897-1912) *Vorlesungen über die Theorie der automorphen Funktionen*, B. G. Teubner, Leipzig, Berlin, 1897-1912 (2 vols.).

Friedman, M. (1992) *Kant and the Exact Sciences*, Harvard Univ. Press, Cambridge (Mass.), London, 1992.

Galilei, G. (1638), *Discorsi e dimostrazioni matematiche intorno a due nuove scienze [...]*, appresso gli Elsevirii, Leida, 1638.

Garber, D. and Zabell, S. (1979) "On the Emergence of Probability", *Archive for History of Exact Sciences*, **21**, 1979, 33-53.

Garceau, B. (1968) Judicium. *Vocabulaire, sources, doctrine de Saitn Thomas d'Aquin*, Inst. d'Études Médiévales, Montréal and Vrin, Paris, 1968.

Gardies, J.-L. (1984) *Pascal entre Eudoxe et Dedekind*, Vrin, Paris, 1984

Gilain, C. (1991) "Sur l'histoire du théorèm fondamental de l'algèbre: théorie des équations et calcul intégral", *Archives for the History of Exact Sciences*, **42**, 1991, 91-136.

Girard, A. (1629) *Invention nouvelle en algèbre*, G. Jansson Blaeuw, Amsterdam, 1629.

Giusti, E. (1984) "Gli 'errori' de Cauchy e i fondamenti dell'analisi", *Bolletino di storia delle scienze matematiche*, **4**, 2, 1984, 24-54.

Glas, E. (1985) "On the dynamics of mathematical change in the case of Monge and the French Revolution", *Stud. Hist. Phil. Sci.*, **17**, 1985, 249-268.

Gödel, K. (1944) "Russell's Mathematical Logic", in P. A. Schilpp (ed. by), *The philosophy of Bertrand Russell*, Northwestern Univ. Press, Evanston (Illinois) and Chicago, 1944, 125-153; in Benacerraff and Putnam (1983), 447-469 and in Gödel (CW), II, 119-141.

Gödel, K. (1961) "The Modern Development of the Foundations of Mathematics in Light of Philosophy", first publication, in Gödel (CW), III, 375-387.

Gödel, K. (CW) *Collected Works* (ed. by S. Feferfan, J. W. Dawson, S. C. Kleene, G. H. Moore, R. M. Solovay and J. van Heijenort), Oxford Univ. Press, Oxford, 1986-1995 (3 vols.).

Goldstine, H. H. (1980) *A history of the calculus of variations from the 17th through the 19th century*, Springer-Verlag, Heidelberg, 1980.

Grabiner, J. V. (1978) "The Origins of Cauchy's Theory of the Derivative", *Historia Mathematica*, **5**, 1978, 379-409.

Grabiner, J. V. (1981) *The Origins of Cauchy's Rigorous Calculus*, M.I.T. press, Cambridge (Mass.), 1981.

Granger, G.-G. (1968) *Essai d'une philosophie du style*, A. Colin, Paris, 1968.

Granger, G.-G. (1982) "On the Notion of Formal Content", *Social Research*, **49**, 1982, 359-382.

Grattan-Guinness, I. (1972) (in collaboration with J. R. Ravetz) *Fourier 1768-1830*, M.I.T. press, Cambridge (Mass.), 1972.

Grice, H. P. and Strawson P. F. (1971) "In Defense of a Dogma", in S. Munsat (ed. by), *The Analytic-Synthetic Distinction*, Wadsworth Publishing Comp., Inc., Belmont (CA), 111-127.

Grimsley, R. G. (1963) *Jean d'Alembert (1717-1783)*, Clarendon Press, Oxford, 1963.

Grisard, P. (w.d.), *François Viète, mathématicien de la fin du seizième siècle*, thèse de Doctorat, EHESS, Paris, without date (2 vols.).

Gueroult, M. (1946) "Substance and the Primitive Simple Notion in the Philosophy of Leibniz", *Philosophical and Phenomenological Research*, **7**, 1946, 293-315, in M. Gueroult, *Etudes sur Descartes, Spinoza, Malebranche et Leibniz*, G. Olms, Hildesheim, 1970, 229-251.

Haack, S. (1993) "Peirce and Logicism: Notes Towards an Exposition", *Transactions of the C. S. Peirce Society*, **29**, 1993, 33-56.

Hacking, I. (1975) *The Emergence of Probability*, Cambridge Univ. Press, Cambridge, 1975.

Hacking, I. (1984) "Leibniz and Descartes: Proof and Eternal Truths", in T. Honderich (ed. by), *Philosophy Through its Past*, Penguin Books, Harmondsworth (Middlesex), 1984, 207-224.

Hald, A. (1990) *A History of Probability and Statistics and Their Applications before 1750*, John Wiley and Sons, New York, 1990.

Hankel, H. (1874) *Zur Geschichte der Mathematik in Alterthum und Mittelalter*, B. G. Teubner, Leipzig, 1874.

Hausdorff, F. (1914) "Bemerkung über den Inhalt von Punktmengen", *Math. Ann.*, **75**, 1914, 428-433.

Heath, T. L. (1921) *A history of Greek Mathematics*, Clarendon Press, Oxford, 1921 (2 vols.).

Hegel, G. W. (W) *Werke*, Duncker und Humblot, Berlin and Liepzig, 1832-1887 (19 vols.).

Heiberg, J. L., "Paralipomena zu Euklid", *Hermes*, **38**, 1903, 46-74, 161-201, 321-356.

Heidegger, M. (1927) *Sein und Zeit*, Max Niemeyer, Halle, 1927.

Herivel, J. (1975) *Joseph Fourier, the Man and the Physicist*, Clarendon Press, Oxford, 1975.

Hermann, J. (1729) "Consideratio curvarum in punctum positione datum projectarum, et de affectionibus earum inde pendentibus", *Comm. Acad. Sci. Imp. Petropolitanae*, **4**, 1729 (pub. 1735), 37-46.

Hesse, O. (1861) *Vorlesungen über analytische Geometrie der Raumes*, Teubner, Leipzig, 1861.

Hilbert, D. (1899) *Grundlagen der Geometrie*, in *Festschrift zur Feier der Enthüllung des Gauss- Weber-Denkmals in Göttingen*, herausg. von dem Fest-Comitee, Teubner, Leipzig, 1899.

Hilbert, D. (1926) "Über das Unendliche", *Mathematische Annalen*, **95**, 1926, 161-190; English translation in, Benacerraf and Putnam, (1983), 183-201.

Hintikka, J. (1973) *Logic, Language-Games and Information*. Kantian Themes in the Philosophy of Logic, Clarendon Press, Oxford, 1973.

Hintikka, J. (1975) *Time and Necessity. Studies in Aristotle's theory of modality*, Clarendon Press, Oxford, 1975.

Hintikka, J. and Remes, U. (1974) *The Method of Analysis: Its Geometrical Origin and Its General Significance*, Reidel, Dordrecht, 1974.

Hintikka, J. and Remes, U. (1976) "Ancient Geometrical Analysis and Modern Logic", in R. S. Cohen, et al., (ed. by), *Essays in Memory of Imre Lakatos*, Reidel, Dordrecht, 1976, 253-276.

Hofmann, J. E. (1960) "Über zahlentheoretische Methoden Fermats und Eulers, ihre Zusammenhänge und ihre Bedeutung", *Archive for History of Exact Sciences* **1**, 1960-1962, 122-159.

Hölder, O. (1924) *Die Mathematische Methode*, Springer, Berlin, 1924.

Holton, G. (1978) *The scientific imagination: case studies*, Cambridge Univ. Press, Cambridge, London, 1978.

Houser, N. (1993) "On 'Peirce and Logicism': A Response to Haack", *Transactions of the C. S. Peirce Society*, **29**, 1993, 57-67.

Hull, K. (1994) "Why Hanker After Logic? Mathematical Imagination, Creativity and Perception in Peirce's Systematic Philosophy", *Transactions of the C. S. Peirce Society*, **30**, 1994, 285-295.

Husserl, E. (1929) *Formale und Transzendentake Logik*, Niemeyer, Halle, 1929.

Husserl, E. (1935-1936) *Die Krisis der europäischen Wissenschaften und die transzendentale Phänomenologie*, M. Ninjhoff, The Hague, (1935-1936).

Huygens. C. (1657) "De Ratiociniis in Ludo Aleae", in F. Van Schooten, *Exercitationum mathematicorum libri quinque*, lib. V, J. Elsevirii, Lugduni Batavorum, 1657, 519-534.

Israel, G. (1990) "Federigo Enriques: A Psychologistic Approach for the Working Mathematician", in M. A. Notturno (ed. by) *Perspectives on Psychologism*, Brill, Leiden, New York, København, Köln, 1989, 426-457.

Itard, J. (1956) *La géométrie des Descartes*, Les conférences du palais de la découverte, Paris, série D, 39, 1956; reprinted in J. Itard, *Essais d'Histoire des Mathématiques*, réunis et introduits par R. Rashed, Blanchard, Paris, 1984, 269-279.

Jahnke, H. N. (1992) "Beweisbare Widersprüche-Komplementarität in der Mathematik", in E. P. Fischer, H. S. Herzka and K. H. Reich (ed. by), *Widersprüchliche Wirklichkeit*, Piper, München, 98-130.

Kant, I. (A) *Kritik der reinen Vernunft*, Hartknoch, Riga, 1781; the pages 1-405 are in Kant (SA), IV, 1911, 1-252.

Kant, I. (B) *Kritik der reinen Vernunft*, Hartknoch, Riga, 1787; *in* Kant (SA), III, 1911, 1-552.

Kant, I. (1790) *Kritik der Urteilskraft*, Lagarde und Friederich, Berlin und Libau, 1790; in Kant (SA), V, 165-485.

Kant, I. (CJM) *Kant's Critique of aestetic judgment*, transl. with seven introductory essays, notes and analytical index, by J. C. Meredith, Clarendon Press, Oxford, 1911.

Kant, I. (CS) *Critique of Pure Reason*, English translation by N. K. Smith, Mac Millan, London, Basingstoke, 1929[1], 1933[2].

Kant, I. (JL) "Logik, ein Handbuch zu Vorlesungen", edit by G. B. Jäsche, F. Nicolovius, Könisberg, 1800; in Kant (SA), IX, 1-150.

Kant, I. (LY) "Jäsche Logic", in I. Kant, *Lectures on Logic*, translated and edited by J. M. Young, Cambridge Univ. Press, Cambridge, 1992, 519-640.

Kant, I. (SA) *Kant's gesammelte Schriften* (herausg. von der Königlich-Preußischen Akademie der Wissenschaften, afterwards Deutsche Akademie der Wissenschaften zu Berlin, afterwards Akademie der Wissenschaften der D.D.R.), G. Reimer and (from 1922) W. de. Gruyter, Berlin, 1900 ff.

Kitcher, P. (1975) "Bolzano's Ideal of Algebraic Analysis", *Studies in History and Philosophy of Science*, **6**, 1975, 229-269.

Klein, F. (1872) *Vergleichende Betrachtungen über neuere geometrische Forschungen*, Deichert, Erlangen, 1872; new ed. quot.: *Das Erlangen Programm*, Akademische Verlagsgesellschaft Geest & Portig K.-G. (Ostwalds Klassiker der exakten Wissenschaften, 253), Leipzig, 1974.

Klein, F. (1893) *Einleitung in die höhere Geometrie*, handwritten notes by F. Schilling, Göttingen, 1893; 3-rd ed. quot.: *Vorlesungen über höhere Geometrie*, bearb. und herausg. von W. Blaschke, Springer, Berlin, 1926.

Klein, F. (1908-1909) *Elementarmathematik vom hoeheren Standpunkte*, B. G. Teubner, Leipzig, 1908-1909 (2 vols.); English transl. quot., from the 3-rd ed. (Springer, Berlin, 1924-1928, (3 vols.)), by E. R. Hedrick and C. A. Noble: Dover, New York, 1939.

Klein, F. (1926-1927) *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, Springer, Berlin, 1926-1927 (2 vols.)

Klein, J. (1934-1936) "Die griechische Logistik und die Entstehung der Algebra," *Quellen und Studien zur Geschichte der Mathematik, Astronomie und Physik*, Abteilung B: *Studien*, **3**, 1934, 18-105 and 122-235; English transl. quot. by Eva Brann: *Greek mathematical thought and the origin of algebra*, M.I.T. Press, Cambridge (Mass.), 1968 (the appendix contains J.W. Smith's English translation of Viète (1591*a*)).

Kline, M. (1972) *Mathematical thought from ancient to modern times*, Oxford Univ. Press, Oxford, New York, 1972.

Knorr, W. (1983) "Construction as Existence Proof in Ancient Geometry", *Ancient Philosophy*, **3**, 1983, 125-148.

Knorr, W. R. (1986) *The Ancient Tradition of Geometric Problems*, Birkhäuser, Boston, Basel, Stuttgart 1986.

Kolmogorov, A. (1932) "Zur Deutung der intuitionistischen Logik", *Mathematische Zeitschrift*, **35**, 1932, 58-65.

Kuhn, T. S. (1962) *The Structure of Scientific Revolutions*, Univ. of Chicago Press, Chicago, 1962.

L'Hôpital, G. F. Marquis de (1696) *Analyse des Infiniment petits, pour l'intelligence des lignes courbes*, de l'Imprimerie Royale, Paris, 1696.

Lachterman, D. (1989) *The Ethics of Geometry*, Routledge, London, New York, 1989.

Lacroix, S.-F. (1797-1798) *Traité du calcul différentiel et du calcul intégral*, Duprat, Paris, 1797-1798 (2 vols.).

Lagrange, J. L. (1788) *Mécanique analytique*, la veuve Desaint, Paris, 1788; in Lagrange (OS), XI-XII.

Lagrange, J. L. (1797) *Théorie des fonctions analytiques*, impr. de la République, Paris, an. V (*Journal de l'École Polytechnique*, cah. 9); in Lagrange (OS), IX.

Lagrange, J. L. (1798) *Traité de la résolution des équations numériques de tous les degrés*, Duprat, Paris, an VI; in Lagrange (OS), VIII.

Lagrange, J. L. (1799) "Discours sur l'objet de la Théorie des Fonctions analytiques", *Journal de L'Ecole Polytechnique*, cah. 6, t. II, 232-235.

Lagrange, J. L. (1806) *Leçons sur le calcul des fonctions*, nouv. éd. revue, corrigée et augmentée par l'auteur, Courcier, Paris, 1806; in Lagrange (OS), X.

Lagrange, J. L. (LEN) "Leçons de Lagrange à l'École Normale de l'an III", in Dhombres (1992*a*), 193-260 (with introdutions and notes of A. Dahan: Dahan (1992) and 261-265).

Lagrange, J. L. (OS) *Œuvres de Lagrange*, publiées par les soins de J.-A. Serret [et G. Darboux], Gauthiers-Villars, Paris 1867-1892 (14 vols.).

Lakatos, I. (1976) *Proofs and Refutations*, ed. by J. Worral and E. Zahar, Cambridge Univ. Press, Cambridge, London, 1976.

Lakatos, I. (PP) *Philosophical Papers*, Cambridge Univ. Press, 1978 (vol. I: *The Methodology of Scientific Research Programmes*; vol. II: *Mathematics, Science and Epistemology*).

Laplace, P. S. (1799-1825) *Traité de mécanique celeste*, Duprat, after Mme Ve Courcier, after Bachelier, Paris, 1799-1825 (5 vols.)

Laplace, P. S. (LEN) "Leçons de Lagrange à l'École Normale de l'an III", in Dhombres (1992*a*), 45-140 (with introdutions and notes of J. Dhombres: Dhombres (1992*a*) and 141-167).

Leibniz, G. W. (1684) "Nova methodus pro maximis et minimis, itamque tangentibus, qua nec fractas, nec irrationales quantitates moratur, et singulare pro illis calculi genus", *Acta Eruditorum*, **3**, 1684, 467-473.

Leibniz, G. W. (A) *Sämtliche Schriften und Briefe*, Leipzig-Berlin, Akademie der Wissenschaften zu Berlin, 1923-...

Leibniz, G. W. (GM) *Leibnizens mathematische Schriften* (herausg. von C. I. Gerhardt), Asher & Comp., Berlin, 1849-1850: voll. I-II, H. W. Schmidt, Halle, 1855-1863: voll. III-VII; repr., G. Olms, Hildesheim, 1961-62.

Leibniz, G. W. (GP) *Die philosophischen Schriften von G. W. Leibniz*, (herausg. von C. I. Gerhardt), Weidmann, Berlin, 1875-1890 (7 vols.); rist. anast. G. Olms, Hildesheim, 1965.

Leibniz, G. W. (VE) *Vorausedition zur Reihe VI — Philosophische Schriften — in der Ausgabe der Akademie der Wissenschaften*, Manuskriptdruck ad usum collegialem, Münster, Leibniz-Forschungsstelle.

Lektorsky, V. (1980) *Subject, Object, Cognition*, Progress Publ., Moscow, 1980.

Lenoir, T. (1979) "Descartes and the Geometrization of Thought: The Methodological Background of Descartes's Géométrie", *Historia Mathematica*, **4**, 1979, 355-379.

Lewis, D. (1973) *Counterfactuals*, Harvard University Press, Cambridge (Mass.), 1973.

Loria, G. (1923) "Qu'est-ce que la géométrie analytique?" *L'Enseignement Mathématique*, **23**, 1923, 142-147.

Loria, G. (1924) "Da Descartes a Fermat a Monge a Lagrange. Contributo alla storia della geometria analitica", *Memorie dell'Accademia dei Lincei*, Cl. Sci. Fis. Mat. e Nat., Serie 5a, **14**, 1924, 777-845.

Loria, G. (1932) "A. L. Cauchy in the history of analytic geometry", *Scripta Mathematica*, **1**, 1932, 123-128.

Lovejoy, A. O. (1936) *The great chain of being; a study of the history of an idea. The William James lectures delivered at Harvard university*, Harvard Univ. Press, Cambridge, Mass., 1936.

Lützen, Jesper (1983) "Euler's vision of a general partial differential calculus for a generalized kind of function", *Mathematics Magazine*, **56**, 1983, 299-306.

Maddy, P. (1990) *Realism in Mathematics*, Oxford. Univ. Press, Oxford, 1990.

Mäenpää, P. (1993) *The Art of Analysis. Logic and History of Problem Solving*, PhD thesis, University of Helsinki, 1993.

Magnus, W. (1974) *Braid groups. A Survey*, in M. F. Newman (ed. by), *International Conference on the Theory of Groups. Proceedings*, Springer, Berlin, New York, 1974 (SLNM 372), 463-487.

Mahoney, M. (1968) "Another Look at Greek Geometrical Analysis," *Archive for History of Exact Sciences*, **5**, 1968, 318-348.

Mahoney, M. S. (1973) *The mathematical career of Pierre de Fermat (1601-1665)*, Princeton Univ. Press, Princeton (N. J.), 1973.

Martin-Löf, P. (1982) "Constructive Mathematics and Computer Programming", in L. J. Cohen et al., (ed by.), *Proceedings of the Sixth International Congress of Logic, Methodology and Philosophy of Science* (Hannover 1979), North-Holland, Amsterdam, 1982, 153-175.

Martin-Löf, P. (1984) *Intuitionistic Type Theory*, Notes by G. Sambin of a series of lectures given in Padua (June 1980), Bibliopolis, Napoli, 1984.

Mehrtens, H. (1990), *Moderne-Sprache-Mathematik [...]*, Suhrkamp, Frankfurt a. Main, 1990.

Milhaud, G. (1921) *Descartes Savant*, Alcan, Paris, 1921.

Molland, A. G. (1976) "Shifting the Foundations: Descartes's Transformation of Ancient Geometry", *Historia Mathematica*, **3**, 1976, 21-49.

Monge, G. (1799) *Géométrie Descriptive. Leçons données aux écoles normales de l'an 3 de la République*, Baudovin, Paris, an VII.

Monge, G. (LEN) "Leçons de Monge à l'École Normale de l'an III", in Dhombres (1992*a*), 305-453 (with introdutions and notes of B. Belhoste and R. Taton, 267-303 and 455-459).

Monge, G. and Hachette, M. (1802) "Application d'Algèbre à la Géométrie", *Journal de l'Ecole Polytéchnique*, chaier 11 (Messidor, an X: 1802), 143-172.

Montmort, R. (1713) *Essai d'analyse sur les jeux de hazard*, 2nd ed., Quillau, Paris, 1713.

Moore, G. E. (1922) *Philosophical Studies*, Harcourt, Brace, New York, 1922; quot. ed.: Routledge & Kegan Paul, London, 1974.

Morse, J. (1981) *The Reception of Diophantus' Arithmetic in the Renaissance*, Ph.D dissertation, Princeton University, 1981.

Mueller, I. (1981) *Philosophy of Mathematics and Deductive Structure in Euclid's Elements*, MIT Press, Cambridge (Mass.), 1981.

Mugnai, M. (1992) *Leibniz's Theory of Relations*, Steiner, Stuttgart, 1992.

Murphey, M. G. (1961) *The Development of Peirce's Philosophy*, Harvard Univ. Press, Cambridge, 1961.

Nayrīzī, al- (EEC) *Anaritii in decem libros priores Elementorum Euclidis commentarii, ex interpretazione Gherardi Cremonensis [...]*, edidit M. Curtze, supplementum of Euclid (OO), Teubner, Leipzig, 1899.

Neugebauer, O. (1938) "Über eine Methode zur Distanzbestimmung Alexandria-Rom bei Heron", *Kongelige Danske Videnskabernes Selskabs Skriften*, **26**, 2, 1938, 21-24.

Newton, I. (1687) *Philosophiae Naturalis Principia Mathematica*, J. Streater, Londini, 1687; 2nd ed. W. P. Catabrigiæ, 1713; 3rd ed. apud G. & J. Innys, Londini, 1726; English transl of the 3-rd ed. by Andrew Motte (1729), revides. by F. Cajori, Univ. of California Press, Berkeley, 1934.

Newton, I. (1704) *Opticks [...] Also two treatises of the species and Magnitudine of Curvilinear Figures*, S. Smith & B. Walford, London, 1704; ed. quot. (based on the 4rt ed., W. Innys, London 1730), with a foreword by A. Einstein and an introduction by E. Whittaker: Dover Publications, New York, 1952.

Newton, I. (MF) *The method of fluxions and infinite series with its application to the geometry of curve lines*, translated by J. Colson, printed by H. Woodfall, sold by J. Nourse, London 1736.

Newton, I. (MP) *The mathematical papers of Isaac Newton*, ed. by D. T. Whiteside, Cambridge Univ. Press, 1967-1981 (8 vols.).

Nordström, B., Petersson, K. and Smith, J. (1990) *Programming in Martin-Löf's Type Theory—An Introduction*, Oxford Univ. Press, Oxford, 1990.

O'Neill, J. (1991) *Worlds without Content: Against Formalism*, Routledge, London, 1991.

Ore, O. (1948) *Number theory and its history*, McGraw-Hill, New York, 1948.

Otte, M. (1989) "The Ideas of Hermann Grassmann in the Context of the Mathematical and Philosophical Tradition since Leibniz", *Historia Mathematica*, **16**, 1989, 1-35.

Otte, M. (1990) "Arithmetic and Geometry: Some Remarks on the the Concept of Complementary", *Studies in Philosophy and Education*, **10**, 1990, 37-62.

Otte, M. (1992) "Das Prinzip der Kontinuität", *Math. Semesterberichte*, **39**, 1992, 105-125.

Otte, M. (1993) "Kontinuitätsprinzip und Prinzip der Identität des Ununterscheidbaren", *Studia Leibnitiana*, **36**, 1993, 70-89.

Otte, M. (1994*a*) *Das Formale, das Soziale und das Subjektive. Eine Einführung in die Philosophie und Didaktik der Mathematik*, Suhrkamp, Frankfurt a. M., 1994.

Otte, M. (1994*b*) "Intuition and Logic in Mathematics", in D. F. Robitaille, D. H., Wheeler and C. Kieran (ed. by), *Selected Lectures from the 7th International Congress on Mathematical Education*, Les Press de l'Univ. de Laval, Sainte-Foy (Québec), 1994, 271-284.

Otte, M. (1995) "La philosophie des mathématiques de Charles S. Peirce (1839-1914)", in Panza and Pont (1995), 89-108.

Palummbo, M. (1984) *Immaginazione e matematica in Kant*, Laterza, Bari, Roma, 1984.

Panza, M. (1992) "La forma della quantità. Analisi algebrica e analisi superiore: il problema dell'unità della matematica nel secolo dell'illuminismo", *Cahiers d'histoire et philosophie des sciences*, **38-39**, 1992.

Panza, M. (1995*a*) "L'intuition et l'évidence. La philosophie kantienne et les géométries non euclidiennes: relecture d'une discussion", in Panza and Pont (1995), 39-87.

Panza, M. (1995*b*) "Platonisme et intentionnalité", in Panza and Salanskis (1995), 85-132.

Panza, M. (fc) "Quelques distinctions à l'usage de l'historiographie des mathématiques", Proceedings of the Conference, *Herméneutique, textes et sciences*, Cerisy-la-Salle, September 1994 (ed. by F. Rastier and J.-M. Salanskis), forthcoming.

Panza, M. and Pont, J.-C. (1995) (sous la direction de) *Les savants et l'épistémologie vers la fin du XVIIIème siècle*, Blanchard, Paris, 1995.

Panza, M. and Salanskis, J.-M. (1995) (sous la direction de) *L'objectivité mathématique. Platonismes et structures formelles*, Masson, Paris, 1995.

Pappus (CH) *Pappi Alexandrini Collectionis quae supersunt, e lehri manu seriptis edidit, latina interpretatione et commendariis instruxit Fridericus Hultsch [...]*, Weidmann, Berolini, 1876-1878 (3 vols.).

Pappus (CJ) *Book 7 of the Collection [by] Pappus of Alexandria*, edited with [English] transl. and commentary by A. Jones, Springer-Verlag, New York, Heidelberg, 1986.

Pappus (CVE) *La Collection mathématique* (Trad. de P. Ver Eecke), Desclée de Brouwer et C., Paris, Bruges, 1933.

Parrini, P. (1990) "Sulla teoria kantiana della conoscenza: verità, forma, materia", in F. Alessio, E. Garroni, M. Mamiani, V. Mathieu, M. Miglio, M. Mori, and P. Parrini, *Kant. Lezioni di aggiornamento*, Zanichelli, Bologna, 1990, 35-67.

Parsons, C. (1983) *Mathematics in Philosophy*, Cornell Univ. Press, Ithaca (N.Y.), 1983.

Parsons, C. (1995) "Quine and Gödel on Analyticity", in P. Leonardi and M. Santambrogio (ed. by), *On Quine*, Cambridge Univ. Press, Cambridge, 1995, 297-313.

Pasch, M. (1882) *Vorlesungen über neuere Geometrie*, Teubner, Leipzig, 1882.

Pasini, E. (1966) *Corpo e funzioni cognitive in Leibniz*, F. Angeli, Milano, 1996.

Peano, G. (1901) "Les définitions mathématiques", in *Compte rendu du Deuxième Congres International des Mathématiques, Paris 1900*, Gauthier-Villars, Paris 1901 (3 vols.), III, 279-288.

Pécot, J.-B. (1992) *Histoire des relations d'orthogonalité*, thése de Doctorat, Université de Nantes, 1992 (4 vols.).

Peirce, C. S. (1885) "On the Algebra of Logic. A Contribution to the Philosophy of Notation", *American Journal of Mathematics*, **7**, 1885, 180-202; in Peirce (W), V, 162-190.

Peirce, C. S. (CCL) *Reasoning and the Logic of Things. The Cambridge Conferences Lectures of 1898* (ed. by K. L. Ketner, with an introduction by K.L. Ketner and H. Putnam, Harvard Univ. Press, Cambridge, London, 1992.

Peirce, C. S. (CP) *Collected Papers of Charles Sanders Peirce* (ed. by C. Hartshorne and P. Weiß, (vols. I-VI) and A. W. Burks, (vols. VII-VIII), Harvard Univ. Press, Cambridge (Mass.), 1931-1958 (8 vols.).

Peirce, C. S. (Ms) *Manuscript, after Richard S. Robin, Annotated Catalogue of the Papers of Charles S. Peirce*, Univ. of Massachusetts Press, Amherst, 1967.

Peirce, C. S. (NEM) *The New Elements of Mathematics by Charles S. Peirce* (ed. by C. Eisele), Mouton, The Hague, Paris and Humanities Press, Atlantic Highlands (N. J.), 1976 (4 vols.).

Peirce, C. S. (W) *Writings of Charles S. Peirce. A Chronological Edition* (ed. by C. J. W. Kloesel) Indiana Univ. Press, Bloomington, Indianapolis, 1982-1992 (5 vols. appeared containing writings from 1857 to 1886).

Plato (OC) *Œuvres complètes*, Gallimard, Etitions de la Pléiade, Paris, 1950.

Poincaré, H. (1891) "Les géométries non-euclidiennes", *Revue générale des sciences pures et appliquées*, **2**, 1891, 769a-774b; republished in Poincaré (1902), 35-50.

Poincaré, H. (1894) "Sur la nature du raisonnement mathématique", *Revue de Métaphysique et de Morale*, **2**, 1894, 371-384; republished in Poincaré (1902), 1-16.

Poincaré, H. (1902) *La science et l'hypothèse*, Flammarion, Paris, 1902.

Poincaré, H. (1905) *La science et l'hypothèse*, Flammarion, Paris, 1905.

Poincaré, H. (1905) *La valeur de la science*, Flammarion, Paris, 1905.

Pólya (1945) *How to Solve It*, Princeton Univ. Press, Princeton (N. J.), 1945.

Poncelet, J. V. (AAG) *Applications d'analyse et de géométrie qui ont servi, en 1822, de principal fondement au* Traité des propriétés projectives des figures*par J.-V. Poncelet*, éd. par V. M. A. Mannheim et T. F. Moutard, Mallet-Bachelier, Paris, 1862-1864 (2 vols.).

Proclus (PEEL) *Procli diadochi in primum Euclids Elementorum librum, ex recognitione G. Friedlein*, B. G. Teubner, Leipzig, 1873; English translation by G. Morrow, quot.: Princeton Univ. Press, Princeton (N. J.), 1970.

Putnam, H. (1975) *Mathematics, Matter and Method*, Cambridge Univ. Press, Cambridge, London, New York, 1975.

Quine, W. v. O. (1953) *From a Logical Point of View*, Haward Univ. Press, Cambridge (Mass.) 1953.

Quine, W. v. O. (1968) "Onthological Relativity", *The Journal of Philosophy*, **65**, 1968, 185-212.

Rescher, N. (1967) *The Philosophy of Leibniz*, Prentice-Hall, Englewood Cliffs (N. J.), 1967.

Reye, T. (1866-1867) *Die Geometrie der Lage*, C. Rümpler, Hannover, 1866-1867 (2 vols.); 4th ed. verb. und verm., Baumgartner's Buchhanlung, Leipzig, 1899 (3 vols.).

Ross, W. D. (1949) *Aristotle's Prior and Posterior Analytics* (A revised text with introduction and commentary by W. D. Ross), Clarendon Press, Oxford, 1949.

Rossi, P. (1962) *I filosofi e le macchine*, Feltrinelli, Milano, 1962.

Rowe, D. E. (1989) "Klein, Hilbert, and the Göttingen mathematical tradition", *Osiris*, 2nd ser., **5**, 1989, 186-213.

Russell, B. (1903A) *The Principles of Mathematics*, Cambridge Univ. Press, Cambridge, 1903.

Russell, B. (1903*b*) "Recent Works on the Philosophy of Leibniz", *Mind* **13**, 1903, 177-201.

Salanskis, J. - M. (1991) *L'herméneutique formelle*, éd. du CNRS, Paris, 1991.

Salanskis, J.-M. (1995) "Platonisme et philosophie des mathématiques", in Panza and Salanskis (1995), 179-212.

Santambrogio, M. (1992) *Forma e oggetto*, Il Saggiatore, Milano 1992.

Scharlau, W. (1979) "Zur Entstehung der Reinen Mathematik", in *Epistemologische und soziale Probleme der Wissenschaftsentwicklung im frühen 19. Jahrhundert* (ed. by the Institut für Didaktik der Mathematik der Universität Bielefeld), Arbeitstagung im Zentrum für interdisziplinäre Forschung in Bielefeld vom 27-30. Nov. 1979, 267-284.

Schuster, J. (1980) "Descartes' mathesis universalis; 1618-1628", in Gaukroger, S. W., (ed. by) *Descartes; philosophy, mathematics and physics*, Harvester Press, Sussex, 41-96.

Scott, J. F. (1938) *The Mathematical Work of John Wallis, D.D., F.R.S. (1616-1703)*, Taylor and Francis, Ltd. London, 1938.

Scriba, C. J. (1960-1962) "Zur Lösung des 2. Debeaunischen Problems durch Descartes. Ein Abschnitt aus der Frühgeschichte der inversen Tangentenaufgaben", *Archive for History of Exact Sciences*, **1**, 1960-1962, 406-419.

Searle, J. (1992) *The Rediscovery of the Mind*, MIT Press, Cambridge (Mass.), 1992.

Sebestik, J. (1992) *Logique et mathématique chez Bolzano*, Vrin, Paris, 1992.

Sinaceur, H. (1991) *Corps et modèles*, Vrin, Paris, 1991.

Smith, R. (1983) *Aristotle's Prior Analytics* (translation with introduction, notes and commentary by R. Smith), Haeken P. C., Indianapolis, Cambridge, 1983.

Stäckel, P. (1894) "Abhandlungen über Variations-rechnung, erster Theil: Abhandlungen von Joh. Bernoulli (1696), Jac. Bernoulli (1697) und Leonhard Euler, (1744)", *Ostwalds klassiker der exakten Wissenschaft*, 4b, Engelmann, Leipzig, 1894 (contains a partial German translation of Euler (1744): chapters 1, 2, 5 and 6).

Stigler, S. (1986) *The History of Statistics*, Belknap Press of Harvard University Press, Cambridge (Mass.), 1986.

Struik, D. J. (1969) *A Source Book in Mathematics, 1200-1800*, Harvard Univ. Press, Cambridge (Mass.), 1969.

Szabó, A. (1974) "Working backwards and proving by synthesis", in Hintikka and Remes (1974), 118-130.

Taton, R. (1951) *L'Œuvre scientifique de Monge*, P.U.F., Paris, 1951.

Tharp, L. (1989-1991) "Myth and Mathematics: A Conceptualistic Philosophy of Mathematics", *Synthese*, **81**, 1989, 167-201 and **88**, 1991, 179-199.

Thiel, C. (1988) "Begriff und Geschichte der Abstraktion", in K. Prätor (ed. by), *Aspekte der Abstraktionstheorie, Aachener Schriften zur Wissenschaften-Theorie, Logic und Sprachphilosophie*, **2**, 1988, 36-48.

Tieszen, R. (1989) *Mathematical Intuition: Phenomenology and Mathematical Knowledge*, Kluwer, Dordrecht, 1989.

Tieszen, R. (1994) "Mathematical Realism and Gödel's Incompleteness Theorems", *Philosophia Mathematica*, 3-rd ser., **2**, 177-201.

Tieszen, R. (1995) "Mathematics", in B. Smith and D. Smith (ed. by), *Cambridge Companions to Philosophy: Husserl*, Cambridge Univ. Press, Cambridge, 438-462.

Timmermans, B. (1995) *La résolution des problèmes de Descartes à Kant*, P.U.F., Paris, 1995.

Todhunter, I. (1949) *A History of the Mathematical Theory of Probability from the Time of Pascal to that of Laplace*, Chelsea Publishing Company, New York, 1949.

Toepell, M. M. (1986) *Über die Entstehung von David Hilberts 'Grundlagen der Geometrie'*, Vandenhoeck und Ruprecht, Göttingen, 1986.

Trabal, P. (1995) *Le sens commun, les mathématiques et les sciences : une approche de la sociologie des sciences par une étude des représentations sociales des mathématiques et des sciences*, thèse de Doctorat, EHESS, Paris, 1995.

Turing, A. (1936) "On Computable Numbers, with an Application to the Entscheidungsproblem", *Proceedings of the London Mathematical Society*, **42**, 230-265.

Van der Waerden, B. L. (1930-1931) *Moderne Algebra*, unter Benutzung von Vorlesungen von E. Artin und E. Nöther, Springer, Berlin, 1930-1931 (2 vols.); 2nd ed., 1937-1940 (2 vols.).

Varignon, P. (1701) "Des forces centrales, ou des pesanteurs necessaires aux planetes pour leur faire décrire les orbes qu'on leur a supposés jusqu'icy", *Hist. Acad. Roy. Sci. [Paris], Mém. Math. et Phy.*, 1701 (publ. 1703), 218-237.

Varignon, P. (1704) "Nouvelle formation de spirales beaucoup plus différentes entr'elles que tout ce qu'on peut imaginer d'autres courbes quelconques à l'infini; avec les touchantes, les quadratures, les déroulemens, & les longueurs de quelques-unes de ces spirales qu'on donne seulement ici pour éxemples de cette formation générale", *Hist. Acad. Roy. Sci. [Paris], Mém. Math. et Phy.*, 1704 (publ. 1706), 69-131.

Viète, F. (1591*a*) *In artem analyticem Isagoge*, Mettayer, Turonis, 1591; English transl. by J. W. Smith in Klein (1968), 312-353.

Viète, F. (1591*b*) *Zeteticorum libri quinque*, Mettayer, Turonis, 1591.

Viète, F. (1593) *Francisci Vietæ Variorum de rebus mathematicis responsorum, liber VIII. Cuius præcipua capita sunt, De duplicatione Cubi, et Quadratione Circuli. Quæ claudit* próxeiron, *seu Ad Usum Mathematici Canonis Methodica*, Mettayer, Tvronis, 1593; a translation into French, with a commentary is due to appear soon.

Viète, F. (1615) *De æquqtiones recognitione et emendatione tractatus duo*, J. Laquehay, Paris, 1615.

Viète, F. (IV) *Introduction en l'art analytic, ou, Nouvelle algebre de Francois Viete [....]. Traduit en notre langue & commenté & illustré par I. L. sieur de Vaulezard*, I. Jacquin, Paris, 1630; new ed. quot., in the *Corpus des Œuvres de Philosophie en langue française*: [J. L.] Vaulézard, *La nouvelle algèbre de H. Viète*, Fayard, Paris, 1986.

Vuillemin, J. (1960) *Mathématiques et métaphysique chez Descartes*, P.U.F., Paris, 1960.

Wallis, J.(1656) *Arithmetica Infinitorum, sive Nova Methodus Inquirendi in Curvilineorum Quadraturam, aliaque difficiliora Matheseos Problemata*, L. Lichfield, Oxonii, 1656 (in J. Wallis, *Operum Mathematicorum Pars Altera [...]*, L. Lichfield, Oxonii, 1956, separated pagination).

Wang, H. (1987) *Reflections on Kurt Gödel*, MIT Press, Cambridge (Mass.), 1987.

Westfall, R. S. (1980) *Never at Rest A biography of Isaac Newton*, Cambridge Univ. Press, Cambridge, London, 1980.

Weyl, H. (1910) "Ueber die Definitionen der mathematischen Begriffe", *Mathematisch-naturwissenschaftliche Blaetter*, **7**, 1910, 93-95 and 109-113.

Weyl, H. (1913) "Die Idee der Riemannschen Flaeche", Teubner, Leipzig und Berlin, 1913.

Whitehead, A. N. and Russell, B. (1910-1913) *Principia Mathematica*, Cambridge Univ. Press, Cambridge, 1910-1913 (3 vols.).

Whiteside, D. T. (1960-1962) "Patterns of Mathematical Thought in the Later Seventeenth Century", *Archive for History of Exact Sciences*, **1**, 1960-1962, 179-388.

Wiener, N. (1923) "On the Nature of Mathematical Thinking", *Australian Journal of Psychology and Philosophy*, **1**, 1923, pp. 268-272; in *Collected Works*, MIT Press, Cambridge (Mass.), 1976-1985 (4 vols.), vol. I, 234-238.

Wiener, N. (1930) "Generalized Harmonic Analysis", *Acta Mathematica*, **55**, 1930, 117-258.

Wiener, N. (1938) "The historical background of harmony analysis", *Amer. Math. Soc. semicentenial publ.*, **2**, 1938, 56-58.

Woodhouse, R. (1810) *A treatise on isoperimetrical prọblems and the calculus of variations*, Cambridge Univ. Press, Cambridge, 1810.

Wright, C. (1983) *Frege's conceptions of Numbers as Objects*, Aberdeen Univ. Press, Aberdeen, 1983.

Zeuthen, H. G (1886) *Die Lehre von die Kegelschnitten im Altertum*, A. F. Host & Sohn, Kopenhagen, 1896.

Zeuthen, H. G. (1893) *Forelæsning over Mathematikens Historie, Oldtid og Middelalder*, Høst & Søns, Kjøbenhavn; 2-nd ed. revised by O. Neugebauer, 1949.

Zeuthen, H. G. (1896) "Die geometrische Construction als 'Existenzbeweis' in der antiken Geometrie, *Mathematische Annalen*, **47**, 1896, 222-228.

INDEX OF NAMES

A

B

C

Boston Studies in the Philosophy of Science

Editor: Robert S. Cohen, *Boston University*

1. M.W. Wartofsky (ed.): *Proceedings of the Boston Colloquium for the Philosophy of Science, 1961/1962.* [Synthese Library 6] 1963 ISBN 90-277-0021-4
2. R.S. Cohen and M.W. Wartofsky (eds.): *Proceedings of the Boston Colloquium for the Philosophy of Science, 1962/1964.* In Honor of P. Frank. [Synthese Library 10] 1965 ISBN 90-277-9004-0
3. R.S. Cohen and M.W. Wartofsky (eds.): *Proceedings of the Boston Colloquium for the Philosophy of Science, 1964/1966.* In Memory of Norwood Russell Hanson. [Synthese Library 14] 1967 ISBN 90-277-0013-3
4. R.S. Cohen and M.W. Wartofsky (eds.): *Proceedings of the Boston Colloquium for the Philosophy of Science, 1966/1968.* [Synthese Library 18] 1969 ISBN 90-277-0014-1
5. R.S. Cohen and M.W. Wartofsky (eds.): *Proceedings of the Boston Colloquium for the Philosophy of Science, 1966/1968.* [Synthese Library 19] 1969 ISBN 90-277-0015-X
6. R.S. Cohen and R.J. Seeger (eds.): *Ernst Mach, Physicist and Philosopher.* [Synthese Library 27] 1970 ISBN 90-277-0016-8
7. M. Čapek: *Bergson and Modern Physics.* A Reinterpretation and Re-evaluation. [Synthese Library 37] 1971 ISBN 90-277-0186-5
8. R.C. Buck and R.S. Cohen (eds.): *PSA 1970.* Proceedings of the 2nd Biennial Meeting of the Philosophy and Science Association (Boston, Fall 1970). In Memory of Rudolf Carnap. [Synthese Library 39] 1971 ISBN 90-277-0187-3; Pb 90-277-0309-4
9. A.A. Zinov'ev: *Foundations of the Logical Theory of Scientific Knowledge (Complex Logic).* Translated from Russian. Revised and enlarged English Edition, with an Appendix by G.A. Smirnov, E.A. Sidorenko, A.M. Fedina and L.A. Bobrova. [Synthese Library 46] 1973 ISBN 90-277-0193-8; Pb 90-277-0324-8
10. L. Tondl: *Scientific Procedures.* A Contribution Concerning the Methodological Problems of Scientific Concepts and Scientific Explanation.Translated from Czech. [Synthese Library 47] 1973 ISBN 90-277-0147-4; Pb 90-277-0323-X
11. R.J. Seeger and R.S. Cohen (eds.): *Philosophical Foundations of Science.* Proceedings of Section L, 1969, American Association for the Advancement of Science. [Synthese Library 58] 1974 ISBN 90-277-0390-6; Pb 90-277-0376-0
12. A. Grünbaum: *Philosophical Problems of Space and Times.* 2nd enlarged ed. [Synthese Library 55] 1973 ISBN 90-277-0357-4; Pb 90-277-0358-2
13. R.S. Cohen and M.W. Wartofsky (eds.): *Logical and Epistemological Studies in Contemporary Physics.* Proceedings of the Boston Colloquium for the Philosophy of Science, 1969/72, Part I. [Synthese Library 59] 1974 ISBN 90-277-0391-4; Pb 90-277-0377-9
14. R.S. Cohen and M.W. Wartofsky (eds.): *Methodological and Historical Essays in the Natural and Social Sciences.* Proceedings of the Boston Colloquium for the Philosophy of Science, 1969/72, Part II. [Synthese Library 60] 1974 ISBN 90-277-0392-2; Pb 90-277-0378-7
15. R.S. Cohen, J.J. Stachel and M.W. Wartofsky (eds.): *For Dirk Struik.* Scientific, Historical and Political Essays in Honor of Dirk J. Struik. [Synthese Library 61] 1974 ISBN 90-277-0393-0; Pb 90-277-0379-5
16. N. Geschwind: *Selected Papers on Language and the Brains.* [Synthese Library 68] 1974 ISBN 90-277-0262-4; Pb 90-277-0263-2
17. B.G. Kuznetsov: *Reason and Being.* Translated from Russian. Edited by C.R. Fawcett and R.S. Cohen. 1987 ISBN 90-277-2181-5

18. P. Mittelstaedt: *Philosophical Problems of Modern Physics*. Translated from the revised 4th German edition by W. Riemer and edited by R.S. Cohen. [Synthese Library 95] 1976
ISBN 90-277-0285-3; Pb 90-277-0506-2
19. H. Mehlberg: *Time, Causality, and the Quantum Theory*. Studies in the Philosophy of Science. Vol. I: *Essay on the Causal Theory of Time*. Vol. II: *Time in a Quantized Universe*. Translated from French. Edited by R.S. Cohen. 1980
Vol. I: ISBN 90-277-0721-9; Pb 90-277-1074-0
Vol. II: ISBN 90-277-1075-9; Pb 90-277-1076-7
20. K.F. Schaffner and R.S. Cohen (eds.): *PSA 1972*. Proceedings of the 3rd Biennial Meeting of the Philosophy of Science Association (Lansing, Michigan, Fall 1972). [Synthese Library 64] 1974 ISBN 90-277-0408-2; Pb 90-277-0409-0
21. R.S. Cohen and J.J. Stachel (eds.): *Selected Papers of Léon Rosenfeld*. [Synthese Library 100] 1979 ISBN 90-277-0651-4; Pb 90-277-0652-2
22. M. Čapek (ed.): *The Concepts of Space and Time*. Their Structure and Their Development. [Synthese Library 74] 1976 ISBN 90-277-0355-8; Pb 90-277-0375-2
23. M. Grene: *The Understanding of Nature*. Essays in the Philosophy of Biology. [Synthese Library 66] 1974 ISBN 90-277-0462-7; Pb 90-277-0463-5
24. D. Ihde: *Technics and Praxis*. A Philosophy of Technology. [Synthese Library 130] 1979
ISBN 90-277-0953-X; Pb 90-277-0954-8
25. J. Hintikka and U. Remes: *The Method of Analysis*. Its Geometrical Origin and Its General Significance. [Synthese Library 75] 1974 ISBN 90-277-0532-1; Pb 90-277-0543-7
26. J.E. Murdoch and E.D. Sylla (eds.): *The Cultural Context of Medieval Learning*. Proceedings of the First International Colloquium on Philosophy, Science, and Theology in the Middle Ages, 1973. [Synthese Library 76] 1975 ISBN 90-277-0560-7; Pb 90-277-0587-9
27. M. Grene and E. Mendelsohn (eds.): *Topics in the Philosophy of Biology*. [Synthese Library 84] 1976 ISBN 90-277-0595-X; Pb 90-277-0596-8
28. J. Agassi: *Science in Flux*. [Synthese Library 80] 1975
ISBN 90-277-0584-4; Pb 90-277-0612-3
29. J.J. Wiatr (ed.): *Polish Essays in the Methodology of the Social Sciences*. [Synthese Library 131] 1979 ISBN 90-277-0723-5; Pb 90-277-0956-4
30. P. Janich: *Protophysics of Time*. Constructive Foundation and History of Time Measurement. Translated from German. 1985 ISBN 90-277-0724-3
31. R.S. Cohen and M.W. Wartofsky (eds.): *Language, Logic, and Method*. 1983
ISBN 90-277-0725-1
32. R.S. Cohen, C.A. Hooker, A.C. Michalos and J.W. van Evra (eds.): *PSA 1974*. Proceedings of the 4th Biennial Meeting of the Philosophy of Science Association. [Synthese Library 101] 1976 ISBN 90-277-0647-6; Pb 90-277-0648-4
33. G. Holton and W.A. Blanpied (eds.): *Science and Its Public*. The Changing Relationship. [Synthese Library 96] 1976 ISBN 90-277-0657-3; Pb 90-277-0658-1
34. M.D. Grmek, R.S. Cohen and G. Cimino (eds.): *On Scientific Discovery*. The 1977 Erice Lectures. 1981 ISBN 90-277-1122-4; Pb 90-277-1123-2
35. S. Amsterdamski: *Between Experience and Metaphysics*. Philosophical Problems of the Evolution of Science. Translated from Polish. [Synthese Library 77] 1975
ISBN 90-277-0568-2; Pb 90-277-0580-1
36. M. Marković and G. Petrović (eds.): *Praxis*. Yugoslav Essays in the Philosophy and Methodology of the Social Sciences. [Synthese Library 134] 1979
ISBN 90-277-0727-8; Pb 90-277-0968-8

37. H. von Helmholtz: *Epistemological Writings*. The Paul Hertz / Moritz Schlick Centenary Edition of 1921. Translated from German by M.F. Lowe. Edited with an Introduction and Bibliography by R.S. Cohen and Y. Elkana. [Synthese Library 79] 1977
ISBN 90-277-0290-X; Pb 90-277-0582-8
38. R.M. Martin: *Pragmatics, Truth and Language*. 1979
ISBN 90-277-0992-0; Pb 90-277-0993-9
39. R.S. Cohen, P.K. Feyerabend and M.W. Wartofsky (eds.): *Essays in Memory of Imre Lakatos*. [Synthese Library 99] 1976 ISBN 90-277-0654-9; Pb 90-277-0655-7
40. Not published.
41. Not published.
42. H.R. Maturana and F.J. Varela: *Autopoiesis and Cognition*. The Realization of the Living. With a Preface to 'Autopoiesis' by S. Beer. 1980
ISBN 90-277-1015-5; Pb 90-277-1016-3
43. A. Kasher (ed.): *Language in Focus: Foundations, Methods and Systems*. Essays in Memory of Yehoshua Bar-Hillel. [Synthese Library 89] 1976
ISBN 90-277-0644-1; Pb 90-277-0645-X
44. T.D. Thao: *Investigations into the Origin of Language and Consciousness*. 1984
ISBN 90-277-0827-4
45. Not published.
46. P.L. Kapitza: *Experiment, Theory, Practice*. Articles and Addresses. Edited by R.S. Cohen. 1980 ISBN 90-277-1061-9; Pb 90-277-1062-7
47. M.L. Dalla Chiara (ed.): *Italian Studies in the Philosophy of Science*. 1981
ISBN 90-277-0735-9; Pb 90-277-1073-2
48. M.W. Wartofsky: *Models*. Representation and the Scientific Understanding. [Synthese Library 129] 1979 ISBN 90-277-0736-7; Pb 90-277-0947-5
49. T.D. Thao: *Phenomenology and Dialectical Materialism*. Edited by R.S. Cohen. 1986
ISBN 90-277-0737-5
50. Y. Fried and J. Agassi: *Paranoia*. A Study in Diagnosis. [Synthese Library 102] 1976
ISBN 90-277-0704-9; Pb 90-277-0705-7
51. K.H. Wolff: *Surrender and Cath*. Experience and Inquiry Today. [Synthese Library 105] 1976 ISBN 90-277-0758-8; Pb 90-277-0765-0
52. K. Kosík: *Dialectics of the Concrete*. A Study on Problems of Man and World. 1976
ISBN 90-277-0761-8; Pb 90-277-0764-2
53. N. Goodman: *The Structure of Appearance*. [Synthese Library 107] 1977
ISBN 90-277-0773-1; Pb 90-277-0774-X
54. H.A. Simon: *Models of Discovery* and Other Topics in the Methods of Science. [Synthese Library 114] 1977 ISBN 90-277-0812-6; Pb 90-277-0858-4
55. M. Lazerowitz: *The Language of Philosophy*. Freud and Wittgenstein. [Synthese Library 117] 1977 ISBN 90-277-0826-6; Pb 90-277-0862-2
56. T. Nickles (ed.): *Scientific Discovery, Logic, and Rationality*. 1980
ISBN 90-277-1069-4; Pb 90-277-1070-8
57. J. Margolis: *Persons and Mind*. The Prospects of Nonreductive Materialism. [Synthese Library 121] 1978 ISBN 90-277-0854-1; Pb 90-277-0863-0
58. G. Radnitzky and G. Andersson (eds.): *Progress and Rationality in Science*. [Synthese Library 125] 1978 ISBN 90-277-0921-1; Pb 90-277-0922-X
59. G. Radnitzky and G. Andersson (eds.): *The Structure and Development of Science*. [Synthese Library 136] 1979 ISBN 90-277-0994-7; Pb 90-277-0995-5

60. T. Nickles (ed.): *Scientific Discovery.* Case Studies. 1980 ISBN 90-277-1092-9; Pb 90-277-1093-7
61. M.A. Finocchiaro: *Galileo and the Art of Reasoning.* Rhetorical Foundation of Logic and Scientific Method. 1980 ISBN 90-277-1094-5; Pb 90-277-1095-3
62. W.A. Wallace: *Prelude to Galileo.* Essays on Medieval and 16th-Century Sources of Galileo's Thought. 1981 ISBN 90-277-1215-8; Pb 90-277-1216-6
63. F. Rapp: *Analytical Philosophy of Technology.* Translated from German. 1981 ISBN 90-277-1221-2; Pb 90-277-1222-0
64. R.S. Cohen and M.W. Wartofsky (eds.): *Hegel and the Sciences.* 1984 ISBN 90-277-0726-X
65. J. Agassi: *Science and Society.* Studies in the Sociology of Science. 1981 ISBN 90-277-1244-1; Pb 90-277-1245-X
66. L. Tondl: *Problems of Semantics.* A Contribution to the Analysis of the Language of Science. Translated from Czech. 1981 ISBN 90-277-0148-2; Pb 90-277-0316-7
67. J. Agassi and R.S. Cohen (eds.): *Scientific Philosophy Today.* Essays in Honor of Mario Bunge. 1982 ISBN 90-277-1262-X; Pb 90-277-1263-8
68. W. Krajewski (ed.): *Polish Essays in the Philosophy of the Natural Sciences.* Translated from Polish and edited by R.S. Cohen and C.R. Fawcett. 1982 ISBN 90-277-1286-7; Pb 90-277-1287-5
69. J.H. Fetzer: *Scientific Knowledge.* Causation, Explanation and Corroboration. 1981 ISBN 90-277-1335-9; Pb 90-277-1336-7
70. S. Grossberg: *Studies of Mind and Brain.* Neural Principles of Learning, Perception, Development, Cognition, and Motor Control. 1982 ISBN 90-277-1359-6; Pb 90-277-1360-X
71. R.S. Cohen and M.W. Wartofsky (eds.): *Epistemology, Methodology, and the Social Sciences.* 1983. ISBN 90-277-1454-1
72. K. Berka: *Measurement.* Its Concepts, Theories and Problems. Translated from Czech. 1983 ISBN 90-277-1416-9
73. G.L. Pandit: *The Structure and Growth of Scientific Knowledge.* A Study in the Methodology of Epistemic Appraisal. 1983 ISBN 90-277-1434-7
74. A.A. Zinov'ev: *Logical Physics.* Translated from Russian. Edited by R.S. Cohen. 1983 [*see also* Volume 9] ISBN 90-277-0734-0
75. G-G. Granger: *Formal Thought and the Sciences of Man.* Translated from French. With and Introduction by A. Rosenberg. 1983 ISBN 90-277-1524-6
76. R.S. Cohen and L. Laudan (eds.): *Physics, Philosophy and Psychoanalysis.* Essays in Honor of Adolf Grünbaum. 1983 ISBN 90-277-1533-5
77. G. Böhme, W. van den Daele, R. Hohlfeld, W. Krohn and W. Schäfer: *Finalization in Science.* The Social Orientation of Scientific Progress. Translated from German. Edited by W. Schäfer. 1983 ISBN 90-277-1549-1
78. D. Shapere: *Reason and the Search for Knowledge.* Investigations in the Philosophy of Science. 1984 ISBN 90-277-1551-3; Pb 90-277-1641-2
79. G. Andersson (ed.): *Rationality in Science and Politics.* Translated from German. 1984 ISBN 90-277-1575-0; Pb 90-277-1953-5
80. P.T. Durbin and F. Rapp (eds.): *Philosophy and Technology.* [*Also* Philosophy and Technology Series, Vol. 1] 1983 ISBN 90-277-1576-9
81. M. Marković: *Dialectical Theory of Meaning.* Translated from Serbo-Croat. 1984 ISBN 90-277-1596-3

Boston Studies in the Philosophy of Science

82. R.S. Cohen and M.W. Wartofsky (eds.): *Physical Sciences and History of Physics.* 1984. ISBN 90-277-1615-3
83. É. Meyerson: *The Relativistic Deduction.* Epistemological Implications of the Theory of Relativity. Translated from French. With a Review by Albert Einstein and an Introduction by Milič Čapek. 1985 ISBN 90-277-1699-4
84. R.S. Cohen and M.W. Wartofsky (eds.): *Methodology, Metaphysics and the History of Science.* In Memory of Benjamin Nelson. 1984 ISBN 90-277-1711-7
85. G. Tamás: *The Logic of Categories.* Translated from Hungarian. Edited by R.S. Cohen. 1986 ISBN 90-277-1742-7
86. S.L. de C. Fernandes: *Foundations of Objective Knowledge.* The Relations of Popper's Theory of Knowledge to That of Kant. 1985 ISBN 90-277-1809-1
87. R.S. Cohen and T. Schnelle (eds.): *Cognition and Fact.* Materials on Ludwik Fleck. 1986 ISBN 90-277-1902-0
88. G. Freudenthal: *Atom and Individual in the Age of Newton.* On the Genesis of the Mechanistic World View. Translated from German. 1986 ISBN 90-277-1905-5
89. A. Donagan, A.N. Perovich Jr and M.V. Wedin (eds.): *Human Nature and Natural Knowledge.* Essays presented to Marjorie Grene on the Occasion of Her 75th Birthday. 1986 ISBN 90-277-1974-8
90. C. Mitcham and A. Hunning (eds.): *Philosophy and Technology II.* Information Technology and Computers in Theory and Practice. [*Also* Philosophy and Technology Series, Vol. 2] 1986 ISBN 90-277-1975-6
91. M. Grene and D. Nails (eds.): *Spinoza and the Sciences.* 1986 ISBN 90-277-1976-4
92. S.P. Turner: *The Search for a Methodology of Social Science.* Durkheim, Weber, and the 19th-Century Problem of Cause, Probability, and Action. 1986. ISBN 90-277-2067-3
93. I.C. Jarvie: *Thinking about Society.* Theory and Practice. 1986 ISBN 90-277-2068-1
94. E. Ullmann-Margalit (ed.): *The Kaleidoscope of Science.* The Israel Colloquium: Studies in History, Philosophy, and Sociology of Science, Vol. 1. 1986 ISBN 90-277-2158-0; Pb 90-277-2159-9
95. E. Ullmann-Margalit (ed.): *The Prism of Science.* The Israel Colloquium: Studies in History, Philosophy, and Sociology of Science, Vol. 2. 1986 ISBN 90-277-2160-2; Pb 90-277-2161-0
96. G. Márkus: *Language and Production.* A Critique of the Paradigms. Translated from French. 1986 ISBN 90-277-2169-6
97. F. Amrine, F.J. Zucker and H. Wheeler (eds.): *Goethe and the Sciences: A Reappraisal.* 1987 ISBN 90-277-2265-X; Pb 90-277-2400-8
98. J.C. Pitt and M. Pera (eds.): *Rational Changes in Science.* Essays on Scientific Reasoning. Translated from Italian. 1987 ISBN 90-277-2417-2
99. O. Costa de Beauregard: *Time, the Physical Magnitude.* 1987 ISBN 90-277-2444-X
100. A. Shimony and D. Nails (eds.): *Naturalistic Epistemology.* A Symposium of Two Decades. 1987 ISBN 90-277-2337-0
101. N. Rotenstreich: *Time and Meaning in History.* 1987 ISBN 90-277-2467-9
102. D.B. Zilberman: *The Birth of Meaning in Hindu Thought.* Edited by R.S. Cohen. 1988 ISBN 90-277-2497-0
103. T.F. Glick (ed.): *The Comparative Reception of Relativity.* 1987 ISBN 90-277-2498-9
104. Z. Harris, M. Gottfried, T. Ryckman, P. Mattick Jr, A. Daladier, T.N. Harris and S. Harris: *The Form of Information in Science.* Analysis of an Immunology Sublanguage. With a Preface by Hilary Putnam. 1989 ISBN 90-277-2516-0

105. F. Burwick (ed.): *Approaches to Organic Form.* Permutations in Science and Culture. 1987 ISBN 90-277-2541-1
106. M. Almási: *The Philosophy of Appearances.* Translated from Hungarian. 1989 ISBN 90-277-2150-5
107. S. Hook, W.L. O'Neill and R. O'Toole (eds.): *Philosophy, History and Social Action.* Essays in Honor of Lewis Feuer. With an Autobiographical Essay by L. Feuer. 1988 ISBN 90-277-2644-2
108. I. Hronszky, M. Fehér and B. Dajka: *Scientific Knowledge Socialized.* Selected Proceedings of the 5th Joint International Conference on the History and Philosophy of Science organized by the IUHPS (Veszprém, Hungary, 1984). 1988 ISBN 90-277-2284-6
109. P. Tillers and E.D. Green (eds.): *Probability and Inference in the Law of Evidence.* The Uses and Limits of Bayesianism. 1988 ISBN 90-277-2689-2
110. E. Ullmann-Margalit (ed.): *Science in Reflection.* The Israel Colloquium: Studies in History, Philosophy, and Sociology of Science, Vol. 3. 1988 ISBN 90-277-2712-0; Pb 90-277-2713-9
111. K. Gavroglu, Y. Goudaroulis and P. Nicolacopoulos (eds.): *Imre Lakatos and Theories of Scientific Change.* 1989 ISBN 90-277-2766-X
112. B. Glassner and J.D. Moreno (eds.): *The Qualitative-Quantitative Distinction in the Social Sciences.* 1989 ISBN 90-277-2829-1
113. K. Arens: *Structures of Knowing.* Psychologies of the 19th Century. 1989 ISBN 0-7923-0009-2
114. A. Janik: *Style, Politics and the Future of Philosophy.* 1989 ISBN 0-7923-0056-4
115. F. Amrine (ed.): *Literature and Science as Modes of Expression.* With an Introduction by S. Weininger. 1989 ISBN 0-7923-0133-1
116. J.R. Brown and J. Mittelstrass (eds.): *An Intimate Relation.* Studies in the History and Philosophy of Science. Presented to Robert E. Butts on His 60th Birthday. 1989 ISBN 0-7923-0169-2
117. F. D'Agostino and I.C. Jarvie (eds.): *Freedom and Rationality.* Essays in Honor of John Watkins. 1989 ISBN 0-7923-0264-8
118. D. Zolo: *Reflexive Epistemology.* The Philosophical Legacy of Otto Neurath. 1989 ISBN 0-7923-0320-2
119. M. Kearn, B.S. Philips and R.S. Cohen (eds.): *Georg Simmel and Contemporary Sociology.* 1989 ISBN 0-7923-0407-1
120. T.H. Levere and W.R. Shea (eds.): *Nature, Experiment and the Science.* Essays on Galileo and the Nature of Science. In Honour of Stillman Drake. 1989 ISBN 0-7923-0420-9
121. P. Nicolacopoulos (ed.): *Greek Studies in the Philosophy and History of Science.* 1990 ISBN 0-7923-0717-8
122. R. Cooke and D. Costantini (eds.): *Statistics in Science.* The Foundations of Statistical Methods in Biology, Physics and Economics. 1990 ISBN 0-7923-0797-6
123. P. Duhem: *The Origins of Statics.* Translated from French by G.F. Leneaux, V.N. Vagliente and G.H. Wagner. With an Introduction by S.L. Jaki. 1991 ISBN 0-7923-0898-0
124. H. Kamerlingh Onnes: *Through Measurement to Knowledge.* The Selected Papers, 1853-1926. Edited and with an Introduction by K. Gavroglu and Y. Goudaroulis. 1991 ISBN 0-7923-0825-5
125. M. Čapek: *The New Aspects of Time: Its Continuity and Novelties.* Selected Papers in the Philosophy of Science. 1991 ISBN 0-7923-0911-1

Boston Studies in the Philosophy of Science

126. S. Unguru (ed.): *Physics, Cosmology and Astronomy, 1300-1700.* Tension and Accommodation. 1991 ISBN 0-7923-1022-5
127. Z. Bechler: *Newton's Physics on the Conceptual Structure of the Scientific Revolution.* 1991 ISBN 0-7923-1054-3
128. É. Meyerson: *Explanation in the Sciences.* Translated from French by M-A. Siple and D.A. Siple. 1991 ISBN 0-7923-1129-9
129. A.I. Tauber (ed.): *Organism and the Origins of Self.* 1991 ISBN 0-7923-1185-X
130. F.J. Varela and J-P. Dupuy (eds.): *Understanding Origins.* Contemporary Views on the Origin of Life, Mind and Society. 1992 ISBN 0-7923-1251-1
131. G.L. Pandit: *Methodological Variance.* Essays in Epistemological Ontology and the Methodology of Science. 1991 ISBN 0-7923-1263-5
132. G. Munévar (ed.): *Beyond Reason.* Essays on the Philosophy of Paul Feyerabend. 1991 ISBN 0-7923-1272-4
133. T.E. Uebel (ed.): *Rediscovering the Forgotten Vienna Circle.* Austrian Studies on Otto Neurath and the Vienna Circle. Partly translated from German. 1991 ISBN 0-7923-1276-7
134. W.R. Woodward and R.S. Cohen (eds.): *World Views and Scientific Discipline Formation.* Science Studies in the [former] German Democratic Republic. Partly translated from German by W.R. Woodward. 1991 ISBN 0-7923-1286-4
135. P. Zambelli: *The Speculum Astronomiae and Its Enigma.* Astrology, Theology and Science in Albertus Magnus and His Contemporaries. 1992 ISBN 0-7923-1380-1
136. P. Petitjean, C. Jami and A.M. Moulin (eds.): *Science and Empires.* Historical Studies about Scientific Development and European Expansion. ISBN 0-7923-1518-9
137. W.A. Wallace: *Galileo's Logic of Discovery and Proof.* The Background, Content, and Use of His Appropriated Treatises on Aristotle's *Posterior Analytics.* 1992 ISBN 0-7923-1577-4
138. W.A. Wallace: *Galileo's Logical Treatises.* A Translation, with Notes and Commentary, of His Appropriated Latin Questions on Aristotle's *Posterior Analytics.* 1992 ISBN 0-7923-1578-2
Set (137 + 138) ISBN 0-7923-1579-0
139. M.J. Nye, J.L. Richards and R.H. Stuewer (eds.): *The Invention of Physical Science.* Intersections of Mathematics, Theology and Natural Philosophy since the Seventeenth Century. Essays in Honor of Erwin N. Hiebert. 1992 ISBN 0-7923-1753-X
140. G. Corsi, M.L. dalla Chiara and G.C. Ghirardi (eds.): *Bridging the Gap: Philosophy, Mathematics and Physics.* Lectures on the Foundations of Science. 1992 ISBN 0-7923-1761-0
141. C.-H. Lin and D. Fu (eds.): *Philosophy and Conceptual History of Science in Taiwan.* 1992 ISBN 0-7923-1766-1
142. S. Sarkar (ed.): *The Founders of Evolutionary Genetics.* A Centenary Reappraisal. 1992 ISBN 0-7923-1777-7
143. J. Blackmore (ed.): *Ernst Mach – A Deeper Look.* Documents and New Perspectives. 1992 ISBN 0-7923-1853-6
144. P. Kroes and M. Bakker (eds.): *Technological Development and Science in the Industrial Age.* New Perspectives on the Science–Technology Relationship. 1992 ISBN 0-7923-1898-6
145. S. Amsterdamski: *Between History and Method.* Disputes about the Rationality of Science. 1992 ISBN 0-7923-1941-9
146. E. Ullmann-Margalit (ed.): *The Scientific Enterprise.* The Bar-Hillel Colloquium: Studies in History, Philosophy, and Sociology of Science, Volume 4. 1992 ISBN 0-7923-1992-3

147. L. Embree (ed.): *Metaarchaeology.* Reflections by Archaeologists and Philosophers. 1992 ISBN 0-7923-2023-9
148. S. French and H. Kamminga (eds.): *Correspondence, Invariance and Heuristics.* Essays in Honour of Heinz Post. 1993 ISBN 0-7923-2085-9
149. M. Bunzl: *The Context of Explanation.* 1993 ISBN 0-7923-2153-7
150. I.B. Cohen (ed.): *The Natural Sciences and the Social Sciences.* Some Critical and Historical Perspectives. 1994 ISBN 0-7923-2223-1
151. K. Gavroglu, Y. Christianidis and E. Nicolaidis (eds.): *Trends in the Historiography of Science.* 1994 ISBN 0-7923-2255-X
152. S. Poggi and M. Bossi (eds.): *Romanticism in Science.* Science in Europe, 1790–1840. 1994 ISBN 0-7923-2336-X
153. J. Faye and H.J. Folse (eds.): *Niels Bohr and Contemporary Philosophy.* 1994 ISBN 0-7923-2378-5
154. C.C. Gould and R.S. Cohen (eds.): *Artifacts, Representations, and Social Practice.* Essays for Marx W. Wartofsky. 1994 ISBN 0-7923-2481-1
155. R.E. Butts: *Historical Pragmatics.* Philosophical Essays. 1993 ISBN 0-7923-2498-6
156. R. Rashed: *The Development of Arabic Mathematics: Between Arithmetic and Algebra.* Translated from French by A.F.W. Armstrong. 1994 ISBN 0-7923-2565-6
157. I. Szumilewicz-Lachman (ed.): *Zygmunt Zawirski: His Life and Work.* With Selected Writings on Time, Logic and the Methodology of Science. Translations by Feliks Lachman. Ed. by R.S. Cohen, with the assistance of B. Bergo. 1994 ISBN 0-7923-2566-4
158. S.N. Haq: *Names, Natures and Things.* The Alchemist Jābir ibn Ḥayyān and His *Kitāb al-Aḥjār* (Book of Stones). 1994 ISBN 0-7923-2587-7
159. P. Plaass: *Kant's Theory of Natural Science.* Translation, Analytic Introduction and Commentary by Alfred E. and Maria G. Miller. 1994 ISBN 0-7923-2750-0
160. J. Misiek (ed.): *The Problem of Rationality in Science and its Philosophy.* On Popper vs. Polanyi. The Polish Conferences 1988–89. 1995 ISBN 0-7923-2925-2
161. I.C. Jarvie and N. Laor (eds.): *Critical Rationalism, Metaphysics and Science.* Essays for Joseph Agassi, Volume I. 1995 ISBN 0-7923-2960-0
162. I.C. Jarvie and N. Laor (eds.): *Critical Rationalism, the Social Sciences and the Humanities.* Essays for Joseph Agassi, Volume II. 1995 ISBN 0-7923-2961-9
     Set (161–162) ISBN 0-7923-2962-7
163. K. Gavroglu, J. Stachel and M.W. Wartofsky (eds.): *Physics, Philosophy, and the Scientific Community.* Essays in the Philosophy and History of the Natural Sciences and Mathematics. In Honor of Robert S. Cohen. 1995 ISBN 0-7923-2988-0
164. K. Gavroglu, J. Stachel and M.W. Wartofsky (eds.): *Science, Politics and Social Practice.* Essays on Marxism and Science, Philosophy of Culture and the Social Sciences. In Honor of Robert S. Cohen. 1995 ISBN 0-7923-2989-9
165. K. Gavroglu, J. Stachel and M.W. Wartofsky (eds.): *Science, Mind and Art.* Essays on Science and the Humanistic Understanding in Art, Epistemology, Religion and Ethics. Essays in Honor of Robert S. Cohen. 1995 ISBN 0-7923-2990-2
     Set (163–165) ISBN 0-7923-2991-0
166. K.H. Wolff: *Transformation in the Writing.* A Case of Surrender-and-Catch. 1995 ISBN 0-7923-3178-8
167. A.J. Kox and D.M. Siegel (eds.): *No Truth Except in the Details.* Essays in Honor of Martin J. Klein. 1995 ISBN 0-7923-3195-8

168. J. Blackmore: *Ludwig Boltzmann, His Later Life and Philosophy, 1900–1906.* Book One: A Documentary History. 1995 ISBN 0-7923-3231-8
169. R.S. Cohen, R. Hilpinen and R. Qiu (eds.): *Realism and Anti-Realism in the Philosophy of Science.* Beijing International Conference, 1992. 1996 ISBN 0-7923-3233-4
170. I. Kuçuradi and R.S. Cohen (eds.): *The Concept of Knowledge.* The Ankara Seminar. 1995 ISBN 0-7923-3241-5
171. M.A. Grodin (ed.): *Meta Medical Ethics*: The Philosophical Foundations of Bioethics. 1995 ISBN 0-7923-3344-6
172. S. Ramirez and R.S. Cohen (eds.): *Mexican Studies in the History and Philosophy of Science.* 1995 ISBN 0-7923-3462-0
173. C. Dilworth: *The Metaphysics of Science.* An Account of Modern Science in Terms of Principles, Laws and Theories. 1995 ISBN 0-7923-3693-3
174. J. Blackmore: *Ludwig Boltzmann, His Later Life and Philosophy, 1900–1906* Book Two: The Philosopher. 1995 ISBN 0-7923-3464-7
175. P. Damerow: *Abstraction and Representation.* Essays on the Cultural Evolution of Thinking. 1996 ISBN 0-7923-3816-2
176. G. Tarozzi (ed.): *Karl Popper, Philosopher of Science.* (in prep.)
177. M. Marion and R.S. Cohen (eds.): *Québec Studies in the Philosophy of Science.* Part I: Logic, Mathematics, Physics and History of Science. Essays in Honor of Hugues Leblanc. 1995 ISBN 0-7923-3559-7
178. M. Marion and R.S. Cohen (eds.): *Québec Studies in the Philosophy of Science.* Part II: Biology, Psychology, Cognitive Science and Economics. Essays in Honor of Hugues Leblanc. 1996 ISBN 0-7923-3560-0
     Set (177–178) ISBN 0-7923-3561-9
179. Fan Dainian and R.S. Cohen (eds.): *Chinese Studies in the History and Philosophy of Science and Technology.* 1996 ISBN 0-7923-3463-9
180. P. Forman and J.M. Sánchez-Ron (eds.): *National Military Establishments and the Advancement of Science and Technology.* Studies in 20th Century History. 1996 ISBN 0-7923-3541-4
181. E.J. Post: *Quantum Reprogramming.* Ensembles and Single Systems: A Two-Tier Approach to Quantum Mechanics. 1995 ISBN 0-7923-3565-1
182. A.I. Tauber (ed.): *The Elusive Synthesis: Aesthetics and Science.* 1996 ISBN 0-7923-3904-5
183. S. Sarkar (ed.): *The Philosophy and History of Molecular Biology: New Perspectives.* 1996 ISBN 0-7923-3947-9
184. J.T. Cushing, A. Fine and S. Goldstein (eds.): *Bohmian Mechanics and Quantum Theory: An Appraisal.* 1996 ISBN 0-7923-4028-0
185. K. Michalski: *Logic and Time.* An Essay on Husserl's Theory of Meaning. 1996 ISBN 0-7923-4082-5
186. G. Munévar (ed.): *Spanish Studies in the Philosophy of Science.* 1996 ISBN 0-7923-4147-3
187. G. Schubring (ed.): *Hermann Günther Graßmann (1809–1877): Visionary Mathematician, Scientist and Neohumanist Scholar.* Papers from a Sesquicentennial Conference. 1996 ISBN 0-7923-4261-5
188. M. Bitbol: *Schrödinger's Philosophy of Quantum Mechanics.* 1996 ISBN 0-7923-4266-6
189. J. Faye, U. Scheffler and M. Urchs (eds.): *Perspectives on Time.* 1997 ISBN 0-7923-4330-1
190. K. Lehrer and J.C. Marek (eds.): *Austrian Philosophy Past and Present.* Essays in Honor of Rudolf Haller. 1996 ISBN 0-7923-4347-6

Boston Studies in the Philosophy of Science

191. J.L. Lagrange: *Analytical Mechanics*. Translated and edited by Auguste Boissonade and Victor N. Vagliente. Translated from the *Mécanique Analytique, novelle édition* of 1811. 1997 ISBN 0-7923-4349-2
192. D. Ginev and R.S. Cohen (eds.): *Issues and Images in the Philosophy of Science*. Scientific and Philosophical Essays for Azarya Polikarov. 1997 ISBN 0-7923-4444-8
193. R.S. Cohen, M. Horne and J. Stachel (eds.): *Experimental Metaphysics*. Quantum Mechanical Studies for Abner Shimony, Volume One. 1997 ISBN 0-7923-4452-9
194. R.S. Cohen, M. Horne and J. Stachel (eds.): *Potentiality, Entanglement and Passion-at-a-Distance*. Quantum Mechanical Studies for Abner Shimony, Volume Two. 1997 ISBN 0-7923-4453-7; Set 0-7923-4454-5
195. R.S. Cohen and A.I. Tauber (eds.): *Philosophies of Nature: The Human Dimension*. 1997 ISBN 0-7923-4579-7
196. M. Otte and M. Panza (eds.): *Analysis and Synthesis in Mathematics*. History and Philosophy. 1997 ISBN 0-7923-4570-3

Also of interest:
R.S. Cohen and M.W. Wartofsky (eds.): *A Portrait of Twenty-Five Years Boston Colloquia for the Philosophy of Science, 1960-1985*. 1985 ISBN Pb 90-277-1971-3

Previous volumes are still available.

KLUWER ACADEMIC PUBLISHERS – DORDRECHT / BOSTON / LONDON

$$\frac{\partial^2 z}{\partial a dx}(x+\varepsilon_1 h, a+\varepsilon_2 k)hk = z(x+h,a+k) - z(x,a+k) - z(x+h,a) + z(x,a) \qquad (7')$$

$$0 \le \varepsilon_1 \le 1, 0 \le \varepsilon_2 \le 1$$

$$\frac{\partial z}{\partial a}(x,a+\eta_1 k)k = z(x,a+k) - z(x,a) \qquad 0 \le \eta_1 \le 1 \qquad (8')$$

$$\frac{\partial^2 z}{\partial x \partial a}(x+\eta_1 h, a+\eta_2 k)kh = z(x+h,a+k) - z(x,a+k) + z(x,a) \qquad (9')$$

$$0 \le \eta_1 \le 1, 0 \le \eta_2 \le 1$$

By rearrangement the right sides of (7') and (9') are equal. The left sides may therefore be equated:

$$\frac{\partial^2 z}{\partial a \partial x}(x+\varepsilon_1 h, a+\varepsilon_2 k) = \frac{\partial^2 z}{\partial x \partial a}(x+\eta_1 h, a+\eta_2 k)$$

Letting h and k tend to zero we obtain from the continuity of the second partial derivatives the desired result

$$\frac{\partial^2 z}{\partial a \partial x} = \frac{\partial^2 z}{\partial x \partial a} \qquad (10')$$

This example is rather typical of eighteenth-century calculus theorems and their counterparts in modern analysis[24]. The law of the mean introduces a distinguished value, localizing at a particular number the analytical relation or property in question. The result is then deduced using conditions of continuity and differentiability by means of a limit argument. In Euler's formulation by contrast there was no consideration of distinguished or individual values as such. Euler believed that the essential element in the demonstration was its generality, guaranteed by a formal analytical or algebraic identity. Thus the key step in his proof, the equality of the right sides of equations (7) and (9), was an algebraic identity that ensured the validity of the result.

IV Discussion

Euler perceived that the calculus is concerned ultimately with equations expressing relations of continuous change between variable magnitudes. His thesis concerning the primacy of pure analysis derived from a logical appreciation that geometrical methods and reasonings are extrinsic to the subject. In formulating this view he established the general framework within which analysis would be understood by subsequent researchers of the period, most notably Lagrange.

The distinctive character of Euler's doctrine is apparent when one considers it at a general epistemological level. There is a certain formal quality to his analysis; it arises ultimately from his conception of the subject as the study of primitive abstract relations. In this respect his viewpoint was very different from that of the early pioneers, who conceived of the foundation of the calculus in terms of geometric conceptions, or that of the nineteenth-century researchers, for whom the numerical continuum provided a fundamental structure of interpretation.

The notion of a primitive abstract relation among variables allowed for a direct and general approach to the subject, evident in Euler's derivation of (5) and (10) above. This generality was however of a particular sort, accompanied by a certain inflexibility of outlook. This became apparent during his debate with d'Alembert in the 1750s over the question of the general solution of the wave equation. Faced with some of the restrictions imposed by the precepts of his own theory (and insisted upon by d'Alembert) Euler advocated a rejection of the concept of a functional equation as a strict relation of equality between analytical expressions. As is well known his defence of this viewpoint reduced to *ad hoc* arguments and "visionary" presentiments of a more general mathematics, presented in a few papers; his systematic treatises of the 1750s remained firmly grounded in the established conception of analysis (Lützen 1983) and (Fraser 1989).

It should be emphasized that the rejection of geometric conceptions by Euler and other eighteenth-century researchers was not accompanied by the realization that the calculus could be developed in full logical isolation as part of pure analysis. In Euler's writings the relationship between foundation, theoretical development and problem generation is not worked out. The entire project of the *Methodus inveniendi* consisted of the derivation of differential equations for general problems, each of which embodied characteristics found in a given set of examples from geometry or mechanics. In his subsequent research the separation of analysis from geometry was made more explicit at a theoretical level. His variational investigations however remained centred on the derivation of general differential equational forms. He provided no account of how the problems in question might originate or be generated within this or any other branch of pure analysis.

He sometimes wrote as if problems are things that are external to analysis that guarantee its meaning and validity. In a memoir published in 1758 he investigat-

ed singular solutions to ordinary differential equations, that is, solutions which are not included in the general integral containing arbitrary constants. He took a differential equation and exhibited a particular function $y=f(x)$ that satisfied the equation but was not in the general solution. He wrote: "Concerning the example that I have just set forth, as it is drawn from fantasy, one could doubt whether this case is ever encountered in a real problem. But the same examples that I adduced in order to clarify the first paradox, will serve also to clarify this one" (Euler 1756; OO, ser. 1, XXII, 231)[25]. (The examples in question concerned curves in the plane that satisfied certain tangent conditions.)

The point here is connected to a larger difference of outlook between eighteenth-century and modern mathematics. That the problems of geometry and mechanics should conform to treatment by pure analysis was something that Euler implicitly accepted as a point of philosophical principle. The term "philosophy" (or "metaphysics") is here being used in the sense identified by Daston:

> "The presuppositions (often unexamined) that inform a scientist's work, which may be of either epistemological or ontological import [...] metaphysics is what is left over once the mathematical and empirical content have been subtracted [...]." (Daston 1991, 522)

In the writings of such post-positivist intellectual historians as E. A. Burtt the term 'metaphysics' in this sense referred to very broad assumptions, such as a general Platonic belief among early modern thinkers in the mathematical character of physical reality[26]. We suggest that it is also useful at a more concrete level in explaining certain tacit but definite attitudes displayed by Euler in his research in geometry and analysis.

Demidov writing of the failure of Euler and d'Alembert to understand each other's point of view in the discussion of the wave equation observes:

> "A cause no less important of this incomprehension rests, in our opinion, on the understanding of the notion of a solution of a mathematical problem. For d'Alembert as for Euler the notion of such a solution does not depend on the way in which it is defined [...] rather the solution represents a certain reality endowed with properties that are independent of the method of defining the solution. To reveal these properties diverse methods are acceptable, including the physical reasonings employed by d'Alembert and Euler." (Demidov 1982, 37)

A biographer of d'Alembert (Grimsley 1963, 248) has noted his insistence on "the elementary truth that the scientist must always accept the essential 'giveness' of the situation in which he finds himself." The sense of logical freedom that is inherent in modern mathematics was notably absent in the eighteenth century.

University of Toronto
Institute for the History and Philosophy of Science and Technology
Victoria College

Notes

1 In his history of analytic geometry Boyer (1956, 190) observes that for Euler "analysis was not the application of algebra to geometry; it was a subject in its own right—the study of variables and functions—and graphs were but visual aids in this connection [...] it now dealt with continuous variability based on the function concept [...] only with Euler did it [this meaning of analysis] take on the status of conscious program."

2 Emphasis in the original.

3 This view is most clearly presented by Mahoney (1973, 36 and 39):

> "In the *Introduction to the Analytic Art*, as in the whole of the *Analytic Art* itself, algebra was transformed from a sophisticated sort of arithmetical problem-solving into the art of mathematical reasoning itself, insofar as that reasoning was based on combinatory operations [...] the analytic art rose to a position subsuming all combinatory mathematics, whether arithmetic, geometry, or trigonometry".
>
> "The elevation of algebra from a subdiscipline of arithmetic to the art of analysis deprived it of its content at the same time that it extended its applicability. Viète's *specious logistic*, the system of symbolic expressions set forth in the *Introduction*, is, to use modern terms, a language of uninterpreted symbols. As a formal language, specious logistic can itself generate problems of syntax alone."

4 In his *Die Grundlagen der Arithmetik* (1884, §10) Frege rejected the use of induction (as it was understood in the physical sciences) as a valid principle of arithmetic. He wrote:

> "For here there is none of that uniformity, which in other fields can give the method a high degree of reliability. Leibniz recognized this already: for to his Philathèthe, who had asserted that 'the several modes of number are not capable of any other difference but more or less; which is why they are simple modes, like those of space' ".

He returns the answer:

> "That can be said of time and of the straight line, but certainly not for the figures and still less of the numbers, which are not merely different in magnitude, but also dissimilar. An even number can be divided into two equal parts, an odd number cannot; three or six are triangular numbers, four and nine are squares, eight is a cube, and so on. And this is even more case with the numbers than with the figures; for two unequal figures can be perfectly similar to each other, but never two numbers."

Later in this section Frege continues:

> "In ordinary induction we often make good use of the proposition that every position in space and every moment in time is as good in itself as every other. Our results must hold good for any other place and any other time, provided only that the conditions are the same. But in the case of the numbers this does not apply, since they are not in space or time. Position in the number series is not a matter of indifference like position in space."

5 Our account of Fermat's number theory is based on Ore (1948), Hoffmann (1960-1962) and especially Mahoney (1973, Chapter VI).

6 I quote from Heath translation Euclid (EH).

7 "Tout nombre premier mesure infailliblement une des puissances –1 de quelque progression que ce soit, et l'exposant de la dite puissance est sous-multiple du nombre premier donné –1 [...]."

8 Quoted in translation in Mahoney (1973, 329).

9 We use the term "coordinate geometry" to designate the subject known since around 1800 as "analytic geometry". The first work to contain the latter term in its title was J.B. Biot's *Essai de géométrie analytique* (1803). Loria (1923, 142-143) identifies analytic geometry with the "method of coordinates" and states that it "has as its goal the investigation, with the aid of coordinates, of all figures that are conceivable in the plane or in space." The employment of coordinate methods to investigate the elementary plane and solid geometry of Euclid, the use of transformations to study conic sections and higher-order polynomial curves, more broadly the study by means of coordinate methods of any class of geometric curves, all lie within the province of analytic geometry.

10 Coolidge (1945, 20-21) writes:

> "This dreary problem, whose algebraic solution gives a conic immediately, seems to have haunted the Greek mind. We noted at the beginning of the present chapter Apollonius' statement that others had unsuccessfully tried to solve it. But Apollonius himself does not appear able to carry it through. Certain modern mathematicians have put not a little time and strength into the attempt to complete such proofs by what we might call strictly Greek methods".

He mentions Zeuthen (1886, 126-63) and Heath for his edition of Apollonius (Apollonius CH, cxxxviii-cl).

11 Pappus's discussion is in Pappus (CI, part I). On pp. 587-591 of part two Jones (following Zeuthen (1886)) provides an account of how a synthesis of the four-line locus might have been achieved by earlier Greek mathematicians, especially Aristaeus.

12 Mahoney (1973, ch. 3) provides an account of Fermat's researches in coordinate geometry.

13 With the invention and increasing development of the calculus analytic geometry weakened as an area of research. Boyer (1956, 153-154) writes:

> "In general, l'Hospital (like Descartes) was more interested in analytic geometry as a means of expressing loci algebraically than as a method of deriving the properties of a curve from its equation. This latter aspect he seems to have felt belonged more properly to work in the calculus."

In reference to the eighteenth century he (1956, 193) observes "there was a natural tendency for material on curves to be merged with that on the calculus, and hence analytic geometry sometimes lost its identity."

14 Scott (1938, ch. 4) gives a good account of Wallis' treatise.

15 Westfall (1980, ch. 4) provides an account of Newton's early mathematical researches. Newton's papers from this period are published in Newton (MP, I.).

16 Both Westfall and Whiteside comment on this difference of approach, although neither identify the fundamental character of Newton's innovation as consisting precisely in his decision to use equations between Cartesian variables. Whiteside (1960-1962, 245) writes:

> "The advance Newton has made on Wallis' inductive approach to integrals—taking the upper bound of the integral variable—is that, in allowing a free variable (and its powers) into the pattern, he has been able to use the ordering of coefficients given by powers of the variable to point a more general aspect of the pattern lost in Wallis tabulated numerical instances."

Westfall (1980, 114-115) writes:

> "[...] Newton realized that Wallis's method was more flexible than Wallis himself had realized. It is not necessary always to compare the area under a curve with the area of the same fixed square. In the case of the simple power functions ($y = x, x^2, x^3,...$), for example, any value of x provides a base line that can be divided into an infinite number of segments, and with the corresponding value of y it implicitly defines a rectangle with which the area under the curve can be compared."

17 Varignon does not give the derivation of this equation. It may be obtained from the polar equation

$$\frac{b^2}{2ar} = 1 + \frac{c}{a}\cos\theta \qquad (\theta = \angle\, BCL)$$

by differentiating with respect to θ, eliminating $\sin\theta$ and setting $dz = rd\theta$. Since notation for the trigonometric functions has not yet been invented, Varignon would have worked from an equation of the form

$$\frac{b^2}{2ar} = 1 - \frac{c}{a}\frac{(CM)}{r}$$

where CM is the projection of CL on the axis AB.

18 "Ponamus omnia ista rectangulorum aggregata possibilia, vel omnes viarum possibilium difficultates, repraesentari per ipsas KV, curvae VV odinatas ad rectam GK normales [...]." English translation from Struik (1969, 278).

19 Cf. Jakob Bernoulli (1691), Newton (MF, 176-178) and Newton (MP, III, 312-313) (for the draft from the early 1670s). The seventeenth-century history of this problem is described by Whiteside Newton (MP, III, 308-311) who writes (*ibid.*, 308):

> "The development of this length-preserving transformation in the three decades preceding 1670 is a fascinating case-history in human insight and preconception which has never been systematically explored in the monograph needed to do it full justice."

20 In his treatise on the differential calculus Euler provided a detailed account of this procedure for introducing higher-order differential coefficients. A discussion of this subject is provided by Bos (1974).

21 "Corollarium 8: Hoc ergo pacto quaestiones ad doctrinam linearum curvarum pertinentes ad Analysin puram revocari possunt. Atque vicissim, si huis generis quaestio in Analysi pura sit proposita, ea ad doctrinam de lineis curvis poterit referri ac resolvi".

Scholion 2: "Quanquam huius generis quaestiones ad puram Analysin reduci possunt, tamen expedit eas cum doctrina linearum curvarum coniungere. Quodsi enim animum a lineis curvis abducere atque ad solas quantitates absolutas firmare velimus, quaestiones primum ipsae admodum fierent abstrusae et inelegantes ususque earum ac dignitas minus conspiceretur. Deinde etiam methodus resolvendi huismodi quaestiones, si in solis quantitatibus abstractis proponeretur, nimium foret abstrusa et molesta; cum tamen eadem, per inspectionem figurarum et quantitatum repraesentationem linearem, mirifice adiuvetur atque intellectu facilis reddatur. Hanc ob causam, etsi huius generis quaestiones cum ad quantitates abstractas tum concretas applicari possunt, tamen eas ad lineas curvas commodissime traducemus et resolvemus. Scilicet, quoties aequation eiusmodi inter x et y quaeritur, ut formula quaedam proposita et composta ex x ety y, si ex illa aequatione quaesita valor ipsius y subrogetur et ipsi x determinatus valor tribuatur, maxima fiat vel minima, tum semper quaestionem transferemus ad inventionem lineae curvae, cuius abscissa sit x et applicata y, pro qua illa formula W fiat maxima vel minima, si abscissa x datae magnitudinis capiatur."

22 "Methodus ergo ante tradita multo latius patet, quam ad aequationes inter coordinatas curvarum inveniendas, ut quaepiam expressio $\int Zdx$ fiat maximum mimimumve. Extenditur scilicet ad binas quascunque variabiles, sive eas ad curvam aliquam pertineant quomodocunque, sive in sola analytica abstractione versentur."

23 Carathéodory (1952, xxii) offers a different account of this part of the *Methodus inveniendi*; he writes:

> "[...] die Beispiele, die im ersten Teil desselben Kapitels (Nr. 1 bis 14) behandelt werden, können als Probme für die Kovarianz der Eulerschen Gleichungen bei beliebigen Koordinaten-Transformationen bewertet werden. Somit finden wir im Eulerschen Buche die ersten Ansätzen zu einer Theorie, die erst in unseren Tagen systematisch entwickelt worden ist."

In his index (*ibid.*, lix) of Euler's variational calculus he places these examples under the heading "Kovariante transformation von variationsproblemen." Goldstine (1980, 84) also observes:

> "It is remarkable that as early as 1744 Euler was already concerned with the problem of the invariance of his fundamental equation or necessary condition. In the first part of his Chapter IV he indicates that this fundamental condition remains invariant under 'general' transformations of the coordinate axes [...] he considers a number of examples where x, y are not related by being cartesian, rectangular coordinates, and shows the utility of his ideas on covariance [...]. It is truly in keeping with Euler's genius that he should have worked at ideas that were only to be satisfactorily and completely discussed in modern times."

In our view one should not speak of transformations, invariance or covariance in reference to Chapter Four. Although coordinate transformations had appeared in a memoir published by Hermann (1729) and were employed by Euler in his *Introductio* (1748, II, ch. II; for further references cf. Boyer 1956, ch. 7) they appear nowhere in the *Methodus inveniendi*. Euler does not have to show anything when he writes down the fundamental equation (5) in polar coordinates; its validity is a logical consequence of the generality of the variables in the original derivation. It is unnecessary to invoke concepts of modern differential geometry in order to reach a full appreciation of his theory.

24 Other examples are the fundamental theorem of the calculus, the theorem on the change of variables in multiple integrals and the fundamental lemma of the calculus of variations.

25 "Pour l'example que je viens d'alléguer ici, comme il est formé à fantaisie, on pourrait aussi douter, si ce cas se recontre jamais dans la solution d'un problème réel. Mais les mêmes exemples, que j'ai rapportés pour éclaircir le premier paradoxe, serviront aussi à éclaircir celui-ci."

26 Daston is identifying the sense in which the term metaphysics is used by Burtt and others. She is somewhat critical of this usage because it does not take into account the various actual historical systems of metaphysics which prevailed in the early modern period. To the extent however that the term serves to designate certain extra-scientific or extra-mathematical attitudes in past research it remains a useful concept of historical analysis.

EDITH DUDLEY SYLLA

JACOB BERNOULLI ON ANALYSIS, SYNTHESIS, AND THE LAW OF LARGE NUMBERS

I Introduction

Jacob Bernoulli was the earliest mathematician to prove a law of large numbers. Following in the directions opened by Christiaan Huygens's *On calculations in games of chance* (1657), he knew how expectations could be calculated for games in which the possible outcomes result from the design of game pieces such as dice or cards. He was interested, however, in developing an "art of conjecturing" that would apply mathematics to make prudent decisions in civil, moral, and economic matters. By his proof of the law of large numbers, he believed he had shown that observed relative frequencies could be reliably used in such calculations. Bernoulli's law of large numbers showed that if, for example, one has a die with a one-sixth chance of falling with any given side up, then as the die is repeatedly thrown, it becomes more and more probable that the observed relative frequency of that side being up will fall within some small interval around one-sixth. In the proof of this law, Bernoulli assumed that there are *a priori* equally likely possible cases in a given ratio and demonstrated that, if so, then the observed relative frequencies will tend to converge toward the *a priori* ratio of cases over a large number of trials. He also implied, however, that the truth of this proposition meant that it would be possible to find, within narrow limits, otherwise unknown ratios of cases *a posteriori*, from the outcomes of frequently repeated trials:

> "[...] another way is open to us by which we may obtain what is sought. What cannot be ascertained *a priori* may at least be found out *a posteriori*, that is from the results many times observed in similar situations, since it should be presumed that something can happen or not happen in the future in as many cases as it was observed to happen or not to happen in the past in a similar state of things."[1] (Bernoulli 1713, 224)

Although Jacob Bernoulli was a pioneer in the development of the mathematical theory of probability, his *The Art of Conjecturing* had less immediate influence than it might have had because he left it unfinished at his death. While large parts of the work were completed in the 1680s, well before Bernoulli's death in 1705, the book was not published until 1713, by which time Pierre Rémond de Montmort, Abraham De Moivre, and Nicholas Bernoulli were all active in the

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 79–101.

VOLUME 196

ANALYSIS AND SYNTHESIS IN MATHEMATICS

History and Philosophy

Edited by

MICHAEL OTTE
Institute for Didactics of Mathematics,
University of Bielefeld

and

MARCO PANZA
Centre F. Viète of the History and Philosophy of Sciences,
University of Nantes

KLUWER ACADEMIC PUBLISHERS
DORDRECHT / BOSTON / LONDON

VOLUME 196

ANALYSIS AND SYNTHESIS IN MATHEMATICS

History and Philosophy

Edited by

MICHAEL OTTE
Institute for Didactics of Mathematics, University of Bielefeld

and

MARCO PANZA
Centre F. Viète of the History and Philosophy of Sciences, University of Nantes

KLUWER ACADEMIC PUBLISHERS
DORDRECHT / BOSTON / LONDON

A C.I.P. Catalogue record for this book is available from the Library of Congress.

ISBN 0-7923-4570-3

Published by Kluwer Academic Publishers,
P.O. Box 17, 3300 AA Dordrecht, The Netherlands.

Sold and distributed in the U.S.A. and Canada
by Kluwer Academic Publishers,
101 Philip Drive, Norwell, MA 02061, U.S.A.

In all other countries, sold and distributed
by Kluwer Academic Publishers Group,
P.O. Box 322, 3300 AH Dordrecht, The Netherlands.

Printed on acid-free paper

Printed in the Netherlands

Table of Contents

II. Philosophy

INTRODUCTION

Time and again, philosophy, in trying to untangle the issues surrounding the analytic-synthetic distinction, has doubted that such a distinction can significantly be drawn at all. We think, in face of the varied and age-old discussions on it, that such reflections amount only to one more documentation of the tenacity of the problems behind this distinction. We could even be justified in promoting the thesis that this distinction refers to the complex relationship between the universe of meanings and the universe of objects and thus concerns each domain of human thinking where a form of objectivity is pursued.

If one accepts such a thesis, one will find it very natural that this distinction has so frequently occurred in the history of mathematics and in philosophical discussions about mathematics. Since Plato, we may encounter quite a number of interpretations of the ideas of analysis and synthesis, which are related in one sense or other with mathematical thought. Mathematicians of all ages have appealed to them in order to distinguish different forms and styles in their argumentation and expositions. Philosophers have referred to them for clarification of the specific character of mathematics in its relations to knowledge in general.

In the present volume various instances of the analytic-synthetic distinction are discussed in relation to the history and philosophy of mathematics, and some new perspectives about possible interpretations and consequences are suggested.

Let us briefly recall a number of interpretations of the notions of analysis and synthesis which played a role in history with respect to mathematics.

– The "logical" interpretation. Analysis proceeds from the general to the particular; synthesis advances in the opposite direction.

– The "structuralist" interpretation. Analysis is conceived as the decomposition of a complex construction given as a whole, in order to reduce it to its elementary components. Synthesis is accomplishment of the complex construction, starting from its elements.

– The "methodological" interpretation. Analysis proceeds on the level of the general only; synthesis is concerned with the particular, considering the general in the particular or even individual.

– The "gnoseological" interpretation. A judgment, or more generally a proposition, is synthetic, if it provides new knowledge, otherwise it is analytic.

– The "mereologic" interpretation. A predication is analytic if it assigns a certain entity to the whole of which it is a part; it is synthetic if this entity is connected to a different and independent (even more general) entity.

– The "semantic" interpretation. A true statement is analytic if its truth just depends on the meaning of the terms occurring in it (and it is then always true as long as these meanings do not change). It is synthetic if its truth depends on the particular character of the model to which it refers (and it is then true in some models and false in others).
– The "syntactical" interpretations. A sentence is analytic if it is logically deduced (or can be logically deduced) from a certain class of axioms, satisfying certain conditions. If not, it is synthetic (except if its negation can be logically deduced from the same axioms). The different interpretations of such a class obviously differ according to the conditions which the axioms have to satisfy. You may require them, for example, to be sentences expressing true analytic statements according to the semantic interpretation of analyticity, or sentences expressing true statements which are true only because of the meaning of the logical constants occurring in them, or even, that they are "logical axioms", or finally that they are simply accepted as starting points of deductive reasoning.
– The "phenomenological" interpretation. By this, analysis and synthesis are understood to be different stages or moments or modalities of mental activity. Synthesis is just will, while analysis is deliberation, the complex research which prepares and justifies synthesis.
– The "genetic" interpretation. Analysis proceeds from ideas which are given as such in a certain stage of the evolution of reason to the original ones from which these ideas originate; synthesis composes or connects the original ideas in order to realize the evolutionary process: it is just a figure of the evolution of reason.
– The "representationalist" interpretation. Analysis presents something through its specific details; synthesis expresses some essential features or characteristics of it.
– The "pragmaticist" interpretation. According to this interpretation analytical reasoning depends upon associations of similarity, synthetical reasoning upon associations of contiguity.
– The "programmatic" interpretation. This is expressed in the ideal of the Enlightenment to organize all knowledge in terms of an "analytic" system. Analysis is then the aim of a program of classification of knowledge, according to a genetic, historical and logical order. It is not concerned with problems of existence, since this is rather the problem of synthesis. Synthesis exhibits contents or being, without caring for their concepts and it remains deaf when analysis does not follow.
– The "directional interpretation". In mathematical reasoning or proof, synthesis proceeds from the given or known to that which we have to deduce or construct in order to solve a certain problem or prove a certain theorem; analysis, in contrast, proceeds from the unknown as if it were known, to its possible antecedents until arriving at premises we recognize to be true, proven or known. These premises then serve as the basis of synthesis.

– The "configurational" interpretation. Again in mathematical reasoning or proof, synthesis determines the consequences of certain premises, by producing a tree of successive and related deductions; analysis identifies the functional relations existing in a certain specified domain of known or unknown entities, by transforming them into a functional configuration.
– The "logico-theoretical" interpretation. A mathematical theory is synthetic if its objects are constructs, being introduced by recursive reasoning, or simply by successive descriptions of the repeatable conduct that lead to their exhibition. It is analytic, if its objects are characterized by specifying certain conditions or properties they have to satisfy or share either individually or together as a whole system or domain.
– The "historico-theoretical" interpretation. A mathematical theory is synthetic, if it refers to the classical geometrical objects or arguments or even to the classical theories of proportion, of numbers or magnitudes. It is analytic if it considers its objects as arguments of certain equations (rather than proportions) or operations, or even as functions.
– The "linguistic" interpretation. A mathematical arguments or the formulation of a mathematical problem or proof is synthetic if it uses the language of classical geometry and of the theory of proportions. It is analytic if it uses the language of equations, functions or operations.
– The "disciplinary" interpretations. A version of it is typical for eighteenth century mathematicians, according to whom analysis is a theory in terms of which all of mathematics can be formulated. A modern version of this interpretation states that analysis is a branch of mathematics, variously the mathematical theory including calculus, or the mathematical theory of the continuum, or the domain of all the theories where topological arguments, conditions or problems occur; etc.

Though it is not the intention of the following presentations to give a classification or even an account of the different ways in which mathematicians and philosophers have addressed analysis and synthesis or have discussed the analytic-synthetic distinction, the greater part of the previous interpretations are directly or indirectly discussed in the different articles of the present volume. Other interpretations, less customary or not as explicitly advanced in the history of mathematics and philosophy, are also presented or evoked. Finally, in certain cases, new interpretations are proposed.

So extended an inquiry is motivated by two convictions. First, behind such a wide variety of interpretations a deep unity in meaning and attitude seems to subsist, an invariant kernel, which justifies the use of the same terms to express different distinctions or views. Second, because of this unity and by searching for it, the discussion of the different interpretations of the analytic- synthetic distinction with regard to mathematics, becomes a *Königsweg* for tackling what we see as the essential problem mathematics presents for historical and philosophical considerations, the problem of objectivity as a form of knowledge.

It appears to us that the connections between this fundamental question and the analytic-synthetic distinction become particularly pertinent to the philosophical and methodological discussions about mathematics after 1800. All the different positions in their respective peculiarities, as characterized above, have since then been more or less overshadowed by the contrast between pure and applied mathematics. Expressed in philosophical terms: all kinds of foundationalism became obsolete and at the same time issues of objectivity of knowledge became ever more pressing. It seems as if the general spirit of the problems that was expressed by the terms "analysis" and "synthesis" can now be summarized by what may be called the question of philosophical realism (as opposed to nominalism as well as social individualism).

Towards the end of the eighteenth century a new understanding of cognition, of science and scientific development as well as of philosophy, emerged. More than ever before, the sciences were faced with the inevitability of the complexity of experience. Even though quantitative extensions of knowledge had always led to changes in scientific methods, techniques and theories, this increase in knowledge accelerated to such a degree that the capacity of the traditional information processing technologies, based on the spatial organization of knowledge seemed exhausted. This led to an estrangement of the natural sciences from the mathematically dominated spirit of the past and it also led to new developments in mathematics itself.

Since the turn to the nineteenth century a fundamental transition from thinking in substances (being the subjects of predication) towards relational thinking has occurred. Science no longer aimed at phenomena but at the form of things, and theories became realities *sui generis*. It became just as obvious, however, that every pertinent piece of theoretical knowledge, being part of some idea or model of the real world, will in some way or other take into account that the person having the knowledge is part of the system this knowledge represents. All knowledge presupposes a subject and an object and relations between these two, (which are established by the subject's activity). And as the multiplicity of subjective perspectives grew with the increasing division of labor, it could no longer be overlooked that the subject is not only the dynamical source of knowledge and change, but also its object or task. In as much as all knowledge is concerned with either of these aspects of the subject's role, it has a distinctly bipartite structure, which may be represented in various ways; for instance, in terms of the well-known complementarity of means and objects of human activity.

This complementarity of means, that is signs, and objects now seems to lie at the heart of the analytic-synthetic distinction.

The present volume offers various suggestions to substantiate such a thesis.

We wish to acknowledge our gratitude to the following persons, without whom this project could not have been completed: Lydia Bauer, Michael Detlefsen, Anita von Duhn, Michael Hoffmann, Michael Möse, Marianne Murphy, Gloria Origgi and Klaus Peters.

The financial assistance from the IDM / University of Bielefeld is greatly appreciated. In the process of editing this volume we have also received indispensable help from the Series Editor and from the Publisher's side. We feel particularly grateful to Evelien Bakker and Annie Kuipers.

Michael Otte
Marco Panza

I. History

GIORGIO ISRAEL

THE ANALYTICAL METHOD IN DESCARTES' *GÉOMÉTRIE**

To describe *La Géométrie* as an "*essai*" of the Cartesian method, or as the application of the rules given in *Discours de la méthode*, has paradoxically contributed to an undervaluation of existing connections between this brilliant and famous "*essai*" and Descartes' philosophical work. In a way this is a paradox, considering the fact that this description of *La Géométrie* underlines the dependency of Descartes' only complete mathematical treatment on the method to follow "pour bien conduire sa raison et chercher la vérité dans les sciences" and on the metaphysical principles on which it is based. Nevertheless the connection between *La Géométrie* and the Cartesian method thus established appears weak. Because of this unsatisfactory situation, the essays dedicated to the study of this text appear to be split into "philosophical-humanistic" analyses and "scientific" analyses.

Let us try to clarify the previous statement, beginning with the reasons why the connection between *La Géométrie* and the rules of *Discours de la méthode* appears weak. The fundamental reason lies in the vagueness of the methodological rules expressed in the *Discours* and summarized in the four famous rules governing scientific thought, even if Leibniz's severity seems excessive when he compares them with common recipes and sums them up in the almost obvious rule: "sume quod debes, operare ut debes et habebis quod optas" (Leibniz GP, IV, 329). Nevertheless it is difficult to deny that those who aim at establishing a tight connection between the rules of the *Discours* and the contents of *La Géométrie*, by trying to demonstrate in some way that the latter represent an application of the first, as if Descartes had endeavoured to obtain the results of *La Géométrie* as a direct application of his methodological rules, would be disappointed, and achieve little more than the impression of a vague link. The situation appears different, however, when the whole of Descartes' work is considered, and not only the *Discours*. Then, particularly when referring to the *Regulæ ad directionem ingenii*, it is possible to trace a much tighter connection between Descartes' method and the contents of *La Géométrie*, and at the same time to examine some historiographical questions on viewing Descartes' mathematical work from a different angle. The aim of this article is to attempt to highlight these connections and to briefly consider the historiographical questions mentioned above.

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 3–34.

As an introduction we will use some observations by E.J. Dijksterhuis, which, even though rather general, emphasize the existing link between *La Géométrie* and the *Regulæ*. Dijksterhuis observes that

> "[...] if you really want to get to know Descartes' method, you should not read the enchanting *Discours*, which is more a *causerie* than a treatise, but rather the *Regulæ ad directionem ingenii* [...]. As a matter of fact, the *Regulæ* contain an exposition of the so-called *Mathesis universalis*, which Descartes always considered one of his major methodological discoveries and which he hoped to see applied in all natural sciences." (Dijksterhuis 1961, 542)

Further on he continues:

> "The essay *La Géométrie*, in which Descartes presents his new discovery, fully deserves [...] to be described as a demonstration of the Cartesian method; yet it does not contain an application of the four rules of the *Discours*, to which this essay constitutes an appendix. In fact, the true *Discours de la méthode* is set up by the *Regulæ ad directionem ingenii*." (Dijksterhuis 1961, 543)

Dijksterhuis identifies this methodology in the *Mathesis universalis* and consequently the Cartesian ideal in the process of making science mathematical, which establishes a central role for *La Géométrie* as the first step of this process and as a model for its realization. Nevertheless, the way in which he characterizes the *Mathesis universalis* and the methodology he derives from it is not only vague but also misleading, in a way typical of many ambiguities in historiography dealing with these topics.

First of all Dijksterhuis completely identifies the *Mathesis universalis* with the "algebra speciosa" of Viète: consequently, Descartes' ideal would be nothing but the systematic "application of algebraic methods" to all science. In this way *La Géométrie* is nothing but the application of algebraic methods to geometry[1], which, in part, is true, but in our opinion insufficient to describe the characteristic features of Cartesian geometry. Secondly, Dijksterhuis identifies the deductive Cartesian method with the logical deductive method of modern mathematics, explicitly referring to the axiomatic method, which constitutes its complete codification[2].

In reality, these two comparisons are strictly correlated so that the discussion of one leads directly to the discussion of the other. We will start by commenting on the second comparison, recognizing that it is misleading, which a brief reading of *La Géométrie* demonstrates. As will be clarified later, the Cartesian deductivism clearly has "constructivistic" character: the only kind of reasoning allowed is that which will give an explicit construction of the entity under investigation or the result being demonstrated. Consequently any form of reasoning *ab absurdo* is excluded in Cartesian mathematics; moreover the entities all have to be constructible, which makes it impossible to define them in a conventional or axiomatic way. Furthermore, the admissible deductive chains must be finite; consequently, also the rudimentary forms of inductive reasoning in Descartes' work differentiate from modern mathematical inductive reasoning which, by means of a finite number of steps, makes it possible to pass from the finite to the infinite. Thus Cartesian deductivism is "constructivistic" and "finitistic", *i.e.* far from, if not the opposite of, the "logical-formal" deductivism of modern mathematics.

Descartes seems conscious of the particular nature of his method and its position in comparison with past traditions in mathematics. When Descartes criticizes the "vulgar mathematics" (Descartes AT, X, 376 and LR, 34)[3] of his time, he does not only refer to a sort of intuitive-experimental knowledge, in which the validity of the discoveries is particularly uncertain because of the frailty of the method used to obtain them[4]; he also criticizes the deductivism of classical mathematics, in particular that of the "ancients" and the synthetic method on which it is based (Descartes AT, X, 376 and LG, 34)[5]. Therefore the "analytical" method he proposes is neither an intuitive procedure, which relies on the senses, nor an abstract formal deductive procedure, which is unable to account for the way in which the discovery was reached—similar to the one characterizing the forms of reasoning of ancient mathematics[6].

The difference between the analytical and synthetic methods and Descartes' evaluations of them are shown in an extremely clear manner in a passage of the "answers" to the "second objections" to the *Meditationes*[7]. Here Descartes points out that in the works of the geometer the methods of demonstration are twofold: "l'une se fait par l'analyse ou résolution, et l'autre par la synthèse ou composition" (Descartes 1647, 387 and AT, VII, 155) and he continues:

> "L'analyse montre la vraie voie par laquelle une chose a été méthodiquement inventée, et fait voir comment les effets dépendent des causes; en sorte que, si le lecteur la veut suivre, et jeter les yeux soigneusement sur tout ce qu'elle contient, il n'entendra pas moins parfaitement la chose ainsi démontrée, et ne la rendra pas moins sienne, que si lui-même l'avait inventée.
>
> Mais cette sorte de démonstration n'est pas propre à convaincre les lecteurs opiniâtres ou peu attentifs: car si on laisse échapper, sans y prendre garde, la moindre des choses qu'elle propose, la nécessité de ses conclusions ne paraîtra point; et on n'a pas coutume d'y exprimer fort amplement les choses qui sont assez claires de soi-même, bien que ce soit ordinairement celles auxquelles il faut le plus prendre garde." (Descartes 1647, 387-388 and AT, VII, 155-156)

Therefore the value of the analytical procedure lies in the connection with the "true way", which has made the invention possible, and in the fact that it shows the links of causal dependence: this means that it derives from the "constructive" nature of this method, even if this advantage can be easily lost, if the chain linking the causes and the effects is interrupted, however slightly. The synthetic method proceeds in a different way:

> "La synthèse, au contraire, par une voie tout autre, et comme en examinant les causes par leurs effets (bien que la preuve qu'elle contient soit aussi des effets par les causes), démontre à la vérité clairement ce qui est contenu en ses conclusions, et se sert d'une lungue suite de définitions, de demandes, d'axiomes, de théorèmes et de problèmes, afin que, si on lui nie quelques consequences, elle fasse voir comment elles sont contenues dans les antécédents, et qu'elle arrache le consentement

du lecteur, tant obstiné et opiniâtre qu'il puisse être; mais elle ne donne pas, comme l'autre, une entière satisfaction aux esprits de ceux qui désirent d'apprendre, parce qu'elle n'enseigne pas la méthode par laquelle la chose a été inventée." (Descartes 1647, 388 and AT, VII, 156)

Descartes' description of the procedure of the synthetic method clearly refers to the geometry of the ancients and, in particular, to the model of Euclid. Differing from the analytical method, this procedure gains the reader's consent, using procedures of "coercion" typical of formal logic[8]. Nevertheless, Descartes criticizes the absence of constructivism in it: it "does not teach the method by which the thing has been invented"[9]. The analytical method, on the contrary, has this great advantage, which was also recognized but kept "secret" (Descartes 1647, 388 and AT, VII, 156)[10] by the ancients, and which Descartes, brought to light and exposed as a method.

The difference between the analytical and the synthetic method is discussed by Descartes as an answer to a concluding remark of the *Seconde obiezioni* to the *Meditationes* "collected by Mersenne on the basis of remarks from various theologicians and philosophers" (Descartes 1647, 359), which invites Descartes to procede "more geometrico" in his exposition:

"[...] ce serait une chose fort utile, si, à la fin de vos solutions, après avoir prémièrement avancé quelques définitions, demandes et axiomes, vous concluiez le tout selon la méthode des géomètres, en laquelle vous êtes si bien versé, afin que tout d'un coup, et comme d'une seule illade, vos lecteurs y puissent voir de quoi se satisfaire, et que vous remplissiez leur esprit de la connaissance de la divinité." (Descartes 1647, 365 and AT, VII, 128)

On the one hand Descartes' answer makes clear in which sense he believes to have to accept the invitation to procede "more geometrico"—*i.e.* according to the analytical and not the synthetic method; on the other hand, as he deals with metaphysical matters, he endeavours to show the particular inadequacy of synthesis in these kinds of questions, recognizing that synthesis appears more acceptable in geometrical problems. In specifying this aspect he touches on an issue that is particularly interesting for our topic: he asks himself why synthesis can "be useful when put after analysis" (Descartes 1647, 388 and AT, VII, 156). This derives from the nature of the basic notions of geometry, which, since they are not in contradiction with the senses, are accepted unanimously:

"Car il y a cette différence, que les prémières notions qui sont supposées pour demontrer les propositions géométriques, ayant de la convenance avec les sens, sont reçues facilement d'un chacun; c'est pourquoi il n'y a point là de difficulté, sinon à bien tirer les conséquences, ce qui se peut faire par toutes sortes de personnes, même par les moins attentives, pourvu seulement qu'elles se ressouviennent des choses précedentes; et on les oblige aisement à s'en souvenir, en distinguant autant de diverses propositions qu'il y a de choses à remarquer dans la difficulté proposée, afin qu'elles s'arrêtent séparément sur chacune, et qu'on les leur puisse citer par après, pour les avertir de celles auxquelles elles doivent penser."[11] (Descartes 1647, 388-389 and AT, VII, 156-157)

Therefore it is obvious that the axioms of geometry are not only far from being conventional but also only acceptable as far as their contents of truth are "clear" and "distinct": only for this reason the synthetic method can be useful when introduced in geometry, naturally "après l'analyse". Thus, once again the superiority and priority of the analytical-constructive method over the synthetic-formal one is emphasized. This has led to two errors: the first one to believe that the use of axiomatic procedures is at the centre of the Cartesian "revolution" in mathematics—with Descartes actually dissociating himself from these procedures, even if it is in a "form of contents" typical of the geometry of the ancients; the second one to speak generally of the central position of the "deductive method" (evoking improper associations with the deductive logics of modern mathematics) without specifying and clearly underlining the "constructive" character of this method in Descartes' vision. It is necessary, however, to give an exact definition of this "constructivism". For this purpose it will be useful to re-examine the *Regulæ* in order to show how it can be directly translated into the concept of "geometric construction" and into a precise definition of the forms of such a construction. This leads Descartes to a critical re-examination of the concept of "constructibility" of a geometric figure as it was defined by previous geometry and to the introduction of a new interpretation of such a concept. The Cartesian classification of the curves—which can be considered Descartes' most important contribution to mathematics—is the consequence of such a re-examination and re-definition. In the end the Cartesian classification of the curves is a direct consequence of the general principles of the Cartesian analytical method, which are unfolded in the *Regulæ*.

Before concentrating on this more specific analysis, we have to make some general observations.

We have tried to show that an accurate explanation of the meaning which Descartes attributes to the terms "analytical" and "synthetic" is necessary to fully understand the method he follows in his mathematical arguments. Therefore it is also necessary to give an exact explanation of these terms with respect to the context of Cartesian thought and to their prevalent use at that time, avoiding any reference to a non-specific and thus debatable "general meaning" of these terms in the history of mathematics. This kind of use, uncritical and unrelated to time, is not infrequent in historiography—particularly the one manifesting itself as a kind of by-product of research—and has been the source of quite a few misunderstandings. A typical manifestation of the cumulative historiographic analysis is the incomprehension—or at least the negligence—of the changes in meaning in scientific terminology, subtle transformations of meaning which occur silently in the course of history, below the unchanged surface of its formal appearance. Marc Bloch, having observed how much the term "history" has changed its meaning in the course of 2000 years, has given a sharp comment which should be read, reread and remembered by the historian as a precious *memento*: "Si les sciences devaient, à chacune de leurs conquêtes, se chercher une appellation nouvelle—au royaume des académies que de baptêmes, et de pertes de temps!" (Bloch 1964, 1). Yet historians of science often forget this rule and venture to analyse a context of scientific concepts by taking a meaning for granted that is determined by recurrent terms which have nothing to do with that very context and that is almost always related to a more recent context. In this way the historic specificity of the term, *i.e.* its meaning in relation to the context in which it is used, is changed, with rather negative consequences for a correct understanding of the subject. The use (and abuse) of the term "analytical geometry" in historiography is an evident example of this: the use of the term recurrent in the handbooks of contemporary mathematics or at least of the end of the 19th century has been widely accepted without any closer examination. In our opinion, this point of view is completely inadequate for the specific meaning of "analytical" geometry in Descartes' work.

Both, the terms "synthetic" and "analytic" have a completely different meaning in Descartes' work than in modern and contemporary mathematics. Since the times of Descartes, the modern meaning of the term "synthetic" (*i.e.* the meaning implied from the second half of the 19th century onwards) has undergone radical changes: what was essential in the ancient interpretation (*i.e.* the very demonstrative procedures effectively described by Descartes in the *Meditationes*) was put last and the aspect of the intuitive meaning of the discovery first[12]. Yet the changes undergone by the term "analytic" are even more complex. No doubt we have to speak about a sequence of slight alterations of meaning during a long period of historical development. The history of these changes should be seen within the framework of the history of changes in meaning of the concept of analysis. Neither of these ambitious projects will be carried out here and we will limit ourselves to pointing out some of the historical layers that cover the Cartesian conception of "analytical" geometry. The marked constructive nature of analysis in Cartesian geometry—something nonexisting in the modern meaning of the term—should be an indication of the occurrence of possible historical sedimentation.

Let us now look at historiography (particularly but not solely at the sector of historiography that is linked with research, referred to above). We may even be fortunate enough to witness an ongoing attempt of "concealment"! Actually, J. Dieudonné, after having listed "analytical geometry" among those "pseudosciences" which "it remains to hope we can forget the existence and even the name of" (Dieudonné 1968, 6), continues like this: "Furthermore it is urgent to free the term 'analytical geometry', which, no doubt, is best to indicate one of the most vivid and profound theories of modern mathematics, *i.e.* the one of analytical varieties, compared to 'algebraic geometry', which is the study of 'algebraic varieties'" (*ibid.*, 6). In another piece of writing (where the "liberation" has already taken place) Dieudonné clearly explains the contents which he wants to free the term "algebraic geometry" from:

> "It is absolutely intolerable to use 'analytical geometry' for linear algebra with coordinates, still called 'analytical geometry' in the elementary books. Analytical geometry in this sense has never existed. There are only people who do linear algebra badly, by taking coordinates and this they call analytical geometry. Out with them! Everyone knows that analytical geometry is the theory of analytical spaces, one of the deepest and most difficult theories of all mathematics."(Dieudonné 1970, 140)

It is not our aim to discuss this kind of historical destructions (which are perpetrated by one of the most authoritative voices not only in mathematics but also in the history of mathematics of our time).

Certainly analytical geometry in the sense of "coordinate geometry" has existed. It is important to remember that the term "analytical geometry" did not first appear in Descartes but in the *Introduction* of the first volume of Lacroix' *Traité du calcul différentiel et du Calcul intégral* in the 1797 edition (Lacroix 1797-1798). Lacroix explains that his point of view differs completely from the traditional constructive one:

> "En écartant avec soin toutes les constructions géométriques j'ai voulu faire sentir au Lecteur qu'il existoit une manière d'envisager la géométrie, qu'on pourrait appeler 'Géométrie analytique', et qui consisteroit à déduire les propriétés de l'étendue du plus petit nombre de principes, par des méthodes purement analytiques, comme Lagrange l'a fait dans sa Méchanique à l'égard des propriétés de l'équilibre et du mouvement." (*ibid.*, I, xxv-xxvi)

In spite of recalling Lagrange, Lacroix admits that it was Monge who first presented "sous cette forme l'application de l'Algèbre à la Géométrie" (*ibid.*, I, xxv-xxvi). Actually, his homonymous treatise (Monge and Hachette 1802) still uses this terminology—"application of algebra to geometry"—which, on one hand, conveys the idea of an "ancillary" use of algebra in geometry, on the other hand suggests a one-sided relationship between the two disciplines in one direction only: the use of algebra in geometry as an instrument leads to the need to justify algebraic techniques in terms of the main subject—geometry—and consequently the translation of algebraic operations into geometrical constructions (*i.e.* from geometry to algebra), while the opposite (from algebra to geometry) does not exist. This is exactly Descartes' point of view—which justifies the definition of his approach as "application of algebra to geometry"—but it is not Monge's point of view, as revealed by Lacroix:

> "Qu'on ne croie pas qu'en insistant ainsi sur les avantages de l'Analyse algébrique, je veuille faire le procès à la Synthèse et à l'Analyse géométrique. Je pense au contraire qu'on néglige trop aujourd'hui l'étude des Anciens mais je ne voudrais pas qu'on mêlât, comme on le fait dans presque tous les ouvrages, les considérations géométriques avec les calculs algébriques; il séroit mieux, ce me semble, que chacun de ces moyens fût porté dans des traités séparés, aussi loin qu'il peut aller et que les résultats de l'un et de l'autre s'éclairassent mutuellement en se correspondant pour ainsi dire, comme le texte d'un livre et sa traduction." (Lacroix 1797-1798, I, xxv-xxvi)

Therefore Lacroix' merit is to have given a new name (which also contains an element of continuity in the use of the term "analytical") to a turning-point in geometrical thinking carried out by Monge in the first place. This turning-point consists in having given autonomy to the two disciplines—algebra and geometry—transforming their relationship into a form of specular correspondence. Even more clearly Monge pointed out in his lectures on descriptive geometry held at the *Ecole Normale* of the year III that the student had to

> "[...] se mettre en état d'une part de pouvoir écrire en Analyse tous les mouvements qu'il peut concevoir dans l'espace, et de l'autre de se répresenter perpétuellement dans l'espace le spectacle mouvant dont chacune des opérations analytiques est l'écriture."[13] (Monge LEN, 367 and 1799, 62)

Algebra is no longer a mere instrument to obtain geometrical constructions in an easy way: it offers a translation of the "book" of geometry which one can work with; and, *vice versa*, from the translation it is possible to return to the original text. Therefore every geometrical problem is susceptible of an algebraic treatment that permits reasoning in a somewhat stenographic abbreviated form, which, in the long run, is more powerful than the classic synthetic reasoning; however, a geometrical translation exists of every algebraic formulation. So, it is possible to obtain from every geometrical locus the algebraic equation representing it, which can be manipulated with the autonomous methods of algebra, and vice versa a geometrical locus can be obtained from a given equation.

This specularity has been the essence of modern analytical geometry from Monge and Lacroix onwards. In this concept coordinate geometry no longer plays an accessory or technical but a central role: the role of mediator between algebra and geometry, a kind of dictionary to translate from one text to another, indicating the correspondence between geometrical locus and equation and vice versa. Therefore it is understandable how, in the modern meaning, the notion of analytical geometry has been confounded with the one of "coordinate geometry", exactly because of the central position of this method in guaranteeing the bi-univocal relationship between the two disciplines.

Referring to this interpretation of analytical geometry as the study of the properties of extension based on the recognition of the specularity between algebraic and geometrical operations and on the consequent central position of coordinate geometry we are led back to Fermat and not Descartes. On this point C. B. Boyer is completely right, when he observes that it is in Fermat's work—precisely in his short treatise entitled *Ad locos planos et solidos isagoge* (Fermat TH, I, 4, 91-110)—that "the fundamental principle of analytical geometry is to be found in a precise and clear language" (Boyer 1956, 218). Boyer is also right when he observes that Fermat's phrase stating that a locus exists whenever there are two unknown quantities in a final equation, since the extreme of one of them describes

a straight line or curve, "represents one of the most significant statements in the history of mathematics" (Boyer 1956, 190). This is certainly most important as regards the notion of analytical geometry of Monge and Lacroix mentioned above: actually, Fermat puts forward the principle of bi-univocal correspondence between algebra and geometry in a rather explicit way, when he admits that beginning with an algebraic equation there can be a geometrical locus—a really revolutionary idea for his time. The geometric constructions have lost their central position at a single blow: it is no longer necessary that a curve can be constructed in order to be admissible—which has been fundamental for the priority of geometry over algebra—the curve exists only because the equation is given, it is not defined by a construction but as "the locus of the points that satisfy the equation". The central position of coordinate geometry follows as an obvious necessity. The fact that Fermat's approach is more "modern" than Descartes' has been correctly observed for some time (Taton 1951, 102). Descartes does not admit this vision of geometrical loci at all, nor does he accept the specularity between algebra and geometry or renounces the central position of the concept of construction. Finally, coordinate geometry has a purely technical and accessory role in his work.

At this point some historiographical difficulties arise. Fermat's point of view, though apparently more modern, was certainly not the more influential one: it is well-known that the 17th and part of the 18th century was dominated by the Cartesian geometrical conception; and even when the mathematics of the Enlightenment period and the time of the French Revolution—Lagrange, Monge and Lacroix in particular—distanced itself from the Cartesian tradition this was done silently, underlining in a clear but implicit way the breaking with this tradition. Monge's use of the expression "application of algebra to geometry" reminds us of the continuity with the Cartesian tradition. On the contrary, Lacroix' naming (the introduction of the term "analytical geometry") equals a more explicit separation, but because of the apparent character of continuity, due to the common use of the term "analytical", may not have sufficiently drawn the historians' attention. This different meaning attributed to the concept of analysis, however, is the very basis of the big difference between the Cartesian vision and the "modern" use of "analytical geometry".

It has to be pointed out that the choice of examining the problem of the birth of analytical geometry according to a view typical of a "cumulative" historiography—and thus starting from the modern notion of analytical geometry[14]—has led to serious difficulties and has caused contradictions among numerous historians. So, Gino Loria does not conceal the sense of confusion that overcomes the historian when he tries to determine the birth of analytical geometry:

> "All those who long for knowing the work which is the starting point of literature on coordinate geometry experience a great disappointment since Descartes' *La Géométrie* differs from a modern

treaty of analytical geometry infinitely more than do two expositions, one ancient, one modern of any other mathematical discipline." (Loria 1924, 777)

He continues like this, providing a perfect model of cumulative historiography:

"[...] Descartes (and this also holds true for Fermat) considered the new discipline a simple metamorphosis in the geometry of the ancients from the influence of algebra [...]; so the comparison of the author of the *Discours de la méthode* with Christopher Columbus, who took the conviction to have discovered a new world to his grave, is evident; this state of being blind was transmitted from the Supreme to his immediate disciples [...]." (Loria 1924, 777)

Taton reveals the differences between Fermat and Descartes more skillfully, characterizing the technical aspects of Cartesian geometry quite well:

"[Descartes] avait conçu cette science comme 'une application de l'algèbre à la géométrie', nom qu'elle conservera d'ailleurs jusqu'au prémières décadaes du XIX siècle et que Monge lui-même adoptera, c'est-à-dire comme une technique de structure algébrique, adaptée à la résolution des problèmes d'essence géométrique et spécialement des problèmes des lieux à la manière d'Apollonius. Ainsi, apparaît-elle, non pas comme une branche autonome de la science, mais plûtot comme un outil permettant de résoudre de nombreux problèmes géométriques qui n'entrent pas dans le champs normal d'application directe des propriétés classiques tirées des *Eléments* d'Euclide. Les courbes ne s'y trouvent pas étudiées pour elles-mêmes d'après leurs équations, mais l'intérêt se porte *quasi* exclusivement sur celles qui apparaissent comme solutions de problèmes à résoudre." (Taton 1951, 101)

Most important in the historiography of analytical geometry remains the work of C. B. Boyer (Boyer, 1956), whose merit was to clarify the difference between Fermat's and Descartes' point of view. As Taton, he recognizes that Descartes' geometry is more an application of algebra to geometry than analytical geometry in the sense we understand it today and calls Chasles' definition of analytical geometry as a "*proles sine matre creata*" (Chasles 1875, 94) "unfortunate". And, after having observed that Cartesian geometry has now become a synonym of analytical geometry, but that Descartes' fundamental goal is quite different from the one of modern handbooks, he offers the following characterization of Cartesian geometry:

"Descartes was not interested in the curves as such. He derived equations of curves with one purpose in mind—to use them in the construction of determinate geometrical problems which had been expressed by polynomial equations in a single variable .[...] The method of Descartes is that of coordinate geometry, but his aim is now found in the theory of equations rather in analytic geometry. [...] where Descartes had begun with a locus problem and from this derived an equation of the locus, Fermat conversely was inclined to begin with an equation from which he derived the properties of the curve. Descartes repeteadly refers to the generation of curve 'by a continuous and regular motion'; in Fermat one finds more frequently the phrase, 'Let a curve be given having the equation [...]' The one admitted curves into geometry if it was possible to find their equations, the other studied curves defined by equations."[15] (Boyer 1956, 216-217)

What exactly are the characteristics of Cartesian geometry? They are described by Boyer, when he refers to the differences between Descartes' and Fermat's approach. These differences could be summed up as follows: both Descartes and Fermat were influenced by Viète; Fermat applied Viète's method to the problems of geometrical loci, whereas Descartes renewed the method by introducing the algebraic symbolism, without changing the object of Viète's researches, *i.e.* the geometrical construction of the roots of an equation. This is certainly correct and yet it means that Descartes is nothing but a descendant of Viète: his analytical geometry is the continuation of Viète's *ars analytica* with the introduction of the powerful instrument of algebraic symbolism—without doubt a considerable step forward but not doing justice to Fermat's innovating contribution. This does not mean that it is scandalous to reconsider the significance of the Cartesian work. But as to the connection between Descartes' and Viète's work, it seems that the above-named interpretation is based on a merely technical vision of the question.

There are also other reasons for not being satisfied. The problem of the origin of analytical geometry cannot be solved by simply stating that Descartes' geometry is not the same as modern analytical geometry, and by concluding with renaming it "application of algebra to geometry". Moreover this term goes back to a later date, so that the question why Cartesian geometry (*i.e.* the application of algebra to geometry) originated as "analytical" geometry or at least as the application of "analysis" to geometry remains. This is not a simple question of terminology but a basic problem which must not be disregarded and reduced to a question of names. Once again the answer could be that Descartes is a descendant of Viète[16]. At this point, however, we really are dissatisfied. We have already seen how the notion of "analytical" in Descartes can neither be reduced to the notion of "analytical" of modern analytical geometry nor to the *ars analytica* of Viète. The characteristics of this notion are to be found in the philosophical sense of the term and not in the strictly mathematical sense. It is obvious that the study of Descartes' philosophical work does not provide the key to the understanding of the importance of his mathematical works, it is true, but it is equally evident that, in order to understand the work of a scientist-philosopher like Descartes, an analysis which is restricted to the study of his contribution viewed solely from the angle of the history of geometrical methods is not sufficient.

One of the most important contributions to Descartes' *La Géométrie*, apart from the writings of Boyer[17], is an article by H. J. M. Bos (Bos 1981). Bos's point of view is different from the one prevailing in literature, which he considers unsatisfactory as it aims at solving "the sterile question whether Descartes invented analytical geometry or not" (Bos 1981, 297). Also Bos's approach is strictly internal and in no way detached from a "cumulative" point of view. In this way the "sterile" question is taken up, possibly in the paradoxical form according to which the very "programmatic" intentions of Descartes had restrained the establishment of analytical geometry.

> "The later synthesis of algebraic and geometrical methods into what is now called analytic geometry was possible only because later mathematicians were not aware of (or forgot) the programmatic problems with which Descartes had struggled." (Bos 1981, 298)

Special emphasis must be put on the fact that Bos's analysis is clearly directed towards the topics and problems we have dealt with so far. First of all, Bos demonstrates that the central theme of *La Géométrie* and "the key to understand its underlying structure [...] and programme" (Bos 1981, 332) is the representation of curves. In fact, it is the basis of the relationship between geometry and algebra in Descartes' work, which is connected to the topic of his constructivistic conception and the relation between analysis and synthesis. After a profound analysis of the text, Bos indicates what he considers contradictory: the co-existence of a classical programme (already clearly expressed in 1619) which regards geometry as a science that "constructs" or solves geometrical problems, which changes the ancient classification of the curves only slightly (basing it on the use of machines which are nothing but the generalization of ruler and compasses) and where algebra has no place, and a programme which attributes an important role to algebra[18] and abolishes the ancient classification of curves, and, in doing so, opens the way to the modern distinction between algebraic and transcendental curves. In fact, there are two co-existing programmes, since Descartes never abandons the vision of geometry as science of "constructions" and remains prisoner of some essential difficulties. The main difficulty revealed by Bos is the contradiction which can be found in the criteria of geometrical acceptability of curves in the programme of *La Géométrie*:

> "On the one hand Descartes claimed that he accepted curves as geometrical only if they could be traced by certain continuous motions. This requirement was to ensure that intersections with other curves could be found, and it was induced by the use of the curve as means of construction in geometry. On the other hand Descartes stated that, under certain conditions, curves represented by pointwise constructions were truly geometrical. Pointwise constructions were related to curve equations in the sense that an equation for a curve directly implied its pointwise constructions. Pointwise construction was used primarily for curves that occurred as solutions to locus problems.
> The link between the two criteria is Descartes' argument that pointwise constructible curves can be traced by continuous motions. We have seen that that argument, and hence also the link, is very weak." (Bos 1981, 326)

Looking once again at Cartesian geometry through the lens of modern mathematics, *i.e.* of "analytical geometry", Bos asks himself:

> "Why then did Descartes not cut this Gordian knot in the most obvious way, namely by defining geometrical curves as those which admit algebraic equations? Why did he not simply state that all such curves are acceptable means of construction and that the degrees of their equations determine their order of simplicity? That principle would have removed the contradictions mentioned above." (Bos 1981, 326)

After an accurate analysis Bos comes to the conclusion that the contradiction is based on the co-existence of the two programmes mentioned above, the second of which being the result of a paradigmatical change that occurred between 1619 and 1637. This change, even though it emphasizes the role of algebra, does not modify the nucleus of the first programme and consequently the idea that "geometry is the science that solves geometrical problems by constructing points by means of the intersection of curves" (Bos 1981, 331). The programme of 1619 "may have been impracticable but coherent" (Bos 1981, 331), whereas the programme of 1637 is innovative but incoherent: it introduces algebra without renouncing the link with the old geometrical programme and consequently brings about a series of difficulties.

This explanation, though accurate, is only descriptive: it does not say anything about the motives that led Descartes to take this new position and remain obliged to the old one at the same time. Was it a question of mere attachment to the past? There is one possible answer, on condition of leaving the link with the "sterile" question of Descartes' relationship with analytical geometry definitely behind. In the time between 1619 and 1637 something crucial happened: Descartes' enunciation of the principles of the method. The influence of such an enunciation on the programme of *La Géométrie* is revealed by Bos[19]. However, by restricting the connection to the *Discours de la Méthode*, it is impossible to see the amplitude and complexity of the profound link between geometry and method. The crucial event between 1619 and 1637 is the publication of the *Regulæ*. In this text we find Descartes' so-called attachment to the classical constructive vision of geometry and, at the same time, the importance he attributes to the procedures of algebra.

Attempting to draw a parallel between the changes in Descartes' approach to geometrical problems—which certainly exist and consist in passing from a nearly orthodoxically classical vision to one that attributes an important role to algebraic procedures—Bos refers to Schuster's theses. Schuster maintains that after 1628 Descartes abandoned the programme of *Mathesis universalis* formulated in the *Regulæ* because he had encountered some difficulties in constructing a geometrical theory of equations (Schuster 1980); consequently he turned to algebra in order to solve his technical difficulties. This explanation, however, is not very convincing, not only because Descartes was not easily influenced by technical difficulties or details[20]. In the first place the changes in Descartes' approach to geometry are reduced to merely technical reasons. Secondly, it is taken for granted that after 1628 Descartes abandoned his programme of *Mathesis universalis*: this means that all connections between the *Regulæ* and *La Géométrie* are disregarded, which really is something impossible to do. Moreover it appears arbitrary to talk about a programme of *Mathesis universalis* which Descartes was to have developed in detail, since he considered the enunciation of the methodical rules of reasoning more important than anything else. Last but not least the traditional conception of geometry which Descartes adhered to in 1619 is claimed to be the exact opposite of the method expressed in the *Regulæ* and does not leave any space for the algebraic approach. We shall see that this does not hold true: in the method enunciated in the *Regulæ* the algebraic procedures, though under a constructive framework, have a fundamental role. It is not true that in 1628 Descartes formulated the *Regulæ* as a specular translation of his geometry of 1619; nor is it true that after 1628 a programme that did not exist came to a crisis for technical reasons. The contrary holds true: exactly in 1628 the determination of the principles of a new method by Descartes induced a radical change in his consideration of geometrical problems. On the one hand, this method is "analytical" and consequently chooses methods used in algebra and it is a "constructive" analytical method and therefore uses the constructive procedures of classical geometry as its point of reference. On the other hand, this constructive analytical procedure, which will be described shortly, completely changes the picture of geometry, in particular the criteria of representation and admission of curves, where progress and difficulties analysed by Bos become evident. It remains doubtful, however, whether Descartes ever worried about those difficulties or even perceived them.

Therefore the crucial knot is to be found in Descartes' method, which is both, analytical and constructive. Such a method needs algebra as a universal language, which reflects the generality of the method, but at the same time it is constructive and does not admit leaps or lacerations in its procedures. Descartes strives to unite these two requirements: therefore the contradictions in his text do not arise from the co-existence between two different visions of geometry but represent the difficulties of one coherent vision[21], which is based on a philosophical programme and not on one of mathematical nature. The Cartesian "incoherences" only exist when they are viewed under the point of view of modern analytical geometry, which requires a balanced co-existence between geometry and algebra: Descartes, however, was not at all interested in cutting the "Gordian knot". For him this "Gordian knot" did not exist, nor could he have solved it—not because he was attached to an ancient vision of geometry but because it would have been in contradiction with his methodological approach. Let us rather have a look at the subordination of algebra to geometry, which is no residue of the past but a necessary consequence of the Cartesian methodical principles.

A purely internalist historiographic vision can make it even more difficult to value the significance of Descartes' contribution and its position in the history of science. Our opinion, which is also based on the big influence that *La Géométrie* had for more than a century, is that Descartes' contribution meant an enormous methodological revolution: this is why his success went beyond the importance of

the results. In order to understand the reasons, there is no need to go back to the traditional connection between *La Géométrie* and the *Discours de la méthode*: in the *Regulæ ad directionem ingenii* it is possible to trace a stronger connection which permits re-reading *La Géométrie*, bringing to light aspects of great importance which, up to now, may have been valued in a one-sided way. The re-reading of the work combined with the sound contributions of historiography, in particular those of Boyer and Bos, provides a satisfactory picture of the basic arguments of the Cartesian text.

Within the limits of this article it is impossible to embark on a detailed and exhaustive analysis of the *Regulæ*, let alone *La Géométrie*, the contents of which will be taken for granted. Here we shall limit ourselves to undo the most important conceptual knots of the Cartesian analytical method emerging from the *Regulæ*. They can be described as follows.

The first point is the affirmation that knowledge is achieved in a two-fold way: by "intuition" (an elementary act the basis of which is not any unreliable information provided by the senses or by imagination but the conception of a "pure attentive spirit" which does not leave any doubts as to what has been understood[22] and is the matrix of the formation of clear and distinct ideas) and by "deduction" (with deduction being a chain of intuitions). It follows that reasoning, being invariably based on the use of "concatenations of elementary acts of intuition", is deductive—which is the second fundamental point.

The third aspect centers on the "constructive" character of the deductive procedure: the chain of deductions on which it is based must not be interrupted, the result has to be reached without leaps. In the process of reasoning one term must not approach another pre-existing term but all links and relations between them have to be indicated so that a chain of intuitions connecting them is constructed. Its validity is proved by the fact that the deductive chain can be run through time and again in an "ordered" and "continuous" movement, which makes it possible to verify whether the construction which conducts to the final truth is valid.

The fourth aspect deals with the possibility of reducing any differences between objects to "differences between geometrical figures": this is the first form of the Cartesian notion of reducing differences to differences of extension in the *Regulæ*, which is basic to the Cartesian quantitative conception of the Universe. In the *Regulæ* this idea does not immediately appear as a metaphysical principle (the possibility of reducing any object to extension), but as an intuitive aid to represent the relations which are difficult to conceive in a form more accessible to intuition. But this first presentation is followed by an interpretation which outlines very clearly the explicit metaphysical valence which the concept of extension will assume in subsequent works. In fact, Descartes proposes a quantitative interpretation of the Universe, centering on mathematics, *i.e. Mathesis universalis*, completely different from the "vulgar" mathematics of his time, and a universal knowledge that permits reducing the analysis of any phenomenon to problems of "order" and "relations". In the deductive chain of reasoning every intuition can be compared with the subsequent one, as in the comparison of two quantities. So, deductive reasoning is transformed, or rather reveals its true nature as a sequence of concatenated relations: in it there occurs what happens in mathematical progression, where every term is determined by the relationship with the preceding term. By means of the language of algebra, deductive reasoning is translated into a sequence of proportions—hence the fundamental role of the theory of proportions. Another important consequence is the following: it has been maintained that, due to the constructive character of the deductive procedure, no ring of the chain can be left out, nor can there be any data without having defined the procedure which, starting from a further well-known truth, permits obtaining it (*i.e.* by way of construction). Therefore the translation of the deductive procedure into algebraic language (*i.e.* by means of equations via the theory of proportions) is "one-directional"; it is possible to move from the deductive procedure to algebra, but not the other way round, since there are no constructive procedures expressed by algebra. In the specific field of the relations between algebra and geometry this implies that their relations are one-directional: it is possible to pass from the geometrical problem to its algebraic translation (provided that the algebraic operations applied are geometrically and thus constructively justified), but not to do the opposite, since no "algebraic problems" as such exist. The *Mathesis universalis*, which reflects the constructive form of deductive reasoning and the universality of the well-defined relations that exist among the objects of the Universe, only contemplates problems of geometrical construction.

The fifth and last aspect is the following: in all the *Regulæ* there is a parallel between "arts" and science, between the procedures of the "mechanical arts" and the constructive procedure of deductive reasoning. This is one of the great number of aspects of the Cartesian mechanistic conception. This parallelism is translated into a parallelism between the procedures of the mechanical "arts" and geometrical constructions and is the basis of the definition of the new criterion of demarcation between admissible and inadmissible curves, introduced by Descartes, which made it possible to go beyond the ancient classification of the curves and reclassify them: an important result, since, apart from some significant but not decisive differences, it coincides with the modern classification of algebraic and transcendental curves.

Let us now examine more closely the significance and implications of the five aspects mentioned above, looking at the form in which they are presented and taken up in the *Regulæ*.

Before doing so, however, we want to discuss to two general topics which represent a sort of "*leit-motiv*" of the *Regulæ*. The first is the refusal of specialistic knowledge in favour of the unity of learning. This is the central theme of

Regula I[23], but also recurs in many other passages and has important consequences for mathematics. For Descartes the study of specific mathematical problems is of no interest at all:

> "neque enim magni facerem has regulas, si non sufficerent nisi ad inania problemata resolvenda, quibus Logistae vel Geometrae otiosi ludere consueverunt; sic enim me nihil aliud praestitisse crederem, quam quod fortasse subtilius nugarer quam caeteri. Et quamvis multa de figuris et numeris hic sum dicturus, quoniam ex nullis disciplinis tam evidentia nec tam certa peti possunt exempla, quicumque tamen attente respexerit ad meum sensum, facile percipiet me nihil minus quam de vulgari Mathematica hic cogitare, sed quamdam alima me exponere disciplinam, cujus integumentum sint potius quam partes." (Descartes AT, X, 373-374 and LR, 30-32)

In a certain sense he explicitly declares that he neither is nor wants to be a mathematician; he uses mathematics (*i.e.* certain mathematics, different from the "vulgar" mathematics of his time) to determine the principles of a universal method of reasoning[24].

The second theme is the refusal of a historical approach to science: *Regula III* establishes a distinct opposition between historical and scientific learning. According to Descartes, we would not be able to "express a firm judgement on a given question", even if we read all the works of the ancients. In the following he enunciates most clearly the opposition mentioned above: "in fact, we seem to have learned from history and not from science" (AT, X, 367 and LR, 16-18). It is evident that this opposition is a result of Descartes' need to proclaim the necessity of leaving previous learning aside so as to promote the development of a science free from the prejudices of bookish learning. But in doing so he accomplishes more than a tactical move: he actually establishes the basis of one of the cornerstones of modern sciences, which has greatly influenced research and its view of the role of history: it is a matter of affirming the uselessness of historical learning in the determination of trends in scientific research and its opposition to the acquisition of scientific learning. As will be shown, this point of view also helps Descartes to deal with traditional principles without being prejudiced, re-examining them independent of any reference to historical tradition and only because of their conceptual value: an impartiality which is particularly important at the moment he abolishes the classification of the curves, consolidated by a time-honoured tradition.

Let us now return to the analysis of the five fundamental topics of the *Regulæ*. The first two of them are already clearly enunciated in *Regula III*: the only two acts of intellect suitable to obtain knowledge without errors are "intuition" and "deduction". It has to be emphasized that, by defining intuition as the "firm conception of a pure and attentive spirit that is born by the light of reasoning alone", Descartes underlines that this act is purely intellective and differentiates it from the "unreliable testimony of the senses" or the "deceptive judgement of imagination" (AT, X, 368 and LR, 20). In order to explain the changes in the use of the term, he refers to its Latin meaning "*intuitus*"[25]. "Deduction", on the other hand, is the means to get to know other things (most things, actually) which are not evident by themselves, provided that they are deduced from true and known principles by way of a chain of elementary acts of intuition and consequently controllable at each step. The difference between the first and second act mainly consists in the fact that the latter needs a "movement" or a "succession". This "movement" is the key to the deductive process: in fact, it is a "continuous and uninterrupted movement of thought with a clear intuition of everything"[26] (AT, X, 369 and LR, 22). Here the influence of two fundamental principles of the Cartesian conception is to be found: the principle of "continuity" and the principle of "completeness". The consequence is a conception of the Universe as a "continuum" free from lacerations and interruptions: it is well-known that Descartes completely refused the existence of a vacuum. As to processes of reasoning (which are of the same nature as material processes), these principles are reflected in the concept of continuity of the deductive chain and in the absence of ruptures and interruptions. It has to be emphasized that the two terms are not synonymous and their meanings do not even partially overlap. This is made clear in *Regula VII*, from which emerges that "continuous" means "which does not stop", "without pauses", "arriving at the very end of the course" and follows all necessary concatenations so as to make up for the weaknesses of memory, which is unable to seize the whole course of reasoning at once. Moreover, it is evident that the deductive chain moves in one direction—from the introduction to the conclusion: by going through the chain time and again in a continuous movement (faster and faster), it is possible to leave the role of memory aside and obtain a sort of global intuition of the whole[27]. Consequently continuity presents itself as a characteristic feature that leads to the comprehension of the whole. *Vice versa*, the fact, that in the act of deducing, the movement of thought is uninterrupted implies that it is not permitted to skip any ring of the chain; the conclusions are no longer certain[28].

Let us have a look at the consequences which these characteristics of deductive reasoning have on the "status" of geometry (as well as that of physics, since the possibility of a gap is negated): geometrical reasoning must be constructive, as it is based on chains of steps, each depending on the preceding one. The geometrical object is only imaginable as constructed by such a succession. Therefore the geometrical point cannot be seen "isolated": when the geometrical object (*e.g.* a curve) is constructed, it has to be explained how to pass from one point to the next one in a "continuous" and uninterrupted procedure. As in physical space, also in geometrical space there can be no gap. From this results Descartes' inconceivability of the notion of geometrical locus assigned in an abstract way by means of an equation and not defined by a construction. The above-mentioned refusal of reasoning *ab absurdo* equally depends on this vision (it is not constructive: skipping

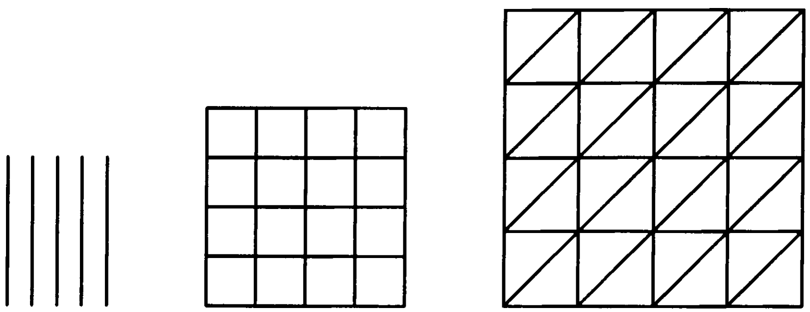
all rings of the chain, it directly compares the last one with the first one and is not one-directional).

It has been considered useful to look at the implications of the Cartesian notion of geometry, so as not to keep apart two important and intimately interrelated aspects. In doing so, however, we have not yet specified the role of geometry in Descartes' notion. It is explained in *Regula IV*, which contains some famous passages on the meaning of the *Mathesis universalis*. We are not going to spend much time on such a well-known topic, but want to emphasize the parallels between Descartes' criticism of the particular sciences and the claim that a form of universal knowledge is necessary, as well as his criticism of the way to do mathematics (geometry and arithmetics). The latter emerges from tradition and the claim to "true" mathematics, certainly well-known to the ancients as the easiest and most necessary science of all to form and prepare the mind to understand other more elevated sciences[29]. In order to understand what this is all about, however, it is not sufficient to refer to etymology, according to which "Mathematics" simply means "science", because in this case also Music, Optics and Mechanics would have the same right as Geometry to be called Mathematics[30]. The substance of Mathematics (which makes it a universal science or *Mathesis universalis*) is the study of everything that is connected with order and measure, "no matter whether these measures are to be found in numbers, figures, stars, sounds or in any other objects"[31] (AT, X, 377-378 and LR, 38).

The link between *Mathesis universalis* and the deductive procedure is evident: just as *Mathesis universalis* searches order in things, according to *Regula V* "the whole method consists in order and the arrangement of the things towards which the mind has to be directed in order to discover some truths"[32] (AT, X, 379 and LR, 42). It follows that the classification of things must no longer be attained by means of categories, as in scholastic philosophy, but "according to deductive order"[33]. Finally, "in order to attain science, all things leading to our goal, and every singular one in particular, have to be run through in a continuous and uninterrupted movement of thought and have to be understood in a sufficient and methodical enumeration"[34].

The concept of "sufficient enumeration" or "induction" could be considered a rudimentary version of the principle of mathematical induction known in modern mathematics. As the only certain procedure Descartes puts induction next to intuition, since it defines inference in each point of the chain. Nevertheless it is quite a rudimentary concept of induction: it is true that the link between subsequent steps is decisive, but all steps have to be examined: moreover, in Descartes' work this notion is not subjected to a clear concept of numeration of the steps (as is shown in Descartes' example on circles in *Regula V*)[35]. The distance separating the Cartesian induction from the modern one still refers to the theme of constructivism, which is well-explained by the contents of *Regula XI*. It has been observed that each step has to be controlled, constructed, and nothing can be left out or taken for granted, while two verifications are sufficient in modern induction and from these infinite cases are deduced. However, Descartes' opinion on the infinite is well-known: his finitism is the second fundamental aspect, mentioned above. In the *Principia*[36], for instance, he distinguishes between infinite and indefinite, reserving the first attribute to God. In this text Descartes declares that he will never deal with discussions of the infinite, since he considers it ridiculous that "finite" beings pretend to say something about the infinite. This also explains the boundaries of the relationship between the Cartesian notion of "continuum" and the modern one: the idea of completeness and lack of interruptions has nothing to do with continuity in the modern sense of the word: therefore Descartes' completeness is a sort of "discrete continuity".

As has been pointed out, *Mathesis universalis* also deals with the study of measure. The answer to the question whether there exist any parallels between mathematics and the deductive method also at this level is positive; in order to establish this connection, however, another crucial knot of Cartesian thinking has to be considered: the concept of extension and the notion that every object can be reduced to characteristics of extension.This is another aspect of Cartesian philosophy too well-known to deal with it here: we shall limit ourselves to say something on how it is presented in the *Regulæ*. It is in *Regula XII* that a representation of differences between objects (and related representations) as differences between figures is introduced. Descartes observes that there is nothing wrong with conceiving the differences that exist between colours like white, blue and red as differences between figures, as the following or similar ones:



In this way the introduction of useless entities is avoided and an extremely natural representation is recurred to. Moreover, the infinite number of figures is sufficient to describe all differences between sensitive objects. In this form the notion of the quantitative description of differences between sensitive objects is

introduced in the *Regulæ*: it will be expressed in the different shape of two geometrical figures and consequently in their relationship. This is a crucial passage: by establishing the central position of the concept of extension, the central role of geometry as science of extension in the learning process is also established.

Nevertheless, the form which the principle of reduction to extension assumes in the *Regulæ* somewhat differs not only from the metaphysical version of the *Principia*[37], but apparently also from the meaning implicit in the text of *La Géométrie*. In fact, in the *Regulæ* differences are described by means of references to illustrated extensions applying to imagination and, in a certain sense, juxtaposed to quantity. In *La Géométrie* this juxtaposition disappears, actually it turns to a hierarchical relationship: in fact, the role of imagination disappears and extension is reduced to quantity by means of algebra. More precisely, the quantitative description of differences by means of extension is realized in a purely intellectual way and the instrument of such a realization is the algebraic description. We have spoken of apparent differences between *Regulæ* and *Géométrie*; the following propositions represent a decisive step forward: generic references to an illustrated representation of differences are abandoned and are described in terms of the theory of proportions—at this point the decisive step towards the introduction of algebra has been taken. Furthermore, as we shall see, the concept of "problem with unknown quantities" and thus the concept of equations is introduced. Last but not least the final part of the first book (incomplete as well) makes evident that Descartes had a clear notion of the central role of algebra.

Before continuing with these topics, we will point out an important consequence of what has been mentioned before: the relationship between extension and quantity thus established shows the subordinate position of algebra compared to the central one of geometry. Geometry comes first: as science of extension it is the instrument to describe and analyse the substance of things: algebra has the essential but subordinate role to make it possible to treat extension as quantitative description and not as a complex of figurations perceptible by imagination alone. This hierarchy is clearly reflected in *La Géométrie* (from the first pages onwards), when Descartes tries to justify the introduction of algebraic operations by means of geometry (demonstrating "how arithmetical calculations refer to geometrical operations"[38] (AT, VI, 369)).

In *Regula XII* we find the distinction between simple propositions and "*quæstiones*": the first only need the distinct intuition of the object and the methods of reasoning expounded in the first twelve rules (which are the ones we have summarized up to this point), whereas the second (object of the remaining twelve rules[39]) regard the problems which are "perfectly understood"[40], even though the solution is unknown. These abstract problems in algebra and geometry lead to three kinds of questions: *a*) Which are the signs to recognize the object that is being searched? *b*) What can it be deduced from? *c*) How does the close dependency of these things manifest itself? In order to solve these problems, the deductive procedure is no longer sufficient, an "art" (*i.e.* Descartes' *ars analytica*, which he sets about to expound) has to be introduced: it consists in the "development of something that depends on many other" *simul implicatis*[41] (AT, X, 429 and LR, 140). This "art" is nothing else than the method of solving problems where "unknown quantities" appear (as is explained by *Regula XIII*, introducing the concept of designating something unknown by something that is known) and thus the "art" of solving equations. Even though this procedure is different from the deductive one (developing what is unknown from what is known), there is a tight link with the constructive procedures of deduction, particularly because the questions dealt with are perfectly determined[42].

Regula XIV contains the last important step towards the translation of "perfectly understood" problems into algebraic form. In fact, Descartes observes that any knowledge that is not gained by simple intuition is gained by comparison. In distinct objects common characteristics are to be found in relations and proportions which have to be reduced to equalities. But in virtue of former considerations, only quantity is susceptible to this reduction and it is extension that has to be chosen from the quantities. So the formulation of a perfectly determined problem is nothing else than the reduction of proportions to equalities. In this rule we observe a transition from the definition of these differences between things by means of figures to a definition of these differences by means of relations or proportions of quantities of extension. The intervention of algebra as an instrument, however, has not yet occurred: this happens in *Regula XVI*, where algebra is explicitly introduced as a more compact instrument of symbolical representation[43] than the geometrical-spatial signs which Descartes referred to in the preceeding rules; he did so in order to give an example of the translation of relations (or differences) between things into relations of extension. This rule is also important as a significant step towards algebra: it eliminates the distinction between root, square root, third root etc., all reduced to the language of the theory of proportions.

It is in *Regula XVII*, however, that the procedure which Descartes suggests in order to solve a perfectly determined problem is expound more clearly, having been translated into equations or a chain of proportions. In our opinion, a direct parallel can be established between this procedure and the one expound in *La Géométrie*.

Descartes observes that, while there exists an easy and direct way to solve a problem in a deductive manner which permits to pass easily from one term to another—it is the one of the direct concatenation exposed in *Regula XI*—the situation is different in the case of perfectly understood problems. Let us continue with Descartes' words:

> "Nunc igitur si dependentiam singularum ad invicem, nullibi interrupto ordinem, intueamur, ut inde inferamus quomodo ultima a prima dependeat, difficultatem directe percurremus; sed contra, si ex eo quod primam et ultimam certo modo inter se connexas esse cognoscemus, vellemus deducere quales sint mediae quae illas conjungunt, hunc omnino ordinem indirectum et praeposterum sequeremur. Quia vero hic versamur tantum circa quaestiones involutas, in quibus scilicet ab extremis cognitis quaedam intermedia turbato ordine sunt cognoscenda, totum hujus loci artificium consistet in eo quod, ignota pro cognitis supponendo, possimus facilem et directam quaerendi viam nobis proponere, etiam in difficultatibus quantumcunque intricatis; neque quicquam impedit quominus id semper fiat, cum supposuerimus ab initio hujus partis, nos agnoscere eorum, quae in quaestione sunt ignota, talem esse dependentiam a cognitis, ut plane ab illis sint determinata, adeo ut si reflectamus ad illa ipsa, quae primum occurrunt, dum illam determinationem agnoscimus, et eadem licet ignota inter cognita numeremus, ut ex illis gradatim et per veros discursus caetera omnia etiam cognita, quasi essent ignota, deducamus, totum id quod haec regula praecepit, exequeremur [...]" (Descartes AT, X, 460-461 and LR, 200)

Descartes declares that he wants to reserve the examples of this method for the subsequent *Regula XXIV*, which is missing: in our view, however, these examples can be found in *La Géométrie*.

The following *Regula XVII* teaches us that only four operations (addition, subtraction, multiplication and division) are sufficient to establish these mutual dependencies, which consents to reduce the definition of mutual dependencies to a sequence of proportions. The following step (*Regula XIX*) is to search as many quantities expressed in different ways as there are unknown variables. When the equations have been found and all remaining operations have been completed (*Regula XX*), all equations of this kind have to be reduced to a single one, "*i.e.* to the one whose terms occupy the minimum degrees on the scale of quantities, in continuous proportion according to which they must be arranged" (*Regula XXI*; Descartes AT, X, 469 and LR, 216).

Let us now turn to the first pages of *La Géométrie*. Here we find the translation of the procedure just expound with analogue terms and the same methodical sequence. It will suffice to read the following passage to verify this statement:

> "[...] voulant resoudre quelque problesme, on doit d'abord le considerer comme desia fait, & donner des noms a toutes les lignes qui semblent necessaires pour le construire, aussy bien a celles qui sont inconnuës qu'aux autres. Puis, sans considerer aucune difference entre ces lignes connues & inconnues, on doit parcourir la difficulté selon l'ordre qui monstre, le plus naturellement de tous, en quelle sorte elles dependent mutuellement les unes des autres, iusques a ce qu'on ait trouvé moyen d'exprimer une mesme quantité en deux façons: ce qui se nomme une Equation, car les termes de l'une de ces deux façons sont esgaux a ceux de l'autre. Et on doit trouver autant de telles Equations qu'on a supposé de lignes qui estoient inconnuës. Ou bien, s'il ne se trouve pas tant, & que, nonobstant, on n'omette rien de ce qui est desiré en la question, cela tesmoigne qu'elle n'est pas entierement determinée; et lors, on peut prendre a discretion des lignes connues, pour toutes les inconnues ausquelles ne correspond aucune Equation. Après cela, s'il en reste ancore plusieurs, il se faut servir par ordre de chascune des Equations qui restent aussy, soit en la considerant toute seule, soit en la comparant avec les autres, pour expliquer chascune de ces lignes inconnuës, & de faire ainsi en les desmelant, qu'il n'en demeure qu'une seule, esgale a quelque autre qui soit connuë, ou bien dont le quarré, ou le cube, ou le quarré de quarré, ou le sursolide, ou le quarré de

cube, &c., soit esgal a ce qui se produist par l'addition, ou soustraction, de deux ou plusieurs autres quantités, dont l'une soit connuè, & les autres coient composées de quelques moyennes proportionnelles entre l'unité & ce quarré, ou cube, ou quarré de quarré, & c., multipliées par d'autres connuès. Ce que i'escris en cete sorte:

$$z \propto b,$$

ou $$z^2 \propto -az+bb,$$

ou $$z^3 \propto +az^2+bbz-c^3,$$

ou $$z^4 \propto +az^3+cz^3+d^4,$$

& c.

C'est a dire: z, que ie prens pour la quantité inconnuè, est esgale a b; ou le quarré de z est esgale au quarré de b, moins a multiplié par z ; ou le cube de z est esgal a a multiplié par le quarré de z, plus le quarré de b multiplié par z, moins le cube de c; & ainsi des autres.

Et on peut tousiours reduire ainsi toutes les quantités inconnuès a une seule, lorsque le Problesme se peut construire par des cercles & des lignes droites, ou aussy par des sections coniques, ou mesme par quelque autre ligne qui ne soit que d'un ou deux degrés plus composée. [...] ie me contenteray icy de vous avertir que, pourvû qu'en demeslant ces Equations on ne manque point a se servir de toutes les divisions qui seront possibles, on aura infalliblement les plus simples termes ausquels la question puisse estre reduite." (Descartes AT, VI, 372-374)

The close link between the general methodical principles enunciated in the *Regulæ* and their application in *La Géométrie* is more than evident. Actually, we could say that almost the whole procedure to "develop" the unknown quantity into equations described in *La Géométrie* is already contained in the *Regulæ*.

Let us now examine the last of the five fundamental themes which have been considered the nucleus of the *Regulæ*: the question of the relationship between mechanical arts and geometry. The first reference to this question appears in *Regula VIII*, where Descartes, after having given various examples on the use of the method, continues like this:

"Haec methodus siquidem illas ex mechanicis artibus imitatur, quae non aliarum ope indigent, sed tradunt ipsaemet quomodo sua instrumenta facienda sint. Si quis enim unam ex illis, ex.gr., fabrilem vellet exercere, omnibusque instrumentis esset destitutus, initio quidem uti cogeretur duro lapide, vel rudi aliqua ferri massa pro include, saxum mallei loco sumere, ligna in forcipes aptare, aliaque ejusmodi pro necessitate colligere: quibus deinde paratis, non statim enses aut cassides, neque quidquam eoruim quae fiunt ex ferro, in usus aliorum cudere conaretur; sed ante amnia malleos, incudem, forcipes, et reliqua sibi ipsi utilia fabricaret. Quo exemplo docemur, cum in his initiis nonnisi incondita quaedam praecepta, et quae videntur potius mentibus nostris ingenita, quam arte parata, poterimus invenire, non statim Philosophorum lites dirimere, vel solvere Mathematicorum nodos, illorum ope esse tentandum: sed iisdem prius utendum ad alia, quaecumque ad veritatis examen magis necessaria sunt, summo studio perquirenda; cum praecipue nulla ratio sit, quare difficilius videatur haec eadem invenire, quam ullas questiones ex iis quae in Geometria vel Physica aliisque disciplinis solent proponi". (Descartes AT, X, 397 and LR, 76-78)

Descartes' interest in machines and mechanical arts as a natural consequence of his mechanistic conception is well-known. Nevertheless, as Paolo Rossi points out, "for Descartes the effective progress of science depends on the work of theorists. Technology as such does not contribute to the progress of scientific learning at all" (Rossi 1962, 111). Rossi remembers Baillet's description of Descartes' project to build some halls at the College de France: the craftsmen involved were taught the scientific principles of making machines work by professors of mathematics and physics. Technology remains subordinate to science, it has to follow its principles, in particular its methodological principles, and not the product but its principle of realization is of interest. In this way Descartes reveals a conception which, in a certain sense, is closer to a technological approach than to a technical one; the main difference, however, is that the relationship between science and technology is somewhat sterile, as technology is considered subordinate to science. In any case Descartes is interested in technology as deriving from methodical principles, since for him this is the practical verification of the world's mechanical nature. His description of the mechanical arts, where nobody proceeds at random but first prepares the necessary tools following methodical principles, shows that he sees a concrete connection between some historical forms of the "arts" and his method. This connection becomes less vague and boils down to a concrete reference in *Regula X*, where the importance of the simpler arts, which are "are ruled by order", is dealt with, those of craftsmen making cloths or carpets or embroideries "similar to number combinations and arithmetical operations"[44]. Embroidery is particularly interesting, as it links the characteristics of these arts, i.e. being simple and methodical, to a specification of their procedures: it is close to the theory of proportions. Therefore these arts appear as a concrete representation of the concatenated, continuous and uninterrupted movement which is the nucleus of the method. We know that one of the outstanding characteristics of technical development in France at the time of Descartes was the diffusion of the textile industry based on the use of the power loom (Dockès 1969). So Descartes' reference appears in no way fortuitous: in a new innovative technique like the one of power-loom weaving, Descartes saw the expression of a conception of the mechanical arts based on method in a double sense: in a general sense, since the methodical principle is put before the specific realization (the way of weaving is more important than the product itself) and in a specific sense, because—as is evident in the case of the power-looms—the functioning of the instrument is based on a concatenation of coordinate movements following one another according to a well-defined rule. This concatenation is determined by precise number relations and consequently based on the theory of proportions. All crucial conceptional knots of the Cartesian method (continuous and uninterrupted movement, theory of proportions) can be found in these examples of mechanical arts.

Several times it has been pointed out that Descartes' famous instrument of movable squares, or rather the instrument to multiply proportions, which appears in *La Géométrie* and has a fundamental role in the classification of curves, had been invented by him long before the publication of *La Géométrie*. This kind of power-loom seems to be a further manifestation of a predilection for the procedures of the mechanical arts based on the theory of proportions, clearly expressed in the *Regulæ*. In any case, Descartes' instrument of movable squares permits the geometrical representation of a sequence of proportions and so it is nothing but the concrete translation of a continuous and uninterrupted movement, the subsequent steps of which are all concatenated according to precise and perfectly determined relations. Although this does not fully meet the necessary qualification of constructibility of the Cartesian conception, it stands for the prototype of a class of instruments that conform to these qualifications (Bos 1981). Descartes referred to this instrument, when he proposed a new classification of "admissible" curves which was to substitute the classical subdivision into "geometrical" curves (*i.e.* curves that can be drawn with ruler and compasses or planar loci), curves obtained by cutting a section (*i.e.* conics or linear loci) and "mechanical" curves (resulting from the "chaotic" motion of a point). This classification was based on the preference of ruler and compasses and could only be changed after their priviledged position had been abolished and different criteria introduced. Descartes, however, did not have the slightest reason to insist on recognizing the ancient classification—neither on the methodological nor the technical level ruler and compasses were to be preferred, since they only represented a partial and episodic working method as to the methodological principles. An instrument like the one of movable squares instead constituted their complete and faithful translation.

It is most interesting to read Descartes' comment on the problem of the classification of curves, which is characterized by the "anti-historic" spirit mentioned above.

> "Les anciens ont fort bien remarqué qu'entre les Problesmes de Geometrie, les uns sont plans, les autres solides, & les autres lineaires: c'est a dire que les uns peuvent estre construits en ne traçcant que des lignes droites & des cercles; au lieu que les autres ne le peuvent estre, qu'on n'y employe pour le moins quelque section conique; ni enfin les autres, qu'on n'y employe quelque autre ligne plus composée. Mais je m'estonne de ce qu'il n'ont point outre cela, distingué divers degrés entre ces lignes plus composées, & ie je sçaurois comprendre pourquoy il les ont nomées Mechaniques, plustot que Geometriques." (Descartes AT, VI, 388)

Descartes' astonishment could seem somewhat strange, if it were not regarded in the above-mentioned anti-historic context. Further on he emphasizes that mechanical curves do not derive their names from the fact that they are drawn by machines, because otherwise also the curves drawn with ruler and compasses, which actually are machines, too, would have to be rejected. We know, however, that in the ancient classification 'mechanical' has a different meaning and, at least in Greek tradition, ruler and compasses have an intellectual value—they represent ideal perfection (exactly like the machine of movable squares in Descartes' intentions). But Descartes continues as if he was not aware of this:

> "Ce n'est pas non plus a cause que les instrumens qui servent a les tracer, estant plus composés que la reigle & le compas, ne peuvent estre si iustes [...]." (Descartes AT, VI, 388-389)

Otherwise they would have to be rejected also from the mechanical curves

> "[...] ou c'est seulement la iustesse du raisonnement qu'on recherche, & qui peut sans doute estre aussy parfaite, touchant ces lignes, que touchant les autres autres." (Descartes AT, VI, 389)

So 'mechanical' does not mean inexact—on the contrary, mechanical procedures are based on exactness. On the other hand, Descartes does not even consider the possible meaning of 'mechanical', *i.e.* "generated by motion". In this way he discards all possible hypotheses of interpretation, only to demonstrate the incoherence of the ancients, and concludes, declaring that he does not want to change any names that have already been accepted by use. In doing so, however, he has deprived them from their original meaning: henceforth—though only conventionally—'geometrical' refers to what is precise and exact and 'mechanical' to what is not. 'Mechanical' alone does not mean anything any longer: it neither means "generated by motion" nor "obtained by means of a machine". Both of these meanings would only obstruct Descartes' new classification, which accepts many of the curves that, according to the old classification, were considered "mechanical" as admissible curves. Now the term 'mechanical' only serves to denote the opposite of something that is perfectly determined—the contrary of 'geometrical', which is well-determined. The names remain unchanged, the line of demarcation of the meanings changes.

Geometry is the science whose object is the measure of bodies. Therefore there is no reason to exclude composite lines in favour of simple lines,

> "pourvû qu'on les puisse imaginer descrites par un mouvement continu, ou par plusieurs qui s'entresuivent & dont les derniers soient entierement reglés par ceux qui les precedent: car, par ce moyen, on peut tousiours avoir une connoissance exacte de leur mesure." (Descartes AT, VI, 390)

Here the usual criterion, already familiar to us, re-emerges: constructibility by means of a continuous, uninterrupted and coordinate movement. This criterion (of which we want to emphasize its "constructive element") is the true conceptual core of Cartesian geometry. This makes the reference to coordinate geometry appear secondary, if not marginal, whereas the classification of curves obtained by Descartes by the conceptual use of the instrument of movable squares is most important. It is well-known that the classification is not complete—due to its constructive character which does not assume the order of the curve as an element of classification, as would happen in the case of a point of view based on the concept of geometrical locus, *i.e.* starting from the algebraic equation: it skips several steps and therefore does not obtain all algebraic curves, as one would expect on the basis of permitted operations, which are the algebraic ones. This topic has been widely discussed and studied in the literature[45]. There is no doubt, however, that the Cartesian classification is almost a complete step towards the modern distinction of the curves between algebraic and transcendental curves, which will be explicitly codified by Leibniz.

Here we conclude our analysis, which is not aimed at going through these specific themes already widely analysed by other exhaustive studies but rather at showing that certain specific themes (like the one of the classification of the curves or the position of Cartesian geometry in the history of analytical geometry) are brought into a new light by the analysis of the relationship between the method expound in the *Regulæ* and *La Géométrie*. Cartesian Geometry no longer appears as a step in the formation of analytical geometry in the modern sense of the word. It is the result of a very particular view of mathematics, in which the concept of geometrical extension has a central role. Descartes' geometry is "analytical", not because it highlights coordinate method, but because it recalls a methodological principle (indeed "analytical") centered upon the "deductive" and "constructive" procedures of reasoning which are the heart of Cartesian philosophy.

University of Rome "La Sapienza"
Department of Mathematics

Notes

* This essay is a revised English version of the paper "Dalle *Regulæ* alla *Géométrie*" published in Italian in the book *Descartes: il Metodo e i Saggi*, Atti del Convegno per il 350° anniversario della pubblicazione del *Discours de la Méthode* e degli *Essais* (G. Belgioioso, G. Cimino, P. Costabel, G. Papuli, eds.), *Acta Enciclopedica* no. 18 (2 voll.), Istituto della Enciclopedia Italiana, Roma, 1990: vol. 18**, pp. 441-474. We thank the *Istituto della Enciclopedia Italiana* for the authorization to publish a new English version of the essay.

1 This is the meaning that Dijksterhuis attributes to the term "analytical geometry"; Descartes is considered its creator: "With the introduction of the new symbolic algebra in geometry he actually became the creator of analytical geometry and consequently the author of one of the most fundamental reforms in mathematics." (Dijksterhuis 1961, 543)

2 The "possibility of setting out propositions in deductive chains" which Cartesius speaks of is identified with the possibility of "expressing the acquired knowledge in axioms". Dijksterhuis continues: "The intention of the cartesian method is [...] to make scientific thought occur [...] through deduction, starting out with axioms, and through algebra." (*ibid.*, 542)

3 The original edition of the *Regulæ* is Descartes (ROP).

4 Descartes talks about these demonstrations "quae casu saepius quam arte inveniuntur, et magis ad oculos et imaginationem pertinent quam ad intellectum." (Descartes AT, X, 376 and LR, 34-36)

5 Descartes remembers to have read nearly everything from the beginning that is usually taught in Arithmetics and Geometry and comments: "Sed in neutra Scriptores, qui mihi abunde satisfecerint, tunc forte incidebant in manus: nam plurima quidem in iisdem legebam circa numeros, quae subductis rationibus vera esse experiebar; circa figuras vero, multa ispismet oculis quondammodo exhibebant et

ex quibusdam consequentibus concludebant; sed quare haec ita se habeant, et quomodo invenirent, menti ipsi non satis videbantur ostendere." (*ibidem*, AT, X, 375 and LR, 32)

6 Modern axiomatic mathematicians consider it the basis of their method. See J. Dieudonné's numerous references to the work of Euclid as a point of reference for the logical-deductive axiomatic method (he repeats that in order to find a valid reference modern axiomatics have to go back in history to Euclid). Cf. *e.g.* Dieudonné (1939).

7 The original edition is Descartes (1641). There exists a French translation of this text published in Descartes' lifetime: Descartes (1647). The quotations are taken from this translation (which Baillet maintains to be preferable to the Latin one), while references will be given both to it and to (Descartes AT, VII).

8 Note, in particular, the clear reference to the method of proving *ab absurdo* ("afin que, si on lui nie quelques consequences, elle fasse voir comment elles sont contenues dans les antécédents "), which Descartes implicitly declares not to wish to include in his method (as a consequence of his refusal of synthesis).

9 The fact that this formulation and the one of the *Regulæ* quoted in note 5 are nearly identical is rather important.

10 Also on that point the *Regulæ* and the *Meditationes* agree. The previous passage actually continues like this: "Les anciens géomètres avaient coutume de se servir seulement de cette synthèse dans leurs écrits, non qu'ils ignorassent entièrement l'analyse, mais, à mon avis, parce qu'ils en faisaient tant d'état, qu'ils la reservaient pour eux seuls, comme un secret d'importance" (Descartes 1647, 388). In the *Regulæ*: "Cum vero postea cogitarem, unde ergo fieret, ut primi olim Philosophiae inventores neminem Matheseos imperitum ad studium sapientiae vellent admittere, tanquam haec disciplina omnium facillima et maxime necessaria videretur ad ingenia capessendi aliis majoribus scientiis erudienda et praeparanda, plane suspicatus sum, quamdam eos Mathesim agnovisse valde diversam a vulgari nostrae aetatis [...]." (Descartes AT, X, 376 and LR, 34)

11 The contrary happens in metaphysics, where "la principale difficulté est de concevoir clairement et distinctement les prémières notions." (Descartes 1647, 389 and AT, VII, 157)

12 In modern mathematics the term 'synthetic' has taken a different meaning. It is true that the reaction to the "subordination" of geometry to algebra appeared as a return to the ancient world: exactly to "synthetic" geometry, which was seen as a way of doing geometry in an autonomous manner and not subordinated to the use of analytical procedures. This tendency was called "purism" (with the Italian mathematician Luigi Cremona as one of the most important exponents), since it suggested to restore the use of "pure" methods in geometry, which were free from any reference to algebra. In the "purist" movement, however, the return to intuition took a predominant, if not nearly obsessive, role. Reasoning in a "synthetic" way did not only mean to proceed with a sequence of logical operations which were to correlate the geometrical characteristics of the entities studied without recurring to algebraic instruments, but to "see" the result, to know it by intuition, make it evident for imagination. The prevailing trend of synthetic geometry of the 19th century expressed a reconquest of the "intuitive geometrical spirit" over the "abstract analytical spirit". Despite refusing the excesses of Cremonian "purism" later on, the Italian geometrical school defended "synthetic" geometry right to the bitter end: not so much as a refusal of the algebraic instrument but so as to support a vision of the "synthetic" method based on the use of intuition or, more precisely, on the psychological acquisition of geometrical concepts. For further details see Israel (1990).

13 For further detail cf. Taton (1951), 79-92.

14 Not from the one by Dieudonné, but the one by Monge-Lacroix, the influence of which reaches up to recent times.

[15]Cf. also Itard (1984, 277): "Descartes affirme plusieurs fois que les courbes organiques conduisent à une équation algébrique. Il n'affirme ni nie jamais la proposition réciproque."

[16]Cf. *ibid.*, 277-278: "A se placer au niveau élémentaire, la *Géométrie* de Descartes est un ouvrage parmi bien d'autres, et les accusations de plagiat [...] pleuvent de toutes part. [...] On pourra toujours trouver chez tel ou tel auteur contemporain ou plus ancien telle ou telle des idées émises par Descartes dans sa Géométrie."

[17]It is impossible to give even an approximate report on the vast secondary bibliography. So we will only remind of some texts which are among the closest to the subject we have been dealing with (and consequently close to the general lines we have been following), apart from the ones already quoted. First of all the important work by J. Vuillemin, to which we owe an essential contribution to bring the philosophic theme closer to the mathematical theme in Descartes' work: Vuillemin (1960). In this respect also Lenoir (1979). It specifies the connection between *Regulæ* and *Géométrie*, presenting, however, only a general analysis of the relations between the two. See also: Molland (1976), Coolidge (1940), Milhaud (1921), Granger (1968), Dhombres (1978), Scriba (1960-1962) and Schuster (1980).

[18] Despite the fact, as Bos observes, that "nowhere in the *Géométrie* did Descartes use an equation to introduce or to represent a curve." (Bos 1981, 322)

[19] Bos observes: "[...] the use of the key words, clear and distinct [...] show that Descartes saw a parallel between the series of interdependent motions in [a] machine, all regulated by the first motions, and the "long chains of reasoning" in mathematics, discussed in the *Discours de la Méthode*, which provided each step in the argument is clear, yield results as clear and certain as their starting point." (*ibidem*, 310)

[20] There is much evidence of that. In his texts numerous sentences like the following famous one can be found: "Mais ie ne m'areste point a expliquer cecy plus en detail, a cause que ie vous osterois le plaisir de l'apprendre de vous mesme, & l'utilité de cultiuer vostre esprit en vous y exerçant, qui est, a mon avis, la principale qu'on puisse tirer de cete science. Aussy que ie n'y remarque rien de si difficile, que ceux qui seront un peu versés en la Geometrie commune & en l'Algebre, & qui prendront garde a tout ce qui est en ce traité, ne puissent trover." (Descartes AT, VI, 374). The original edition of *La Géométrie* is Descartes (1637).

[21] On the other hand even Bos observes: "Although there were contradictions in the structure and the programme, there was an underlying unity of vision." (Bos 1981, 332)

[22] "Per intuitum intelligo, non fluctuantem sensuum fidem, vel male componentis imaginationis judicium fallax; sed mentis purae et attentae tam facilem distinctumque conceptum, ut de eo, quod intellegimus, nulla prorsus dubitatio relinquatur; seu, quod idem est, mentis purae et attentae non dubium conceptum, qui a sola rationis luce nascitur [...]." (Descartes AT, X, 368 and LR, 20)

[23] "Si quis igitur serio rerum veritatem investigare vult, non singularem aliquam debet optare scientiam: sunt enim omnes inter se conjunctae et a se invicem dependentes; sed cogitet tantum de naturali rationis lumine augendo [...]." (Descartes AT, X, 361 and LR. 6)

[24] It is interesting to note that Descartes refuses to establish any parallels between science and arts at the beginning of the Regulæ (cf. *Regula I*), due to the different nature of arts, where the employment of one speciality interferes with the employment of another, whereas, according to Descartes, the opposite holds true for the sciences, which are linked in such a way that they are more easily assimilated as a whole than separately.

[25] The verb "intueor" is understood above all in the sense of "consider attentively", "ponder over".

[26] "Sed hoc ita faciendum fuit, quia plurimae res certo sciuntur, quamvis non ipsae sint evidentes, modo tantum a veris cognitisque principiis deducantur per continuum et nullibi interruptum cogitationis motum singula perspicue intuentis."

[27] "Quamobrem illas continuo quodam imaginationis motu singula intuentis simul et ad alia transeuntis aliquoties percurram, donec a prima ad ultimam tam celeriter transire didicerim, ut fere nullas memoriae partes relinquendo, rem totam simul videar intueri." (Descartes AT, X, 388 and LR, 58-60)

[28] To be more precise, we should point out that Descartes' meaning of "uninterrupted" is the one closest to the modern concept of "continuous", in particular the one suggested by mathematical terminology. However, they only conform in part. We could say that the principles of completeness and continuity correspond to the Cartesian principles of continuity and absence of interruption respectively, with some translation of the meanings; but on the whole they convey an idea that is rather close to the one suggested by the concept of continuum used in modern mathematics.

[29] "[...] tanquam haec disciplina omnium facillima et maxime necessaria videretur ad ingenia capessendis aliis majoribus scientiis erudienda et præparanda." (Descartes AT, X, 375 and LR, 34)

[30] "[...] nam cum Matheseos nomen idem tantum sonet quod disciplina, non minori jure, quam Geometria ipsa, Mathematicae vocarentur." (Descartes AT, X, 377 and LR, 36)

[31] "Quod attentius consideranti tandem innotuit, illa omnia tantum, in quibus ordo vel mensura examinatur, ad Mathesim referri, nec interesse utrum in numeris, vel figuris, vel astris, vel sonis, aliove quovis objecto, talis mensura quaerenda sit."

[32] "Tota methodus consistit in ordine et dispositione eorum ad quae mentis acies est convertendo, ut aliquam veritatem inveniamus."

[33] This consequence is discussed in *Regula VI*.

[34] "Ad scientiae complementum oportet omnia et singula, quae ad institutum nostrum pertinent, continuo et nullibi interrupto cogitationis motu perlustrare, atque illa sufficienti et ordinata enumeratione complecti." (Descartes AT, X, 387 and LR, 58)

[35] A cumulative historian could say that Descartes did not have the concept of natural number.

[36] See First Part, Sections 24, 25, 26, 27.

[37] In the *Principia* extension is defined as the main attribute of a body (Part I, Sect. 53) and it is confirmed that the nature of a body only consists in being a substance with extension (Part II, Sect. 4). Moreover it is affirmed that size does not differ from what is big nor does number differ from what is numbered but through thought. Despite a substantial coherence of the two texts, the ways to establish identity between matter and extension are different. In the *Regulæ* it is methodical, whereas in *Principia* it is metaphysical.

[38] "Comment le calcul d'Arithmétique se rapporte aux operations de Géométric.Dijksterhuis "

[39] We only possess the ones from XIII to XXI (the last three are without comments).

[40] These differ from the "imperfectly understood" problems, which are part of physics and should have been dealt with by Descartes in his last twelve rules.

[41] "[...] sed unum quid ex multis simul implicatis dependens tam artificiose evolvendo, ut nullibi major ingenii capacitas requiratur, quam ad simplicissimam illationem faciendam."

[42] "Sed insuper ut quaestio sit perfecta, volumus illam omnino determinari, adeo ut nihil amplius quaeratur, quam id quod deduci potest ex datis [...]." (Descartes AT, X, 431 and LR, 142)

[43] Algebra consists in abstracting the terms of difficulty from numbers in order to examine their nature. Cf. *Regula XVI*. (Descartes AT, X, 457 and LR 194)

[44] "[...] non statim in difficilioribus et arduis nos occupari oportet, sed levissimas quasque artes et simplicissimas prius esse discutiendas, illasque maxime, in quibus magis ordo regnat, ut sunt artificum

qui telas et tapetia texunt, aut mulierum quae acu pingunt, vel fila intermiscent texturae infinitis modis variatae; item omnes lusus numerorum et quaecumque ad Arithmeticam pertinent, et similia [...]." (Descartes AT, X, 404 and LR, 92)

45 See, in particular, Boyer (1956), Vuillemin (1960) and Bos (1981).

ENRICO PASINI

ARCANUM ARTIS INVENIENDI: LEIBNIZ AND ANALYSIS

"Mathematics is an experimental science. The formulation and testing of hypotheses play in mathematics a part not other than in chemistry, physics, astronomy, or botany" (Wiener 1923, 271).

I Introduction

Leibniz was undoubtedly a many-sided man, and a polymathic mind, if ever there was one. The concept of analysis is notoriously, for its part, a polycephalous monster, and nearly all its meanings are spread through Leibniz's multifarious works, where the philosophical, epistemological, logical, and mathematical receptions of the term seem to be inextricably interwoven. Much the same is true of its counterterm, synthesis, and thus their mutual relation itself presents various aspects.

A thorough survey of these varieties lies far beyond the scope of the present study, and they have already supplied the subject-matter of some very good accounts (in particular Duchesneau 1993, 55-104). Here we shall just try to find some traces of what Goethe would have called a "red thread"—like the one he saw metaphorically twisted throughout the literary cordage of Ottilie's diary in the *Wahlverwandtschaften*. Analysis is introduced by Leibniz in juridical, scientifical, mathematical, or philosophical contexts, under different conditions and with different purposes; but even for such manifold uses should exist some common ground and univocal meaning. The analysis of thoughts and that of truths, the analysis of problems and that of things, all imply slightly or consistently different proceedings, and nevertheless they must perform somehow one and the same operation.

In a very general sense, analysis is for Leibniz, like for anyone else, the resolution of something complex into simpler elements. A procedure of this kind is applied, for instance, to physical objects by natural scientists. As Leibniz writes to des Billettes in 1697, they make use of "a certain analysis of sensible bodies, [protracted only to an extent] useful for the practice of their discipline" (Leibniz A, I, 13, 656). Depending on their object, such practices can in principle proceed in perfectly symmetrical manners, either from individual entities to universal features, or from universal concepts to particular instances. Thus Martial Gueroult distinguished two aspects of analysis with respect to Leibniz, one that "goes from

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 35–46.

the concrete to the abstract; this is the one which tends to ascend indefinitely towards the simple notions"; and another one "which, on the contrary, goes from the abstract to the concrete and, in principle, from the less to the more real" (Gueroult 1946, 251). There are Leibnitian texts on the analysis of physical bodies confirming this interpretation[1], but it is anyway somewhat too vague to be useful outside the immediate terrain of application.

II Truth Conditions

A first preciser specification of analysis, and a distinguishing one as for Leibniz's thought, is its application to truths, that is, as it may also be called, "conceptual" analysis:

> "According to Leibniz, truths of reason in general, and logical truths in particular, are necessary and eternal, true in all possible worlds, provable (*i.e.* reducible to identical propositions) in a finite number of steps, and hence 'analytic' in the strong sense (namely, the conceptual analysis that shows that the concept of the predicate is contained in that of the subject can be actually performed)" (Dascal 1988, 27).

Here a "truth" is the description of a state of fact expressed by one or several propositions in the form "subject-predicate" (substance-state), *i.e.* each proposition specifying a property of a determinated substance at a determinated instant of time—a property as such or a property acting as a non-relational "*requisitum*" to a relational state of things (Mugnai 1992). Leibniz writes in the § 33 of the *Monadology*:

> "When a truth is necessary, its reason can be found by analysis, resolving it into more simple ideas and truths, until we come to those which are primitive." (Leibniz GP, VI, 612)

In every propositional truth, the predicate is someway contained in the subject, connected by conditions that can be shown by analysis—just like mathematical theorems, Leibniz adds notably, "are reduced by analysis to Definitions, Axioms and Postulates" (*ibid.*).

So there must also be a reason, or a chain of reasons, for all truths of fact, that is to say, for contingent truths. They concern the sequences of events that constitute the universe of created beings, in which "the analysis into particular reasons might go on into endless detail" (*ibid.*, 613), because of the immense variety of things in nature and the infinite division of bodies.

> "There is an infinity of present and past forms and motions which join to make up the efficient cause of my present writing; and there is an infinity of minute tendencies and dispositions of my soul, which contribute to make its final cause." (*ibid.*)

And all this minuteness involves infinite other contingent objects and events, "each of which still requires a similar analysis" (*ibid.*). As Leibniz once briefly

condensed his theory of contingency, the root of contingency lies in the infinite (*radix contingentiae est in infinitum*): truths of fact are contingent, because no analysis can exhaust the infinite complexity of their truth conditions.

We are confronted here with the most general sense of the term, in which the concept of analysis is restricted to its fundamental elements. In so far as this is meant, it is true what Rescher maintains: that for Leibniz "'analysis' is a logical process of a very rudimentary sort, based on the inferential procedures of *definitional replacement* and *determination of predicational containment* through explicit use of logical processes of inference" (Rescher 1967, 23). But it's easy to find quite different epistemological conceptions of analysis in Leibniz's writings, in particular when questions concerning the scientific method are dealt with.

III There is Method in't

Leibniz felt a lively interest in the advancement of medical knowledge and of its methods. In a *De scribendis novis medicinae elementis*, written in 1680-82, we find the following remarks on the difference between analysis and synthesis in the study of pathology:

> "The method is truly analytical when, for every function, we investigate its media, or organs, and their modes of operating; thus we acquire knowledge of the body from [the knowledge of] its parts. After having completed this, we'll return to the synthesis, coordinating everything to the one, and we'll describe the prime motor, the instruments of motion (both the liquid and the solid ones), their connections, and the whole economy of the animal." (Pasini 1996, 214)

The synthesis is then drawn from theoretical principles, namely the Galenic distinction of vessels, humours and spirits, out of which Leibniz's favourite definition of the animal body as an "hydraulo-pneumatical-pyrobolical engine" can easily be deduced.

Synthesis is here an *a priori* proceeding, while analysis is a method to acquire empirical knowledge. Both contribute to the investigation of physiology, but analysis seems to act as first, being the chief means to systematically gather information, whereas synthesis represents the correct foundation by which it is possible to gain systematicity for the information collected. This conception, of course, is not in any way peculiar of Leibniz[2].

If we read further in the *De scribendis novis medicinae elementis*, towards the end we encounter again the opposition of analysis and synthesis; this time the matter is not the method of investigation, but the communication of knowledge. Both analysis and synthesis again play a defined role: this is quite relevant, since the idea that analysis pertains mainly to discovery and synthesis to explaining and teaching is at Leibniz's time very close to a commonplace.

"Duplex Methodus tractandi Morbos", he declares, *"una Analytica per symptomata, altera Synthetica per causas"* (*ibid.*, 217). Disease can be considered analytically, based upon symptoms, or synthetically, based upon causes. It is important to teach first the true analysis of illness, writes Leibniz further, namely "the art both to inquire into the signs, and to identify an illness by means of signs" (*ibid.*). Synthesis will be taught only after giving a specimen of analysis, *i.e.* "a general healing method, which is to the pathological synthesis what algebra is to the elements of geometry" (*ibid.*). Here again we see Leibniz draw a parallel with mathematics, and in particular between the method of analysis in general, and algebra—that is, for a mathematician of his time, analysis in the most proper sense.

IV The Anatomy of Wit

Leibniz maintains, more in general, that inventive people who make discoveries and enlarge knowledge usually proceed in two ways: "per Synthesin sive Combinationem et per analysin" (Leibniz VE, 1362), as we read in a *De arte characteristica et inventoria*. Combination, or synthesis, is a conjunction of thoughts, maybe even arbitrary, so devised as to let some new knowledge arise. Analysis requires dwelling upon the proposed subject, and to resolve its concept into other simpler concepts, or to determinate its requisite elements or components.

Leibniz observes that all inventive spirits are either more combinatorics or more analytical in disposition. A combinatorics wit can recall things past and connect them to present needs and experiences. Analytics thoroughly examine present things, but remain so immersed in their object as to limit their power of observation. Combinatorics spirits are superior, because their ability is a rare gift: *"Combinare vero remota promte, non est cujusvis"* (*ibid.*, 1363).

In the second version of a programmatic sketch *De arte combinatoria scribenda*, Leibniz remarks analogously:

> "I must premise a chapter concerning the difference between the analytical and the combinatory method, and the difference between analytical and combinatory wits." (Leibniz VE, 1098)

Analytical wits, according to him, are more short-sighted, so to speak, while combinatorics ones are rather long-sighted (*"Analytici magis Myopes; Combinatorii magis similes presbites"*, *ibid.*, 1099): in fact, in analysis it is suitable to pay attention to fewer things, but with more precision, whereas combinatorics considers many things together, and much more perspectively; thus analysis has more in common with miniature painting, and combinatorics with large-scale sculpture.

Analysis is much easier to apply, since it consists of definable procedures:

> "Once a procedure of analysis is detected, it requires only attention, or firmness of mind [...] and indeed there are such people, whose wit is not vagabond, and who are able to reckon in their imagination, even without paper and pencil." (*ibid.*)

Combinatorics, on the contrary, requires to quickly and promptly browse a manifold of subjects, and to treat them in unexpected ways. Their practical instruments also differ: people with a weaker imagination make use of figures and symbols to better focus questions, while those with a weaker memory and unable to represent many things together, are helped by the use of tables. *"Characteristica vera et tabulis et analysi auxiliatur"* (*ibid.*).

In the art of discovery, that is in the course of knowledge, both analytic and combinatorics spirits, as we read in the *De arte characteristica et inventoria*, will particularly profit of a method. The method is described in general: *"Methodus inveniendi consistit in quodam cogitandi filo id est regula transeundi de cogitatione in cogitationem"* (*ibid.*). Method means something that provides the thinking processes with a leading thread, *i.e.* with a rule regulating the movement from one thought to the other. The rule must consist in a palpable instrument: as the compass rules the hand in correctly tracing a circle, for correct thinking *"instrumentis quibusdam sensibilibus indigemus"* (*ibid.*). These palpable instruments of thought are again tables for the combinatorics and characters—symbols—for the analysis[3].

"Characterem voco quicquid rem aliam cogitanti repraesentat" (*ibid.*)—a character is anythings that represents another thing to a thinking person. If we could keep the things themselves before us, we would have less need for such characters. The representation is based on some relation or rule of correspondence between them: so the ellipse represents a circle by being its projection. Models and figures of things can be considered as characters: they too are crafted so as to express the essence of the thing. Characters do not need to be similar to the objects they represent: numerical symbols express correctly the properties of number, but they do not resemble them.

V Thought Instruments

This conception of the method as an instrument, or a collection of instruments and techniques, rather than a set of precepts, marks one of the most important differences between Leibniz and the greater part of his contemporaries, notably Descartes. For Leibniz a method "is" an instrument, and an instrument, in the method of analysis, is an algorithm based on characters. Hence, on non-mathematical ground too, analysis is in principle a symbolic operation for Leibniz. Moreover, systems of symbolic operations, *i.e.* algorithms, can legitimately be used, both for the comprehension and organization of existing knowledge and for the creation of new knowledge, also outside their traditional grounds.

The construction of general methods for the acquisition, sharing and transmission of knowledge, in the form of complex algorithmic instruments for logical and conceptual calculus, is an idea that dates back to the young Leibniz. Adolescent, he devised an "alphabet of human thoughts": it will grow into one of Leibniz's greatest projects, that of an art of discovery based on a "characteristic" (art of characters or symbols) of general use for combinatorics and analysis at the same time.

An analysis of our thoughts (*analyse de nos pensées*), states Leibniz in 1684, is "of the greatest importance both for judging and for inventing" (Leibniz A, I, 4, 342). This analysis of thought, he specifies elsewhere, "*respondet analysi characterum*", corresponds to a symbolic analysis, in that characters can express our thoughts and their relations, thus providing our reasonings with a "mechanical thread" (Leibniz VE, 811). This idea is explained more clearly in many programmatic essays, one of which received the not particularly original title of *Initia et specimina scientiae novae generalis* ("First steps and examples of a new general science"). Leibniz distinguishes between dialectics, or analysis of opinions, and analysis of truths; the latter, he affirms, is the secret for the development of the art of invention and discovery:

> "I shall also add the vulgar analysis of human judgements, *i.e.* the principles on which human opinions are based, that are dialectic and ought not to be despised. It wouldn't be necessary to bring them into surer principles, only with the purpose to confirm something we already know. But since the whole secret of the art of discovery [*totum arcanum artis inveniendi*], by virtue of which human science could make an immense progress, depends on the analysis of truths (that is the emendation of our thoughts), it is convenient to proceed to the highest levels of analysis." (Leibniz VE, 702)

This art will comprehend a method to perform rigorous demonstrations in any field, "equal or even superior to mathematical ones, which suppose many elements that here could be demonstrated" (*ibid.*). It is a wholly new calculus that, according to Leibniz, is at work, in every human reasoning and is nevertheless as accurate as arithmetical or algebraic calculations are.

The same concepts are repeated ever and again in Leibniz's countless manifestoes for this new discipline:

> "Since when I had the pleasure to considerably improve the art of discovery, or analysis, of the mathematicians, I began to have certain new views, that is, to reduce all human reasoning to a sort of calculus, which would be of use in discovering a truth in so far as it is possible *ex datis*, *i.e.* from what is given or known." (Leibniz GP, VII, 25)

A universal writing would also result from it, that "would be like a sort of general algebra, and would provide the means to perform reasoning by calculation" (*ibid.*, 26): such a calculus would not only be an instrument for learning and research, but it would be an infallible judge of controversies as well, offering a way to solve disputes by simple reckoning.

Leibniz explains this extended meaning of calculus in a letter he wrote to Tschirnhaus in 1678: "*Nihil enim aliud est Calculus, quam operatio per characteres, quae non solum in quantitate, sed et in omni alia ratiocinatione locum habet*" (Leibniz GM, IV, 462). A calculus is nothing else than an operation performed by means of characters—that is, an algorithm of symbolic analysis—that takes place not only with quantities, but in any kind of reasoning as well.

VI The Place of Analysis

The place of analysis in this more general frame is, as one may expect, quite variable. In a short and schematic note, Leibniz lists the chapters for a work to be entitled *Guilielmi Pacidii Plus Ultra sive Initia et specimina scientiae generalis*. There we find among others the following arrangement of analysis and synthesis, combinatorics and discovery, *mathesis* and art of invention:

> " 10. De arte inveniendi
> 11. De synthesi seu arte combinatoria
> 12. De Analysi.
> 13. De combinatoria speciali, seu scientia formarum, sive qualitatum in genere sive de simili et dissimili
> 14. De Analysi speciali seu scientia quantitatum in genere seu de magno et parvo
> 15. De mathesi generali ex duabus praecedentibus composita." (Leibniz GP, VII, 49-50)

Analysis and combinatoric in general seem to be tied to the art of invention; two more specific versions, that concern quantity and form, are presented as the two branches that compose universal mathesis[4].

Another, more detailed program is rubricated *Initia et specimina scientiae generalis*. It describes at length the structure of a complex work, dedicated to the "*instauratione et augmentis scientiarum*" (Leibniz GP VII, 57). After a first book dedicated to the logical form of arguments and to the ways to determine the eternal truths, the second book should treat *de arte inveniendi*, the "art of discovery, namely that of the tangible thread by which investigation is ruled", and of its divisions, "*ejusque artis speciebus*", namely combinatorics and analytics (*ibid.*).

In the *Fundamenta calculi ratiocinatoris* (1688-1689) Leibniz defines the calculus used in the universal art of characters as follows: "A calculus or operation consists in the exhibition of relations, performed by the transmutation of formulas according to some prescribed rule" (Leibniz VE, 1205); again, it might well be an exemplary definition of the analytical proceedings. And anyway, for Leibniz, any analytical calculation is a formal argument: as we read in a letter to the palatine countess Elisabeth of 1678:

> "un calcul d'analyse est un argument *in forma*, puisqu'il n'y a rien qui y manque, et puisque la forme ou la disposition de tout ce raisonnement est cause de l'evidence." (Leibniz A, II, 1, 437)

When Leibniz defines combinatorics in his *De artis combinatoriae usu in scientia generali* (of 1683-84), he states that "*Combinatoria agit de calculo in universum*", the combinatorics art deals with every aspect of the calculus, "that is to say, with universal marks or characters [...] and their rules, dispositions and processes, or with formulas universally. Of this general calculus, the algebraic calculus is a species, *i.e.* the one based on the laws of multiplication" (Leibniz VE, 1354).

If even combinatorics reveals itself blatantly to be framed just like analysis, on the other hand mathematical analysis is clearly, as Leibniz himself often affirms, a specimen of the *ars characteristica*. In 1691 Leibniz writes to Huygens that:

> "The best and most convenient feature in my new calculus is this: that it exhibit truths by means of a sort of analysis, without any of those efforts of the imagination, that often succeed only by chance, and thus gives us the same advantage over Archimedes that Vieta and Descartes let us gain over Apollonius." (Leibniz GM, II, 104)

The infinitesimal calculus, he means, frees the geometer from the need to concentrate on the geometrical situation of the problem in order to devise a helpful construction, such as the insertion of a suitable ad-hoc linear segment.

Three months later Leibniz hammers again the qualities of his calculus in Huygens' mind, and he supports his argument with an example:

> "I remember that, as I once studied the cycloid, my calculus presented to me the greater part of the discoveries that have been made on the subject, nearly without any need for meditation. Indeed, what I like best in this calculus, is that it gives us the same advantage in the field of Archimedean geometry that Vieta and Descartes have given us in Euclidean and Apollonian geometry, since it exempts us from working with the imagination." (*ibid.*, 123)

In fact, from the study of the function it is possible to exhibit numerous geometrical properties of the curve, by way of analysis: "*Caeteraque omnia circa cycloidem inventa, pluraque alia similiter ex tali calculo analytice derivantur.*" (Leibniz GM, II, 118)

VII Calculus on My Mind

Leibniz often intends by "analysis" a particular analytical method or a set of analytical techniques, developed by other mathematicians, and from some writings of his, one might imagine that "*quot sunt capita, tot sunt analyses*". Leibniz is clearly conscious of the novelty and peculiarity of his mathematical discoveries. He writes in 1692:

> "I have developed a new analysis concerning the infinite; it is quite different from Cavalieri's geometry of indivisibles and from Wallis' arithmetic of infinite series, since it doesn't depend on lines as the former, nor on numerical series, as the latter, but it is general, and thus symbolic or Specious. But instead of the vulgar analytical calculus applying to powers and roots, it performs the calculus of differences and summations." (Leibniz GM, V, 263-264)

"Vulgar" analysis (*i.e.* the algebra of Descartes, his mathematical and philosophical *tête de turque*) is often reprehended by Leibniz, since it doesn't comprehend some of the most fascinating concepts of seventeenth century mathematics (infinitesimals, imaginary numbers), nor some of the most important objects of Leibniz's analytic research (transcendent relations, the theory of determinants).

A very important methodological distinction is drawn in a famous letter addressed to Antonio Magliabecchi. There are two forms of analysis, states Leibniz here; first comes the analysis of Vieta and Descartes, that is considered by the moderns to be the only analysis, and "that solves every problem, studying the relation of the unknown to the known quantities" (Leibniz GM, VII, 312). The other one has its scope in reducing the problem "to a different problem, easier than the first one" (*ibid.*). The latter was known also to the ancients, as it appears, for instance, from the *Data*. In writing to Huygens, Leibniz explains this distinction as that between analysis "*per saltum*" and "*per gradus, cum problema propositum reducimus ad aliud facilius*" (Leibniz GM, II, 116-117). The first one is more absolute, but the second often works better.

In *De methodis synthetica et anagogica applicandis in algebra*, the synthetic method is defined analogously: "*cum problema difficile soluturi incipimus a facilioribus*" (Leibniz VE, 1095). Leibniz also observes that algebra performs a fake synthesis, in treating the unknown quantities as if they were known. The anagogic method is that of pure analysis, "*quae nihil syntheseos habet*" (*ibid.*); and the "*Data veterum*" are of pertinence to the anagogic method, that hence appears to be the heir of the method described to Magliabecchi. Here we proceed backwards, "always reducing the problem to another, easier problem. And this is my method" (*ibid.*), adds Leibniz, used for ordinary equations, but also for the resolution of the ordinates of a curve, viz. in transcendent problems.

Another front is to be opened soon. As Leibniz writes to Mélchisedec Thévenot in 1691:

> "Since I believe that geometry and mechanics have now become fully analytical, I have devised to extend the calculus to other subjects, even to subjects that until now nobody thought would have supported it." (Leibniz A, I, 7, 356)

And he adds, as usual: "Here I mean by 'calculus' every notation representing a reasoning, even without any relationship to numbers" (*ibid.*).

In 1679, four years after the completion of his work on the fundamentals of the infinitesimal calculus, Leibniz writes to Huygens: "Mais apres tous les pro-gres que j'ay faits en ces matieres, je ne suis pas encor content de l'algebre" (Leibniz GM, II, 18-19), after all the work I did with algebra I think we need something different and more powerful in treating with geometrical entities. It is "une autre analyse proprement geometrique ou lineaire, qui nous exprime directement *situm*" (*ibid.*, 19): an analysis specific to loci, *i.e.* an analytic topology. Algebra represents quantities by appropriate symbols and operations: other symbols and operations can calculate forms, angles, orientations, movements, in their qualitative aspects too.

The most important use of this analysis, anyway, is to help in geometrical reasoning: "on trouve ainsi par une espece de calcul", the same words used to describe the advantages of infinitesimal analysis, "tous ce que la geometrie enseigne jusqu'aux elemens d'une maniere analytique et determinée" (*ibid.*, 26). By this calculus it is possible to determine analytically everything that belongs to geometry, up to its most fundamental elements.

As an obvious example of immaterial cognitive technology, this new *analysis situs* is, of course, an art of characters, and an art of invention: "Cette caracteristique", adds Leibniz, will even express in symbols all mechanical structures and will help us to find new geometrical constructions, "à trouver de belles constructions", since it contains at one time both the procedures of calculus and of construction (Leibniz GM II, 30-31).

VIII An Engine for Your Thoughts

"*Quod omnium maxime quaero est Machina, quae pro nobis faciat operationes analyticas, quemadmodum Arithmetica a me reperta facit numericas*" (Leibniz A, VI, 3, 412). What I most desire, Leibniz writes already in 1674, is a machine that performs analytical operations, just as the calculating machine he invented carries out the arithmetic ones. This idea of an analytical engine is hindered, one may say, by the inadequacy of its programming language, since "the universal analysis depends on the development of a universal character" (Leibniz A, VI, 3, 413). Meanwhile, for the use of complex reasonings, it is acceptable to surrogate the required special-purpose characters with generic characters, such as the letters used in geometry[5]. But in general the signs we presently use to compose analytical formulas, adds Leibniz, can't suitably express the mental operations involved in their treatment by means of simple analytical procedures as transpositions or linear transformations. Anyway, it is not an impossible task, since "*omnes cogitationes non sunt nisi simplices complicationes idearum*" (*ibid.*): thoughts derive in the ultimate analysis from simple components, simply combined, as words are composed by simple letters, and the complex apparatus of thoughts needs only to be brought back to such simplicity.

But in reality our thoughts are not so transparent: even if we were able to perform thorough analyses of the concepts we use, we would not *ipso facto* be aware of its results at any moment of our thinking processes: "when a notion is very composite, we can't think of all its ingredients together, as with an intuitive notion" (Leibniz GM, IV, 610). Leibniz discusses such issues in his *Meditationes de cognitione, veritate et ideis*, a short essay published in 1684 and dealing mainly with the classification of ideas into clear, distinct, obscure, adequate etc. We have a distinct notion of something, Leibniz affirms, if our knowledge contains enough marks to discern it from all similar objects. But "in most cases, in particular when a very complex Analysis is required, we can't represent intuitively the whole nature of the object, and we use signs instead" (*ibid.*).

This sort of reasoning, says Leibniz, can be called "blind reasoning, or also symbolic reasoning, as we make use of in Algebra and Arithmetic, and indeed at every moment" (*ibid.*). Symbols, like those of analysis, are the true instruments of thought: in particular, they are for human thought a sort of indispensable blind-flying instruments—under conditions where normal thought is "blind-thought". "*Et huius generis cogitationes*", in Leibniz's words, "*soleo vocare caecas, quibus nihil apud homines frequentius aut necessarium magis*" (Leibniz A, VI, 2, 481). This is the most intimate kernel and the real operational mode of human thought: that it operates mostly by means of symbols, that is to say it operates in the same way as algebraic algorithms, or analytical algorithms do—those of the "literal" or "specious" analysis. That's why this last one is so successful, and useful, and sure in matters so difficult and general as reasoning and problem solving: "*Hinc Symbolica illa recentiorum analysis [...] tanti est ad celeriter et secure ratiocinandum usus*" (*ibid.*).

The *cogitatio caeca* or *symbolica* finally is, according to Leibniz, in itself the best human instrument for problem solving, that is to say for the augmentation of "both knowledge, and happyness" (*ibid.*)—and mathematical analysis mirrors it. Not bad, in the end.

Notes

1 In the *De modo perveniendi as veram corporum analysin* of 1677: "Duplex est resolutio: una corporum in varias qualitates per phenomena seu experimenta, altera, qualitatem sensibilium in causas sive rationes per ratiocinationem" (Leibniz GP, VII, 268). If we combine such analyses with experiments, adds Leibniz, we'll easily determine the causes of any quality found in any physical subject.

2 For instance, a quite conformable statement can be read in Newton's *Optics*: "The Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phænomena proceeding from them" (Newton 1704, 405).

3 It must be observed that the instruments intended for the combinatorics are mostly traditional, static and trite; the instruments for analysis powerfully embody innovation.

4 And in the *Elementa nova matheseos universalis* (written between 1684 and 1687): "*Tradetur et Synthesis et Analysis, sive tam Combinatoria, quam Algebra.*" (Leibniz VE, 987).

5 In this way, if the specific knowledge that enters in a logical calculation is already set up, it will be easier to coordinate this particular specimen of the art to the general frame of the universal characteristic.

CRAIG G. FRASER

THE BACKGROUND TO AND EARLY EMERGENCE OF EULER'S ANALYSIS

I Introduction

In cultivating analysis Euler is sometimes seen as someone whose primary achievement was the development of tendencies in the Leibnizian school. Typical here is Bourbaki's statement (Bourbaki 1974, 246) that he carried "the Leibnizian formalism to an extreme" thereby "completing the work of Leibniz". A somewhat different view is expressed by Boyer (Boyer 1939, 243) who calls attention to Euler's originality: "Most of his predecessors had considered the differential calculus as bound up with geometry, but Euler made the subject a formal theory of functions which had no need to revert to diagrams or geometrical conceptions"[1].

The present paper is devoted to a study of the role of analysis in the background to and early development of Euler's mathematical research. Euler's *Methodus inveniendi lineas curvas* of 1744 (Euler 1744), the first systematic treatise on what would later become known as the calculus of variations, is here identified as the locus classicus for the initial emergence of a fully analytical conception of the calculus. The work contained many of the technical and notational innovations that would be elaborated in his mid-century textbooks on infinitesimal analysis. In addition, in chapter four of the treatise Euler developed the subject in a way that exhibited its analytical character at a deeper theoretical level.

To understand the origins of Euler's programme we first provide a survey of analytical conceptions in earlier mathematics. We then turn to a consideration of the relevant parts of the *Methodus inveniendi*, ending with a discussion of the mathematical and philosophical character of his approach to analysis.

II Analytical Methods in Early Modern Mathematics

It is possible to trace a continuous development in European mathematics that begins in the thirteenth century and leads by 1700 to the extensive employment of symbolic methods. Techniques of analysis came to play an important role in such distinct areas as the theory of determinate equations, arithmetic, coordinate geometry and the calculus. Our survey will focus on the emergence of the concepts

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 47–78.

of equation and variable, and on the question of the degree to which symbolic methods formulated essential mathematical features of the subject under study.

II.1 ANALYTIC ART

The concept of analysis and the name itself became part of early modern mathematics largely as a result of the work of François Viète. His essay of 1591, *In artem analyticen isagoge* (1591*a*), initiated a series of researches by himself and such contemporaries of his as Marino Ghetaldi and Thomas Harriot that together contributed to the widespread employment in the seventeenth century of symbolic mathematical methods.

A substantial historical literature, deriving from the work of Jakob Klein (Klein 1934-1936), emphasizes Viète's modernity as a mathematician. It is suggested that his notion of specious logistic involved a theoretical widening of the concept of magnitude to include both arithmetic and geometric quantity. In adapting ideas from Diophantus's arithmetic to the realm of geometric analysis he was led to generalize Diophantus's concept of species. According to Klein (*ibid.*, 166-167), "the *eidos* concept, the concept of the 'species', *undergoes a universalizing extension while preserving its tie to the realm of numbers. In the light of this general procedure, the species*, or as Viète also says, the 'forms of things'[...] *represent 'general' magnitudes simply*"[2].

Associated with this general concept of number, it is suggested, there emerged in his analytic art, with its use of symbols to represent unknowns and parameters, a structural, syntactic approach to mathematics[3]. Because the terms of his system could be given different interpretations in arithmetic and geometry the purely combinatorial properties of operations performed on analytical expressions were exhibited as an object of interest.

Klein's essay and the historical writings it has inspired have resulted in a renewed interest in Viète's algebra and have led to a better appreciation of his role in early modern mathematics. We will however argue in what follows that suggestive and informative as Klein's essay has been, his whole thesis must be qualified at certain fundamental points.

The widening of the concept of magnitude that is attributed to Viète had already taken place and was well assimilated within algebraic practice at least a century before he wrote. Algebra was known as "the art of the thing and the power" or "the great art" or "the greater part of arithmetic". The progress of symbolic methods consisted of the replacement of the largely rhetorical procedures inherited from Islamic mathematicians by ones that used a syncopated or partial formalism in the solution of problems involving the determination of an unknown quantity. Study of quadratic, cubic and quartic equations led to the introduction of expressions denoting the roots of non-square numbers; thus magnitudes traditionally regarded as geometrical entities were denoted as numbers within the confines of what was essentially an arithmetical algebra.

In emphasizing the radical character of the Viètan concept of magnitude, Klein has overlooked the full *mathematical* significance inherent in the assimilation (well established by 1590) of surd numbers into arithmetical algebra.He is to be sure aware of this earlier tradition, writing that "the new number concept [...] already controlled, although not explicitly, the algebraic expressions and investigations of Stifel, Cardano, Tartaglia, etc." (*ibid.*, 178). Nevertheless he concludes of the cossist school that "in its whole mode of operating with numbers and number signs, its self-understanding fails to keep pace with these technical advances. This algebraic school becomes conscious of its own 'scientific' character and of the novelty of its 'number' concept only at the moment of direct contact with the corresponding Greek science, *i.e.*, with the *Arithmetic* of Diophantus" (*ibid.*, 148). To this one may reply in two ways. Self-consciousness on the part of researchers, however significant, is not necessary in order for important conceptual advances to take place; the latter may be, as they were for the cossist algebraists, logical concomitants of technical developments within the subject itself. Second, if indeed an explicit awareness of conceptual advance is present it is necessary to show how this influenced and shaped the direction of mathematical research.

Another difficulty with Klein's thesis is that it understates the extent to which Viète situated his notion of species within a classical Euclidean theory of magnitude. He seems to have regarded the general magnitudes of his specious logistic as geometrical entities. He uses the words "ducere" and "adplicare", terms denoting geometric operations, in his definition of the multiplication and division of magnitudes (writing for example, "magnitudinem in magnitudinem ducere"), and retains dimensional homogeneity as a fundamental principle. His vision of a general theory of quantity applicable to either number or line segments was already realized in *Elements* V, a part of the Euclidean canon that he drew upon in chapter 2 of his *Analytic Art.* (Advocates of the notion of "symbolic magnitude" never explain how book V of the *Elements*—a general theory of magnitude without symbols in the Viètan sense—is possible.)

Certainly Viète showed a stronger interest in mathematical method than had earlier researchers. To attribute to him a radical new syntactic or structural conception of mathematics seems however doubtful. He viewed analysis not as an autonomous subject but as an "art", as a tool in solving problems, be they ones in geometry, the theory of equations or Diophantine arithmetic. The content of mathematics was for him not a system of relations but a set of concrete problems in these subjects. His notational innovations were developed within this historically particular programme of research. His technical vocabulary and fondness for formal categories indicate the continued influence on him of scholastic thought. Incongruous mathematical elements were contained in his attempt to adapt ideas

from Diophantine arithmetic, essentially a work of rational number theory, to the art of algebra as it was employed in the solution of geometrical problems.

Viète's conceptual advances, the introduction of distinct symbols for variables and parameters and the adoption of an operational formalism, represented a significant contribution to mathematical method. They provided an orderly and uniform notation for handling the material on algebraic identities and polynomial equations that had appeared in Cardano's *Ars Magna* (Cardano 1545), and permitted the emergence of "the first consciously articulated theory of equations" (Mahoney 1973, 36). Perhaps most important mathematically, his notational system allowed one to investigate the relationship between the coefficients of a polynomial and the structure of its roots; it must be said however that this last line of investigation developed slowly and only became established in the later eighteenth century.

Of considerable conceptual significance, particularly for the later development of the calculus, was the idea of a function. The notion of a general expression $f(A)$ defined in terms of the variable A was present in embryonic form in Viète's system, where the square of the magnitude denoted by the symbol A was denoted by an expression ("A quadratus") that itself contained A. Instead of the "res" and the "census" of traditional algebra, separate terms denoting distinct entities, one now had a notation that reflected the underlying operations performed on the magnitudes being represented. That the functional idea could only receive a somewhat limited development by Viète was a consequence of the fact that he viewed his symbol "A" not as a variable in the full sense but as an unknown, an object whose value was to be determined in the course of the solution of a problem (Boyer 1956, 60). His definition of an equation, "the coupling of an unknown magnitude with a known" reflected this particular perspective.

II.2 Theory of Numbers

The figures of Euclidean plane geometry are coherent unitary objects whose identity is defined in terms of certain universal attributes, such as being three-sided or being right-angled. Results in geometry become theorems by virtue of the inherent generality of figures as mathematical objects. As commentators from Leibniz to Frege have emphasized, whole numbers—the objects of arithmetic—are different sorts of things, possessing particular individual characteristics[4]. Propositions in Euclidean arithmetic (*Elements* VII, VIII and IX) are formulated in terms of classes of numbers, such as being prime, being perfect, or being a member of a geometric progression. These classes are delineated rhetorically, without the aid of symbolic notation.

It is ironic that Viète turned to Diophantus's *Arithmetic*, a work of rational number theory, as a source of inspiration for developing methods in algebra and

geometry, the sciences (for him) of continuous magnitude. An opposite sort of irony characterized Pierre Fermat's extensive researches in theoretical arithmetic[5]. In his study of geometry he adopted Viète's system of notation, using it to formulate mathematically the idea of coordinate geometry. He also studied the *Arithmetic* carefully and greatly extended the results contained there, in the process laying the foundation of modern number theory. Throughout these latter researches he employed a predominately rhetorical mode of presentation. Although he used hindu-arabic numerals and some signs for arithmetic operations, his statement and demonstration of theorems were presented in words without the aid of symbolic notation.

The style of Fermat's writings is illustrated by a comparison with Euclid, whose mode of expression in number theory was also rhetorical. Consider Euclid's assertion (*Elements* IX, 36) that a number of the form $2^{p-1}(2^p-1)$ is perfect if 2^p-1 is prime[6]: "If as many numbers as we please beginning from an unit be set out continuously in double proportion, until the sum of all becomes prime, and if the sum multiplied into the last make some number, the product will be perfect".

Consider now Fermat's original statement of what is known as Fermat's little theorem, the assertion (in modern mathematical language) that p divides $a^{p-1}-1$, where a and p are relatively prime numbers[7]: "Without exception, every prime number measures one of the powers -1 of any progression whatever, and the exponent of the said power is a submultiple of the given prime number -1" (Fermat, TH, V. 1, 209).

In his rhetorical expression as well as in his interest in integral rather than rational solutions Fermat seemed to be looking past Diophantus to the arithmetic books of Euclid's *Elements* as a source of inspiration. In 1657 he explicitly criticized the use of geometrical considerations in arithmetic (presumably because they entailed conceptions of continuous magnitude) and, appealing to Euclid, urged that "arithmetic redeem the doctrine of whole numbers as a patrimony of its own"[8]. Although many problems of rational arithmetic reduced to ones of whole-number arithmetic it was also the case that certain interesting questions in the latter subject became trivial when the class of permissible solutions was extended to rational numbers. It is very possible that his disinclination to use literal notation derived from a desire to emphasize the autonomy of whole-number arithmetic.

There is it must be noted some evidence that Fermat privately employed algebraic methods in his arithmetic researches, and some of his correspondents suspected him of having done so. His contemporary Descartes made use of formulas to express arithmetical results. Nevertheless, in all of his extant writings, in all of the different phases of his research, Fermat did not employ symbolic algebraic notation.

The awkwardness of rhetorical formulations and the need for more and more detailed statements of results eventually imposed restrictions on the sort of theory

that could be developed. Fermat's decision not to give a fuller account of his researches may have derived in part from the demands that such a mode of exposition entailed. The concept of an arithmetic variable—an entity that could assume any of a given set of whole-number values—was central to the progress of number theory as it was to develop after him. It enabled one to reify in formulas expressions and relations that could then be studied or manipulated at will in the course of the investigation.

It should nevertheless be remembered that at the most fundamental level it was numbers and their properties, and not any system of relations embodying these properties, which constituted the fundamental subject of the theory of numbers. The role of the variable was not an essential one; each symbolic statement could always be re-expressed in terms of a proposition about classes of numbers.

II.3 Coordinate Geometry[9]

Euclid and Apollonius had derived results about curves that express relations of equality between magnitudes associated with these figures, relations that are valid for an arbitrary point taken on the perimeter of the curve. In *Elements* III, 36 one is given a point D outside of a circle and asked to draw from it two lines; the first DB is tangent to the circle and the second DCA cuts the circle at the points C and A (fig. 1). Euclid showed that the square on DB is equal to the rectangle on DC and DA. In book I of the *Conics* Apollonius introduced the ellipse as the section obtained by intersecting a plane with an oblique circular cone (fig. 2). Such a cone is formed by the lines joining the perimeter of a circle to a point not in the plane of the circle. Let PP' be a given axis through the centre of the ellipse and let Q be a point on the perimeter of the ellipse. Consider the line VQ of intersection of the plane of the ellipse and the plane of that circle through V which is parallel to the base; Q is the point where the line meets the ellipse. The line VQ is called an "ordinate". In I, 15 Apollonius showed that the rectangle on PV and VP' is in a given constant ratio to the square on VQ.

In these propositions the curve is introduced and the given relation is then exhibited as a property satisfied by it. The relation represents one of several properties and is not regarded as defining or definitively expressing the curve. The primary purpose of the results is found in the solution of other problems. In *Elements* IV Euclid used III, 36 in his construction of the regular pentagon. In *Conics* III Apollonius employed the theory of the earlier books in his investigation of the problem of the locus to three and four lines.

This last problem is of great historical significance for the later development of coordinate and projective geometry and possesses in its own right certain points of conceptual interest. Consider four lines given in position in the plane. It is necessary to determine the locus of points P such that the rectangle formed by the

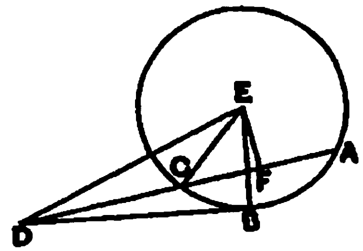


Figure 1

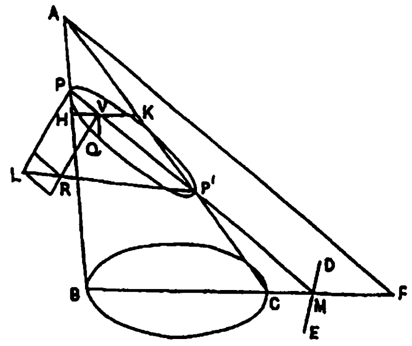


Figure 2

distances from P measured in given directions to the first two lines is in a specified ratio to the rectangle formed by the distances measured in given directions to the other two lines. (In the case of three lines one of the rectangles becomes a square.) It turns out that the locus is in every instance a conic section. In the same book Apollonius provided a detailed discussion of the problem, developing results that would (at least in principle) form the basis for a complete solution[10].

In book VII of his *Collection* Pappus called attention to the three and four line problem and discussed the work of earlier geometers[11]. He also raised the question of the nature of the locus when the number of lines exceeds four. The distances that appear in this problem are magnitudes that are assumed to vary while the relation expressed by the locus condition itself continues to hold. (This relation was expressed in two forms by Pappus, in terms of the ratio of figures or solids, and for the more general case in terms of compound proportions.) What logically distinguishes these magnitudes within the problem is that they vary, and that the locus is produced in consequence of their variation. The concept of a variable would therefore seem to be implicitly present in Pappus's formulation.

The *Collection* became available in Western Europe in 1588 in Commandino's Latin translation (Commandino 1588). When Descartes began to study the locus problem in 1632 he did so having already had some grounding in Viètan algebra and the theory of equations. His *Géométrie* (1637) may be seen as a fairly natural development arising from the application of algebraic methods to a problem of current interest. His approach to the investigation of the locus was very simple. Let AB be one of the lines that are given in position, C be a point on the locus and CB the line segment that is to be drawn from C to AB. Descartes took AB and CB as his given reference lines and let $x=AB$ and $y=CB$ (fig. 3). (Notice that the problem is especially suited to coordinate methods, because the line segment CB from C to AB is always drawn at the same angle to AB.) He calculated

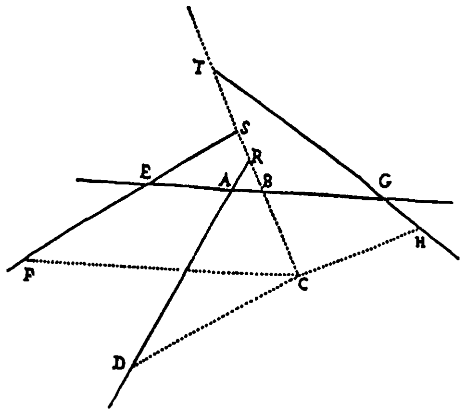


Figure 3

the various distances of the problem in terms of x and y and proceeded to express the locus condition as an indeterminate equation in these variables.

In the original locus problem there were as many variable magnitudes as there were lines given in position. In Descartes' geometry by contrast the problem was reduced to the consideration of two variables connected by means of an equation. His theory opened up the possibility—at least in principle—that continuous variation could be studied by examining how one variable changes with respect to the other within such a relation.

The last question however was one that Descartes never pursued. His investigation remained firmly centred on the classical problem of constructing solutions to geometrical problems. His interest in equations was based primarily on the role they played in such solutions. Within this programme it was necessary to determine points on a curve by means of acceptable instruments of construction (Bos 1981).The curve enjoyed a dual status, as something that was a solution to a geometrical problem and as something that could itself be used as a tool in the construction of a solution. The study of indeterminate equations yielded information about the associated curves, while determinate equations could be solved to obtain particular points on the curve.

Fermat's writings from the same period demonstrate a better appreciation of the general methodological character of coordinate geometry. In his *Ad locos et solidos isagoge* of 1637 (TH, I, 4, 91-110) he enunciated the principle that to any equation in two variables there corresponds a curve in the plane, one given by means of the graphical method of his coordinate system[12]. He was however primarily interested in geometrical loci problems, in which the final equation is always

an algebraic or polynomial relation. His continued interest in restoring Greek mathematical works indicated the strong classical character of his investigation.

Throughout the early history of coordinate geometry there seems to have been little interest in the mathematical investigation by means of graphical techniques of arbitrary relations among magnitudes, abstractly considered. The familiar modern use of graphs to represent the behaviour of virtually any two related quantities that are found anywhere was notably absent during the period.

II.4 The Calculus

II.4.1 *Equations*

While established research in coordinate geometry remained centred on geometrical construction a whole new line of investigation was opened up with the growing interest in quadrature and tangent problems. Early work on what later became the calculus was connected with the programme of study set forth in Van Schooten's Latin edition of Descartes's *Géométrie* (Descartes, 1659-1661). Out of these developments came a new part of mathematics, one that soon achieved considerable prominence as an area of research[13].The relevant history has been well documented in the literature. Our discussion will be confined to two examples which illustrate some of the conceptual and technical issues associated with the role of the equation in the early calculus.

The first example involves a comparison of Wallis's *Arithmetical infinitorum* (1656) and Newton's researches on infinite series from the 1660s. Wallis was a proponent of the new analysis and employed symbolic notation freely in his book. His primary goal was to investigate quadratures and cubatures by means of arithmetic methods involving infinite numerical series. In Proposition XIX he considered the series

$$\frac{0+1=1}{1+1=2}=\frac{1}{2}=\frac{1}{3}+\frac{1}{6}, \qquad \frac{0+1+4=5}{4+4+4=12}=\frac{1}{3}+\frac{1}{12}$$
$$\frac{0+1+4+9=14}{9+9+9+9=34}=\frac{7}{18}=\frac{1}{3}+\frac{1}{18}, etc.$$

It is clear that when the number of terms become infinite the value of the series is 1/3. (Wallis wrote down the general formula for the numerator as $\frac{l+1}{3}l^2+\frac{l+1}{6l}l^2$.

He showed how this result may be used to obtain the ratio of the area under a parabola to the circumscribed rectangle, and the ratio of the volume of a cone to

the circumscribed cylinder. He proceeded in the treatise to extend the result, and through the skilful and extensive use of interpolation went very far in obtaining numerical series expressions for various quadratures[14].

In the winter of 1664-1665 Newton began to study the *Arithmetica infinitorum*, research which he carried out at the same time he was reading Van Schooten's edition of the *Géométrie*. He recorded his progress in notebooks which have survived[15]. His fundamental innovation was to reformulate Wallis' investigation in terms of equations between Cartesian coordinate variables. By setting the problem in this way he made relations between continuously changing magnitudes the central object of study. An equation implies the existence of a relation that remains valid as the variables change continuously in value. It is this fundamental fact—the continuous and permanent character of the relation, its persistence differentially in the neighbourhood of each real number—that was exploited by Newton in expressing the connection between the equation of the curve and the formula for its quadrature. This fact would also be the basis for his subsequent investigation, set forth in the 1669 paper *De analysi*, relating the quadrature of a curve to its equation by means of differentiation[16].

Although Wallis was an advocate of the new analysis he did not make essential use of relations among variable magnitudes in his investigation. His approach was not "analytical" in the deeper sense discernible in Newton's early work on infinite series and quadratures.

Our second example concerns some later work of Newton and the French mathematician Pierre Varignon. The motion of a freely moving particle acted upon by a central force was the subject of book one of Newton's *Principia mathematica* (1687) as well as of a memoir by Varignon published by the Paris Academy in 1703 (Varignon 1701). Both men established that motion in an ellipse with the force centre at one focus implies an inverse-square force law. In a break with his early mathematical work of the 1660s Newton abandoned Cartesian analytical methods, turning instead to a kind of infinitesimal-geometrical theory of limits. Varignon by contrast used techniques of the recently established Leibnizian calculus in his solution.

In Proposition VI and its corollaries Newton had derived a measure for the force in terms of geometrical quantities associated with the curve. In the next few propositions he calculated the force law when the trajectory was assumed to have a given form. In Proposition XI he considered the case of the ellipse. In fig. 4 the point P is the position of the particle on the ellipse at a given instant, C is the centre of ellipse, S is one of the foci and the centre of the force, and CA and CB are the semi-major and semi-minor axes. Through P draw the tangent RP. The line DCK is drawn through C parallel to the tangent intersecting the ellipse at the points D and K. The lines CP and CD are then conjugate axes of the ellipse corresponding to the point P. Let E be the intersection of the SP and DC. Draw the

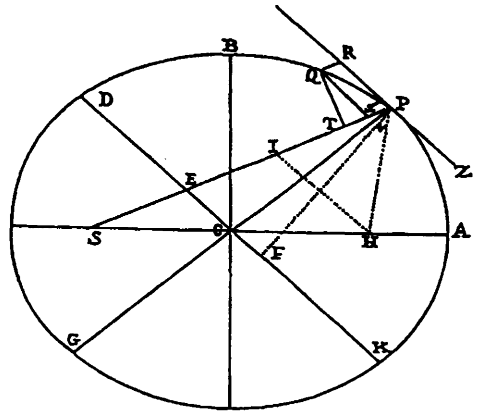


Figure 4

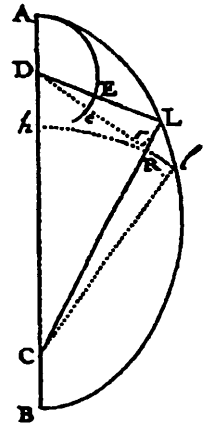


Figure 5

perpendicular PF from P to Dk. Let Q be a point on the ellipse near P. Draw the line Qv parallel to the tangent intersecting the conjugate diameter PCG at v. In the course of his derivation Newton made use of the following two equations:

$$Gv \cdot Pv : Qv^2 = PC^2 : CD^2$$
$$CA : PF = CD : CB$$

These he presented as known properties of the ellipse; of the second relation he noted that it had been "demonstrated by the writers on the conic sections."(Note that the first of these relations is the one from Apollonius's *Conics* I, discussed earlier.) He also proved that the quantity EP is a constant equal to the semi-major axis CA. Using this fact and the above relations he was able to show that the force is inversely proportional to the distance SP.

Varignon began by expressing the trajectory relative to a coordinate system in which the variables are the distance r from the force centre and the quantity z, where dz is defined as the projection of the element of path-length ds on the perpendicular to the radius. The tangential component of the force is equated to the expression dds/ddt, where s is the path length and t is the time. The derivation of the inverse-square law for the case of the ellipse is a model of simplicity. Consider the ellipse with major axis AB, foci D and C and force centre at C (fig. 5). Set $AB=a$, $DC=c$ and $b^2=a^2-c^2$. Let L be a point on the ellipse, $CL=r$. If l is a point close to L and the perpendicular lR is drawn to CL then the differential $dz=Rl$. Varignon gave the equation of the ellipse in the form[17]

$$bdr = dz\sqrt{4ar - 4rr - bb}$$

Using the relation $ds^2{=}dr^2{+}dz^2$ and the area law $rdz{=}dt$ he reexpressed this equation in the form

$$\frac{4a-4r}{r}=\frac{bbds^2}{dt^2}$$

Differentiation of this equation with respect to t led to the expression $\frac{2a}{b^2r^2}$ for the force, which yielded the desired result.

Both Newton and Varignon employed equations that express relations between continuously varying magnitudes and in this sense both of their derivations may be said to be analytical. There were however important differences of approach. In Newton's solution the ellipse with its various properties acts as a synthetic geometrical object, controlling the form of the derivation. In Varignon's memoir by contrast the ellipse is specified by a single equation between two variables relative to a fixed coordinate system. The entire mathematical content of the problem is reduced to the study of this equation; all of the properties of the ellipse needed for the solution are contained in it. The solution therefore evolves through a mechanical application of the differential algorithm.

II.4.2 *Graphical Techniques*

The curve was an object of considerable mathematical and physical interest throughout the seventeenth and eighteenth centuries. A few examples from the period 1680-1740 illustrate this point. The study of the relations that subsist between the lengths of curves gave rise to a theory of elliptic integrals. In work in the calculus of variations classes of curves constituted the primary object of study. In analytical dynamics attention was concentrated on determining the relation between trajectories and force laws. In the theory of elasticity researchers studied the shape of static equilibrium assumed by an elastic lamina under various loadings, as well as the configurations of a vibrating string.

The curve also played a fundamental and very different role in the conceptual foundation of the calculus. The situation is illustrated by work in problems of maxima and minima, an important part of the subject. In the very first published paper in the calculus Leibniz (1684) used his differential algorithm to derive the optical law of refraction from the principle that light follows the path of least time. He considered the points E and C on opposite sides of a line SS separating two optical media (fig. 6). It is necessary to find the point F on SS such that a ray of light travelling the path EFC does so in the least time. The time of transit from

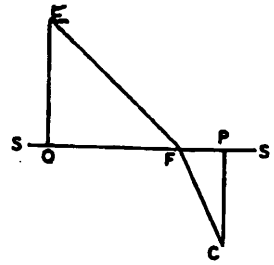


Figure 6

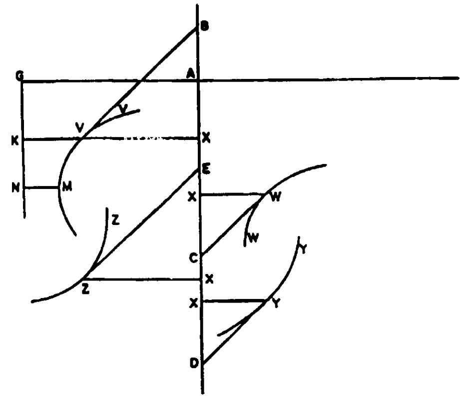


Figure 7

E to F is equal to the product of the distance EF and a constant equal to the reciprocal of the velocity in the first medium; this product Leibniz regarded as a rectangle of sides EF and a given constant line r. The time from F to C was likewise regarded as a rectangle of sides FC and a line h. The total time of transit along EFC is therefore equal to the sum of these rectangles. Leibniz (*ibid.* 1684) wrote: "Let us assume that all such possible sums of rectangles, or all possible paths, are represented by the ordinates KV of curve VV perpendicular to the line GK" (fig. 7)[18]. Letting $x{=}QF{=}GK$ be the abscissa and $w{=}KV$ be the ordinate he had in fig. 7 a curve VVM representing the time of transit as a function of the distance x from Q to F. He calculated this time as an expression in x and applied the differential theory he had previously introduced for curves to obtain the path given by the known law of refraction.

In this problem the primary object of interest is the relation between two magnitudes, the distance QF and the time of transit that corresponds to this distance. Although there is nothing in the nature of this relation that logically entails a geometric interpretation Leibniz nevertheless chose to represent it graphically by means of a curve. He could then apply his differential algorithm which had been introduced earlier for the analysis of curves.

Graphical procedures had been employed by Galileo in his *Discorsi* (1638) to relate the speed of a falling body to the time of its descent. They had become common in mathematical treatises by the late seventeenth century. Barrow in his *Lectiones geometricae* (1670) represented quadrature relationships in this way. In his *Principia mathematica* (1687) Newton investigated the inverse problem of central-force particle motion. In Propositions XXXIX and XLI of book one he graphed the force as a function of the projection of position on the orbital axis and

analyzed the resulting curve to arrive at expressions for the particle's trajectory. Jakob Bernoulli employed graphical methods throughout his researches of the 1690s. In his study of the elastica the relation between the restoring force and the distance along the lamina was superimposed in graphical form on the diagram of the actual physical system.

The first textbook on the differential calculus, l'Hôpital's *Analyse des infiniment petits* (1696), was a systematic attempt to ground the calculus in a theory of curves. The way in which this was done by him and other researchers of the period has been documented in the historical literature (Bos 1974). Of particular interest for the present discussion is his treatment of problems of maxima and minima. These problems were explicitly formulated as ones of finding the maximum or minimum ordinate of a curve. The equation of condition $dy = 0$ or $dy = \infty$ was deduced by considering successive values of dy and noting that about a maximum or minimum ordinate these values must change in sign. In several examples, each of which gave rise to a relation between two variables, he used graphical techniques to refer the problem of finding an extremum to the consideration of an associated curve.

In the ninth example l'Hôpital introduced a curve *AEB* (fig. 8) given in position and two fixed points *C* and *F*. Consider a variable point *P* on the curve and let $CP=u$ and $PF=z$. Consider a quantity (what would later be called a function) composed in some definite way from the variables u and z. It is necessary to find the point *P* so that this quantity is a maximum or a minimum. To solve this problem l'Hôpital joined the points *C* and *F* to form a base axis *CF*. The ordinates *QM* and *OD* give the values of the quantity corresponding to the points *P* and *E*. In contrast to the primary curve *AEB* the curve *MD* joining *M* and *D* is a purely

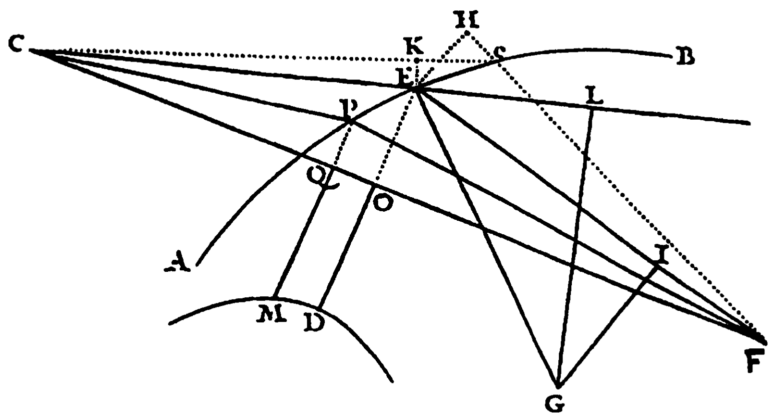


Figure 8

logical construction expressing the quantity as a function of position along *CF*. L'Hôpital observed that at *P* "the ordinate *QM* which becomes *OD* must be the greatest or least of all companion ordinates." He derived using the differential algorithm a solution in the particular case where the quantity is equal to $au+z^2$ (a constant), obtaining $adu+2zdz = 0$ or $du : dz = 2z : a$ as the differential equation which defines *P*.

The grounding of basic calculus procedures in terms of the properties of the curve, and the common practice of representing relations between magnitudes graphically by means of curves, led to a tendency to see the early calculus as something that was essentially geometrical. The term "fine geometry" employed at the time conveys the contemporary understanding. At the most fundamental level the geometrical character of the early calculus conditioned how the subject was understood, allowing it to be experienced intellectually as an interpreted, meaningful body of mathematics.

II.4.3 *Coordinate Systems*

It is clear that graphical methods played a role in the early calculus that would later be filled by the function concept. An example of this is Varignon's 1706 memoir "Nouvelle formation des spirales" (1704). The paper is devoted to the investigation of curves given in terms of polar variables. Although Cartesian geometry was originally developed for oblique and orthogonal coordinates there had been an early interest in other reference systems. Study of Archimedes's *On spirals* led in the seventeenth century to the invention of transformations that correlated areas expressed in terms of polar quantities to ones defined in terms of Cartesian coordinates. In the writings of Cavalieri, Roberval, James Gregory, Barrow, Newton and Jakob Bernoulli there was an interest in applying calculus-related procedures to curves expressed in polar quantities. In Varignon's own earlier work in orbital dynamics (as we saw in § II.4.1) he considered expressions for the force that were functions of the distance from the particle to a given centre; it was therefore natural that polar quantities were employed to analyze the resulting motion.

In his 1706 memoir Varignon considered a fixed reference circle *ABYA* with centre *C* (fig. 9). A "courbe génératrice" *HHV* is given; a point *H* on this curve is specified by the perpendicular ordinate *GH*, where *G* is a point on the axis *xCX* of the circle. The line *CX* is conceived as a ruler that rotates with centre *C* in a clockwise direction tracing out a spiral *OEZAEK*. Consider a point *E* on the spiral. With centre *C* draw the arc *EG*. Let c = the circumference of the reference circle *ABYA*, x = arc *AMB*, $CA=a$, $CE=y$, $GH=z$ and $AD=b$ a constant line. The arc x is defined by the proportion $c{:}x=b{:}z$. Varignon wrote what he called the "équation générale de spirals à l'infini" as $cz=bx$. By substituting the value for z

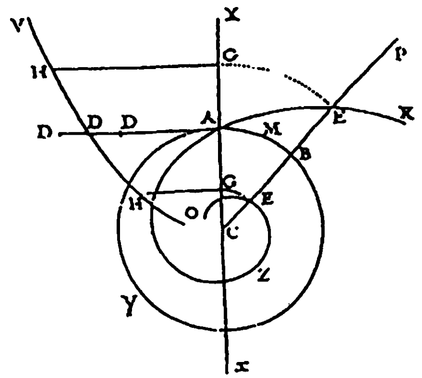


Figure 9

given by the nature of the generating curve into this equation the character of the spiral was revealed. Depending on whether the generating curve was a parabola, hyperbola, logarithm, circle, etc., the corresponding spiral was called parabolic, hyperbolic, logarithmic, circular etc.

That one could introduce curves in a polar reference system by considering arbitrary relations between the radius and the pole angle was presented by Varignon as a substantial advance. Earlier mathematical researches had concerned such special cases as the parabolic spiral. In Varignon's dynamical investigations the trajectory was something that was logically given as part of the physical problem. In the present paper by contrast the "equation" of the spiral is formulated *a priori* in terms of Cartesian coordinates in the associated "generating curve". The latter embodies in graphical form the functional relationship between the polar variables and acts as a standard model to which this relationship may be referred.

A prominent subject of Varignon's paper, the rectification of polar curves, is of interest from the viewpoint of the conceptual foundations of analysis. Newton and Jakob Bernoulli had independently studied the path-lengths of pairs of associated curves, one member given in Cartesian and the other in polar coordinates[19]. The Cartesian formula for the differential element of path length is $ds^2=dx^2+dy^2$, where x is the ordinate and y the abscissa; the polar expression of the same quantity is $ds^2=dx^2+x^2d\theta^2$, where x is now the radius and θ is the polar angle. If the element of length is assumed to be the same along both curves (so that there respective lengths for a given value of x are equal) we are led to the differential equation $dy=xd\theta$ relating the respective coordinate variables. It was clear for example that the integral $\int_a^b \sqrt{1+x^2}\,dx$ gives both the length along the parabola $y=\frac{1}{2}x^2$ as well

as the length along the Archimedean spiral $x=\theta$. The rectification of the spiral, a mechanical curve, was reduced to that of the simpler and better known conic section, a result of considerable interest to mathematicians of the period. Varignon's study of rectification consisted in large part in the extension and further development of this result.

The common use of non-Cartesian coordinates in the early calculus was in the computation of geometric quantities associated with the curve. Thus polar coordinates were employed in certain problems because they provided a suitable measure of the radius of curvature of a curve. The geometrical object was given and the coordinate description was varied for the purposes of investigation. Varignon's paper pointed in the opposite direction. Contained in his study, if only implicitly, was the realization that the same formula could receive distinct geometric interpretations, depending on the meaning assigned to the coordinate variables of the problem. The interpretation of the formula $\int_a^b \sqrt{1+x^2}\,dx$ in the preceding paragraph will differ depending on whether x is regarded as an orthogonal or a polar variable. This conclusion suggested more generally the possible existence of a stable analytical core for the calculus. The work of Euler that we shall we consider in the next section was based in large part on the elevation of this insight to an explicit and systematic programme of research in infinitesimal analysis.

III Euler's Analysis

III.1 By the early eighteenth century symbolic methods were common in Continental mathematics. In the infinitesimal calculus especially there were strong analytical elements in the researches of the Bernoullis, Varignon, Taylor (English, but an important influence on the Continent), Hermann, Fagnano, Riccati, and others, elements that were combined however with pervasive geometric modes of representation.

Euler became established as a mathematician of note during the decade of the 1730s. He was a young man in his twenties, a member of the St. Petersburg Academy of Sciences and a colleague of Hermann, Daniel Bernoulli and Goldbach. His interest in analysis is evident in writings from this period, including his major treatise on particle dynamics, *Mechanica sive motus scientia analytice exposita* (1736). Although the theme of analysis was well established at the time there was in his work something new, the beginning of an explicit awareness of the distinction between analytical and geometrical methods and an emphasis on the desirability of the former in proving theorems of the calculus.

The direction of Euler's research in the later 1730s and early 1740s may be followed in his work in the calculus of variations, leading up to the publication in

1744 of his *Methodus inveniendi*. His investigation began from earlier results of Jakob Bernoulli, Brook Taylor and Johann Bernoulli. Jakob and Taylor's researches were linked by an appreciation at the level of technical approach for the analytical solution of isoperimetric problems. By contrast, Johann's major memoir of 1719, an extended exposition of his brother's ideas, emphasized a more geometric approach to the same subject. Although Euler had been Johann's student in Basel his own conception of variational calculus seems to have evolved under the influence of Jakob and Taylor (Fraser 1994).

III.2 The *Methodus inveniendi* contained many of the advances that would be systematically developed by Euler in his later treatises: the function concept; the notion of a trigonometric function and the associated notation; and a uniform procedure for introducing higher-order differentials. At a deeper level the work expressed an appreciation for the mathematical possibilities of a more abstract approach to analysis.

A typical problem of the early calculus involved the determination of a magnitude associated in a specified way with a curve. To find the tangent to a curve at a point it was necessary to determine the length of the subtangent there; to find the maximum or minimum of a curve one needed to calculate the value of the abscissa that corresponded to an infinite subtangent; to find the area under a curve it was necessary to calculate an integral; to determine the curvature at a point one had to calculate the radius of curvature.

The calculus of variations extended this paradigm to classes of curves. In the fundamental problem of the *Methodus inveniendi* it is required to select that curve from among a class of curves which makes a given magnitude expressing some property a maximum or minimum. More precisely, Euler considered curves that are represented analytically by means of relations between x and y in terms of an orthogonal coordinate system (fig. 10). The magnitude that is to be maximized or minimized is expressed as a definite integral

$$W = \int Z dx \qquad (\text{from } x = a \text{ to } x = b), \tag{1}$$

a formula that quantifies in analytical terms the given extremal property. Z is regarded by Euler as a "function" of x, y and the differential coefficients (*i.e.*, derivatives) $p, q, r, \ldots$ of y with respect to x. The latter are given by the relations $dy=pdx$, $dp=qdx$, $dq=rdx$, ..., a procedure for introducing higher-order derivatives that was Euler's own invention[20].

Near the beginning of his treatise Euler (Euler 1744, 13) noted that a purely analytical interpretation of the theory is possible. Instead of seeking the curve which renders W an extremum one seeks that "equation" between x and y which

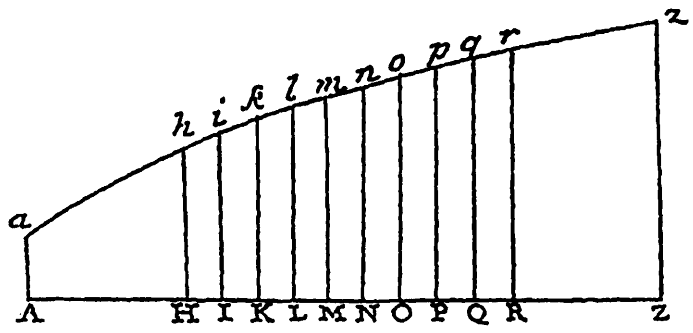


Figure 10

among all such equations when introduced into (1) renders the quantity W a maximum or minimum. He wrote:

> "Corollary 8. In this way questions in the doctrine of curved lines may be referred back to pure analysis. Conversely, if questions of this type in pure analysis be proposed, they may be referred to and solved by means of the doctrine of curved lines.
> *Scholium* 2. Although questions of this kind may be reduced to pure analysis, nevertheless it is useful to consider them as part of the doctrine of curved lines. For though indeed we may abstract from curved lines and consider absolute quantities alone, so these questions at once become abstruse and inelegant and appear to us less useful and worthwhile. For indeed methods of resolving these sorts of questions, if they are formulated in terms of abstract quantities alone, are very abstruse and troublesome, just as they become wonderfully practical and are made simple to the understanding by the inspection of figures and the linear representation of quantities. So although questions of this kind may be applied equally to abstract and concrete quantities it is most convenient to formulate and solve them by means of curved lines. Thus if a formula composed of x and y is given, and that equation between x and y is sought such that, the expression for y in terms of x given by the equation being substituted, there is a maximum or minimum; then we can always transform this question to the determination of the curved line, whose abscissa is x and ordinate is y, for which the formula W is a maximum or minimum, if the abscissa x is assumed to have a given magnitude."[21] (Euler 1744, 14)

Euler's view seems to have been that while it is possible in principle to approach the calculus of variations purely analytically it is more effective in practice to refer problems to the study of curves. This conclusion could hardly have seemed surprising. Each of the various examples and problems which historically made up the subject had as its explicit goal the determination of a curve; the selection of such objects was part of the defining character of this part of mathematics. What is perhaps noteworthy about Euler's discussion is that he should have considered the possibility at all of a purely analytical treatment.

III.3 The main body of variational results, presented in chapters two and three, is formulated throughout in terms of the properties of curves. Euler's approach is indicated by his derivation of the fundamental necessary condition known in the modern subject as the Euler (or Lagrange-Euler) differential equation. He developed his derivation with reference to fig. 11, in which the line *amnoz* is the hypothetical extremalizing curve. The letters M, N, O designate points of the x-axis AZ infinitely close together. The letters m, n, o designate corresponding points on the curve given by the ordinates Mm, Nn, Oo. Let $AM=x$, $AN=x'$, $AO=x''$ and $Mm=y$, $Nn=y'$, $Oo=y'$. The differential coefficient p is defined by the relation $dy=pdx$; hence $p=dy/dx$. We have the following relations

$$p = \frac{y' - y}{dx}$$
$$p' = \frac{y'' - y'}{dx} \tag{2}$$

Suppose now that we are given a determinate "function" Z containing x, y and $p=dy/dx$.The integral (1) was regarded by Euler as an infinite sum of the form $...+Z, dx+Zdx+Z'dx+...$, where Z, is the value of Z at x-dx, Z its value at x and Z' its value at $x+dx$, and where the summation begins at $x=a$ and ends at $x=b$. Let us increase the ordinate y' by the infinitesimal "particle" $n\nu$, obtaining in this way a comparison curve *amvoz*. Consider the value of (1) along this curve. By hypothesis the difference between this value and the value of (1) along the actual curve will be zero. The only part of (1) that is affected by varying y' is $Zdx+Z'dx=(Z+Z')dx$. Euler wrote:

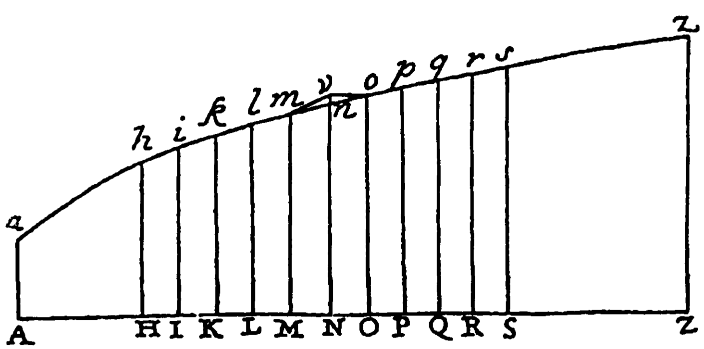


Figure 11

$$dZ = Mdx + Ndy + Pdp$$
$$dZ' = M'dx + N'dy' + P'dp' \tag{3}$$

He proceeded to interpret the differentials in (3) as the infinitesimal changes in Z, Z', x, y, y', p, p' that result when y' is increased by nn. From (2) we see that dp and dp' equal nn/dx and $-nn/dx$. (These changes are presented in the form of a table, with the variables in the left column and their corresponding increments in the right column.) Hence (3) becomes

$$dZ = P \cdot \frac{n\nu}{dx}$$
$$dZ' = N' \cdot n\nu - P' \cdot \frac{n\nu}{dx} \tag{4}$$

Thus the total change in $\int_a^b Zdx$ equals $(dZ+dZ')dx = n\nu\cdot(P+N'dx-P')$. This expression must be equated to zero. Euler set $P'-P=dP$ and replaced N' by N. He therefore obtained $0=Ndx-dP$ or

$$N - \frac{dP}{dx} = 0 \tag{5}$$

as the final equation of the problem.

Equation (5) is the simplest instance of the Euler differential equation, yielding a necessary condition that must be satisfied by the extremalizing arc. In modern notation it is written $\frac{\partial f}{\partial y} - \frac{d}{dx}\left(\frac{\partial f}{\partial y'}\right) = 0$. Its derivation by Euler was a major theoretical achievement, representing the synthesis in one equational form of the many special cases and examples that had appeared in the work of earlier researchers.

The remainder of chapter two consists of the presentation of a large number of examples as well as the extension of the variational theory to the case where higher-order derivatives of y with respect to x appear in the integrand Z of (1). In chapter three, mathematically the most advanced of the treatise, Euler considered problems where variables that satisfy certain auxiliary relations are introduced into the integrand Z of the variational integral (1). This investigation, which was motivated by examples involving the constrained gravitational motion of particles

in resisting media, led once again to an analytical solution in the form of differential equations.

III.4 The basic variational problem of maximizing or minimizing (1) involves the selection of a curve from among a class of curves. In the derivation of (5) the variables x and y are regarded as the orthogonal Cartesian coordinates of a curve. Each of the steps in this derivation involves reference to the geometrical diagram in Figure 11. In chapter four, however, Euler returned to the point of view that he had indicated at the beginning of the treatise. In the opening proposition the variational problem is formulated as one of determining that "equation" connecting two variables x and y for which a magnitude of the form (1) (given for the general case where higher-order derivatives and auxiliary quantities are contained in Z) is a maximum or minimum. In his solution he noted that such variables can always be regarded as orthogonal coordinates and so determine a curve. The solution then follows from the theory developed in the preceding chapters. In the first corollary he wrote:

> "Thus the method presented earlier may be applied widely to the determination of equations between the coordinates of a curve which render any given expression ∂Zdx a maximum or a minimum. Indeed it may be extended to any two variables, whether they involve an arbitrary curve, or are considered purely in analytical abstraction."[22] (Euler 1744, 129)

Euler illustrated this claim by solving several examples using variables other than the usual rectangular Cartesian coordinates. In the first example he employed polar coordinates to find the curve of shortest length between two points. We are given (fig. 12) the points A and M and a centre C; it is necessary to find the shortest curve AM joining A and M. Let x be the pole angle ACM and y the radius

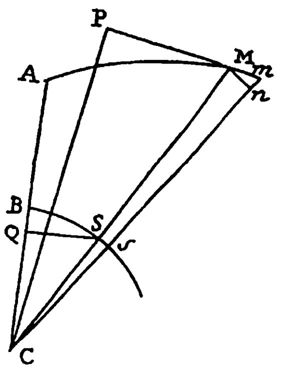


Figure 12

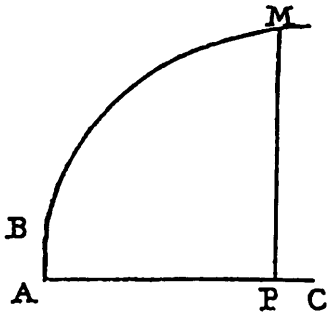


Figure 13

CM. Because the differential element of path-length is equal to $\sqrt{dy^2+y^2dx^2}$ the formula for the total path-length is $\int dx\sqrt{yy+pp}$, where $pdx=dy$ and the integral is taken from $x=0$ to $x=\angle ACM$. Here x does not appear in the integrand Z of the variational integral, so that $dZ=Ndy+Pdp$. The equation (5) gives $N=dP/dx$ so that we have $dZ=dPp+Pdp$ and a first integral is $Z+C=Pp$, where C is a constant. Since $ZZ=\sqrt{yy+pp}$ we have

$$C+\sqrt{(yy+pp)}=\frac{pp}{\sqrt{(yy+pp)}} \quad i.\ e.: \ \frac{yy}{\sqrt{(yy+pp)}}=Const.=b$$

Let PM be the tangent to the curve at M and CP the perpendicular from C to this tangent. By comparing similar triangles in fig. 12 we see that $Mm:Mn=MC:CP$. Since $Mm=dx\sqrt{y^2p^2}$, $Mn=ydx$ and $MC=y$ it follows that $CP=\dfrac{y^2}{\sqrt{y^2+p^2}}$. Hence CP is a constant. Euler concluded from this property that the given curve AM is a straight line.

Note that Euler was completely comfortable with polar coordinates; gone is the Cartesian "generating curve" Varignon had employed in his investigation of 1706 in order to introduce general curves using polar quantities. In the second example he displayed a further level of abstraction in his choice of variables. Here we are given the axis AC with the points A and P, the perpendicular line PM and a curve ABM joining A and M (fig. 13). Given that the area $ABMP$ is some given constant value we must find that curve ABM which is of the shortest length. Euler set the abscissa $AP=t$, the ordinate $PM=y$ and let x equal the area under the curve from A to P. We have $dx=ydt$ and the variational integral becomes $\int\sqrt{\dfrac{dy^2+dx^2}{yy}}$.

Because x does not appear in the integrand we obtain as before the first integral $Z=C+pP$. Substituting the expressions for Z and P into this integral we obtain

$$\frac{\sqrt{(1+yypp)}}{y}=C+\frac{ypp}{\sqrt{(1+yypp)}}$$

Letting $dx=ydt$ we obtain after some further reductions the final equation $t = c \pm \sqrt{(bb - yy)}$. Hence the desired curve is the arc of a circle with its centre on the axis AP.

A range of non-Cartesian coordinate systems had been employed in earlier mathematics but never with the same theoretical import as in Euler's variational analysis. Here one had a fully developed mathematical process, centred on the consideration of a given analytically-expressed magnitude, in which a general equational form was seen to be valid independent of the geometric interpretation conferred upon the variables of the problem. Thus it is not at all essential in the reasoning employed in the derivation of (5) that the line AZ be perpendicular to Mm (fig. 11); indeed it is clear that the variable x need not be a length nor even a coordinate variable in the usual sense. As Euler observed in the first corollary, the variables of the problem are abstract quantities, and fig. 11 is simply a convenient geometrical visualization of an underlying analytical process[23].

Euler's statement at the beginning of the treatise that it was possible to consider the subject as one of "pure analysis" seemed somewhat speculative. By showing in chapter four how the basic variational problem and its solution could be interpreted abstractly he had supplied this view with a considerable degree of mathematical credibility.

III.5 Refinement

Although Euler in 1744 clearly recognized the essential analytical character of the variational calculus his insight was not fully developed in his treatise. Its title "Method of finding curves..." indicated that the primary object of study continued to be the curve. In his later variational writings, in part in response to Lagrange's research, he developed and refined further the conception outlined in chapter four. More generally there was an increasing emphasis on analysis throughout his mathematical work. Conceptually, the most significant change was the explicit replacement of the geometric curve by the analytical relation (conceived as a functional equation between two variables) as the fundamental concept of the variational theory; instead of selecting a curve from among a class of curves it was now required to select a relation from among a class of relations.

The function concept played a dual role in Euler's emerging programme. The functional equation $y=f(x)$ enabled one to conceive analytically of arbitrary relations between the variables x and y. In addition, the notion of an expression composed of variables and constants (denoted for example by Z in the formulation of (1)) allowed the formal statement of general propositions and made it possible to express the content of the subject in purely analytical terms.

A relation between variables is regarded by Euler as a primitive of the theory; it is not further conceptualized, as it would be in later real-variable calculus, in terms of the numerical structure of the continuum of values assumed by each variable. This notion of a primitive abstract relation in large part defined the distinctive character of his approach to analysis. The point in question is illustrated by his demonstration of theorems of the calculus. We will consider one example in detail. At the same time he was composing the *Methodus inveniendi* he published a memoir (Euler 1734-1735) containing an analytical proof of the theorem on the equality of mixed partial differentials. He was motivated in doing so by a belief that a geometrical demonstration would be "drawn from an alien source". He considered a quantity z that is a function of the variables x and a. If dx and da are the differentials of x and a, let $e, f,$ and g denote the values of z at $(x+dx, a)$, $(x, a+da)$ and $(x+dx, a+da)$. Euler differentiated z holding a constant to obtain

$$Pdx = e-z \tag{6}$$

Here P denotes the differential coefficient, in later mathematics the partial derivative of z with respect to x. He differentiated Pdx holding x constant

$$Bdxda = g-f-e+z \tag{7}$$

He then differentiated z holding a constant to obtain

$$Qda = f-z \tag{8}$$

Finally he differentiated Qda holding x constant:

$$Cdadx = g-e-f+z \tag{9}$$

By rearrangement of terms the right sides of (7) and (9) are seen to be equal. Equating the left sides Euler obtained

$$B = C \tag{10}$$

which is the desired result.

In later real analysis this argument would be reformulated using the law of the mean and a limit argument. Suppose $z=z(x,a)$ and its first and second partial derivatives are defined and continuous on a rectangular region in the x–a plane. For x and a in this region we have by the law of the mean for small h and k the four equations

$$\frac{\partial z}{\partial x}(x+\varepsilon_1 h,a)h = z(x+h,a) - z(x,a) \qquad 0 \le \varepsilon_1 \le 1 \tag{6'}$$

$$\frac{\partial^2 z}{\partial a dx}(x+\varepsilon_1 h, a+\varepsilon_2 k)hk = z(x+h,a+k) - z(x,a+k) - z(x+h,a) + z(x,a) \qquad (7')$$

$$0 \le \varepsilon_1 \le 1, 0 \le \varepsilon_2 \le 1$$

$$\frac{\partial z}{\partial a}(x,a+\eta_1 k)k = z(x,a+k) - z(x,a) \qquad 0 \le \eta_1 \le 1 \qquad (8')$$

$$\frac{\partial^2 z}{\partial x \partial a}(x+\eta_1 h, a+\eta_2 k)kh = z(x+h,a+k) - z(x,a+k) + z(x,a) \qquad (9')$$

$$0 \le \eta_1 \le 1, 0 \le \eta_2 \le 1$$

By rearrangement the right sides of (7') and (9') are equal. The left sides may therefore be equated:

$$\frac{\partial^2 z}{\partial a \partial x}(x+\varepsilon_1 h, a+\varepsilon_2 k) = \frac{\partial^2 z}{\partial x \partial a}(x+\eta_1 h, a+\eta_2 k)$$

Letting h and k tend to zero we obtain from the continuity of the second partial derivatives the desired result

$$\frac{\partial^2 z}{\partial a \partial x} = \frac{\partial^2 z}{\partial x \partial a} \qquad (10')$$

This example is rather typical of eighteenth-century calculus theorems and their counterparts in modern analysis[24]. The law of the mean introduces a distinguished value, localizing at a particular number the analytical relation or property in question. The result is then deduced using conditions of continuity and differentiability by means of a limit argument. In Euler's formulation by contrast there was no consideration of distinguished or individual values as such. Euler believed that the essential element in the demonstration was its generality, guaranteed by a formal analytical or algebraic identity. Thus the key step in his proof, the equality of the right sides of equations (7) and (9), was an algebraic identity that ensured the validity of the result.

IV Discussion

Euler perceived that the calculus is concerned ultimately with equations expressing relations of continuous change between variable magnitudes. His thesis concerning the primacy of pure analysis derived from a logical appreciation that geometrical methods and reasonings are extrinsic to the subject. In formulating this view he established the general framework within which analysis would be understood by subsequent researchers of the period, most notably Lagrange.

The distinctive character of Euler's doctrine is apparent when one considers it at a general epistemological level. There is a certain formal quality to his analysis; it arises ultimately from his conception of the subject as the study of primitive abstract relations. In this respect his viewpoint was very different from that of the early pioneers, who conceived of the foundation of the calculus in terms of geometric conceptions, or that of the nineteenth-century researchers, for whom the numerical continuum provided a fundamental structure of interpretation.

The notion of a primitive abstract relation among variables allowed for a direct and general approach to the subject, evident in Euler's derivation of (5) and (10) above. This generality was however of a particular sort, accompanied by a certain inflexibility of outlook. This became apparent during his debate with d'Alembert in the 1750s over the question of the general solution of the wave equation. Faced with some of the restrictions imposed by the precepts of his own theory (and insisted upon by d'Alembert) Euler advocated a rejection of the concept of a functional equation as a strict relation of equality between analytical expressions. As is well known his defence of this viewpoint reduced to *ad hoc* arguments and "visionary" presentiments of a more general mathematics, presented in a few papers; his systematic treatises of the 1750s remained firmly grounded in the established conception of analysis (Lützen 1983) and (Fraser 1989).

It should be emphasized that the rejection of geometric conceptions by Euler and other eighteenth-century researchers was not accompanied by the realization that the calculus could be developed in full logical isolation as part of pure analysis. In Euler's writings the relationship between foundation, theoretical development and problem generation is not worked out. The entire project of the *Methodus inveniendi* consisted of the derivation of differential equations for general problems, each of which embodied characteristics found in a given set of examples from geometry or mechanics. In his subsequent research the separation of analysis from geometry was made more explicit at a theoretical level. His variational investigations however remained centred on the derivation of general differential equational forms. He provided no account of how the problems in question might originate or be generated within this or any other branch of pure analysis.

He sometimes wrote as if problems are things that are external to analysis that guarantee its meaning and validity. In a memoir published in 1758 he investigat-

ed singular solutions to ordinary differential equations, that is, solutions which are not included in the general integral containing arbitrary constants. He took a differential equation and exhibited a particular function $y=f(x)$ that satisfied the equation but was not in the general solution. He wrote: "Concerning the example that I have just set forth, as it is drawn from fantasy, one could doubt whether this case is ever encountered in a real problem. But the same examples that I adduced in order to clarify the first paradox, will serve also to clarify this one" (Euler 1756; OO, ser. 1, XXII, 231)[25]. (The examples in question concerned curves in the plane that satisfied certain tangent conditions.)

The point here is connected to a larger difference of outlook between eighteenth-century and modern mathematics. That the problems of geometry and mechanics should conform to treatment by pure analysis was something that Euler implicitly accepted as a point of philosophical principle. The term "philosophy" (or "metaphysics") is here being used in the sense identified by Daston:

> "The presuppositions (often unexamined) that inform a scientist's work, which may be of either epistemological or ontological import [...] metaphysics is what is left over once the mathematical and empirical content have been subtracted [...]." (Daston 1991, 522)

In the writings of such post-positivist intellectual historians as E. A. Burtt the term 'metaphysics' in this sense referred to very broad assumptions, such as a general Platonic belief among early modern thinkers in the mathematical character of physical reality[26]. We suggest that it is also useful at a more concrete level in explaining certain tacit but definite attitudes displayed by Euler in his research in geometry and analysis.

Demidov writing of the failure of Euler and d'Alembert to understand each other's point of view in the discussion of the wave equation observes:

> "A cause no less important of this incomprehension rests, in our opinion, on the understanding of the notion of a solution of a mathematical problem. For d'Alembert as for Euler the notion of such a solution does not depend on the way in which it is defined [...] rather the solution represents a certain reality endowed with properties that are independent of the method of defining the solution. To reveal these properties diverse methods are acceptable, including the physical reasonings employed by d'Alembert and Euler." (Demidov 1982, 37)

A biographer of d'Alembert (Grimsley 1963, 248) has noted his insistence on "the elementary truth that the scientist must always accept the essential 'giveness' of the situation in which he finds himself." The sense of logical freedom that is inherent in modern mathematics was notably absent in the eighteenth century.

University of Toronto
Institute for the History and Philosophy of Science and Technology
Victoria College

Notes

1 In his history of analytic geometry Boyer (1956, 190) observes that for Euler "analysis was not the application of algebra to geometry; it was a subject in its own right—the study of variables and functions—and graphs were but visual aids in this connection [...] it now dealt with continuous variability based on the function concept [...] only with Euler did it [this meaning of analysis] take on the status of conscious program."

2 Emphasis in the original.

3 This view is most clearly presented by Mahoney (1973, 36 and 39):

> "In the *Introduction to the Analytic Art*, as in the whole of the *Analytic Art* itself, algebra was transformed from a sophisticated sort of arithmetical problem-solving into the art of mathematical reasoning itself, insofar as that reasoning was based on combinatory operations [...] the analytic art rose to a position subsuming all combinatory mathematics, whether arithmetic, geometry, or trigonometry".
>
> "The elevation of algebra from a subdiscipline of arithmetic to the art of analysis deprived it of its content at the same time that it extended its applicability. Viète's *specious logistic*, the system of symbolic expressions set forth in the *Introduction*, is, to use modern terms, a language of uninterpreted symbols. As a formal language, specious logistic can itself generate problems of syntax alone."

4 In his *Die Grundlagen der Arithmetik* (1884, §10) Frege rejected the use of induction (as it was understood in the physical sciences) as a valid principle of arithmetic. He wrote:

> "For here there is none of that uniformity, which in other fields can give the method a high degree of reliability. Leibniz recognized this already: for to his Philathèthe, who had asserted that 'the several modes of number are not capable of any other difference but more or less; which is why they are simple modes, like those of space' ".

He returns the answer:

> "That can be said of time and of the straight line, but certainly not for the figures and still less of the numbers, which are not merely different in magnitude, but also dissimilar. An even number can be divided into two equal parts, an odd number cannot; three or six are triangular numbers, four and nine are squares, eight is a cube, and so on. And this is even more case with the numbers than with the figures; for two unequal figures can be perfectly similar to each other, but never two numbers."

Later in this section Frege continues:

> "In ordinary induction we often make good use of the proposition that every position in space and every moment in time is as good in itself as every other. Our results must hold good for any other place and any other time, provided only that the conditions are the same. But in the case of the numbers this does not apply, since they are not in space or time. Position in the number series is not a matter of indifference like position in space."

5 Our account of Fermat's number theory is based on Ore (1948), Hoffmann (1960-1962) and especially Mahoney (1973, Chapter VI).

6 I quote from Heath translation Euclid (EH).

7 "Tout nombre premier mesure infailliblement une des puissances −1 de quelque progression que ce soit, et l'exposant de la dite puissance est sous-multiple du nombre premier donné −1 [...]."

8 Quoted in translation in Mahoney (1973, 329).

9 We use the term "coordinate geometry" to designate the subject known since around 1800 as "analytic geometry". The first work to contain the latter term in its title was J.B. Biot's *Essai de géométrie analytique* (1803). Loria (1923, 142-143) identifies analytic geometry with the "method of coordinates" and states that it "has as its goal the investigation, with the aid of coordinates, of all figures that are conceivable in the plane or in space." The employment of coordinate methods to investigate the elementary plane and solid geometry of Euclid, the use of transformations to study conic sections and higher-order polynomial curves, more broadly the study by means of coordinate methods of any class of geometric curves, all lie within the province of analytic geometry.

10 Coolidge (1945, 20-21) writes:

> "This dreary problem, whose algebraic solution gives a conic immediately, seems to have haunted the Greek mind. We noted at the beginning of the present chapter Apollonius' statement that others had unsuccessfully tried to solve it. But Apollonius himself does not appear able to carry it through. Certain modern mathematicians have put not a little time and strength into the attempt to complete such proofs by what we might call strictly Greek methods".

He mentions Zeuthen (1886, 126-63) and Heath for his edition of Apollonius (Apollonius CH, cxxxviii-cl).

11 Pappus's discussion is in Pappus (CI, part I). On pp. 587-591 of part two Jones (following Zeuthen (1886)) provides an account of how a synthesis of the four-line locus might have been achieved by earlier Greek mathematicians, especially Aristaeus.

12 Mahoney (1973, ch. 3) provides an account of Fermat's researches in coordinate geometry.

13 With the invention and increasing development of the calculus analytic geometry weakened as an area of research. Boyer (1956, 153-154) writes:

> "In general, l'Hospital (like Descartes) was more interested in analytic geometry as a means of expressing loci algebraically than as a method of deriving the properties of a curve from its equation. This latter aspect he seems to have felt belonged more properly to work in the calculus."

In reference to the eighteenth century he (1956, 193) observes "there was a natural tendency for material on curves to be merged with that on the calculus, and hence analytic geometry sometimes lost its identity."

14 Scott (1938, ch. 4) gives a good account of Wallis' treatise.

15 Westfall (1980, ch. 4) provides an account of Newton's early mathematical researches. Newton's papers from this period are published in Newton (MP, I.).

16 Both Westfall and Whiteside comment on this difference of approach, although neither identify the fundamental character of Newton's innovation as consisting precisely in his decision to use equations between Cartesian variables. Whiteside (1960-1962, 245) writes:

> "The advance Newton has made on Wallis' inductive approach to integrals—taking the upper bound of the integral variable—is that, in allowing a free variable (and its powers) into the pattern, he has been able to use the ordering of coefficients given by powers of the variable to point a more general aspect of the pattern lost in Wallis tabulated numerical instances."

Westfall (1980, 114-115) writes:

> "[...] Newton realized that Wallis's method was more flexible than Wallis himself had realized. It is not necessary always to compare the area under a curve with the area of the same fixed square. In the case of the simple power functions ($y = x, x^2, x^3, \ldots$), for example, any value of x provides a base line that can be divided into an infinite number of segments, and with the corresponding value of y it implicitly defines a rectangle with which the area under the curve can be compared."

17 Varignon does not give the derivation of this equation. It may be obtained from the polar equation

$$\frac{b^2}{2ar} = 1 + \frac{c}{a}\cos\theta \qquad (\theta = \angle BCL)$$

by differentiating with respect to θ, eliminating $\sin\theta$ and setting $dz = r d\theta$. Since notation for the trigonometric functions has not yet been invented, Varignon would have worked from an equation of the form

$$\frac{b^2}{2ar} = 1 - \frac{c}{a}\frac{(CM)}{r}$$

where CM is the projection of CL on the axis AB.

18 "Ponamus omnia ista rectangulorum aggregata possibilia, vel omnes viarum possibilium difficultates, repraesentari per ipsas KV, curvae VV odinatas ad rectam GK normales [...]." English translation from Struik (1969, 278).

19 Cf. Jakob Bernoulli (1691), Newton (MF, 176-178) and Newton (MP, III, 312-313) (for the draft from the early 1670s). The seventeenth-century history of this problem is described by Whiteside Newton (MP, III, 308-311) who writes (*ibid.*, 308):

> "The development of this length-preserving transformation in the three decades preceding 1670 is a fascinating case-history in human insight and preconception which has never been systematically explored in the monograph needed to do it full justice."

20 In his treatise on the differential calculus Euler provided a detailed account of this procedure for introducing higher-order differential coefficients. A discussion of this subject is provided by Bos (1974).

21 "Corollarium 8: Hoc ergo pacto quaestiones ad doctrinam linearum curvarum pertinentes ad Analysin puram revocari possunt. Atque vicissim, si huis generis quaestio in Analysi pura sit proposita, ea ad doctrinam de lineis curvis poterit referri ac resolvi".

Scholion 2: "Quanquam huius generis quaestiones ad puram Analysin reduci possunt, tamen expedit eas cum doctrina linearum curvarum coniungere. Quodsi enim animum a lineis curvis abducere atque ad solas quantitates absolutas firmare velimus, quaestiones primum ipsae admodum fierent abstrusae et inelegantes ususque earum ac dignitas minus conspiceretur. Deinde etiam methodus resolvendi huismodi quaestiones, si in solis quantitatibus abstractis proponeretur, nimium foret abstrusa et molesta; cum tamen eadem, per inspectionem figurarum et quantitatum repraesentationem linearem, mirifice adiuvetur atque intellectu facilis reddatur. Hanc ob causam, etsi huius generis quaestiones cum ad quantitates abstractas tum concretas applicari possunt, tamen eas ad lineas curvas commodissime traducemus et resolvemus. Scilicet, quoties aequation eiusmodi inter x et y quaeritur, ut formula quaedam proposita et composta ex x et y, si ex illa aequatione quaesita valor ipsius y subrogetur et ipsi x determinatus valor tribuatur, maxima fiat vel minima, tum semper quaestionem transferemus ad inventionem lineae curvae, cuius abscissa sit x et applicata y, pro qua illa formula W fiat maxima vel minima, si abscissa x datae magnitudinis capiatur."

22 "Methodus ergo ante tradita multo latius patet, quam ad aequationes inter coordinatas curvarum inveniendas, ut quaepiam expressio $\int Zdx$ fiat maximum mimimumve. Extenditur scilicet ad binas quascunque variabiles, sive eas ad curvam aliquam pertineant quomodocunque, sive in sola analytica abstractione versentur."

[23] Carathéodory (1952, xxii) offers a different account of this part of the *Methodus inveniendi*; he writes:

> "[...] die Beispiele, die im ersten Teil desselben Kapitels (Nr. 1 bis 14) behandelt werden, können as Probme für die Kovarianz der Eulerschen Gleichungen bei beliebigen Koordinaten-Transformationen bewertet werden. Somit finden wir im Eulerschen Buche die ersten Ansätzen zu einer Theorie, die erst in unseren Tagen systematisch entwickelt worden ist."

In his index (*ibid.*, lix) of Euler's variational calculus he places these examples under the heading "Kovariante transformation von variationsproblemen." Goldstine (1980, 84) also observes:

> "It is remarkable that as early as 1744 Euler was already concerned with the problem of the invariance of his fundamental equation or necessary condition. In the first part of his Chapter IV he indicates that this fundamental condition remains invariant under 'general' transformations of the coordinate axes [...] he considers a number of examples where x, y are not related by being cartesian, rectangular coordinates, and shows the utility of his ideas on covariance [...]. It is truly in keeping with Euler's genius that he should have worked at ideas that were only to be satisfactorily and completely discussed in modern times."

In our view one should not speak of transformations, invariance or covariance in reference to Chapter Four. Although coordinate transformations had appeared in a memoir published by Hermann (1729) and were employed by Euler in his *Introductio* (1748, II, ch. II; for further references cf. Boyer 1956, ch. 7) they appear nowhere in the *Methodus inveniendi*. Euler does not have to show anything when he writes down the fundamental equation (5) in polar coordinates; its validity is a logical consequence of the generality of the variables in the original derivation. It is unnecessary to invoke concepts of modern differential geometry in order to reach a full appreciation of his theory.

[24] Other examples are the fundamental theorem of the calculus, the theorem on the change of variables in multiple integrals and the fundamental lemma of the calculus of variations.

[25] "Pour l'example que je viens d'alléguer ici, comme il est formé à fantaisie, on pourrait aussi douter, si ce cas se recontre jamais dans la solution d'un problème réel. Mais les mêmes exemples, que j'ai rapportés pour éclaircir le premier paradoxe, serviront aussi à éclaircir celui-ci."

[26] Daston is identifying the sense in which the term metaphysics is used by Burtt and others. She is somewhat critical of this usage because it does not take into account the various actual historical systems of metaphysics which prevailed in the early modern period. To the extent however that the term serves to designate certain extra-scientific or extra-mathematical attitudes in past research it remains a useful concept of historical analysis.

EDITH DUDLEY SYLLA

JACOB BERNOULLI ON ANALYSIS, SYNTHESIS, AND THE LAW OF LARGE NUMBERS

I Introduction

Jacob Bernoulli was the earliest mathematician to prove a law of large numbers. Following in the directions opened by Christiaan Huygens's *On calculations in games of chance* (1657), he knew how expectations could be calculated for games in which the possible outcomes result from the design of game pieces such as dice or cards. He was interested, however, in developing an "art of conjecturing" that would apply mathematics to make prudent decisions in civil, moral, and economic matters. By his proof of the law of large numbers, he believed he had shown that observed relative frequencies could be reliably used in such calculations. Bernoulli's law of large numbers showed that if, for example, one has a die with a one-sixth chance of falling with any given side up, then as the die is repeatedly thrown, it becomes more and more probable that the observed relative frequency of that side being up will fall within some small interval around one-sixth. In the proof of this law, Bernoulli assumed that there are *a priori* equally likely possible cases in a given ratio and demonstrated that, if so, then the observed relative frequencies will tend to converge toward the *a priori* ratio of cases over a large number of trials. He also implied, however, that the truth of this proposition meant that it would be possible to find, within narrow limits, otherwise unknown ratios of cases *a posteriori*, from the outcomes of frequently repeated trials:

> "[...] another way is open to us by which we may obtain what is sought. What cannot be ascertained *a priori* may at least be found out *a posteriori*, that is from the results many times observed in similar situations, since it should be presumed that something can happen or not happen in the future in as many cases as it was observed to happen or not to happen in the past in a similar state of things."[1] (Bernoulli 1713, 224)

Although Jacob Bernoulli was a pioneer in the development of the mathematical theory of probability, his *The Art of Conjecturing* had less immediate influence than it might have had because he left it unfinished at his death. While large parts of the work were completed in the 1680s, well before Bernoulli's death in 1705, the book was not published until 1713, by which time Pierre Rémond de Montmort, Abraham De Moivre, and Nicholas Bernoulli were all active in the

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 79–101.

field of mathematical probability and in direct communication with each other, so that they tended to be more influenced by each other than by Jacob Bernoulli's work directly[2]. Because of this publication history, it may be difficult to discern Jacob Bernoulli's personal understanding of the foundations of mathematical probability and hence difficult to understand what he intended to accomplish through his proof of the law of large numbers. Ian Hacking, in particular, has raised problems about the correct understanding of Bernoulli's intended interpretation of the law of large numbers (Hacking 1975, ch. 17, 154-165)[3]. These problems are compounded by the fact that Bernoulli's work breaks off immediately after his proof. In one sense it does not matter what Bernoulli intended, since the proof of the theorem holds mathematically no matter how Bernoulli himself understood it. Nevertheless, we may more easily place Jacob Bernoulli within in the history of probability theory if his own interpretation of his work is understood. If I seem to belabor my criticism of Hacking's discussion of Bernoulli's work, it is because it has been influential in shaping subsequent research concerning the early history of probability theory.

Why, then, did Bernoulli believe that his proof of the law of large numbers implied that, if one makes a sufficient number of observations, it is possible to discover the ratio of cases, within narrow limits, *a posteriori* in a trustworthy way? Why did he believe that his proof was such a significant achievement, more significant than if he had discovered a way to square the circle—a discovery which, even if it would have been great, would have been of little use?[4] Is there evidence elsewhere in his work in general and in *The Art of Conjecturing* in particular that would help to answer this question?

In this paper I attempt to discern Jacob Bernoulli's understanding of the significance and use of his law of large numbers by first examining what Bernoulli had to say on mathematical methodology, and in particular on the uses of mathematical analysis and synthesis. For Bernoulli, a mathematical synthesis moves from what is prior and better known to what is posterior, but a mathematical analysis lacks this sense of direction. When Bernoulli contrasts an analytic method to a synthetic one, by an analytic method he almost always means an algebraic one. The central lemmas of Bernoulli's proof of the law of large numbers are algebraic and so analytic in his sense.

After examining what Bernoulli had to say about analysis and synthesis and how he went about proving the law of large numbers, I then describe how the law of large numbers and its proof fit into Bernoulli's more general world view. Jacob Bernoulli developed his art of conjecturing or doctrine of chances with the understanding that God has designed the universe to follow natural laws or regularities and that we only use ideas of chance where we lack knowledge of the underlying causes—not that these underlying causes do not in fact exist. To God everything is known and certain. In Bernoulli's view, the law of large numbers shows that over

the long run the underlying regularities of nature will manifest themselves. Finally, Bernoulli's particular use of algebra and of the properties of binomial expansions to prove the lemmas that form the core of his demonstration of the law of large numbers fit with this "God's eye" view of the universe, in which everything is immediate and there is no scope for ordering into what is mathematically prior or posterior. Thus Jacob Bernoulli's ideas about God and the world combine with his reliance on algebra in proving the law of large numbers to explain what has seemed so problematic to critics like Hacking about Bernoulli's intended interpretation of his law of large numbers: why he "assumed" the existence of a ratio of cases in his proof of the law of large numbers and nevertheless believed that the proof justified the use of observed frequencies to discover such ratios to a close approximation. Thus an understanding of Bernoulli's ideas of analysis and synthesis helps to clear up modern philosophical perplexities about his intended interpretation of the law of large numbers.

II Jacob Bernoulli on Analysis and Synthesis

Part I of *The Art of Conjecturing* is a reprinting with notes of Christiaan Huygens's *On Calculations in Games of Chance*. In it Huygens, and Bernoulli following him, frequently derive expectations in games of chance iteratively, by building up from the simplest cases (for instance to find players' relative expectations when one more round will determine the winner) to more complex cases (for instance to find the players' relative expectations when the game is broken off considerably before the end). In games in which each player's chances depend on those of other previous players and *vice versa*, however, Huygens and Bernoulli sometimes use simultaneous equations to determine the expectations. About this resort to algebra, Bernoulli says in his note on the first problem of Huygens's Appendix:

> "Now since all these chances are different and unknown and since any preceding chance depends on the following chance and the following chance in turn on the preceding [...] it follows that this Problem cannot be solved, at least by the Author's method [...] otherwise than by means of algebraic analysis."[5] (1713, 50)

But Bernoulli seems to think a synthetic approach is preferable. Thus, earlier, in his note on Huygens's Proposition XIV, Bernoulli writes:

> "The Author in this Problem is compelled for the first time to employ algebraic analysis, while in the preceding only synthesis was used. The difference between these two is that in all the former propositions the expectation sought was derived from other expectations that were either totally known and given, or, indeed, not known, but naturally prior and simpler, and not dependent in turn upon that sought. For this reason, it was possible, by beginning with the aid of the simplest of all of them, to proceed step by step to unravel other more complex cases without any analysis. Here, however, the matter is different [...]. It is worthwhile to have observed this, so that by a clear

> example it may appear what the difference is between the two methods and when one or the other is to be turned to."[6] (*ibid.*, 47-48)

Bernoulli follows this by suggesting his own alternative method that can be used both when synthesis is normally used and when algebraic analysis had been resorted to:

> "I have said that it cannot be done following in the author's footsteps. There is, however, still another special way by which I may pursue what is sought short of any analysis. This additional way may also be usefully employed in what follows. Let us, in place of the two alternate players, hypothesize infinitely many players, to each of whom in order, one after the other, only one throw is conceded [...]. (*ibid.*, 60-61)
> Further, the method familiar to us may also be used with regard to this hypothesis, nor is this method less compatible with questions that are commonly solved by synthesis alone than with those that require analysis."[7] (*ibid.*, 48)

Since Bernoulli's terminology alternates between "algebraic analysis [*analysis algebraica*]" and simply "analysis [*analysis*]", it is clear that by "analysis" he often means, in our terms, simply algebra. Elsewhere, following Huygens, he calls "analysis" the working out of the solution to a problem (*ibid.*, 2-3)[8]. On the other hand, as is clear from his definitions of analysis and synthesis, he does sometimes have a directional differentiation between analysis and synthesis in mind. In Bernoulli's terminology a "synthesis" is mathematical reasoning that goes step by step from what is prior and already known to what is at first unknown, while "analysis" is a line of mathematical reasoning that may involve recursion and/or solution of simultaneous equations. Discussing a problem in which three players in turn draw stones without replacing them from an urn originally containing 12 stones, Bernoulli states that in the end one comes down to known chances, so that the problem can then be reversed to build up a synthesis from the simplest cases:

> "If, again, the sense of the problem were that the stones taken from a common supply of 12 were not replaced after being taken from the urn, then the first player indeed would, after playing, take third place and the third player second place and the second player first place, but, on that account, the players would not exchange among themselves chances equal to those that existed at the start, as happened under the preceding hypothesis. Rather, they would continually acquire new chances, different from the earlier chances, because of the changed number of stones. These chances would be simpler to the extent that more black stones were withdrawn and such that finally they end in chances that are altogether known. On account of this, we can begin, using the Author's accustomed method, from the simplest cases, and proceed backwards through all the intermediate cases, arriving finally at the case proposed in the question, having used the method of synthesis."[9] (*ibid.*, 59)

In sum, Bernoulli uses the word "synthesis" in the sense standard from the time of the Greeks to mean a demonstration beginning from axioms, postulates, or what is prior and better known and moving to what was previously not known or not proved. "Analysis," on the other hand, for him as for the Greeks, is a method that does not begin from what is better known, but from something not

known, or not yet proved. Bernoulli is unlike the Greeks, however, because he has a method of analysis in mind, namely algebra, or the solution of simultaneous equations with unknowns.[10] While there is a perennial question about Greek geometrical analysis, because it seems to assume unjustifiably that the deductions of the analysis will always be reversible to construct the desired synthesis (Mahoney 1968), there is no such problem with algebraic analysis, which is, in this sense, directionless. Thus Bernoulli understands Huygens's *On Calculations in Games of Chance* to exhibit or demonstrate a small number of approaches or methods, both synthetic and analytic, by which problems concerning games of chance may be solved. While a synthetic method may be more natural, building up from the prior and better known to what is sought, an algebraic method also achieves the desired results, and that without the necessity of being supplemented by a synthesis.

III *A Priori*, *A Posteriori*, and the Law of Large Numbers

When in Part IV of *The Art of Conjecturing* Bernoulli introduces his law of large numbers, he does not use concepts of analysis and synthesis to indicate directions of reasoning, but rather the concepts of *a priori* and *a posteriori*. After the lines quoted above (at note 1), Bernoulli goes on:

> "If, for example, there once existed three hundred people with the same age and body type as Titius now has, and you observed that two hundred of them died before the end of a decade, while the rest lived longer, you could safely enough conclude that there are twice as many cases in which Titius also may die within a decade as there are cases in which he may live beyond a decade. Likewise if someone for several years past should have observed the weather and noted how many times it was clear or rainy or if someone should have very frequently watched two players at a game and should have seen how many times this or that player won, just by doing so one would have discovered the ratio that probably exists between the numbers of cases in which the same outcomes can happen or not happen in the future in circumstances similar to the previous ones."[11] (1713, 224-225)

The method of arguing *a posteriori*, or empirically, in this period could also be called "analysis," as Isaac Newton does in his famous Query 31 of the *Opticks*:

> "As in Mathematicks, so in Natural Philosophy, the Investigation of difficult Things by the Method of Analysis, ought ever to precede the Method of Composition. This Analysis consists in making Experiments, and Observations, and in drawing general Conclusions from them by Induction [...]. By this way of Analysis we may proceed from Compounds to Ingredients, and from Motions to the Forces producing them; and in general, from Effects to their Causes, and from particular Causes to more general ones, till the Argument end in the most general. This is the Method of Analysis: And the Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phaenomena proceeding from them and proving the Explanations." (Newton 1704, 404-405)

Here Newton links the methods of analysis and synthesis in mathematics and in physics, explaining physical analysis as induction from experimental data. This might lead us to believe that Jacob Bernoulli also intended his *a posteriori* method to be based on induction. But in turning to the proof of his law of large numbers, Bernoulli shows no concern about problems of induction. Rather, in order to justify the use of observed frequencies as the basis for decisions or predictions, Bernoulli thinks he needs to show two things. First, he wants to demonstrate that as the number of observations increases, the probability that the *a posteriori* observed ratio of outcomes corresponds closely to an *a priori* ratio also increases—this is something even ordinary people commonly assume, but they do not know how to prove it:

> "This empirical way of determining the number of cases by experiment is neither new nor uncommon. The author of *The Art of Thinking* [*i.e.*, Antoine Arnauld], a man of great acuteness and talent, made a similar recommendation in Chapter 12 and following of the last part [*i.e.*, Part IV], and everyone consistently does the same thing in daily practice. Neither should it escape anyone that to judge in this way concerning some future event it would not suffice to take one or another experiment, but a great abundance of experiments would be required, given that even the most foolish person, by some instinct of nature, alone and with no previous instruction (which is truly astonishing), has discovered that the more observations of this sort are made, the less danger there will be of error. But although this is naturally known to everyone, the demonstration by which it can be inferred from the principles of the art is hardly known at all, and, accordingly, it is incumbent upon us to expound it here."[12] (1713, 225)

But, second, beyond demonstrating the effect of increasing numbers of observations, Bernoulli also wants to prove that the process does not reach a limit of certainty or probability beyond which greater probability is impossible:

> "But I would consider that I had not achieved enough if I limited myself to demonstrating this one thing, of which no one is ignorant. Something else remains to consider, which perhaps no one has thought about up to this point. It remains, namely, to ask whether as the number of observations increases, so the probability increases of obtaining the true ratio between the numbers of cases in which some event can happen and not happen, such that this probability may eventually exceed any given degree of certainty. Or whether, instead, the problem has an asymptote, so to speak; whether, that is, there is some degree of certainty that may never be exceeded no matter how far the number of observations is multiplied, so that, for example, we may never be certain that we have discovered the true ratio of cases with more than a half or two-thirds or three-fourths parts of certainty."[13] (*ibid.*)

With this introduction, Bernoulli then goes on to his proof, which assumes *a priori* ratios exist, although they may or may not be known. What then is the relationship of analysis and synthesis, or the relationship of the *a priori* and the *a posteriori*, in this proof? Given Bernoulli's earlier discussions of mathematical analysis and synthesis, we should expect him to take a consistent position on these matters[14].

IV Bernoulli's Proof of the Law of Large Numbers

In order to investigate this question further, it will be worthwhile to examine Bernoulli's proof of his law of large numbers. Bernoulli achieves his proof by first demonstrating five lemmas concerning the terms of a binomial expansion. He then is able to prove his law essentially by showing how the various terms of the binomial expansion correspond to possible outcomes of nt trials of a situation in which there are r cases for a positive outcome and s cases for a negative one, $t = r + s$, and n is some large integer. Todhunter states the essentials of the proof quite clearly and succinctly:

> "We will now state the purely algebraical part of the theorem. Suppose that $(r+s)^{nt}$ is expanded by the Binomial Theorem, the letters all denoting integral numbers and t being equal to $r + s$. Let u denote the sum of the greatest term and the n preceding terms and the n following terms. Then by taking n large enough the ratio of u to the sum of all the remaining terms of the expansion may be made as great as we please. If we wish that this ratio should not be less than c it will be sufficient to take n equal to the greater of the two following expressions:
>
> $$\frac{\log c + \log(s+1)}{\log(r+1) - \log r}\left(1 + \frac{s}{r+1}\right) - \frac{s}{r+1},$$
>
> and
>
> $$\frac{\log c + \log(r-1)}{\log(s+1) - \log s}\left(1 + \frac{r}{s+1}\right) - \frac{1}{s+1}.$$
>
> [...] Let us now take the application of the algebraical result to the Theory of Probability. The greatest term of $(r+s)^{nt}$, where $t = r+s$ is the term involving $r^{nr}s^{ns}$. Let r and s be proportional to the probability of the happening and failing of an event in a single trial. Then the sum of the $2n+1$ terms of $(r+s)^{nt}$ which have the greatest term for their middle term corresponds to the probability that in nt trials the number of times the event happens will lie between $n(r-1)$ and $n(r+1)$, both inclusive; so that the ratio of the number of times the event happens to the whole number of trials lies between $\frac{r+1}{t}$ and $\frac{r-1}{t}$. Then, by taking for n the greater of the two expressions in the preceding [...], we have the odds of c to 1 that the ratio of the number of times the event happens to the whole number of trials lies between $\frac{r+1}{t}$ and $\frac{r-1}{t}$." (Todhunter 1949, 71-72)

Now, because the central work of the proof is done by means of lemmas concerning any binomial expansion, it is not immediately clear whether Bernoulli would consider the reasoning in his proof of the main theorem to have been analytic or synthetic. But Todhunter's labelling of the lemmas as "the purely algebraical part of the theorem," provides a needed clue: the five lemmas are in a sense Bernoulli's analysis of the problem, while the synthesis is what Todhunter calls "the application of the algebraical result to the Theory of Probability" and what Bernoulli himself calls the demonstration of the principal proposition[15]. In the proofs of his lemmas, Bernoulli takes it for granted that mathematicians know the series expansions of binomials to various powers, and he treats them as pure mathematics, abstracted from any particular application[16]. He raises the possibility in a scholium that someone may object to the way he has made use of infinites in his proof of Lemmas 4 and 5, but provides an alternative interpretation for such objectors that requires only finite numbers and not infinites[17]. The proof of the second lemma is an informal induction[18].

Based on the algebraic analysis of the lemmas, Bernoulli's proof of the law of large numbers is synthetic, starting from what is known through the lemmas and moving to prove the desired conclusion. How he gets from the pure mathematics of the lemmas to the proof of his law of large numbers is, in his terms, simply "by the application of the foregoing lemmas to the present purpose", that is, by interpreting the terms of the binomial expansion as expressing the numbers of ways in which various possible outcomes of a series of observations can occur. Bernoulli writes:

> "*Demonstration.* Let *nt* be the number of observations to be taken, and let us ask how great is the expectation or how great is the probability, that they will all be fecund except for, first, none, then 1, 2, 3, 4, etc. sterile. But since in any observation there are, by hypothesis, *t* cases at hand, and of them *r* are fecund and *s* sterile, and the individual cases of one observation can be combined with the individual cases of the other, and those combined can be joined again with the individual cases of the third, fourth, etc., it is easy to see that this situation fits the Rule in the Notes appended to the end of Proposition XIII. [*sic*, should be XII] in Part I, and its Corollary 2, which contains a general formula, with the help of which it is seen that the expectation of no sterile observations is $r^{nt}:t^{nt}$, of one $\frac{nt}{1}r^{nt-1}s:t^{nt}$, of two $\frac{nt(nt-1)}{1\cdot 2}r^{nt-2}ss:t^{nt}$ of three $\frac{nt(nt-1)(nt-2)}{1\cdot 2\cdot 3}r^{nt-3}s^3:t^{nt}$ and so forth. Consequently, omitting the common denominator t^{nt} the degrees 'of probability or the numbers' of cases in which it can happen that all the experiences are fecund, or all except one sterile one, or all except 2, 3, 4, etc. are expressed in order by $r^{nt}, \frac{nt}{1}r^{nt-1}s, \frac{nt(nt-1)}{1\cdot 2}r^{nt-2}ss, \frac{nt(nt-1)(nt-2)}{1\cdot 2\cdot 3}r^{nt-3}s^3$ etc. Now these, in fact, are the terms of the power *nt* of the binomial *r+s*, investigated just now in our lemmas. Then all the rest is completely evident. Indeed, it is clear from the nature of the progression that the number of cases that combine *ns* sterile experiences with *nr* fecund ones is the maximum term M, or the term that *ns* terms preceed and *nr* follow, by Lemma 3."[19] (Bernoulli 1713, 236-237)

Thus Bernoulli bases his demonstration upon the algebraic lemmas, interpreting the terms of the binomial expansion in terms of the probabilities of various outcomes of a series of observations. The largest term of the binomial expansion represents the numbers of ways in which the ratio of fertile to sterile observations may equal the underlying ratio of cases.

V Cases (*casus*) and Bernoulli's Conceptions of God and the World

What in Jacob Bernoulli's larger world view justified his belief that cases of death, or of weather, or of winning at tennis, and so forth could be represented by the terms of a binomial expansion as he represented them in his proof of the law of large numbers? Or why would a prudent physician or insurance agent be wise or justified in using observed ages at death of people in various situations to calculate the life expectancies of living people of given age and circumstances? Not only did Bernoulli not know *what* the fundamental *a priori* ratios of cases were for diseases or the weather or any other of the political, moral, or economic situations to which he hoped to apply the art of conjecturing, but also, from our point of view, he did not know *that* there were fundamental *a priori* ratios of cases.

Leibniz raised this objection in correspondence with Bernoulli near the end of the latter's life, arguing that the risks of various diseases are not known and, in fact, may not be stable. In response, Bernoulli admitted that the situation quite likely changes over time. Modern life expectancy, Bernoulli reasoned, was doubtless different from the life expectancy in Biblical times. Nevertheless Bernoulli was optimistic that there was enough stability in the real world for his *a posteriori* method to be useful. A central reason for this optimism was that even if we do not know anything about the ratios in real world cases, God knows. Things are uncertain to us, but not to God:

> "All things under the sun, which are, were, or will be, in themselves and objectively always have the highest certainty. This is evident concerning past and present things, since by the very fact that they are or were, these things cannot not exist or not have existed. Nor should there be any doubt about future things, which in like manner, even if not by the necessity of some inevitable fate, nevertheless by divine foreknowledge and predetermination, cannot not be in the future. Unless, indeed, whatever will be will occur with certainty, it is not apparent how the praise of the highest Creator's omniscience and omnipotence can prevail."[20] (*ibid.*, 210-211)

Responding directly to Leibniz's argument, Bernoulli said:

> "Let me remove a few objections which certain learned men have raised against these views. [...] They object first that the ratio of stones is different from the ratio of diseases or changes in the air: the former have a determinate number, the latter an indeterminate and varying one. I reply to this that both are considered to be equally uncertain and indeterminate with respect to our knowledge. On the other hand, that either is indeterminate in itself and with respect to its nature can no more be conceived by us than it can be conceived that the same thing at the same time is both created and not created by the Author of nature: for whatever God has made, he has, by that very act, also determined at the same time."[21] (*ibid.*, 227)

Thus Jacob Bernoulli did not believe that nature or even human life is inherently statistical or probabilistic[22] (Daston 1992). Although he was not sure how human freedom could be reconciled with the fact that God determines and foresees everything that will happen, Bernoulli nevertheless believed that everything is determined by God[23]. Humans use probabilistic reasoning, he said, not because the world is inherently governed by chance, but because we do not know its hidden mechanisms. The laws of physics and the initial conditions determine which side of a die will fall facing up. We say that each face has a one-sixth chance of facing upwards because we do not know the exact initial conditions or perhaps all the laws of physics, but the fall of the die is nonetheless physically determined.

Bernoulli's understanding *that* there are *a priori* ratios of cases in real world situations helps to elucidate the cryptic statement with which *The Art of Conjecturing* ends:

> "Whence at last this remarkable result is seen to follow, that if the observations of all events were continued for the whole of eternity (with the probability finally ending in perfect certainty) then everything in the world would be observed to happen in fixed ratios and with a constant law of alternation. Thus in even the most accidental and fortuitous we would be bound to acknowledge a certain quasi necessity and, so to speak, fatality. I do not know whether Plato already wished to assert this result in his dogma of the universal return of things to their former positions, in which he predicted that after the unrolling of innumerable centuries everything would return to its original state."[24] (*ibid.*, 239)

To a modern eye this passage seems to mean only that if all events of all eternity are taken into account, then they will have some ratio to each other, whatever that may be. Bernoulli, however, when he says, "fixed ratios and with a constant law of alternation," implies that there will be some lawlike ratios of integers, small or large, but not unrecognizable as such.

Up to this point, I have been translating "casus" when it appears in Bernoulli's Latin as "cases," as when he says in introducing his *a posteriori* method of determining the ratios of cases, "it ought to be anticipated that something can happen or not happen in the future in as many cases as it was observed to happen or not to happen in the past in a similar state of things."[25] (*ibid.*, 224) When, in the eighteenth century, other authors writing about games of chance translated "casus" in this sense into English, they almost always translated it as "chances." If I translated Bernoulli as saying, "it ought to be anticipated that something can happen or not happen in the future with as many chances as it was observed to happen or not to happen in the past in a similar state of things," then Bernoulli might seem to believe that chance was intrinsic to physical reality and not only to our thinking about it.

What, then, is a "casus" for Bernoulli? His models or metaphors for *casus* come first of all from games. *Casus* sometimes correspond to stones to be drawn out of an urn[26]. Dice and cards also provide common models. With stones in an urn there may be many stones of the same color any of which is equally likely to be drawn. Cards provide a more complicated set of possibilities of a similar type. With normal dice, on the other hand, each die might be thought to have an equal proclivity for falling with any of its faces up. Hence, with stones in an urn or cards

or dice, the "casus" correspond to separable aspects of physical reality, but their ease in occurring may depend on various factors configuring the situation, as well as on the items themselves. It is not essential to these models that the game pieces themselves have some inherent "proclivity" to exhibit one or another case, that is, that they have in themselves some inherent "probability" of appearing one way or another. Jacob Bernoulli in *The Art of Conjecturing* never uses "probability [*probabilitas*]" to refer to inherent properties or proclivities of stones or cards or dice, but always to refer to degrees of certainty about the truth of propositions.

While Abraham De Moivre in his *The Doctrine of Chances* consistently uses "chances" with regard to alternative possibilities, his statements about these chances show that he too did not believe that the underlying reality was governed by chance as we understand it. In a typical problem, he says:

> "To find the Probability of throwing a Chance assigned a given number of times without intermission, in any given number of Trials." (De Moivre 1718, 254)

Here the "chance assigned" could be anything, say to throw a 7 with two dice: the "chance assigned" is some specific outcome, one of several possible outcomes.

After De Moivre has discussed the law of large numbers he says:

> "*Chance*, as we understand it, supposes the *Existence* of things, and their general known *Properties*: that a number of Dice, for instance, being thrown, each of them shall settle upon one or other of its Bases. After which, the *Probability* of an assigned Chance, that is of some particular disposition of the Dice, becomes as proper a subject of Investigation as any other quantity or Ratio can be.
>
> But *Chance*, in atheistical writings or discourse, is a sound utterly insignificant: It imports no determination to any *mode of Existence*; nor indeed to *Existence* itself, more than to *non-existence*; it can neither be defined nor understood: nor can any Proposition concerning it be either affirmed or denied, excepting this one, 'That it is a mere word.'" (*ibid.*, 253)

Shortly before this passage, De Moivre wrote:

> "From what has been said, it follows, that Chance very little disturbs the Events which in their natural Institution were designed to happen or fail, according to some determinate Law; for if in order to help our conception, we imagine a round piece of Metal, with two polished opposite faces, differing in nothing but their colour, whereof one may be supposed to be white, and the other black; it is plain that we may say, that this piece may with equal facility exhibit a white or black face, and we may even suppose that it was framed with that particular view of shewing sometimes one face, sometimes the other, and that consequently if it be tossed up Chance shall decide the appearance [...] yet the appearances, either one way or the other, will perpetually tend to a proportion of Equality [...]. What we have said is also applicable to a Ratio of Inequality [...]. And thus in all Cases it will be found, that *altho' Chance produces Irregularities, still the Odds will be infinitely great, that in the process of Time, those Irregularities will bear no proportion to the recurrency of that Order which naturally results from* Original Design." (*ibid.*, 250-251)

Thus, for De Moivre, and I suggest also for Jacob Bernoulli, there are laws of nature which in the long run will appear, however "chance" may obscure them in the short run. Moreover, according to De Moivre, it is God who has determined and continues to determine these regularities, not some intrinsic propensities or proclivities of material bodies:

> "[...] such Laws, as well as the original Design and Purpose of their Establishment, must all be *from without*; the *Inertia* of matter, and the nature of created Beings, rendering it impossible that any thing should modify its own essence, or give to itself, or to any thing else, an original determination or propensity. And hence, if we blind not ourselves with metaphysical dust, we shall be led, by a short and obvious way, to the acknowledgment of the great *Maker* and *Governour* of all; *Himself all-wise, all-powerful* and *good*." (*ibid.*, 252)

From this point of view, then, for De Moivre (and for Bernoulli as well) it is clear that consistently observed frequencies of events in the world reveal the laws of nature or structures built into the universe no less than faces built into a die:

> "As, upon the Supposition of a certain determinate Law according to which any Event is to happen, we demonstrate that the Ratio of Happenings will continually approach to that Law, as the Experiments or Observations are multiplied: so, *conversely*, if from numberless Observations we find the Ratio of the Events to converge to a determinate quantity, as to the Ratio of P to Q; then we conclude that this Ratio expresses the determinate Law according to which the Event is to happen.
> For let that Law be expressed not by the Ratio P:Q, but by some other, as R:S; then would the Ratio of the Events converge to this last, not to the former: which contradicts our *Hypothesis*. And the like, or greater, Absurdity follows, if we should suppose the Event not to happen according to any Law, but in a manner altogether desultory and uncertain; for then the Events would converge to no fixt Ratio at all." (*ibid.*, 251-252)

Thus De Moivre's "chances," no less than Bernoulli's "casus"/"cases", reflect the laws of nature built into the existence of things and not something "desultory." They come "from without," that is from God or the First Cause, who, in creating, gives determination to creation. If there is chance in creation, it is only because God, like a dice maker, has designed into creation certain features that will result in the appearance of events with certain frequencies, as the designer of a die designs the die to come up one-sixth of the time on each of its faces. It is these features of God's design that can be found out *a posteriori* by observing the ratios of outcomes in the world over sufficiently long periods of time.

That there will be ratios in events observed over long periods of time results from God's design, but ratios observed *a posteriori* will not always correspond to the most fundamental structures of reality. In his commentary on Huygens, Bernoulli at first assumed that the ratios of cases used in the calculations resulted from the nature of the game pieces, but as he went on he noticed that Huygens sometimes treated the numerator and denominator of a fraction representing an expectation as if they represented numbers of cases, even if they were derived in a different way:

> "It helps here to observe that the Author supposes that any expectation expressed as a fraction may also be considered as if it resulted from as many cases for obtaining the stake a as are indicated by the numerator of the fraction and as many cases for obtaining nothing as are signified by the difference between the denominator and the numerator, notwithstanding that perhaps that expectation was arrived at in another way. Thus although the person who undertakes to throw two sixes in two tries arrives at his expectation of $(71/1296)a$ by a case for obtaining a and 35 cases for $(1/36)a$, nevertheless one could judge him to obtain it by 71 cases for obtaining a and 1225 cases for 0." (Bernoulli 1713, 29)

Thus the ratios of cases observed in wins and losses of tennis players over time may not correspond directly to some basic features of the minds or bodies of the players or their equipment, but to complex interactions of many factors. In an early consideration of tennis published in 1686, Bernoulli stated that the underlying ratio of cases may be incommensurable[27].

Mathematically, "cases" enter Bernoulli's proof of the law of large numbers in two ways. First of all, there are the fundamental cases with which the proof begins, that is r cases for a fertile outcome and s cases for a sterile one. But after nt observations have been made, there are also more complex cases, first the case in which all outcomes are fertile or positive, then the case in which the first trial is sterile, but the rest fertile, and so forth. The largest term of the binomial expansion is shown to represent the numbers of cases in which the individual outcomes are in the ratio of the underlying cases (corresponding to the two terms of the binomial, r and s). The probability that the ratio of outcomes will fall within some small interval around the ratio corresponding to the ratio of the underlying cases is explained to be proportional to the sum of a certain number of terms of the binomial expansion on either side of the largest term. Then the ratio between this sum and the sum of all the terms outside the limits is shown to increase without limit as the number of trials, nt, increases. A very large number of trials is required if it is desired that there be a very high probability that the ratio fall within very narrow limits. In interpreting this result, Bernoulli assumes that he is looking for ratios of integers and he talks about finding, determining, or discovering the ratio[28]. He seems to take it for granted that it will be obvious what the "real ratio" of cases is, even if the observed ratio of frequencies after many trials should deviate from it very slightly[29].

Did Bernoulli think that there really were in the outside world "cases" corresponding to various diseases or other possible causes of death and that by examining statistics for death rates he could discover underlying causes? Given his remark in commenting on Huygens's treatment of the numerators and denominators of expressions for expectation as if they referred to numbers of cases, I conclude that Bernoulli thought that the ratios found by experience might not represent fundamental cases or causes in the external world, but that they would represent the result of complex interactions of such cases or causes. In the proof of the law of large numbers, at first the cases are simple successes and failures, but once one has observed nt trials, then the cases become not just the r cases for success and s

cases for failure in a single trial, but instead the case of *nt* successes, the cases of *nt*–1 successes combined with one failure, etc., up through the case of *nt* failures. When Bernoulli talks about diseases or the weather, he sometimes talks as if the diseases would be the cases, but twice (in a letter to Leibniz and in *The Art of Conjecturing*) he chooses the word "tinder" (*fomitem*), which seems to be a purposefully vague or multivalent word with some connotations like "seed" or "germ"[30].

The one work in which Bernoulli did apply his method to a concrete situation was his *Letter to a Friend* on the game of tennis, published together with the *Ars Conjectandi* in 1713. His idea was that it would be possible to take the ratios of points or games that players won when playing against each other and to use these ratios to predict, for instance, the likelihood of victory when such individuals played as parts of doubles teams. The sorts of factors that Bernoulli then considered were, for instance, whether the opponents would consistently try to hit to the weaker player, whether the player who has to hit more balls will become tired sooner, etc., such things meaning that the strength of a team could not be supposed to be simply equal to the strength of the better player, nor simply the average between the two players.

In the introduction to the *Letter to a Friend*, Bernoulli writes as if he were cognizant of our question about the physical meaning of the concept of "cases." Bernoulli writes that his friend has seen a thesis of his concerning the game of tennis and:

> "[...] you ask me if these propositions contain some reality that can be demonstrated or if they are only founded on pure conjectures made in the air and which have nothing solid about them. According to what you say, you cannot conceive that the forces of players can be measured by numbers, much less that one can draw the conclusions from them that I have drawn."[31] (*ibid*., new numeration, 1)

After referring to games of chance in which the numbers of cases are known *a priori*, he discusses games of skill in which they are not:

> "[...] it is not the same with games that depend only or in part on the genius, the industry, or the application of the players, such as tennis, chess, and most card games. It is very clear that one could not know how to determine by their causes or *a priori*, as one says, how much one person is more knowledgeable than another, more skillful, or more able, unless one had a perfect knowledge of the nature of the soul and of the disposition of the organs of the human body, which the thousand hidden causes that interact make absolutely impossible. But this does not prevent one from knowing this almost as certainly *a posteriori*, by the observation of the outcome many times repeated, doing what can be done even in games of pure chance when one does not know the number of cases that can occur."[32] (*ibid*., new numeration, 2)

Bernoulli then goes on to describe the drawing of tickets from an urn without knowing the number of tickets of each kind that it contains. If, he says, he drew out a black ticket a hundred times and a white ticket two hundred times, he would not hesitate to conclude that the number of white tickets was about double the

number of black tickets. Having referred to his proof of the law of large numbers, Bernoulli then says that the same reasoning can be applied to games of skill. If, he says, he observed two men playing tennis and one man won 200 or 300 points while the other won 100, then he would judge with sufficient certainty that the first man was a two or three times better player than the second. The first player would have, so to speak, two or three times as many cases or causes making him win as the other[33]. Thus in the one concrete application that we have, Bernoulli makes no claims of knowing what in the real world corresponds to his cases or causes, only that the observed ratio of outcomes can be used as a ratio of cases in making judgments or predictions. If he knew what percentage of the time player *A* had beaten player *B* over a long series of games in the past, this did not mean that Bernoulli knew what it was that made one player more or less likely to win, but only that he thought he could predict the future reliably or with probability.

VI Algebra and the Law of Large Numbers

With this discussion about the meaning of "casus" or "cases" in hand, let me return to an examination of Bernoulli's proof of the law of large numbers. Whatever else "cases" or, for that matter "chances" were, they were always countable, or represented by integers. One always has some number of cases or chances for some outcome, never a fractional amount. The fact that Bernoulli's intuitive understanding of the "cases" is, to use modern terminology, digital rather than analog, may explain why, even though the infinitesimal calculus was in development by this time, he did not think to try proving the law of large numbers using calculus or even geometry, but instead used algebra[34]. Once Bernoulli began to think of his law of large numbers in algebraic terms, the mathematics itself may have become for him a model of the processes he was dealing with. The fact is that in the algebraic part of Bernoulli's proof of the law of large numbers, that is in the lemmas which are "pure mathematics" and which, indeed, contain the whole proof aside from its "application" or interpretation in terms of possible outcomes or expectations, there is no "prior" or "posterior," but everything is, so to speak, at the same cognitive level. One considers, as if laid out together in an array, all the possible outcomes of *nt* trials. This is not like the analysis of a game in which one round of the game precedes the next and in which the ratios of cases or chances may change depending upon the outcomes of the various rounds. Time is not a factor (nor is "sampling" from a larger population). The mathematics takes a "God's eye" point of view, in which every possibility is present and on an equal footing. On the other hand, each "snapshot" of the situation is for some *nt* number of observations. One chooses the level of risk one is willing to take (or the probability of being correct that one requires) and then determines how many observations are necessary to keep the risk that low (or the probability of being correct that high). As the number of trials is never infinite, the risk is never zero (or the probability never one). One may always be wrong. As long as *nt* is not infinite, it is always possible to observe a ratio of frequencies that does not reflect the underlying law of nature. All the law of large numbers tells you is what the chances are that you are wrong or the probability that you are right or very nearly so. What the art of conjecturing then provides as a mathematical instrument for decision making is knowledge of how to maximize your expectations before the fact and how to measure the chances that you may be wrong. If, after acting on the basis of the art of conjecturing, you lose, you nevertheless have the consolation of knowing you followed prudent strategy[35].

VII Summary

In this paper, I have made the following points. Jacob Bernoulli had notions of mathematical analysis and synthesis that were not atypical of his times. For him, a mathematical synthesis moves from what is prior and better known, while a mathematical analysis may move in any direction, sometimes deducing what is mathematically prior or better known from what is mathematically posterior. Like many others of his time, even those who, like himself, were in the process of developing infinite or infinitesimal analysis, Jacob Bernoulli when he used the word "analysis" frequently meant nothing more than algebra. Previous historians of probability theory, and in particular Ian Hacking, have questioned Bernoulli's intended interpretation of his law of large numbers, because his proof of the theorem presupposes that there are *a priori* ratios of cases and yet the theorem is supposed to justify discovering these ratios *a posteriori*. Bernoulli's world view, like that of De Moivre, indeed assumes that the universe displays design and that this design is incorporated in laws of nature that undergird observed frequencies. To God, Bernoulli says, all things are known and certain in the past and present and in the future as well. The law of large numbers shows that, despite temporary fluctuations, in the long run the structure of the world will manifest itself. The lemmas of Bernoulli's proof of the law of large numbers, that is the algebraic parts of the proof or the analysis, mirror this "God's eye" perspective on the universe in the sense that there is nothing prior or posterior, but all is equally present and evident. They are pure mathematics and self-contained. All that is required to apply them to prove the law of large numbers is to interpret them to apply to the outcomes of experiments. Thus, both Bernoulli's world view and the way in which he used algebra to prove his lemmas explain why he saw no problem in assuming the existence of *a priori* ratios of cases when he was proving the law of large numbers—and then boasting that the proof of the law of large numbers shows why one can reliably discover ratios of cases *a posteriori*. The ratios of cases so discovered were not necessarily ratios of fundamental underlying causes, but rather ratios of cases that could be prudently used, with the rest of the art of conjecturing, to make decisions in civic, moral, and economic situations.

North Carolina State University
Department of History

Notes

1 The translations of the *Ars Conjectandi* for this paper are my own, part of a joint project with Glenn Shafer to publish an English translation of *The Art of Conjecturing* with supporting materials. I shall quote in notes the original texts.

> "Verum enimvero alia hic nobis via suppetit, qua quaesitum obtineamus; & quod a priori elicere non datur, saltem *a posteriori*, hoc est, ex eventu in similibus exemplis multoties observato eruere licebit; quandoquidem praesumi debet, tot casibus unumquodque posthac contingere & non contingere posse, quoties id antehac in simili rerum statu contigisse & non contigisse fuerit deprehensum."

2 Nicholas Bernoulli was familiar with his uncle Jacob Bernoulli's work in mathematical probability long before the work was published. In 1709 Nicholas defended a mathematical-legal thesis *De Usu Artis Conjectandi in Jure*, that made use of his uncle's ideas, and throughout the period just before the publication of Jacob Bernoulli's *Ars Conjectandi*, Nicholas collaborated with Montmort in their work on probability theory, culminating in the publication of a number of letters from Nicholas to Montmort in the second edition of Montmort's *Essai d'analyse sur les jeux de hazard* (1713). These letters included an alternative approach to proving a law of large numbers. De Moivre's first publication in probability theory was his *De mensura sortis seu de probabilitate eventuum in ludis a casu fortuito pendentibus* in *Philosophical Transactions* (1711). This was followed in 1718 by his *The Doctrine of Chances: or, A Method of Calculating the Probability of Events in Play* (1718). De Moivre first dealt with Bernoulli's proof of the law of large numbers in his *Miscellanea Analytica de Seriebus et Quadraturis* (1730). In the first edition of his *Doctrine of Chances*, he only said, at the end of the preface:

> "Before I make an end of this Discourse, I think myself obliged to take Notice, that some years after my specimen was printed, there came out a Tract upon the Subject of Chances, being a Posthumous Work of Mr. James Bernoully, wherein the Author has shown a great deal of Skill and Judgment, and perfectly answered the Character and great Reputation he hath so justly obtained [...]."

The tone was set for all these later works by Christiaan Huygens, *De Ratiociniis in Ludo Aleae* as it appeared in Latin translation in F. Van Schooten, *Exercitationum mathematicarum* (1657). In 1692 John Arbuthnot published an English translation of much of Huygens's book (Arbuthnot 1692). Montmort said, in the first edition of *Essay d'analyse sur les jeux de hazard* (1708, iii-vi) that he was motivated to attempt to calculate expectations in games of chance by the reports about the manuscript of Jacob Bernoulli's *Ars Conjectandi*, made in the *éloges* at the time of Jacob's death.

3 Here what he writes (1975, ch. 17, 154-165):

> "Chapter 5 of Part IV of *Ars conjectandi* proves the first limit theorem of probability theory. The intended interpretation of this result is still a matter of controversy, but there is no dispute about what Bernoulli actually proved [...]. Bernoulli proves what is now called the weak law of large numbers [...]. Bernoulli's proof is chiefly a consequence of his earlier investigation of combinatorics, for it proceeds by summing the middle terms in the binomial expansion. Notice that this result is a theorem of pure probability theory, and holds under any interpretation of the calculus [...]. Bernoulli's exposition has a basic difficulty that has led to repeated misinterpretation. It is still a matter of

controversy [...]. Bernoulli plainly wants to estimate an unknown parameter *p*. His favourite example is the proportion of white pebbles in an urn. An *estimator* is a function F from data to possible parameter values, in this case, possible values of *p*. Bernoulli uses an *interval estimator* which maps given data onto a set of possible values of *p*, 'bounded by two limits' [...]. Inevitably [...] we come to consider his problem as one of estimating an unknown aleatory probability, or chance. Moreover, we wonder if he wanted to know the epistemic probability that a given estimate of chance was correct [...]. We are [...] confident that Bernoulli did not make any simply fallacious 'inverse' use of his theorem [...]. He thought [his theorem] had application to inverse inference, but does not make clear exactly why."

Stephen Stigler (1986, 66), also brings into question the correct interpretation of the significance of Bernoulli's theorem: "This modern synopsis is inaccurate in several respects, however, as is the occasional claim that Bernoulli presented the first example of an interval estimate of probability." Lorraine Daston (1988, 188-190), chides Hacking more generally for anachronism in his interpretation of Bernoulli's ideas:

> "In Ian Hacking's thoughtful discussion of the *Ars conjectandi*, for example, Bernoulli emerges as both more prescient and more quaint than a less anachronistic reading would warrant. On the one hand, Hacking credits Bernoulli with anticipating a frequentist 'security level' for inductive inference [*corr. ex* influence] [...] and on the other, he saddles Bernoulli with a 'useful equivocation' between *de re* and *de dicto* senses of possibility and corresponding epistemic and physical senses of probability."

On Bernoulli's inverse use of his law, cf. also *ibid.*, 234ff. A general corrective to Hacking's history of the emergence of probability is to be found in Garber and Zabell (1978). The process of translating Bernoulli has made it clear to me that Bernoulli's use of the term "*probabilitas*" is always epistemic—the word is never used by Huygens or by Bernoulli in the first three parts of the *Ars Conjectandi* dealing with games of chance.

4 Cf. Bernoulli (W, vol. III, 88; from Bernoulli's notebook *Meditationes*, p. 91): "NB. Hoc inventum pluris facio quam si ipsam circuli quadraturam dedissem, quod si maximè reperiretur, exigui usûs esset."

5 "Quoniam enim omnes istae sortes differentes sunt et incognitae, earumque praecedens quaelibet a sequente et postrema vicissim a prima dependet, uti ex subjuncta operatione constabit, non poterit Problema istud Auctoris saltem methodo, per ea quae ad Propos. ult. annotata sunt, aliter quam mediante analysi algebraica expediri."

6 "Auctor in hoc Problemate primum adhibere cogitur analysin algebraicam, cum in praecedentibus sola synthesi usus fuisset: cuius differentiae ratio est, quod in illis omnibus expectatio quaesita fluebat ex aliis expectationibus vel in totum cognitis et datis, vel incognitis quidem, at natura prioribus ac simplicioribus, et quae ab hac vicissim non dependebant; quapropter incipiendo ab omnium simplicissimis earum ope gradatim pergere poterat ad enodandos alios casus magis magisque compositos absque analysi ulla. Secus vero se hic res habet; nam expectationem meam, quam possideo cum collusorem ordo jaciendi tangit, Auctoris more aestimare non possum, nisi cognitam habuero sortem, quam acquiro ubi vices jaciendi ad me devolvuntur: sed et hanc cognoscere nequeo, nisi priorem illam compertam habeam, quae tamen ea ipsa est quam quaerere intendo; unde cum utraque sit incognita, et altera ab altera vicissim dependeat, non possunt Auctoris vestigiis insistendo aliter quam analyseos ope ex se mutuo elici: id quod operae pretium est observasse, ut utriusque methodi discrimen, et quando haec illave in usum vertenda sit, perspicuo aliquo exemplo pateret."

7 "Dixi, Auctoris vestigiis insistendo non posse; datur enim adhuc alia peculiaris via, qua quaesitum consequi possum citra analysin ullam, et quam in sequentibus quoque utiliter adhibere licet. Fingamus loco duorum alternatim ludentium infinitos Collusores, quibus singulis ordine uni post alterum singuli tantum concedantur jactus [...]."

"Methodus porro nobis familiaris etiam in praesente hypothesi locum habet; neque enim hanc magis respuunt eae quaestiones, quae communiter sola synthesi solvuntur, quam quae analysi opus habent."

8 Huygens's Preface addressed to Franciscus Schooten (1657, 519) begins,

> "Cum in editione elegantissimorum ingenii Tui monumentorum, quam prae manibus nunc habes, Vir Clarissime, id inter coetera Te spectare sciam, ut varietate rerum, quarum tractationem instituisti, ostendas quam late se protendat divina Analytices scientia, facile intelligo [...]."

And in ending (*ibid.*, 520) Huygens says,

> "Horum Problematum nonnulla in fine operis addidisse me invenies, omissa tamen analysi, cum quod prolixam nimis operam poscebant, si perspicue omnia exequi voluissem, tum quod relinquendum aliquid videbatur exercitationi nostrorum, si qui erunt, Lectorum."

Bernoulli then echoes Huygens's reference to the working out of solutions to problems as "analysis" (1713, 49): "Coronidis loco Auctor Tractatui suo subjunxit sequentia quinque Problemata, sed omissa analysi vel demonstratione, quam Lectori eruendum reliquit."

9 "Si porro sensus Problematis sit, ut assumpti in commune calculi 12 non reponantur, postquam ex urna exempti fuerint; observandum est, quod per continuam eductionem calculorum nigrorum, primus quidem collusor transeat in locum tertii, tertius in locus secundi, secundus in locum primi, non idcirco tamen pariter sortes, quas ab initio ludi habuere, invicem permutent, ut factum fuit in praecendente hypothesi, sed quod subinde alias novas et a prioribus diversas ob mutatum calculorum numerum acquirant, easque tamen simpliciores quo plures calculi nigri educti fuerint, atque ita comparatas, ut tandem desinant in sortes omnino cognitas. Quapropter incipiendo consueta Auctoris methodo ab omnium simplicissimis, et pergendo retro per omnes intermedias, perveniemus ultimo sola synthesi utendo ad casum in quaestione propositum."

10 Cf. Boyer (1968, 97-98 ("Perhaps more genuinely significant is the ascription to Plato of the so-called analytic method [...]. Plato seems to have pointed out that often it is pedagogically convenient, when a chain of reasoning from premises to conclusion is not obvious, to reverse the process. One might begin with the proposition that is to be proved and from it deduce a conclusion that is known to hold."); 210 ("Pappus describes analysis as 'a method of taking that which is sought as though it were admitted and passing from it through its consequences in order to something which is admitted as a result of synthesis.' That is, he recognized analysis as a 'reverse solution,' the steps of which must be retraced in opposite order to constitute a valid demonstration."); 352 ("Viète had been one of the first to use the word 'analysis' as a synonym for algebra"); 418-419 ("One who has read our chapters on Greece will see that Wallis was far better as a mathematician than as a historian, for he equates algebra (or the analytics of Viète) with the ancient geometrical analysis.")).

11 "Nam si ex. gr. facto olim experimento in tercentis hominibus ejusdem, cujus nunc Titius est, aetatis & complexionis, observaveris ducentos eorum ante exactum decennium mortem oppetiisse, reliquos ultra vitam protraxisse, satis tuto colligere poteris, duplo plures casus esse, quibus & Titio intra decennium proximum naturae debitum solvendum sit, quam quibus terminum hunc transgredi possit. Ita si quis a plurimis retro annis ad coeli tempestatem attenderit, notaveritque, quoties ea serena aut pluvia extiterit: aut si quis duobus ludentibus saepissime adstiterit, videritque quoties hic aut ille ludi victor evaserit, eo ipso rationem detexerit, quam probabiliter habent inter se numeri casuum, quibus iidem eventus praeviis similibus circumstantiis & posthac contingere ac non contingere possunt."

12 "Atque hic modus empiricus determinandi numeros casuum per experimenta neque novus est neque insolitus; nam et Celeb. Auctor Artis cogitandi magni acuminis et ingenii Vir Cap. 12 et seqq. postremae Partis haud dissimilem praescribit, et omnes in quotidiana praxi eundem constanter observant. Deinde nec illud quenquam latere potest, quod ad judicandum hoc modo de quopiam eventu non sufficiat sumsisse

unum alterumque experimentum, sed quod magna experimentorum requiratur copia; quando et stupidissimus quisque nescio quo naturae instinctu per se et nulla praevia institutione (quod sane mirabile est) compertum habet, quo plures ejusmodi captae fuerint observationes, eo minus a scopo aberrandi periculum fore. Quanquam autem hoc naturaliter omnibus notus sit, demonstratio, qua id ex artis principiis evincitur, minime vulgaris est, et proin nobis hic loci tradenda incumbit [...]."

13 "Ubi tamen parum me praestiturum existimarem, si in hoc uno, quod nemo ignorat, demonstrando subsisterem. Ulterius aliquid hic contemplandum superest, quod nemini fortassis vel cogitando adhucdum incidit. Inquirendum nimirum restat, an aucto sic observationum numero ita continuo augeatur probabilitas assequendae genuinae rationis inter numeros casuum, quibus eventus aliquid contingere et quibus non contingere potest, ut probabilitas haec tandem datum quemvis certitudinis gradum superet: an vero Problema, ut sic dicam, suam habeat Asymptoton, hoc est an detur quidam certitudinis gradus quem nunquam excedere liceat, utcunque multiplicentur observationes, puta, ut nunquam ultra semissem, aut 2/3, aut 3/4 certitudinis partes certi fieri possumus, nos veram casuum rationem detexisse."

14 I make this point because of Hacking's discussion of Bernoulli and the law of large numbers. Cf. Hacking (1975 as quoted above, note 3, and 159):

> "Remember, however, that at the time Bernoulli wrote, the problem of induction had not yet been stated as a central problem of philosophy [...]. One thing Bernoulli was *not* trying to do was to solve some publicized problem of induction, for when he wrote there was none."

15 Cf. Bernoulli (1713, 236):

> "*Propos. Princip.* Sequitur tandem Propositio ipsa, cujus gratia haec omnia dicta sunt, sed cujus demonstrationem sola Lemmatum praemissorum applicatio ad praesens institutum absolvet."

Cf. also (*ibid.*, 228):

> "Ut prolixae rem demonstrationis qua licet brevitate et perspicuitate expediam, conabor omnia reducere ad abstractam Mathesin, depromendo ex illa sequentia Lemmata, quibus ostensis caetera in nuda applicatione consistent."

16 He says, *e.g.*, at the start of his demonstration of lemma 3 (*ibid.*, 229): " Nota res est inter Geometras, quod potestas nt binomii $r+s$, hoc est $(r+s)^{nt}$ hac serie exprimitur [...]." Cf. also his use of "abstractam Mathesin" in the quotation at the preceding note (15).

17 "It may be objected against Lemmas 4 and 5, by those who are not accustomed to speculations about the infinite, that even if, in the case of an infinite number n, the factors in the expressions for the ratios M/L and M/Λ, namely $nr \pm nm 1, 2, 3$, *etc* [...]. I cannot reply to this uneasiness better than by showing how to assign an actually finite number to n, or a finite power to the binomial, so that the sum of the terms within the bounds L and Λ will have to the sum of terms outside a ratio larger than a given ratio however large [...]. When this has been shown, it will be seen that the objection necessarily also collapses."

18 Cf. (*ibid.*, 229):

> "Every integral power of a binomial $r+s$ is expressed by one more term than the number of units in the index of the power. Thus a square is composed of 3 terms, a cube of 4, a biquadrate of 5, and so forth, as is known."

19 "*Dem.* Ponatur numerus capiendarum observationum nt, & quaeratur, quanta sit expectatio, seu quanta probabilitas, ut omnes existant foecundae, exceptis primo nulla, dein una, duabus, 3, 4 &c. sterilibus. Quandoquidem autem in qualibet observatione praesto sunt ex hyp. t casus, eorumque r foecundi & s steriles, & singuli casus unius observationis cum singulis alterius combinari, combinatique rursus cum singulis tertiae, 4 tae &c. conjungi possunt, facile patet, huic negotio quadrare Regulam Annotationibus Prop. XIII. [*sic*, should be XII] primae Part. in fine subnexam, & ejus Corollarium secundum, quod universalem formulam continet, cujus ope cognoscitur, quod expectatio ad nullam observationem sterilem sit $r^{nt} : t^{nt}$, ad unam $\frac{nt}{1} r^{nt-1} s : t^{nt}$, ad duas steriles $\frac{nt(nt-1)}{1 \cdot 2} r^{nt-2} ss : t^{nt}$, ad tres $\frac{nt(nt-1)(nt-2)}{1 \cdot 2 \cdot 3} r^{nt-3} s^3 : t^{nt}$ & sic deinceps; adeoque (rejecto communi nomine t^{nt}) quod gradus probabilitatum seu numeri casuum, quibus contingere potest, ut omnia experimenta sint foecunda, vel omnia praeter unum sterile, vel omnia praeter duo, 3, 4 &c. sterilia, ordine exprimantur per $r^{nt}, \frac{nt}{1} r^{nt-1} s, \frac{nt(nt-1)}{1 \cdot 2} r^{nt-2} ss, \frac{nt(nt-1)(nt-2)}{1 \cdot 2 \cdot 3} r^{nt-3} s^3$, &c. ipsissimos nempe terminos potestatis nt binomii $r+s$, in Lemmatis modo nostris excussae: unde jam caetera omnia oppido manifesta sunt. Patet enim ex progressionis natura, quod numerus casuum, qui cum ns sterilibus experimentis nr foecunda adducunt, sit ipse terminus maximus potestatis M, utpote quem ns termini praecedunt, & nr sequuntur, per Lemm. 3."

20 "Omnia, quae sub Sole sunt vel fiunt, praeterita, praesentia sive futura, in se & objective summam semper certitudinem habent. De praesentibus et praeteritis constat; quoniam eo ipso, quo sunt vel fuerunt, non possunt non esse vel fuisse: nec de futuris ambigendum, quae pariter etsi non fati alicujus inevitabili necessitate, tamen ratione tum praescientiae tum praedeterminationis divinae non possunt non fore; nisi enim certo eveniant quaecunque futura sunt, non apparet, quo pacto summo Creatori omniscientiae & omnipotentiae laus illibata constare queat."

21 "Objiciunt primo, aliam esse rationem calculorum aliam morborum aut mutationum aeris; illorum numerum determinatum esse, horum indeterminatum & vagum. Ad quod respondeo, utrumque respectu cognitionis nostrae aeque poni incertum & indeterminatum; sed quicquam in se & sua natura tale esse, non magis a nobis posse concipi, quam concipi potest, idem simul ab Auctore naturae creatum esse & non creatum: quaecunque enim Deus fecit, eo ipso dum fecit, etiam determinavit."

22 While I do not agree with every part of Daston's analysis, her basic point is soundly established, namely that there is no real chance in Jacob Bernoulli's universe.

23 Cf. (*ibid.*, 211), immediately following the passage quoted at note 19: "Let others dispute how this certainty of future occurrences may coexist with the contingency and freedom of secondary causes; we do not wish to deal with matters extraneous to our goal."

24 "Unde tandem hoc singulare sequi videtur, quod si eventuum omnium observationes per totam aeternitatem continuarentur, (probabilitate ultimo in perfectam certitudinem abeunte) omnia in mundo certis rationibus & constanti vicissitudinis lege contingere deprehenderentur; adeo ut etiam in maxime casualibus atque fortuitis quandam quasi necessitatem, &, ut dicam, fatalitatem agnoscere teneamur; quam nescio annon ipse jam Plato intendere voluerit, suo de universali rerum apocatastasi dogmate, secundum quod omnia post innumerabilium seculorum decursum in pristinum reversura statum praedixit."

25 For Latin see note 1 above.

26 On the urn model, cf. Daston (1988, 237 ff.)

27 Cf. Bernoulli (1686, 238): "Animadverto, subducto calculo, peritias Colllusorum esse incommensurabiles inter se, id est, veram illorum rationem nullo numero posse exprimi, tametsi id fierit prope verum possit." This incommensurability would follow if it was assumed that one player could concede the other a certain number of points in a fair game and then asked how much better the first player must be than the second if this concession of points makes their chances of winning the game equal.

28 Cf. Leibniz (GM, III, 1, 78; letter of Bernoulli to Leibniz, October 3, 1703):

"Unde jam determinare possum, quot observationes instituendae, ut centies, millies, decies millies etc. verisimilius (adeoque tandem ut moraliter certum) sit, rationem inter numeros casuum, quam hoc pacto obtineo, legitimam et genuinam esse."

In (1713, 227), Bernoulli uses the word "inventam," or "found." Earlier (225), he uses the word "detexerit," and (226), he uses the word "determinare."

29 Cf. Stigler (1986), 66:

"Bernoulli [...] dealt only with the case where the numbers of fertile cases (r) and sterile cases (s) were integers, not with the modern situation in which the proportion $\frac{r}{r+s}$ is allowed to range over all real numbers in the interval [0,1]. His aim was to show that, in essence, the exact ratio $\frac{r}{r+s}$ could be recovered with 'moral certainty' for a sufficiently large N [...]. He did view the ratio $\frac{r}{r+s}$ as possibly an approximation to the real state of affairs, and he knew that r and s were not identifiable ($r' = 10r$ and $s' = 10s$ would give the same ratio as r and s). But up to the order of approximation determined by a given $r+s$ he sought to determine the ratio exactly, as his statements and examples make clear."

Hacking, (1975, 158) assumes that Bernoulli uses the observed ratio of cases after some number of trials plus or minus some small error term as an "estimator" of the *a priori* probability of the outcome, and he is concerned that Bernoulli, not having a notation for conditional probability, has confused the probability that the *a priori* probability p falls within a small range around the observed ratio given that prior probability p, with the probability that the p falls within that same small range around the observed ratio given that observed ratio. When he quotes the passage in Bernoulli's letter to Leibniz quoted in the previous note, (163), reads his interpretation into the text by translating: "that the ratio between the number of cases which I estimate is legitimate and genuine [*i.e.* within some allowed error]." While Leibniz and Bernoulli in their letters (April 1703, October 1703) do write of estimating probabilities ("*doctrina de probabilitatibus aestimandis*"), they are thinking of epistemic probabilities rather than directly of frequencies. *The Emergence of Probability* has been very influential, but here and elsewhere Hacking reads later issues back into earlier authors in a way that creates rather than solves problems of understanding what the historical actors intended.

30 Cf, Bernoulli (1713, 226): "Si loco urnae substituamus aerem, ex. gr. sive corpus humanum, quae fomitem variarum mutationum atque morborum intra se, velut urna calculos, continent [...]." Cf. Leibniz, (GM, III, 1, 88). So the diseases could be the cases, but apparently Bernoulli is thinking that some diseases are more common than others, so that some would count for more cases.

31 "Vous me demandez, si ces Propositions renferment quelque realité qui puisse être démontrée, ou si elles ne sont fondées que sur de pures conjectures faites en l'air, et qui n'ont rien de solide; ne pouvant pas concevoir, à ce que vous dites, que l'on puisse mesurer les forces des joueurs par nombres, et encore moins en tirer toutes les conclusions, que j'en ay tirées."

32 "Qu'il n'en est pas de même des jeux, qui dépendent uniquement, ou en partie, du genie, de l'industrie ou de l'adresse des joueurs, tels que sont les jeux de la paume, des échecs, & la pluspart des jeux de cartes; étant bien visible, que l'on ne sauroit déterminer par les causes, ou *à priori*, comme l'on parle, de combien un homme est plus savant, plus adroit ou plus habile qu'un autre, sans avoir une parfaite connoissance de la nature de l'ame, & et la disposition des organes du corps humain, laquelle mille causes occultes, qui y concourent, rendent absolument impossible. Mais cela n'empêche pas, qu'on ne puisse le sçavoir presque aussi certainement, *à posteriori*, par l'observation de l'événement plusieurs fois reiterée, en faisant ce qui se peut pratiquer dans les jeux même de pur hazard, lors qu'on ne sçait pas le nombre des cas, qui peuvent arriver."

33 Cf. Bernoulli (1713, new numeration, 3):

"Je juge par là, avec assez de certitude, que le premier est deux ou trois fois meilleur joueur que l'autre, ayant pour ainsi dire deux ou trois parties d'adresse, comme autant de cas ou de causes qui luy font gagner la bale, là où l'autre n'en a qu'une."

34 Cf. Hald (1990, 263):

"His proof was worked out at the latest in 1690. It must have seemed rather unsatisfactory, to Bernoulli himself as well, when he included it in the manuscript 15 years later in view of the fact that the integral calculus had been developed in the meantime. In 1705 it would have been natural to evaluate the areas (sums) by means of integrals instead of limiting ordinates [...]. The need for a revision of the proof may have been another reason for Bernoulli's hesitation to publish."

I know of no evidence that Bernoulli thought his proof unsatisfactory.

35 John Arbuthnot, in his (1692), a work incorporating an English translation of large parts of Huygens's *De ratiociniis in ludo aleae*, makes this point in his Preface:

"All a wise Man can do in such a Case is, to lay his Business on such Events, as have the most or most powerful second Causes, and this is true both in the great Events of the World, and in ordinary Games [...] that only which is left to me, is to wager where there are the greatest number of Chances, and consequently the greatest probability to gain [...] and tho it is possible, if there are any Chances against him at all, that he may lose, yet when he chuseth the safest side, he may part with this Money with more content (if there can be any at all) in such a Case."

CARLOS ALVAREZ JIMENEZ

MATHEMATICAL ANALYSIS AND ANALYTICAL SCIENCE*

I Introduction

The development of physical sciences during the eighteenth century is inconceivable without also taking into account a major development in mathematical analysis itself. The birth of a new analytical mechanics, of a new physical theory of sound, of a new capillarity theory or, at the beginning of nineteenth century, of the theories of heat and elasticity, all follow nearly the same procedure consisting in the definition of variables and equations to describe the phenomenon. This treatment of physical sciences, that took a long way from purely descriptive approaches, or even of geometrical models, has been qualified as "analytical". This style can be found in the great scientific treatises of eighteenth century as a kind of "method" of approaching natural phenomena.

At first glance it can be said that a physical science becomes "analytical" as soon as mathematical analysis is used to express the equations describing the physical phenomenon. This way of mathematisation contrasts with a previous one, where the description was done by geometry, for example, as was the case in the first science of movement by Galileo or even with Newton's *Principia* whose underlying mathematical style can be considered as a sort of "geometry of limit positions"(De Gant 1986). But the role played by mathematical analysis in the new physical theories is more than just a way for expressing physical concepts that were previously defined; the algorithms through which this new style of mathematisation is realized become also the means for the constitution of the concepts for an analytical science. The role played by mathematical analysis, as the privileged mathematical mean to describe a wide scope of physical phenomena, includes the first descriptions in mechanics up to those of heat theory. In the preface of his *Théorie Analytique de la Chaleur*, Fourier describes this wide application of mathematical analysis:

> "Les équations analytiques, ignorées des anciens géométres, que Descartes a introduites le premier dans l'étude des courbes et des surfaces, ne sont pas restreintes aux propriétés des figures, et à celles qui sont l'objet de la mécanique rationnelle; elles s'étend à tous les phénomènes généraux. Il ne peut y avoir de langage plus universel et plus simple, plus digne d'exprimer les rapports invariables des êtres naturels.

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 103–145.

Considérée sous ce point de vue, l'analyse mathématique est aussi étendue que la nature elle-même; elle définit tous les rapports sensibles, mesure les temps, les espaces, les forces, les températures; cette science difficile se forme avec lenteur, mais elle conserve tous les principes qu'elle a une fois acquis; elle s'accroît et s'affermit sans cesse au milieu de tant de variations et d'erreurs de l'esprit humain". (Fourier 1822, xiij-xiv)

But even if the so called analytical sciences, such as mechanics, probability theory or heat theory, come closer to a model where mathematical analysis plays the central role, it must be said that its particular status—concerning the model that it follows or the model that it imposes—and also the history of its birth and the history of its radical separation from the previous models of explanation, cannot fit into a general explanatory framework.

Let us take the example of mechanics. Since the Newtonian synthesis between celestial mechanics and terrestrial mechanics, it can be said that the two main scientific texts on mechanics from the eighteenth century, J. L. Lagrange's *Mécanique Analytique* (1788) and P. S. Laplace's *Mécanique Celeste* (1799-1725), represent the highest expression of that theoretical movement introduced by Newton in his *Philosophiae Naturalis Principia Mathematica* (1687). With this in mind it could be said that a common point of view ought to be shared by Lagrange and Laplace concerning their approach towards dynamics and its analytical treatment. But we find in Laplace one hypothesis that runs through his *Mécanique Celeste*, and also through other fields, such as capillarity, heat or light, that could hardly be found in Lagrange's texts: it is the hypothesis establishing that these phenomena are the result of the action through distance of certain attractive and repulsive forces between molecules. This model of explanation is clearly expressed in his historical notice of the XII book of his *Mécanique Celeste*:

"Au moyen de ces suppositions, les phénomènes de l'expansion de la chaleur et des vibrations des gaz sont ramenés à des forces attractives et répulsives qui ne sont sensibles qu'à des distances imperceptibles. Dans ma théorie de l'action capillaire, j'ai ramené à semblables forces les effets de la capillarité. Tous les phénomènes terrestres dépendent de ce genre de forces comme les phénomènes célestes dépendent de la gravitation universelle. Leur considération me paraît devoir être maintenant le principal objet de la philosophie mathématique." (Laplace, 1799-1825, V 99)

With Lagrange, on the other hand, the analytical treatment of phenomena seems to go against any hypothesis concerning any physical approach for them. Lagrange's *Mécanique Analitique* is a text where the formal expression of the main concepts, and the role they play therein, make possible the wide scope of applications they have. A remarkable example is given by the principle of "virtual velocities", treated as a kind of axiom of mechanics, which states that a system of forces is in equilibrium if these forces are in an inverse ratio to their virtual speed. Lagrange's general formulation of this principle states

"Si un système quelconque de tant de corps ou points que l'on veut tirés, chacun par des puissances quelconques, est en équilibre, et qu'on donne à ce système un petit mouvement quelconque, en vertu duquel chaque point parcoure un espace infiniment petit qui exprimera sa vitesse virtuelle, la some des puissances, multipliées chacune par l'espace que le point où elle est appliquée parcourt suivant la direction de cette même puissance, sera toujours égale à zero, en regardant comme positifs les petits espaces parcourus dans le sens des puissances, et comme négatifs les espaces parcourus dans un sens opposé." (Lagrange 1788, 11-12)

This principle is immediately expressed through a differential form:

$$Pdp + Qdq + Rdr + \ldots = 0$$

where $P, Q, R, \ldots$ are forces acting on different bodies and $dp, dq, dr, \ldots$ are the differentials of the quantities $p, q, r, \ldots$ which represent the line distances from the bodies where the forces act, to their centers of mass.

The great advantage of this formal expression for the principle of virtual velocities, is that in this way it might be used to solve all the problems that might appear towards equilibrium of forces. In this sense the principle plays the role of principle of unification of the, at least, static of solid bodies and the static of fluids. This unification will make use of a formal calculus particularly well adapted for this purpose, the calculus of variations, that will make possible the reduction of mechanics to analysis, before making the reduction of analysis to algebra. This theoretical reduction is already announced in the preface of the *Mécanique Analytique*:

"On a déjà plusieurs Traités de Mécanique, mais le plan de celui-ci est entiérement neuf [...]. Les méthodes que j'y expose ne demandent ni constructions, ni raisonnements géométriques ou méchaniques, mais seulement des opérations algébriques, assujetties à une marche réguliére et uniforme." (Lagrange 1788, v-vi)

And he states also that

"Ceux qui aiment l'Analyse verront avec plaisir la Máchanique en devenir une nouvelle branche et ne sauront gré d'en avoir étendu ainsi le domaine." (Lagrange 1788, vi)

This way of understanding the analytic methods underlying mechanics as a sort of translation into algebraic means, could be identified with the synthesis which Descartes made between algebra and geometry.

At first glance it could be said that the analytical method can be declared as the inheritor of Cartesian thought: the subordination of geometry to algebra, a procedure well justified by *le Discours de la Méthode* and *la Géométrie*, states that the knowledge of geometric properties of bodies is obtained by an ascension in the order of magnitudes that, just as the order of reasons, follows a way moving from the complex to the simplest one. In this way algebra carries out the role of "reason" in its investigation of spatial "extension", and it also carries out a means of expression which, more than a mere description for phenomena, becomes the means for rational comprehension. By recognizing the origin of this tradition in

Descartes it is possible to say that the analytic methods all live in the theoretical frame of modernity created by him[1].

This treatment and diffusion of formal procedures within calculus, and therein through physical sciences, becomes an ideal which is more than a simple procedure to generalize certain properties; it is considered the most important means to propagate knowledge. A typical example for this is Condillac who conceived that "analytical" procedures were the most appropriate ones to guarantee an accord and fidelity with regards to the nature and methods of verification of ideas. But for the success of this project, a particular language able to transmit the research procedures as well as conceptual changes and transformations, was needed. This language is algebra, since, for Condillac, it is the only well-formed language where nothing is arbitrary (Dhombres 1982-1983).

We think that Laplace shares this point of view concerning the support that analytical-algebraic procedures give to the constitution of knowledge, as well. In his seventh lesson given at the *Ecole Normale*, and maybe because of the great influence that Condillac's thought had therein, Laplace states that

> "Pour bien connaître les propriétés des corps, on a d'abord fait abstraction de leurs propriétés, et l'on n'a vu en eux qu'une étendue figurée, mobile et impénétrable. On a fait encore abstraction de ces deux dernières propriétés générales en considérant l'étendue simple comme figurée. Les nombreux rapports qu'elle présente sous ce point de vue sont l'objet de la géométrie. Enfin, par une abstraction encore plus grande, on n'a envisagé dans l'étendue qu'une quantité susceptible d'accroissement et de diminution; c'est l'objet de la science des grandeurs en général, ou de l'arithmétique universelle, [...]. Ensuite on a restitué successivement aux corps les propriétés dont on les avait dépouillés; l'observation et l'expérience en ont fait connaître de nouvelles, et l'on a déterminé les nouveaux rapports qui naissaient de ces additions successives, en s'aidant toujours des rapports précédemment découverts. Ainsi, la mécanique, l'astronomie, l'optique, et généralement toutes les sciences qui s'appuient à la fois sur l'observation et le calcul, ont été créées et perfectionées. Vous voyes par là que ces sciences diverses s'enchaînent les unes aux autres, et qu'elles ont une source commune dans la science des grandeurs dont l'utile influence s'étend sur toute la philosophie naturelle. Cette méthode de décomposer les objets et de les recomposer pour en saisir parfaitement les rapports, se nomme analyse. L'esprit humain lui est redevable de tout ce qu'il sait avec précision sur la nature des choses."(Laplace LEN, 87)

So far we have talked only about those changes that took place within mechanics and its transformation into an analytical science, but what can be said about other *analytic sciences*? A quick look at Fourier's *Théorie Analytique de la Chaleur* shows clearly that the sense of what "analytic" means here—in what sense is the theory of heat an "analytic theory"—has partially changed. In Fourier's theory of heat there is no attempt to reduce the explanation of heat phenomena to algebraic deductions. Certainly the preface to the *Theorie Analytique* gives a clear idea of the role that mathematical analysis played in the general constitution of the theory, but it is also clear that in this treatise, mathematical analysis is by no means just a subset of algebraic methods. His approach to heat phenomena states that they are not reduced to mechanical theories, since they are not related with

the question of movement and of equilibrium of bodies, nor are they related with attractive or repulsive forces between bodies or molecules. This point of view, clearly different from "Laplacian molecularism" opens new horizons to mathematical physics: his main purpose is to give the mathematical description for the problem of diffusion of heat into a solid body, the question of transmission of heat from one body to another, the question of heat loss. In this sense the analytic questions to be solved are those of finding the correct expression of the "temperature function" v at each point of a solid body when a source of heat is applied at one point o of the body, the question of finding the heat flow after a time t, and the problem of the heat loss, after a time t, at each point of the body, when this source is no longer in contact and ceases its action over the body.

Considering that the value v of the temperature at each point of a body is given through a function $f(x, y, z, t)$ of the variables x, y, z which give the position of the point, and of the variable t which gives the time that a heat source has been in contact with one extreme of the body, the heat flow is given through the differential equation

$$\frac{dv}{dt} = \frac{K}{CD}\left(\frac{\partial^2 v}{\partial x^2} + \frac{\partial^2 v}{\partial y^2} + \frac{\partial^2 v}{\partial z^2}\right)$$

Where K gives the specific conductibility of heat of the body—the heat content transmitted through the body in a unity of time—C is the specific heat capacity—the necessary heat content needed to raise the temperature of a unity of the mass body from the temperature 0, the temperature of the melting ice, to temperature 1, the temperature of boiling water—and D is the density of the body. Now, considering that the heat flow is to be found using this equation, whose particular conditions justify the general solution given through a trigonometric (convergent) series, and considering Fourier's proof that not only this particular function of heat flow, but "any function" can be developed into a trigonometric series, it seems clear that the "mathematical analysis" working in this treatise is not to be identified with a branch of mathematics whose main advantage is its possible reduction to algebra. Already in the introduction to his *Théorie Analytique*, Fourier remarks that new methods, and not only "algebraic deductions", are needed in his treatise:

> "Les équations du mouvement de la chaleur, comme celles qui expriment les vibrations des corps sonores, ou les dernières oscillations des liquides, appartiennent à une des branches de la science du calcul les plus récemment découvertes, et qu'il importait beaucoup de perfectionner. Après avoir établi ces équations différentielles, il fallait en obtenir les intégrales; ce qui consiste à passer d'une expression commune, à une solution propre assujettie à toutes les conditions données. Cette recherche difficile exigeait une analyse spéciale, fondée sur des théorèmes nouveaux dont nous ne pourrions ici faire connaître l'objet. La méthode qui en dérive ne laisse rien de vague et d'indéterminé dans les solutions; elle les conduit jusqu'aux dernières applications numériques, condition

néccessaire de toute recherche, et sans laquelle on n'arriverait qu'à des transformations inutiles." (Fourier 1822, xij)

It seems clear to us that between the two analytic treatises by Lagrange and Fourier respectively, the meaning, the role and the scope of "mathematical analysis" have changed. This change is not only related to any particular style of mathematisation, but it concerns the mathematical theory that constitutes the base and the possibility for all those analytical projects. In other words, we think that there have been some transformations in mathematical analysis, just like there have been some transformations in physical sciences.

In this text we will analyze some of the changes that took place in mathematical analysis in the period between these two analytic treatises, Lagrange's *Mécanique Analytique* and Fourier's *Théorie Analytique de la Chaleur*. However, we have to point out that we will not refer to the "underlying mathematics" of these two treatises; we will not refer to the *Calculus of Variations* nor to *Fourier's Series* or *Fourier's Analysis*. The problem we want to analyze is rather that of the emergence of some concepts of mathematical analysis, mainly those of "continuity" (of functions) and of "convergence" (of series), which determined the development of this branch of mathematics during the nineteenth century. We think that it is the emergence of these concepts which makes possible the dissolution of a link between "algebra" and "analysis", a link that is conceived, and valued by Lagrange, as a relation of "subordination" of the latter to the former. After the dissolution of this particular link, a new shape was given to mathematical analysis, creating a new branch of mathematics, valued "in itself" by Fourier.

The emergence and use of those new concepts will be followed through the evolution of mathematical analysis and the theory of functions, and it could be said that after their appearance algebra itself will not be able to overlook the new "analytic methods"[2].

II The Algebraic Foundation of Mathematical Analysis

Regarding the main transformations within mathematical analysis, it seems that the first great change in the eighteenth century was introduced by Euler, who made the concept of "function" the central one. The reorganization given by his *Introductio in Analysin Infinitorum* (1748) introduced a new attitude towards the field of "quantities". Up to that moment mathematical analysis had been conceived as a kind of algebra of infinitely small or vanishing quantities, out of which the mechanical or geometrical problems could be solved. Euler's *Introductio* gives a new treatment for quantities—constant, variable, infinitely small or infinitely large—through those "calculus expressions" which are "functions". The field of quantities is conceived as being formed out of constant and variable quantities—

which are "like the gender or the species towards the individual" (*ibid.*, I, 4). With a variable quantity it is possible to define another variable quantity through "an analytical expression made out of this quantity and other constant quantities" (*ibid*). The variable quantity obtained by this procedure is a "function" of the first variable quantity, and functions are classified according to the analytical procedures used to define them. The *Introductio* is above all a treatise that intends to give a complete classification for functions, and through them a classification of curve lines. It is in this general scope that algebra becomes the privileged mean to express a function and to develop the theory of functions itself. This algebraic treatment of functions, that constitutes a new branch of mathematical analysis namely "algebraic analysis", became a necessary background that preceded infinitesimal calculus.

Now, even if the main trends for Euler's mathematical analysis are to be found in the algebraic treatment of functions, it must be pointed out that the algebraic form is above all the way through which a variable quantity is transformed in order to define a function, and so a function is more than just an equation through which an unknown quantity is to be found. As a variable quantity, a function runs through different values, depending on the values given to the variable quantity. A function Z of the variable z might be "algebraic" or "transcendent".

"The first ones are obtained through variable quantities that are combined among them by using only the common algebraic operations; the second ones depend on other operations [...]." (*ibid.*, I, 5-6)

Algebraic functions might be "irrational" or "rational", according to whether the variable z is submitted to root operations or is free of them. Another distinction between functions is given after the first one: "rational" functions are always "uniform"—only one value for the function is obtained for each value given to the variable quantity—while "irrational" functions are always "multiform"—many different values for Z might be obtained for each value given to the variable quantity. Now the way in which the quantity Z takes different values, as the variable z runs through different values, is given precisely through an algebraic, analytic, expression. If the algebraic expression is such that Z is a "multiform" function, it might happen not only that for some values of z, Z takes two or more different values, but also that for some values of z, Z might be no longer a real but an imaginary quantity. In this case, the way in which the quantity Z takes its corresponding different (possibly manifold) values might not follow a "continuous course" in the domain of quantities. But for uniform functions, Euler considers valid, because of its algebraic form, the following property: if a uniform function $Z(z)$ takes, for $z = a$, the value $Z = A$, and for $z = b$, the value $Z = B$, then while the variable quantity z runs through the values between a and b, the function Z must take, at least ones, each value between A and B. Euler's argument states that

"Since Z is a uniform function of z, for every real value of z, the function Z takes also a real value, and if the quantity Z, in the first case, when $z = a$, takes the value A, and in the second case, when $z = b$, the value B; then Z could not run from A to B without passing through all the intermediate values. Then if the equation $Z-A = 0$ and the equation $Z-B = 0$, have a real root, the equation $Z-C = 0$ will have also one whenever C lies between A and B." (*ibid.*, I, 20)

With this general property for uniform functions, Euler proves that for a uniform function Z of z, whose highest exponent is an odd number $2n+1$, the function Z has at least one real simple factor. In his proof, besides the intermediate value property, he uses a formal calculus for infinite quantities as if they were any real and finite quantities:

If the function Z is of the form

$$z^{2n+1} + pz^{2n} + qz^{2n-1} + rz^{2n-2} + \dots$$

when $z = \infty$, all the terms disappear in relation to the first one, and the function takes the form $Z = (\infty)^{2n+1} = \infty$; but when $z = -\infty$, the function takes the form $Z = (-\infty)^{2n+1} = -\infty$. Now for any real value C, since C lies between $-\infty$ and ∞, the theorem states that Z cannot run from $-\infty$ to ∞ without passing through C. That means that the equation $Z-C = 0$ has a real root. If $C = 0$ the conclusion is that the function Z has a real simple factor $(z-c)$, where c lies between $-\infty$ and ∞.

Before giving the intermediate value property, Euler stated the two following properties for an entire function:

1. The function given through an algebraic expression of the form

$$z^n + pz^{n-1} + qz^{n-2} + rz^{n-3} + \dots$$

is equal to the product of n simple (linear) factors[3].

2. The simple factors might be real or imaginary, but the imaginary simple factors are always in even number.

Clearly these two properties were sufficient to proof that an entire function whose highest exponent is an odd number has at least one real root, but as we have seen, Euler used the intermediate value property, as if some hidden reason, not explained in his *Introductio*, made the conclusion without this argument illegitimate.

Concerning properties 1 and 2, the first one is obtained directly from the statement that any equation of nth degree has n roots; the second one establishes that imaginary roots are always in even number. For the second property Euler gave no general proof, nevertheless he realized that in some sense this property was closely related with the fact that a polynomial is equal to the product of simple or double real factors. The only argument given by him to support this statement goes as follows: first he assures that if a function $f(x)$ has two simple imaginary factors, then the product of these two factors is a real double factor: without any hypothesis concerning the nature of imaginary roots[4], Euler states that if $P(x)$ denotes the product of the simple real factors of $f(x)$—and so $P(x)$ is real of degree $(n-2)$—, the product of the two imaginary factors is $\dfrac{f(x)}{P(x)}$ which is a real double factor. After this he assures that if a function is the product of four simple imaginary factors, then it can be given as the product of two double real factors; to prove this fact he takes as imaginary quantities those of the form $a + b\sqrt{-1}$. Once this property for functions that are the product of four imaginary factors is proven, Euler makes a generalization: for a function Z of the variable z it is always possible to combine in couples the imaginary factors to obtain a (double) real factor[5], For this argument Euler states simply that

"If there are only two imaginary factors, it is clear that their product will be real, and if there are four imaginary factors their product, as we have seen, can be given as the product of two double real factors of the form fz^2+gz+h. Even if the same proof is not valid for higher powers, it seems clear enough that this property holds for any number of factors, so that instead of $2n$ simple imaginary factors, there will be n double real factors. So any entire function of the variable z is equal to the product of simple or double real factors. If the truth of this proposition is not proved here completely, it will soon become stronger."[6] (*ibid.*, I, 19)

After this argument, which cannot be considered as a proof for the general case, Euler shows, using the two facts: that a polynomial of odd degree has at least a real root, and that those polynomials which are equal to the product of four imaginary factors are equal to the product of two double real factors, that the polynomials

$$a + bz^n + cz^{2n} + dz^{3n}$$

$$a + bz^n + cz^{2n} + dz^{3n} + ez^{4n}$$

$$a + bz^n + cz^{2n} + dz^{3n} + ez^{4n} + fz^{5n}$$

accept the same factorisation by real or double simple factors. These cases confirm the hypothesis that any entire function—any polynomial—is equal to the product of simple or double real factors[7].

"So if there were still some doubts concerning the factorisation of any entire function, they should vanish almost completely." (*ibid.*, I, 117)

In any case, as it will be clearly admitted by Lagrange (1798, note I, 111-113), the proof about the factorization of any polynomial in real factors, the hypothesis about the even number of the imaginary roots, and therefore the nature of the imaginary roots[8], is based on the property that any equation of an odd degree has a real root. Euler considered that the purely algebraic conclusion from the equality in number of roots and the degree of the equation, and of the fact that imaginary roots are always in an even number, could not be used as an argument to prove that an equation of odd degree has a real root. For Euler, the intermediate value property appears already as one which algebra could not ignore.

The intermediate value property which Euler proves for uniform-rational functions explicitly rests on the assumption that once the variable quantity Z has reached two different (real) values, it should run through all the values between them. Two facts of different kind are involved here; first the fact that Z is a uniform-rational function: because of the algebraic nature of the function Z—no roots for the variable z appear—while z runs through all the real values, Z takes only real values and no "jumps" might occur in this case, since the only possibility for a jump is when an irrational or multiform function takes imaginary values. Considering the general algebraic form for a multiform function Z of z:

$$Z^n + PZ^{n-1} + QZ^{n-2} + RZ^{n-3} + \ldots = 0 \tag{1}$$

where P, Q, R, ... are uniform functions of z, the different values of Z are given through the different n roots of the polynomial, but in this case each "root" of the equation is a function of z that might take only real values, or is a function that might take imaginary values for some values of z. Euler gives the example of a "biforme" function $Z^2-2PZ+Q = 0$ (where P and Q are uniform functions of z), where for each value of z the two values of Z are given, the first one by $Z_1(z) = P + \sqrt{P^2 - Q}$, and the second one by $Z_2(z) = P - \sqrt{P^2 - Q}$. So if the uniform function P is such that for every value of z $P^2 > Q$, the two values of Z are always real; but for those z where $P^2 < Q$, the values of Z will be imaginary. And he asserts, again from the intermediate value property, that when both conditions hold and there are some values of z such that $P^2 > Q$, and some other values of z such that $P^2 < Q$, then there must exist at least one value of z between them, such that $P^2 = Q$. In this case the two values of Z coincide and are given through the function P. From the algebraic theory of equations Euler assures that if n is an odd number, at least one of the root-functions is a uniform real function, and whenever a value of z gives an imaginary value for one of the root-functions, this same value of z will give imaginary values for at least another (always in even number) root-function. So if $Z(a) = A$ and $Z(b) = B$, but Z does not take the value C which lies between A and B, it is because while the variable z runs from a to b, Z takes imaginary values.

The second fact related with the intermediate value property deals with the nature of "variable quantities": since they are magnitudes which include all determined quantities, it is in their nature to take all values between two fixed ones. That is why in geometry a variable quantity is representated correctly by a straight line, and a function can be represented by a curved line: a line all of whose points take as abscissa a value of z, and as ordinate the corresponding value(s) of Z. The remarkable fact in Euler's geometric interpretation of functions (when for each value of z the corresponding value of Z is given, then by taking the first one as abscissa and the second as ordinate, a line is obtained) is that a (curved or straight) line is obtained here with "all" the points out of which it is assembled; this makes possible to study geometric curves independent of the idea of "mouvement" or "fluxion" of a point. With this approach even "mechanical curves" might be studied as formed by functions.

> "Even if we can describe mechanically many curve lines by the continuous mouvement of a point which presents the whole curve to our sight, we will consider them as obtained by functions. This approach is more analytic, more general and appropriate to calculation. In this way any function of z will give some straight or curve line and, conversely, any curve line will be related to a function." (Euler 1748, II, 6)

When the function Z is uniform, the curve representing it, will be produced continuously and indefinitely, and at any point of the horizontal axis representing the values of the variable z, a perpendicular line will cut the curve exactly at one point. When the function is multiform, and is given by a polynomial of the previous general form, the curve representing it might be intercepted by a perpendicular straight line in n, $n-2$, $n-4$, ... points; making certain that if n is an odd number, any perpendicular will intercept this curve at least once; but when n is an even number, it may happen that at some points of the horizontal axis a perpendicular line does not intercept the curve representing the function at all, making clear that the intermediate value property "might" fail in this case.

Euler is certain that the intermediate value property depends only on the algebraic nature of the function: if the property fails it is because function Z takes also imaginary values. Besides, "continuity" for functions and for "curves" is conceived by him as a property related with the permanence of the analytic expression: no matter how the curve that represents it looks like, a function (and the curve) is continuous whenever it is obtained through a single analytic expression. That means that "continuity" is a property that is ruled by "analysis"—through the analytic expression—and not by geometry[9]. For a multiform function, even if the curve related to it might be formed by different branches and the intermediate value property does not hold, it is considered by him as a continuous curve (generated by one analytic expression). On the other hand, "discontinuous" curves are for him "mixed" curve, obtained with two or more different functions.

For Euler the way in which a variable quantity runs between two fixed values needs no further description to guarantee the fact that it does it "continuously". Considered as a variable quantity, the variable z bears no "jump" nor any "gap" in the domain of real quantities; and the same happens with the function Z, as long as it remains in the domain of real quantities. For Euler there is no need to state that if the variable z runs "continuously", the analytic law which defines Z also makes it follow a "continuous" path through the values it takes[10].

The continuity of functions—in Euler's sense—is a question that cannot be generally answered just by stating that a function is a variable quantity obtained from another (variable) quantity through an analytic expression; the analytic form has to be given in such a way that the permanence of the analytic expression could be identified without any doubt. Considering the classification given by Euler at the beginning of his *Introductio*, it seems that for algebraic-rational functions there is no problem at all: the polynomial form becomes the mean to express them. For algebraic irrational functions and for transcendent functions the generalization from polynomials to infinite power series becomes a necessary step to be given. Through the infinite power series it might be said that the difference between algebraic and transcendent functions almost vanishes: the possibility to reduce those functions which require the transcendental operations (mainly the logarithmic and the exponential functions) to power series makes them appear as "continuous" (always in Euler's sense) functions, too.

After having analyzed some features of Euler's *continuity of functions*, let us look closely at the expression of a transcendent function in power series. Two general hypotheses concerning the nature of real quantities, are made to justify the development of the logarithmic function in power series: first a formal calculus for infinitely small and infinitely large quantities is used as a generalization of the calculus for finite quantities; secondly a general hypothesis about finite quantities: the assumption that they all can be obtained as the product of an infinitely small and an infinitely large quantity. For the calculation of the power series for the logarithmic function another main algebraic principle is used by Euler: Newton's binomial formula for the case where the exponent is any real quantity. This formula, admitted without proof[11], is here justified as the result of a formal procedure that is already valid in the case of a positive integer exponent.

The series for the logarithmic function will be calculated also by Lagrange and Cauchy, and we will analyze the solutions given by them as a paradigmatic example that will help us to better understand the changes that took place within algebraic analysis from Euler's *Introductio* to Cauchy's *Cours d'Analyse*.

Starting from an arbitrary quantity a and an infinitely small quantity ω, since a^{ω} is > 1 if $a > 1$, then $a^{\omega} = 1 + \psi$, where ψ is another infinitely small quantity. It is possible to write the last one as a function of the first one: $\psi = k\omega$ and $a^{\omega} = 1 + k\omega$. If L denotes the characteristic for logarithms of base a, then $\omega = L(1+k\omega)$ and $i\omega = L(1+k\omega)^i$, and since "it is clear that when the number i increases, the value $(1+k\omega)^i$ goes beyond the value of the unity (*ibid.*, I, 88)", then $(1+k\omega)^i = (1+x)$ and $i\omega = L(1+k\omega)^i = L(1+x)$. Starting from $(1+k\omega)^i = (1+x)$, Euler states $(1+k\omega) = (1+x)^{\frac{1}{i}}$, and then, by a simple algebraic substitution, $i\omega = \frac{i}{k}\left[(1+x)^{\frac{1}{i}} - 1\right]$. By developing the term inside the parenthesis through Newton's formula

$$L(1+x) = i\omega = \frac{i}{k}\left[(1+x)^{\frac{1}{i}} - 1\right] =$$

$$\frac{i}{k}\left[\left(1 + \frac{x}{i} - \frac{(i-1)x^2}{i\cdot 2i} + \frac{(i-1)(2i-1)x^3}{i\cdot 2i\cdot 3i} - \frac{(i-1)(2i-1)(3i-1)x^4}{i\cdot 2i\cdot 3i\cdot 4i} + \ldots\right) - 1\right].$$

When i becomes an infinitely large quantity, Euler establishes that a quotient of the form $\frac{ni-1}{(n+1)i}$, becomes equal to $\frac{n}{n+1}$ and so he finally gets

$$L(1+x) = \frac{1}{k}\left(x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4} + \ldots\right) \qquad (1)$$

From the equality $a^{\omega} = 1 + k\psi$ Euler gets $a^{i\omega} = (1+k\omega)^i$ for any value i; and from Newton's binomial formula this one is equal to

$$1 + ik\omega + \frac{i(i-1)}{2}k^2\omega^2 + \frac{i(i-1)(i-2)}{2\cdot 3}k^3\omega^3 + \frac{i(i-1)(i-2)(i-3)}{2\cdot 3\cdot 4}k^4\omega^4 + \ldots$$

Euler takes a finite number z and makes $i = \frac{z}{\omega}$; so that number i be infinitely large. From this $a^{i\omega} = (1+k\omega)^i = \left(1 + k\frac{z}{i}\right)^i$. And again from Newton's formula

$$\left(1 + k\frac{z}{i}\right)^i = 1 + kz + \frac{(i-1)}{2i}k^2z^2 +$$
$$\frac{(i-1)(i-2)}{2i\cdot 3i}k^3z^3 + \frac{(i-1)(i-2)(i-3)}{2i\cdot 3i\cdot 4i}k^4z^4 + \ldots$$

Since i is an infinitely large quantity, the value of a quotient $\frac{(i-n)}{(n+1)i}$ becomes equal to $\frac{1}{n+1}$, so the series takes the value

$$a^{i\omega} = 1 + kz + \frac{k^2z^2}{2} + \frac{k^3z^3}{2\cdot 3} + \frac{k^4z^4}{2\cdot 3\cdot 4} + \ldots$$

When $i\omega = 1$, the expression

$$a = 1 + k + \frac{k^2}{2} + \frac{k^3}{2\cdot 3} + \frac{k^4}{2\cdot 3\cdot 4} + \ldots \qquad (2)$$

gives the relation between the values a and k. With relation (2) it is possible to state that $a = e^k$, and so $k = \ln(a)$. The series expansion (1) for the logarithmic function is then equal to $L(1+x) = \frac{1}{\ln(a)}\left(x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4} + \ldots\right)$.

Euler's proof is a good example of what a formal procedure, given under the general frame of analytic thought, looks like: the series development for the logarithmic function is obtained through a purely algebraic calculus where the rules for the infinitely small and infinitely large quantities, and also a purely formal justification for Newton's formula, completely fill any conceptual gap that might appear.

Lagrange's point of view concerning algebraic analysis is close to Euler's ideas about the role that algebra has to play in the development of the theory of functions. The intermediate value property will play an important role in relation with the nature of the roots of algebraic equations; but it will play a central role also in the calculation of the remainder of an infinite series, out of which this series could be replaced by a finite polynomial. Besides these facts, the continuity property plays an important role in the proof of the binomial formula as a special case of the Taylor series.

In his *Discours sur l'objet de la théorie des fonctions* (Lagrange 1799), a short but deep manifesto for algebraic analysis, he states that the foundations of mathematical analysis are to be given by the new discipline defined through its relation with algebra: theory of functions. In a sense, Lagrange states that algebra is precisely a theory of functions, since those quantities algebra deals with appear as functions of other quantities. Through this theory of functions differential calculus becomes a particular branch that will no longer need to consider infinitely small quantities, vanishing quantities or fluxions: the methods introduced through the calculus of these infinitely small quantities just try to find out the first terms of the infinite power series development for the function.

> "Il est [...] plus naturel et plus simple de considérer immédiatement la formation des premiers termes du développement des fonctions, sans employer le circuit métaphysique des infiniments petits ou des limites; et c'est ramener le Calcul différentiel à une origine purement algébrique, que de le faire dépendre uniquement de ce développement." (*ibid.* 1799, 234)

This algebraic style rules not only over the power series development of functions, but introduces, above all, a "canonical form" that resumes in itself the reduction of mathematical analysis to algebra. In his *Théorie des Fonctions Analytiques*, all the possible applications of the analytic theory of functions are already contained in the canonical expression for a function given by its Taylor series: it is possible to proceed from the formal expression to the geometrical and mechanical domains. It is also through its formal nature that the theory of analytic functions includes all possible kinds of calculus; not only differential calculus, but also the calculus of variations, "this type of calculus which does not require a new analysis but only a special application of the theory of functions" (Lagrange 1797, 200-201).

Using this approach, Lagrange's theory of functions completes a theoretical program that includes mechanics and the calculus of variations as two moments to give a reduction of mechanics to a purely algebraic reasoning[12].

From the development in power series for the function $f(x+i)$: $f(x+i) = f(x) + ip(x) + i^2q(x) + i^3r(x) + \ldots$ [13], Lagrange obtains the canonical development given through the derived functions[14]

$$f(x+i) = f(x) + if'(x) + \frac{i^2}{2}f''(x) + \frac{i^3}{3\cdot 2}f'''(x) + \ldots \qquad (3)$$

It is through this canonical form that Lagrange calculates the series development for the binomial formula and for the exponential and the logarithmic function $L(1+x)$. In order to give a proof for the binomial formula, Lagrange tries to give the development of $(1+x)^m$ with a power series as an application of the canonical form for the power function $f(x) = x^m$, since then $f(x+i) = (x+i)^m$. In this way, "by the simple rules of arithmetic or the first operations of algebra" (*ibid.*, 15) it is possible to show that the first two terms of $(x+i)^m$ are $x^m + mix^{m-1}$, so clearly, by equating with the series (3), $f'(x) = mx^{m-1}$, and he obtains[15]:

$$(x+i)^m = x^m + imx^{m-1} + \frac{i^2}{2}m(m-1)x^{m-2} + \frac{i^3}{3\cdot 2}m(m-1)(m-2)x^{m-3} + ... \quad (4)$$

In this way Newton's binomial formula (4) is obtained through the "canonical form" for the power function. Once "the first operation of algebra" led him to the first derived function, the series (3) justifies all the rest; there is no need to fall back on the principles of differential calculus for a justification of this formula.

Lagrange considers that formula (4) is valid for every rational number m, but in order to consider it valid when the exponent is any real number, two implicit assumptions are made: first an assumption about the "dense" distribution of rational numbers, secondly the assumption that considering the exponent as a variable, the power function behaves as a continuous function[16]:

> "Comme tout nombre irrationnel peut être renfermé entre des limites rationnelles aussi resserrées que l'on veut, on en pourrait conclure tout de suite la vérité du résultat précédent pour une valeur quelconque irrationelle de m, puis qu'on peut, en resserrant les limites, diminuer l'erreur à volonté." (Lagrange 1806, 16)

Once this binomial formula is proved, the series for the exponential and the logarithmic functions can be obtained. For the function $f(x) = a^x$, $f(x+i) = a^{x+i} = a^x \cdot a^i$, the problem is now to find the first two terms of the series for a^i. By putting $a = 1+b$, and by the binomial series, Lagrange gets:

$$a^i = (1+b)^i = 1 + ib + \frac{i(i-1)}{2}b^2 + \frac{i(i-1)(i-2)}{2\cdot 3}b^3 + ...$$

So after developing the products and rearranging the series for the increasing powers of i, it is easy to see that

$$a^i = (1+b)^i = 1 + i\left(b - \frac{b^2}{2} + \frac{b^3}{3} - \frac{b^4}{4} + ...\right) + ...$$

With these two first terms, Lagrange states that $a^{x+i} = a^x \cdot a^i = a^x(1+iA+...)$, where $A = b - \frac{b^2}{2} + \frac{b^3}{3} - \frac{b^4}{4} + ...$, so by the development (3), he gets $f'(x) = Aa^x$,

and the algorithm to find the derived functions gives $f''(x) = A^2a^x$; $f'''(x) = A^3a^x$; with these functions, the complete series can be obtained:

$$f(x+i) = a^{x+i} = a^x\left(1 + Ai + A^2\frac{i^2}{2} + A^3\frac{i^3}{3.2} + ...\right)$$

From this equality, after dividing by a^x and changing i for x he obtains

$$a^x = 1 + Ax + A^2\frac{x^2}{2} + A^3\frac{x^3}{3.2} + ... \quad (5)$$

When $x = 1$, the value for a is given by $a = 1 + A + \frac{A^2}{2} + \frac{A^3}{3\cdot 2} + ...$

For the value $x = \frac{1}{A}$ $a^{\frac{1}{A}} = 1 + 1 + \frac{1}{2} + \frac{1}{3\cdot 2} + \frac{1}{4\cdot 3\cdot 2} + ...$ which is the number e; $a^{\frac{1}{A}} = e$ or $a = e^A$. Clearly $\frac{1}{A} = L(e)$ and $A = \ln(a)$; so $a = e^A = e^{\ln a}$. When $f(x) = e^x$ the series (5) gives $e^x = 1 + x + \frac{x^2}{2} + \frac{x^3}{3\cdot 2} + ..$ since in this case the value $A = \ln(e) = 1$.

By introducing now as a new function $f(x) = L(x)$, then $x = a^{f(x)}$, and $f(x+i) = L(x+i)$, so $x+i = a^{f(x+i)}$. Again, the series development is solved once the derived functions are found; in this case Lagrange finds[17] $f'(x) = \frac{1}{xA}$. By putting this last function in the form $f'(x) = \frac{1}{A}x^{-1}$, the algorithm already found for the derivation of a function of this form, gives $f''(x) = -\frac{1}{A}x^{-2} = -\frac{1}{Ax^2}$; $f'''(x) = \frac{2}{A}x^{-3} = \frac{2}{Ax^3}, ...$. The development (3) gives:

$$L(x+i) = L(x) + \frac{i}{Ax} - \frac{i^2}{2Ax^2} + \frac{i^3}{3Ax^3} - \ldots$$

By making $x = 1$ and putting x instead of i he finally gets:

$$L(1+x) = \frac{1}{A}\left(x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4} \ldots\right) = \frac{1}{\ln(a)}\left(x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4} \ldots\right)$$

since $A = \ln(a)$. This is the same series development already given by Euler.

With this series, Lagrange tries to find the logarithm of any real quantity y. By making $y = 1 + x$, the series for the logarithm gives

$$L(y) = \frac{1}{\ln(a)}\left((y-1) - \frac{(y-1)^2}{2} + \frac{(y-1)^3}{3} - \frac{(y-1)^4}{4} \ldots\right) \tag{6}$$

which is a convergent series only for those values of y "which are close to the unity" (Lagrange 1797, 20). So Lagrange now comes to the problem of finding the logarithm of any quantity y, even if it is not so close to the unity; that means, even if the series (6) does not converge. Since it is always possible to find another quantity r, big enough, so that $z = \sqrt[r]{y}$ is close to the unity, a new convergent series can be find to calculate the logarithm, no matter how big the quantity y might be. The series for this quantity z is

$$L(z) = \frac{1}{\ln(a)}\left((z-1) - \frac{(z-1)^2}{2} + \frac{(z-1)^3}{3} - \frac{(z-1)^4}{4} \ldots\right)$$

so that

$$L(z) = L(y)^{\frac{1}{r}} = \frac{L(y)}{r} = \frac{1}{\ln(a)}\left((z-1) - \frac{(z-1)^2}{2} + \frac{(z-1)^3}{3} - \frac{(z-1)^4}{4} \ldots\right)$$

and from this Lagrange gets

$$L(y) = \frac{r}{\ln(a)}\left(\left(\sqrt[r]{y}-1\right) - \frac{\left(\sqrt[r]{y}-1\right)^2}{2} + \frac{\left(\sqrt[r]{y}-1\right)^3}{3} - \frac{\left(\sqrt[r]{y}-1\right)^4}{4} \ldots\right) \tag{7}$$

Clearly Lagrange's aim is to make possible the transit from the formal expression of a series, obtained from the general development (3), to the numerical value of a function[18]. But the question raised goes farther and becomes a question about the series development (3). Since this series is obtained by substituting $(x+i)$ for x in $f(x)$, at each step a new function appears:

$$f(x+i) = f(x) + iP(x,i)$$
$$P(x,i) = p(x) + iQ(x,i)$$
$$Q(x,i) = q(x) + iR(x,i) \ldots$$

Each function iP, iQ, iR, ... is zero when $i = 0$, but when i is a very small quantity, these functions take also very small values. Already in the first step, when Lagrange affirms that if $i = 0$ then $f(x+i) = f(x)$, he suggests at the same time that when i is a very small quantity—the term "infinitely small quantity" has been explicitly proscribed from the *Théorie des Fonctions Analytiques*—the reminder iP becomes also a very small quantity and so is the difference between $f(x+i)$ and $f(x)$[19]. To make clear the behavior of these functions, Lagrange considers the curve whose abscissa is equal to i and whose ordinate is given by one of these functions. This curve has a continuous path, so:

> "[...] le course de la courbe s'approchera peu à peu de l'axe avant de le couper et s'en approchera, par conséquent, d'une quantité moindre qu'aucune quantité donnée, de sorte qu'on pourra toujours trouver une abscisse i correspondant à une ordonnée moindre qu'une quantité donnée, et alors toute valeur plus petite de i répondra aussi à des ordonnées moindres que la quantité donnée." (*ibid.*, 12)

This property is in fact a fundamental principle for the whole theory of functions, and it has been always assumed implicitly in the differential calculus and in the calculus of fluxions. With this property a bound for the reminder functions iP, iQ, iR, ... can be given so that more than their specific values, it is possible to have a clear idea of the error, when only a finite number of terms of series I are considered.

Series (3) gives the value of $f(x+i)$, in order to obtain a series development for $f(x)$, Lagrange takes $x-i$ in the place of x in (3) and he obtains

$$f(x) = f(x-i) + if'(x-i) + \frac{i^2}{2} f''(x-i) + \frac{i^3}{3 \cdot 2} f'''(x-i) + \ldots$$

and by making $xz = i$

$$f(x) = f(x-xz) + xzf'(x-xz) + \frac{x^2z^2}{2} f''(x-xz) + \frac{x^3z^3}{3 \cdot 2} f'''(x-xz) + \ldots \tag{8}$$

clearly if $z = 0$ this series reduces to the equality $f(x) = f(x)$, and for $z = 1$ it becomes[20]

$$f(x) = f(0) + xf'(0) + \frac{2}{2} f''(0) + \frac{x^3}{3 \cdot 2} f'''(0) + \ldots \quad . \tag{9}$$

Through this transformation Lagrange's aim is not only to give a series development for $f(x)$, but to obtain the value of $f(x)$ only with a finite number of terms of the series. The series (8) and (9) suggest that it is possible to obtain a value which will come closer and closer to $f(x)$ as more and more terms of the series are added; but the "meaning" of the equality sign in the series (3), (8) or (9) should be, Lagrange thinks, the same as in any equation where both terms are considered to represent exactly the same quantity—out of which the equality sign can be used to link them—, and so equations (3), (8) and (9) are exact only when "all" the terms of the series are really added. But to obtain the value of the function for a specific value of x, the quest for the reminder that could help to avoid the infinite series becomes necessary

> "Tant que ce développment ne sert qu'à la génération des fonctions dérivées, il est indifférent que la série aille à l'infini ou non; il est aussi lorsqu'on ne considère le dévéloppement que comme une simple transformation analytique de la fonction; mais, si on veut l'employer pour avoir la valeur de la fonction dans les cas particuliers, comme offrant une expression d'une forme plus simple à raison de la quantité *i* qui se trouve dégagée de dessous la fonction, alors, ne pouvant tenir compte que d'un certain nombre plus ou moins grand de terms, il est important d'avoir un moyen d'évaluer le reste de la série qu'on néglige, ou du moins de trouver des limites de l'erreur qu'on commet en négligeant ce reste." (Lagrange 1806)

Faced with this problem, Lagrange looks for the value of a "remainder" that helps to find the exact value for $f(x)$ with just a finite number of terms.

From the series development (8) it is possible to write

$$f(x) = f(x-xz) + xP(z)$$

Where $P(0) = 0$. In the case $z = 0$, the development reduces to the equality $f(x) = f(x)$. By deriving the two members of this equation with respect to the variable z, the following equality is obtained

$$f'(x-xz) = P'(z) \tag{10}$$

So the reminder P is obtained by looking for a function of the variable z whose derivative regarding this variable is equal to $f'(x-xz)$, and is such that $P(0) = 0$. Once this condition for the reminder P is given, and if $z = 1$, the equality

$$f(x) = f(0) + xP(1)$$

is obtained.

By following to the next term of the series (8) it is possible to write

$$f(x) = f(x-xz) + xzf'(x-xz) + x^2Q(z)$$

where $Q(0) = 0$. By repeating the process of derivation in both members of the equation, a value for Q' is obtained

$$Q' = zf''(x-xz) \tag{11}$$

Again, when $z = 1$ Lagrange obtains now

$$f(x) = f(0) + xf'(0) + x^2Q(1)$$

Repeating the process again for the expression

$$f(x) = f(x-xz) + xzf'(x-xz) + \frac{x^2z^2}{2} f''(x-xz) + x^3R(z)$$

the value for the reminder R is given through

$$R' = \frac{z^2}{2} f'''(x-xz) \tag{12}$$

and for $z = 1$, the value

$$f(x) = f(0) + xf'(0) + \frac{x^2}{2} f''(0) + x^3R(1)$$

Since the new functions $P(z)$, $Q(z)$, $R(z)$, ... are known through their derivatives—from the relations a, b, c, ...—Lagrange gives upper and lower bounds for them: by defining first a function $F(z)$ such that $F'(z) = z^m Z(z)$, where $Z(z)$ is another function such that $N \leq Z(z) \leq M$ when $a \leq z \leq b$, and if $f(z)$ is another function such that $f'(z) = z^m(M - Z)$, then $f(b) > f(a)$[21]. Since $f'(z) = z^m M - F'(z)$, then $f(z) = \frac{Mz^{m+1}}{m+1} - F(z)$ and the following inequality holds

$$\frac{Mb^{m+1}}{m+1} - F(b) = f(b) > f(a) = \frac{Ma^{m+1}}{m+1} - F(a)$$

from this inequality it is possible to write

$$F(b) < F(a) + \frac{M\left(b^{m+1} - a^{m+1}\right)}{m+1}$$

In a completely similar way, by taking now $f'(z) = z^m(Z - N)$, the following inequality is obtained

$$F(b) > F(a) + \frac{N\left(b^{m+1} - a^{m+1}\right)}{m+1}$$

giving finally

$$F(a) + \frac{N\left(b^{m+1} - a^{m+1}\right)}{m+1} < F(b) < F(a) + \frac{M\left(b^{m+1} - a^{m+1}\right)}{m+1} \tag{13}$$

This is applied to the functions $P(z)$, $Q(z)$, $R(z)$, ... First by assuming that $P = F(z)$, it follows that $P' = F'(z) = f'(x-xz)$, and since it has been assumed that $F'(z) = z^m Z(z)$, by making $m = 0$, then $Z(z) = f'(x-xz)$. Whenever $a = 0$ and $b = 1$, $P(0) = 0 = F(a)$ and $F(b) = P(1)$. In the case that $N \leq f'(x-xz) \leq M$ whenever $0 \leq z \leq 1$, it is possible to obtain from the inequality (13) the inequality: $N < F(b) = P(1) < M$.

In a similar way Lagrange obtains for the function $Q(z)$, by making $m = 1$, that if $N_1 \leq f''(x-xz) \leq M_1$, then $\frac{N_1}{2} < F(b) = Q(1) < \frac{M_1}{2}$. And for the function $R(z)$, by making $m = 2$, that if $N_2 \leq \frac{f'''(x-xz)}{2} \leq M_2$ then $\frac{N_2}{3} < F(b) = R(1) < \frac{M_2}{3}$.

If in the variable quantity $u = x-xz$, the variable z runs through the interval $[0,1]$, then u runs through $[0,x]$, Lagrange concludes then, with the help of the intermediate value theorem, that $N \leq f'(x-xz) = f'(u) \leq M$, and so any value between N and M can be given as $f''(u)$ for some u in $[0,x]$. So the value $P(1)$ takes this form. For the same reason there are values of u such that $Q(1) = \frac{1}{2}f''(u)$ and $R(1) = \frac{1}{2\cdot 3}f'''(u)$. From these facts his conclusion is the following theorem: "En désignant par u une quantité inconnue mais renfermée entre les limites 0 et x, on peut développer successivement toute fonction de x et d'autres quantités quelconques suivant les puissances de x de cette manière

$$f(x) = f(0) + xf'(u)$$

$$f(x) = f(0) + xf'(0) + \frac{x^2}{2}f''(u)$$

...

$$f(x) = f(0) + xf'(0) + \frac{x^2}{2}f''(0) + \frac{x^3}{3\cdot 2}f'''(u) + \ldots \qquad \text{(Lagrange 1797, 49)}$$

So the canonical form (3) and the formal series (9) might be replaced by a finite polynomial, but this substitution does not mean an "error" in the calculus of the value for $f(x+i)$ or $f(x)$.

Two levels in Lagrange's theory of functions become clear: first a purely formal representation for functions where the canonical form (3) carries the reduction of all the theory to the algebraic scope, as well as the application of this theory of functions to geometry and mechanics. The second level is given when the effective calculation is wanted; the fact that the "remainder" of the series exists and takes the general form $\frac{x^n f^n(u)}{n!}$, reduces the canonical form to a finite and effective process.

But as we have seen, Lagrange's arguments are strongly supported by a simple assumption that is hardly justified within the frame of this algebraic function theory: all functions are supposed to behave as "continuous" functions. This assumption was made in relation with the theorem concerning the possibility to bound the reminder functions iP, iQ, iR,... It was made also concerning the intermediate value property of the derived functions.

The property of continuity, treated up to now as an evident truth through a geometric image, is a main tool to justify the passage from the formal representation to the effective calculation. But the need for this property shows, as it was already the case with Euler's algebraic analysis, that a theory of functions can no longer ignore it. Even more, making now a deeper gap between the analytical ideal, identified as an algebraic foundation for function theory, and the means to carry out this ideal, the fundamental proposition for algebra is also involved and needs this continuity property.

The relation between the coefficients of an equation, the degree of this equation and the number of its roots, is a problem that goes back to the algebra of Cardano and Viète. In his *De aequationum recognitione et amendatione* (1615) Viete shows the possibility to write a general equation of third and fourth degree as a product of linear factors. Concerning the relation between the maximum number of roots for an equation and its degree, an important background is given in Girard's *Invention nouvelle en l'algèbre* (1629) and in Descartes' *Géométrie* (1637), related to fact that a polynomial of degree n has n roots and also that it can be divided by each one of the n linear factors formed by these roots.

So, behind the classical form for the fundamental theorem of algebra, stating that a polynomial of degree n with real coefficient has n roots, several problems are involved. Among them the most important are:

1. The problem related with "existence" of the roots for a polynomial.
2. The description of the "form" that these roots may have.
3. The "number" of roots that might exist for a polynomial.
4. To find the roots of the polynomial through the linear factors that divide this polynomial.

Historically the first problem to appear is the one related to the number of roots for an equation. This problem, treated in some sense by Girard and Descartes, also involves the fourth problem: if the nth degree polynomial $P(x)$ is equal to the product $(x-x_1)(x-x_2)...(x-x_n)$, it is because the polynomial admits as many roots as its degree. The equality $P(x)=\prod_{i=1}^{n}(x-x_i)$, states not only that we

can divide $P(x)$ by any of the factors $(x-x_i)$, and so each quantity x_i is a root for $P(x)$; but also that $P(x)$ can be divided by $(x-x_i)$ whenever x_i is a root[22].

Now, when the problem is not to "prove the existence" of the roots, but to justify the equality $P(x)=\prod_{i=1}^{n}(x-x_i)$, then it is necessary to establish which values make the equality possible. As we know, Descartes states that in order to assure that any polynomial $P(x)$ of degree n is equal to a product of linear factors, it might be necessary to "imagine" some of these quantities that make possible the factorization. All through 17th and 18th centuries, the controversy about the nature of these "imaginary quantities" was at the center of all the questions concerning algebraic equations and their roots, until D'Alembert's proof (1746), that these quantities can only have the form $x+y\sqrt{-1}$. An immediate consequence is then that the number of imaginary roots is even[23]. When the degree of the polynomial is odd, the only possibility to admit this fact, and the one stating that the number of roots is equal to the degree of the equation, is that in this case the equation must have at least one real root.

At the beginning of his *Traité de la résolution des equations numériques de tous les degrés*, Lagrange gave two theorems where "the foundation for the theory of equations is given" and for which the continuity becomes necessary:

> "Si l'on a une équation quelconque, et que l'on connaisse deux nombres tels qu'étant substitués seccessivement à la place de l'inconnue de cette équation, ils donnent des résultats des signes contraires, l'équation aura nécessairement au moins une racine rélle dont la valeur sera entre ces deux nombres.
>
> Si, dans une équation qui a une ou plusieurs racines réelles et inégales, on substitue successivement à la place de l'inconnue deux nombres, dont l'un soit plus grand et l'autre soit plus petit que l'une de ces racines, et qui différent en même temps l'un de l'autre d'une quantité moindre que la différence entre cette racine et chacune des autres racines réelles de l'équation, ces deux substitutions donneront nécessairement deux résultats de signes contraires." (Lagrange 1798, 6)

For the proof of the first theorem Lagrange proceeds as follows: if it is possible to write the equation $P(x)$ as the product of linear factors of the form $(x-\alpha_i)$, where a_i is a real or imaginary root, and if by substituting two values p and q in the place of x in the product $\prod_{i=1}^{n}(x-\alpha_i)$, $P(a)$ and $P(b)$ take different signs, then at least one of the factors $(x-\alpha_i)$ changes its sign when substituting x by a and b. But in the product $\prod_{i=1}^{n}(x-\alpha_i)$, whenever one of the roots α_i has the form $a+b\sqrt{-1}$, then another root α_j takes the form $a-b\sqrt{-1}$. Since the product of the two linear factors $\left(x-a-b\sqrt{-1}\right)\left(x-a+b\sqrt{-1}\right)$ is always positive for any value of x, if there is a change in the sign of $P(x)$, this change is produced in a linear factor $(x-\alpha_i)$ where α_i is real. But Lagrange recognizes that there is a circular argument: the theorem about the nature of the imaginary roots, and the form of the linear factors, depend in some way on the first theorem that was to be proven.

Because of this circular argument Lagrange uses a cinematic image which was also used in his lessons at the *Ecole Normale* (LEN), before becoming a "rigorous" proof given in the first note in his 1798 treatise on numerical equations. This new argument is considered a rigorous one since it follows "from the nature of the equation, independently of any of its properties" (Lagrange 1798, note I, 111): by dividing the equation into two parts P and Q, each one of them representing the sum of positive and negative terms, when the value of the variable x is augmented "by insensible degrees" the values P and Q also change by "insensible degrees". By doing this between two values of the variable x which give, the first one $P-Q < 0$, and the second one $P-Q > 0$, then between these two values there must exist at least one value that makes $P = Q$,

> "[...]comme deux mobiles qu'on suppose parcourir une même ligne dans le même sens, et qui, partant à la fois de deux points différents, arrivent en même temps à deux autres points, mais de manière que celui qui était d'abord en arrière se trouve ensuite plus avancé que l'autre, doivent nécessairement se rencontrer dans leur chemin." (*ibid.*, note I, 112)

The fact that a mechanical or geometrical image is used, shows that algebra is unable to introduce and give a theoretical place to this notion itself. This limitation will show exactly how mathematical analysis finds its own and specific scope. The revolution in mathematical analysis caused by Bolzano and Cauchy concerns the reorganization of mathematical analysis on the basis of those concepts that Lagrange already considered as necessary, but that were not clearly conceivable within the frame of a purely algebraic foundation for analysis: the concepts of "convergence" (of series) and of "continuity" (of functions). The introduction of these concepts will not only show a new stage for mathematical analysis, but also a new relation of analysis towards algebra.

III Convergence and Continuity as the Trends of the New Analysis

The introduction of a new concept in mathematics realizes the definition of a new kind of objects. In this case, the new changes in mathematical analysis at the turn of the nineteenth century could be characterized as the transformations that took place within the theory of functions when the new objects known as "continuous functions" and "convergent series" were introduced. To see how the introduction of these new concepts and objects gave a new structure to mathematical analysis, we will look closely at some aspects of the mutual relation between the already existing concepts and the new ones. If mathematical analysis reaches a new "modernity" with the concepts of "convergence" and "continuity", it is because it takes on a new structure once these concepts have been introduced.

The new structure given to mathematical analysis by these new concepts of continuity and convergence emerges from the fact that they introduce a new approach towards the domain of real quantities. The theory of curve lines, as given by Euler in the second part of his *Introductio*, assumes, as we have seen, that the course of values which the function runs through is, as well as the one which the variable runs through, a "continuous" path. This property was automatically assumed from the "analytic nature" (in Euler's terms) of the function; mechanical or geometrical curves could all be seen as the "graph" of an appropriate analytic function. In his theory of curve lines, and in the proof of the intermediate value property—a property which could be deduced from the algebraic nature of functions—there is no special approach towards the "values" that the function takes, as the variable runs through different values. The assumption that an extreme value could not be reached by a function without reaching before all the intermediate values, is enough to deduce the properties related with continuity. Contrary to this style, a new approach towards real quantities, considered as the main condition to articulate the new trends of mathematical analysis, is introduced by the works of Bolzano and Cauchy. Bolzano's *Rein Analytischer Beweis* (1817), and Cauchy's *Cours d'Analyse* (1821) state the basis of this new approach, and with this new approach they give a new sense to what the "analytical style" ought to be.

We think, for example, that the main point of the "purely analytical proof" for the intermediate value property, given by Bolzano, is the proof of the existence of a certain quantity: the "real root" of an equation that takes values of different sign. We want to underline that when Bolzano argues that a purely analytical proof for this theorem is needed, it is not because of some misleading fact about geometry or mechanics, but rather because they are unable to support an argument that is, or should be, a "fundamental" one. Geometry or mechanics could only support a plausible argument, whereas it is necessary to give a foundation for the "truth" of the proposition. In Bolzano's words a proof should not be only a "confirmation" but rather a "justification [*Begründungen*]" (Bolzano 1817, preface, 160). The property to be proved, equivalent to the fact that a function "never reaches a higher value without first going through all lower values" (*ibid.*, preface, 162), is a property of "continuous functions", even if it can be more immediately "seen" as a property of continuous curves. After the radical changes that Euler introduced, and that we have already analyzed, curve lines should be considered as emerging from functions and so the property has to be proved in the scope of (continuous) functions. Even more, since this property has always been admitted as an evident fact of "continuity", the concepts of "continuity" and of "continuous function" have never been explicitly given. Bolzano introduces the concept of continuous function; with this concept he introduces a new objet into mathematical analysis:

> "A function $f(x)$ varies according to the law of continuity for all values of x inside or outside certain limits [...] if [...] the difference $f(x+\omega)-f(x)$ can be made smaller than any given quantity provided ω can be taken as small as we please." (*ibid.*, preface, 162)

The property holding for algebraic equation, as describes by Euler or Lagrange, is a result of the following schema of argumentation; which is the correct way to prove that for any equation $P(x)$ taking values of different sign for two values a and b of the variable x, a real root exists:

> [1.] If two functions of the variable x, $f(x)$ and $g(x)$, vary according to the law of continuity either for *all* values of x or only for those which lie between α and β, and if $g(\alpha) > f(\alpha)$ and $f(\beta) > g(\beta)$, then there is always a certain value of x between α and β for which $f(x) = g(x)$. (ibid., §15, 177)
>
> [2.] Every function of the form
>
> $[P(x) =]\ a+bx^m+cx^n+\ldots+px^r$
>
> in which $m, n,\ldots, r$, designate whole positive exponents, varies according to the law of continuity for all values of x (ibid., §17, 180).
>
> [3.] If a function of the form
>
> $[P(x) =]\ x^n+ax^{n-1}+bx^{n-2}+\ldots+px+q$
>
> in which n denotes a whole positive number, is positive for $x = \alpha$ and negative for $x = \beta$, then the equation
>
> $x^n+ax^{n-1}+bx^{n-2}+\ldots+px+q = 0$
>
> has at least one real root lying between α and β. (ibid., §18, 181)

The purely analytical proof is based on the following auxiliary theorem, which states the existence of the least upper bound for an (upper) bounded set, and which also establishes the necessary relation between the property of "continuity" for function and the property of "continuity" for the domain of real quantities:

> "If a property M does not belong to *all* values of a variable x, but does belong to all values which are less than a certain u, then there is always a quantity U which is the greatest of those of which it can be asserted that all smaller x have property M." (*ibid.*, §12, 174)

By taking as M the property of all those values of x for which $f(x) < g(x)$ (if $\alpha < \beta$ and $f(\alpha) < g(\alpha)$), then for the quantity U, whose existence is guaranteed by the theorem, the continuity of the functions f and g will make $f(U) = g(U)$. For if $f(U) < g(U)$, since f and g are continuous functions, it could be possible to show the existence of a real quantity s, such that $f(U+s) < g(U+s)$, and so U would not meet the condition established by the theorem. By reasoning in a similar way, if

$f(U) > g(U)$ it would be possible to show that $f(U-s) > g(U-s)$ and the same conclusion is obtained about U.

The proof of the auxiliary theorem, which shows the "existence" of the quantity U, goes as follows: if the property M is satisfied for all the values $x < u$, but not for all the values of the variable x, then there exists one number $D > 0$ such that M is not satisfied for all $x < V = u+D$. By considering now the following sequence of values

$$\left\{V_n; V_n = u + \frac{D}{2^n}\right\},$$

with n an increasing number, and $V = V_0 > V_1 > V_2 > \ldots > V_n > \ldots$. Since M is not satisfied for every $x < V_0$, it is possible to ask if there is some V_n such that M holds for every $x < V_n$; if there is no such quantity V_n, then $U = u$ and the theorem is proved. But if there exists a number n such that the property M is satisfied for all $x < V_n$, but not for every $x < V_{n-1}$ (n is the first number with this property) the procedure starts again. Considering now the sequence

$$\left\{W_m; W_m = V_n + \frac{D}{2^{n+m}}\right\}$$

with an increasing number m, and $W_0 = V_{n-1}$—since $V_{n-1} = V_n + \frac{D}{2^n}$. Now M does not hold for every $x < W_0$. Since $W_0 > W_1 > \ldots W_m > \ldots > V_n$, if there is no integer number m such that M holds for every $x < W_m$, then $U = V_n$ and the theorem is proved; but if there is a number m with the desired property (and again it might be assumed that m is the first one), then M is satisfied for every $x < W_m$, but not for every $x < W_{m-1}$. In this case the procedure is repeated again. If it happens that after a finite number of steps the property M holds for every $x < Z_r = u + \frac{D}{2^n} + \frac{D}{2^{n+m}} + \ldots + \frac{D}{2^{n+m+\ldots+r}}$, but there is no positive integer number s such that M holds for every $x < u + \frac{D}{2^n} + \frac{D}{2^{n+m}} + \ldots + \frac{D}{2^{n+m+\ldots+r+s}}$, then $U = Z_r$. If, on the other hand, it is not possible to find such a value, then the sequence of values

$$u, u + \frac{D}{2^n}, u + \frac{D}{2^n} + \frac{D}{2^{n+m}}, \ldots, u + \frac{D}{2^n} + \frac{D}{2^{n+m}} + \frac{D}{2^{n+m+\ldots+r}}, \ldots$$

represents a sequence whose terms increase while the difference between two consecutive terms decreases in a reason that is less than a geometric progression. The quantity U is in this case the "limit" of this sequence. Bolzano assures that:

> "If a sequence of quantities
>
> $F_1(x), F_2(x), F_3(x), ..., F_n(x), ..., F_{n+r}(x), ...$
>
> has the property that the difference between its n-th term $F_n(x)$ and every later term $F_{n+r}(x)$, however far from the former, remains smaller than any given quantity if n has been taken large enough, then there is always a certain ***constant quantity***, and indeed only one, which the terms of the sequence approach, and to which they can come as close as desired if the sequence is continued far enough."[24] (*ibid.*, §7, 171)

Since the sequence u, V_n, W_m, ... Z_r,... has this property, the existence of the quantity U the limit of this sequence, is guaranteed by the last statement.

As we said before, a main point in Bolzano's argument is the proof of the existence of a certain quantity. The existence of the quantity U, which becomes the root for the equation, is given through the auxiliary theorem—stating the existence of the "least upper bound" for a bounded subset of numbers—, whose proof rests upon the convergence of a sequence having the so called Cauchy property[25]. For Bolzano a proof for this last property is possible, and his argument for the existence of a limit for a "Cauchy sequence" is that the assumption of the existence of such limit bears no contradiction:

> "The assumption of an *invariable* quantity with this property of proximity to the terms of our series is not impossible because with this assumption it is possible to determine the quantity as accurately as desired." (*ibid.*)

This means that the existence of the limit quantity can be asserted since its value can be approached as accurately as desired through the successive values of the sequence. The value of the limit of the sequence might not be known, but it is possible to approach this value through the sequence, and this possibility is the main reason to assure the existence of the limit. Otherwise, if there was no real quantity the sequence approaches, the terms of the sequence would not approach each other as they increase; "for anyone who has a correct concept of "quantity" the idea of this value is the idea of a real, *i.e.*, "actual", quantity". Clearly Bolzano's conclusion would not be valid if the domain of real quantities had a "gap"; but he considers that when a sequence behaves as the theorem says, then it is convergent, since for a non convergent sequence the "non approaching behavior" is essential. It is possible to accept the existence of a quantity, being the limit of the series, and then to consider this hypothesis among the rest of "truths" of analysis[26].

But if the existence of the limit of the sequence might be concluded, the property out of which this is deduced, the so called Cauchy property, is far from containing the existence of a limit. In other words, in a Kantian sense, the proposition "any sequence or series having the Cauchy property has a limit" is not "analytical" but "synthetical". Since in the statement "for any positive quantity, no matter how small this might be, there exists a positive number n such that the difference, in absolute value, between the term a_n and any other term a_{n+r} of the sequence, is smaller than the given quantity", there is nothing involving the "existence of a limit value"; and that is why *the existence of the limit must be proved.*

With regards to this point Cauchy's procedure is different. First he works with limits of functions: he proofs that if for the increasing values of the variable x, the difference $f(x+1)-f(x)$ "converges" to a limit k, then the function $\frac{f(x)}{x}$ converges to the same limit. In the proof of this statement, Cauchy makes clear the meaning of the sentence "the difference $f(x+1)-f(x)$ *converges* to a limit k":

> "On pourra donner au nombre h une valeur assez considérable pour que, x étant égale ou supérieur à h, la difference dont il s'agit soit constamment entre les limites $k-\varepsilon$ et $k+\varepsilon$. (si ε est un nombre possitif aussi petit que l'on voudra)." (Cauchy 1821, 54)

For any function, or any sequence which is to be considered as a function $f(1)$, $f(2)$, ... , it converges to a limit k if, given any positive value ε no matter how small it might be, there is a positive number h such that if $n > h$, the term $f(n)$ lies between the limits $k-\varepsilon$ and $k+\varepsilon$. After this explanation of the concept of convergence for sequences, Cauchy explains the convergence of series in detail: for a series $\sum a_i$, let $s_n = \sum_{i=1}^{n} a_i$ be the sum of the first n terms, if the terms of the form s_n form a convergent sequence whose limit is s, the series is convergent and its limit is s (and so it might be written $s = \sum_{i=1}^{\infty} a_i$). Now for a series to be convergent it is necessary that it satisfies the "Cauchy condition": for any positive quantity, no matter how small this might be, there exists a positive number n such that the sum of the terms $a_n+...+a_{n+r}$ of a series $\sum a_i$, is smaller than the given quantity. But for the converse property Cauchy simply states that "when this condition is filled, it can be assured the convergence of the series" (*ibid.*, 126). So he finally considers that concerning the question which was "proved" by Bolzano, really there is nothing to prove.

The relation between convergent sequences and series and continuous functions is a basic one, since whenever the variable quantity x has X as a limit, and $f(x)$ is a continuous function, $f(x)$ becomes a variable quantity whose limit is $f(X)$. That means:

$$\lim_{x \to X} f(x) = f(x) \tag{14}$$

With this basic relation Cauchy proves the intermediate value property: if the function $f(x)$ remains continuous between the two limits $x = x_0$, $x = X$, and if the two values $f(x_0)$ and $f(X)$ have different signs, then it is possible to find a solution for the equation $f(x) = 0$, at least with one real value of the variable x between x_0 and X.

If $x_0 < X$, $h = X - x_0$, and $m > 1$ is an integer number, since the two quantities $f(x_0)$ and $f(X)$ have different signs, it is possible to compare two consecutive terms of the sequence

$$f(x_0), f\left(x_0 + \frac{h}{m}\right), f\left(x_0 + 2\frac{h}{m}\right), \ldots, f\left(X - \frac{h}{m}\right), f(X)$$

and there must exist at least two consecutive terms $f(x_1)$ and $f(X')$ having different signs. Clearly $x_0 < x_1 < X' < X$, and $X' - x_1 = \frac{h}{m} = \frac{1}{m}(X - x_0)$.

Once these consecutive terms x_1 and X' have been found, it is possible to find two values between them, x_2 and X'', giving values $f(x_2)$ and $f(X'')$ of different signs, and holding the conditions $x_1 < x_2 < X'' < X'$, and

$$X'' - x_2 = \frac{1}{m}(X' - x_1) = \frac{1}{m^2}(X - x_0) \quad .$$

By continuing in this way two sequences are given: an increasing sequence of values

$$x_0, x_1, x_2, \ldots \tag{15}$$

and a decreasing sequence

$$X, X', X'', \ldots \tag{16}$$

The terms of sequence (16) are all greater than those of sequence (15), and the difference between two respective terms of these sequences decreases: $X - x_0 = h$,

$$X' - x_1 = \frac{h}{m}, X'' - x_2 = \frac{h}{m^2}.$$

It must be concluded that the terms of the sequences (15) and (16) will converge to a common limit a. Since $f(x)$ is a continuous function, the terms of the sequences

$$f(x_0), f(x_1), f(x_2), \ldots \textit{ and } f(X), f(X'), f(X''), \ldots$$

converge also towards the limit $f(a)$ which must be equal to zero.

Nevertheless, Cauchy states another relation between convergence and continuity. When speaking of a convergent series, since the partial sums $s_n = \sum_{i=1}^{n} a_i$ indefinitely approach a certain limit s, the difference between the limit s and the partial sum decreases as the number n increases. This difference, the "reminder" of the series, is a variable quantity whose limit is zero[27]. The fact that the terms of the series are constant or variable quantities does not change this property of the reminder: to be an infinitely small quantity. Now, when the terms of the convergent series are all continuous functions—each term is a function for which an infinitely small variation for the variable produces an infinitely small variation in the value of the function itself—the variations for the value of the limit function, when infinitely small variations takes place for the variable, are proportional to the variation for the reminder itself, but this last variation must be infinitely small since the reminder itself is already an infinitely small quantity. From this argument Cauchy concludes that:

> "Theorem I: Lorsque les differents terms de la serie sont des fonctions d'une meme variable x, continues par rapport à cette variable dans le voisinage d'une valeur particulière pour laquelle la série est convergente, la somme s de la série est aussi, dans le voisinage de cette valeur particulière, fonction continue de x." (*ibid.*, 131-132)

The conclusion is obtained by stating the properties of a "fixed" object, the limit of the series, from the behavior of a "mobile" object, the reminder of the series; but the properties that can be stated about the reminder are obtained from the existence of the limit: it is the existence of the limit which determines that the reminder must be an infinitely small quantity, and this property is enough, in Cauchy's view, to state that the limit function is continuous when the terms of the series are all continuous functions.

Many articles and texts have been written around this famous "wrong theorem" proved by Cauchy. Some of them have pointed out "why" it is a wrong statement (since Cauchy does not give the precise condition on the way in which the series converges; *i.e.* that the series should be a "uniformly convergent" series); others have tried to point out in which sense Cauchy's argument could be read as a correct statement. But very few have remarked on the "place" that this statement takes in the whole text of 1821: it is used to justify a crucial step in the proof of the binomial formula.

As we said before, the introduction of a new concept is not reduced to the statement of a new definition, the role of continuous functions does not stop with the intermediate value property or with the relation between continuity and convergence. Besides Newton's binomial formula, another outstanding and well-known statement gets a new foundation through the concept of a continuous function: the fundamental theorem of algebra (FTA). And it is precisely through these two propositions that it could hardly be said, that mathematical analysis gets its foundation through algebra. Contrary to this, it will be algebra—and precisely its fundamental theorem—which will find a new proof, and so a new foundation as Bolzano affirmed, through mathematical analysis.

Cauchy's proof of the binomial formula, and the development of the logarithmic series, are given in the scope of the solution of functional equations. The problem is to find the continuous functions that satisfy the following conditions:

1. $\phi(x+y) = \phi(x)\times\phi(y)$
2. $\phi(xy) = \phi(x)+\phi(y)$

The solutions given by Cauchy for these equations are:

1. $\phi(x) = A^x$, with A a positive constant value.
2. $\phi(x) = aL(x)$, with a a constant quantity and L the characteristic of the logarithmic function.

For the solution of these equations the assumption that they should be continuous functions is necessary. As to the first one, Cauchy remarks that the function takes only positive values: from the equality $\phi(x+y) = \phi(x)\cdot\phi(y)$, he gets $\phi(2x) = [\phi(x)]^2$; and by taking $\frac{1}{2}x$ in the place of x he gets now $\phi(x)=\left[\phi\left(\frac{1}{2}x\right)\right]^2$.

By taking a positive number α and a positive integer m, it follows from equation 1 that $\phi(m\alpha) = [\phi(\alpha)]^m$. If now $\beta=\frac{m}{n}\alpha$, from the two equalities $\phi(m\alpha) = [\phi(\alpha)]^m$ and $m\alpha = n\beta$, it follows $\phi(\beta)=\phi\left(\frac{m}{n}\alpha\right)=\left[\phi(\alpha)\right]^{\frac{m}{n}}$. By the "density" of the rational numbers, and from the property of continuity of the function ϕ, Cauchy gets finally $\phi(\mu\alpha)=\left[\phi(\alpha)\right]^{\mu}$. The case $-\alpha = 1$ gives $\phi(\mu)=\left[\phi(1)\right]^{\mu}$, and by taking the limit when $\mu\to 0$, $\phi(0) = 1$. From the initial condition it follows that $\phi(-\mu)=\dfrac{\phi(0)}{\phi(\mu)}=\left[\phi(1)\right]^{-\mu}$, which proofs that for any positive or negative value of the variable x, the equality $\phi(x)=\left[\phi(1)\right]^{x}$ holds. If $A = \phi(1)$, Cauchy gets the solution $\phi(x) = A^x$.

To find the proof of Newton's binomial formula, a problem related with the convergence of a series has to be solved: the only "legitimate" way to prove that the equality

$$(1+x)^{\mu} = 1+\mu x+\frac{\mu(\mu-1)}{2}x^2+\frac{\mu(\mu-1)(\mu-2)}{2\cdot 3}x^3+\ldots$$

holds (for every real number μ), is that the infinite series which is the right member of the equality—which is infinite unless μ represents a positive integer—"converges" to the value of the left member. The "root" tests for the convergence of series, when the terms of the series $\sum_{i=0}^{\infty}u_i$ are functions of the form $u_i(x) = a_i x^i$, takes the form:

Let A be the $\limsup_{n\to\infty}\sqrt[n]{a_n}$. The series converges for every value x between the limits $x=-\frac{1}{A}$ and $x=+\frac{1}{A}$; the series diverges for every x outside these limits (the value A defines the "radius of convergence" of the power series).

For power series, Cauchy proves also the algebraic closure related to the sum and the product: if the two series $\sum_{n=0}^{\infty}a_n x^n$, $\sum_{n=0}^{\infty}b_n x^n$ are convergent for some value of the variable x, and if their respective sums are s and s', the power series $\sum_{n=0}^{\infty}(a_n+b_n)x^n$ is also convergent and its sum is $s+s'$. Under the same conditions, if each one of the series is absolutely convergent, the series $\sum_{n=0}^{\infty}c_n x^n$, with $C_n = \sum_{k+l=n}a_k\cdot b_l$, is a new convergent series whose sum is ss'.

By taking as a general coefficients for the two series

$$a_n = \frac{\mu(\mu-1)(\mu-2)\ldots(\mu-n+1)}{n!} \text{ and } \quad b_n = \frac{\mu'(\mu'-1)(\mu'-2)\ldots(\mu'-n+1)}{n!} \qquad (17)$$

where m and m' are two arbitrary quantities, if $-1 < x < 1$, by the root test states they are ("absolutely") "convergent", and the general term of the "product series" is

$$c_n = \frac{(\mu+\mu')(\mu+\mu'-1)(\mu+\mu'-2)\dots(\mu+\mu'-n+1)}{n!} \tag{18}$$

Cauchy writes $\phi(\mu) = \sum_{n=0}^{\infty} a_n x^n$, and when the coefficients take the values given in (17), it satisfies the equality

$$\phi(\mu) = 1 + \mu x + \frac{\mu(\mu-1)}{2}x^2 + \frac{\mu(\mu-1)(\mu-2)}{2\cdot 3}x^3 + \dots \tag{19}$$

Now for the sum of the second series, Cauchy writes $\phi'(\mu) = \sum_{n=0}^{\infty} b_n x^n$ and it satisfies

$$\phi(\mu') = 1 + \mu' x + \frac{\mu'(\mu'-1)}{2}x^2 + \frac{\mu'(\mu'-1)(\mu'-2)}{2\cdot 3}x^3 + \dots \tag{19'}$$

Clearly $\phi(\mu+\mu') = \sum_{n=0}^{\infty} c_n x^n$, when the coefficients c_n take the form (3); in this way the function $f(m)$ satisfies the equation

$$\phi(\mu)\cdot\phi(\mu') = \phi(\mu+\mu') \tag{20}$$

From equation (19), and by taking $-1 < x < 1$, theorem I assures that $f(m)$ is a continuous function for the variable m that satisfies the functional equation (20) and so $\phi(\mu) = [\phi(1)]^{\mu} = (1+x)^{\mu}$. That means,

$$(1+x)^{\mu} = 1 + \mu x + \frac{\mu(\mu-1)}{2}x^2 + \frac{\mu(\mu-1)(\mu-2)}{2\cdot 3}x^3 + \dots\ , \tag{21}$$

whenever $-1 < x < 1$ for any real value of μ. Newton's binomial formula is completely proven.

As an immediate consequence of this formula, Cauchy gives the series developments for the exponential function e^x, for the natural logarithmic function $\ln(1+x)$

and for the logarithmic function of any base a, $L(1+x)$. First by putting in the equation (21) $\mu = \frac{1}{\alpha}$ and substituting x with αx then

$$(1+\alpha x)^{\frac{1}{\mu}} = 1 + x + \frac{(1-\alpha)}{2}x^2 + \frac{(1-\alpha)(1-2\alpha)}{2\cdot 3}x^3 + \dots$$

if $-1 < \alpha x < 1$—or $-\frac{1}{\alpha} < x < \frac{1}{\alpha}$. Taking the limit when $\alpha \to 0$ the series

$$\lim_{\alpha\to 0}(1+\alpha x)^{\frac{1}{\alpha}} = 1 + x + \frac{x^2}{2} + \frac{x^3}{3\cdot 2} + \dots$$

is convergent for $-\infty < x < \infty$. When $x = 1$, the series

$\lim_{\alpha\to 0}(1+\alpha)^{\frac{1}{\alpha}} = 1 + 1 + \frac{1}{2} + \frac{1}{3\cdot 2} + \dots$ defines the number e, and

$$e^x = \lim_{\alpha\to 0}(1+\alpha x)^{\frac{1}{\alpha}} = 1 + x + \frac{x^2}{2} + \frac{x^3}{3\cdot 2} + \dots \tag{22}$$

By subtracting 1 to each member of equation (21), and then dividing by μ and taking the limit when $\mu \to 0$, he gets

$$\lim_{\mu\to 0}\frac{(1+x)^{\mu}-1}{\mu} = x - \frac{x^2}{2} + \frac{x^3}{3} - \dots \tag{23}$$

and since $(1+x) = e^{l(1+x)}$,

$$(1+x)^{\mu} = e^{\mu l(1+x)} = 1 + \frac{\mu l(1+x)}{1} + \frac{\mu^2[l(1+x)]^2}{2} + \frac{\mu^3[l(1+x)]^3}{2\cdot 3} + \dots$$

and

$$\frac{(1+x)^{\mu}-1}{\mu} = \frac{l(1+x)}{1} + \frac{\mu[l(1+x)]^2}{2} + \frac{\mu^2[l(1+x)]^3}{2\cdot 3} + \dots \tag{24}$$

From (23) and (24) he gets

$$\lim_{\mu\to 0}\frac{(1+x)^{\mu}-1}{\mu}=l(1+x)=x-\frac{x^2}{2}+\frac{x^3}{3}-\ldots \tag{25}$$

whenever $-1 < x < 1$.

For the function $L(1+x)$—the logarithms of base a—Cauchy uses the well-known equality $\frac{L(1+x)}{L(a)}=\frac{l(1+x)}{l(a)}$ and from (22) it follows that

$$L(1+x)=\frac{1}{\ln(a)}\left[x-\frac{x^2}{2}+\frac{x^3}{3}-\ldots\right] \tag{26}$$

Two conditions play a fundamental role in the developments of these functions and also in the proof of Newton's formula: the series must converge, and the functions represented through the series are continuous—the function $\phi(\mu)=[\phi(1)]^{\mu}=(1+x)^{\mu}$ is a continuous function for the real variable μ because of theorem I, the "wrong theorem". Those theorems and series developments that were proved before by Euler and Lagrange are here submitted to these conditions; from now on, mathematical analysis and "analytical style" will be related with them. Mathematical analysis was a branch of mathematics that under the conceptual basis given by Euler, became mainly a theory of functions, and made the natural means to develop functions out of polynomials and infinite series. With Lagrange, the development of functions by a Taylor series achieved the reduction of theory of functions to algebra. In the new scope of mathematical analysis given by Bolzano and Cauchy, the concepts of continuity and convergence rule the extent of the "algebraic generalizations" —the possibility to develop a function through an infinite series is necessarily submitted to the fact that the variable of the function should vary within the radius of convergence of the series.

The proof given by Cauchy for the binomial formula states another feature for the new analytic style: it is possible to finish with the vicious circle—already detected by Euler—, between the binomial formula and Taylor's series for a function. In Lagrange's algebraic theory of functions, the binomial formula appeared as a particular case of the Taylor series for $f(x) = x^n$, although for the justification of the Taylor series development, a proof for the relation $f'(x) = nx^{n-1}$ is needed. This relation is proved precisely by using the binomial formula. For Cauchy two facts are clearly stated: the binomial formula is based on the principles of purely "algebraic analysis"—which in the tradition opened by Euler states that there is no need to call for any principle of differential or integral calculus—and, because of that, it needs no other justification than those coming from basic concepts of continuity and convergence, as we have already seen.

As for the upcoming relation between algebraic analysis and infinitesimal calculus, these concepts state how algebraic analysis should precede infinitesimal calculus: let us just point out that without them it is not possible to define the two main concepts of calculus: in Cauchy's lessons on infinitesimal calculus (1823), and since then, the derivative and the integral of a function are defined as a "limit" (of a quotient or a series). The "definite integral" for a function, with this definition, becomes independent of the derivative of a function. This makes possible and necessary the proof of the fundamental theorem of the calculus.

The core of Cauchy's analytical ideal, as given through his *Analyse Algébrique*, is not only to introduce the concepts that will give the new foundation to infinitesimal calculus. We think that Cauchy's aim is, contrary to Euler and Lagrange, to present algebra as founded by analysis. This aim is finally reached with his proof of the fundamental theorem of algebra (FTA):

> "Theorem 1. For any real or imaginary values for the constants $a_0, a_1, a_2, \ldots, a_{n-1}, a_n$, the equation
>
> $$a_0x^n+a_1x^{n-1}+\ldots+a_{n-1}x+a_n=0 \tag{27}$$
>
> where n is an integer positive number ≥ 1, has always real or imaginary roots."

With this general theorem the following ones are also given

> "Theorem 2. For any real or imaginary values for the constants $a_0, a_1, a_2, \ldots, a_{n-1}, a_n$, the polynomial
>
> $$a_0x^n+a_1x^{n-1}+\ldots+a_{n-1}x+a_n=f(x) \tag{28}$$
>
> is equal to the product of the constant a_0 and n linear factor of the form $x-\alpha-\beta\sqrt{-1}$."

> "Theorem 3. For any real or imaginary values for the constants $a_0, a_1, a_2, \ldots, a_{n-1}, a_n$, the equation
>
> $$a_0x^n+a_1x^{n-1}+\ldots+a_{n-1}x+a_n=0 \tag{29}$$
>
> has always n real or imaginary roots, and it could not have more." (*ibid.*, 343)

According to (28), $f(x)$ is a real or imaginary, but always "entire" function. With this notation, equation (27) states that $f(x) = 0$. By taking $x=u+v\sqrt{-1}$ and by substituting this value in $f(x)$, then $f\left(u+v\sqrt{-1}\right)=\phi(u,v)+\psi(u,v)\sqrt{-1}$, where now $\phi(u,v)$ and $\psi(u,v)$ are real functions of the real variables u and v. Under this new form equation (27) becomes $\phi(u,v)+\psi(u,v)\sqrt{-1}=0$; and this is satisfied only when the two equations

$$\begin{cases}\phi(u,v)=0\\ \psi(u,v)=0\end{cases} \tag{30}$$

are satisfied at the same time, or when the equation $F(u,v)=[\phi(u,v)]^2+[\psi(u,v)]^2=0$ holds. So the proof for FTA becomes the proof of the existence of two real values, u and v, that satisfy the equation $F(u,v) = 0$. Two main properties of the function $F(u,v)$ are obtained: first that this function is not bounded when one of the two values u, v increases more and more

> "La fonction $F(u,v)$ ne peut conserver une valeur finie qu'autant que les deux quantités u, v reçoivent elles-mêmes des valeurs de cette espèce, et devient infiniment grande dès que l'une des deux quantités croît indéfiniment." (Cauchy 1821, 334)

The second property for $F(u,v)$ is that it is also a continuous function of the variables u and v. Now, since $F(u,v) \geq 0$, the two properties for this function, being continuous and becoming infinite whenever u or v become infinite, allow Cauchy to conclude that the function reaches its lower limit with finite values of u and v.

> "$F(u,v)$, variant [avec les variables u,v] par degrés insensibles, et ne pouvant s'abaisser au-dessous de zéro, atteindra une ou plusieurs fois une certaine limite inferieure qu'elle ne dépassera jamais." (*ibid.*, 334-335)

By calling A this lower limit and (u_0,v_0) one couple of values such that $F(u_0,v_0) = A$, Cauchy proves that $A = 0$. Clearly the main point here is the statement that the lower limit A is reached by the continuous function $F(u,v)$—out of which the "existence" of the couple (u_0,v_0) is obtained, and by this the existence of the root of the equation. Once again, as it happened with Bolzano, the goal is the proof of the existence of a quantity (which now could be not only real but also imaginary), and this existence is obtained through a property that the function $F(u,v)$ should hold as a continuous function: this function reaches its lower bound since whenever u or $v \longrightarrow \infty$, $F(u,v) \longrightarrow \infty$[28].

At the end of seventeenth century *Mathematical Analysis* was not a well-recognized mathematical theory. Certainly a new approach towards quantities, requiring the study of entire and infinitely small quantities, became the main attribute of a new *style* of working the algebra of quantities; the need for this new algebra was already justified by the works of Descartes and Leibniz. But as we said before, at the beginning of nineteenth century *Mathematical Analysis* was considered the core of the mathematical expression of physical phenomena. As Fourier stated in

the Introduction to his *Théorie Analytique de la Chaleur*, it is not only a well-recognized mathematical theory, but even the "heart" of all mathematics; the great development of this theory in the nineteenth century in some sense confirmed Fourier's vision. But as we have seen, the methods, the content, and the concepts of Euler or Lagrange that articulate this theory are not the same as in Cauchy or Riemann. Certainly the development of mathematical analysis after Cauchy is not conceivable without the concepts of "continuity" and "convergence", even if wider classes of functions were discussed after Riemann—the class of "integrable functions" which includes "continuous functions" as a particular subclass, the class of measurable functions, the "Baire" functions, etc.

The birth of a new physics in the eighteenth century happened because of an "analytical ideal" that made possible their treatment out of the purely descriptive explanations. Now, it seems to us that the main consequence the "analytical ideal" had for mathematical analysis itself was precisely the need for the production of the concepts of continuity and convergence, that support the theoretical structure for the new analysis and their distinction from purely "algebraic generalizations".

Universidad Nacional Autonoma de México

Notes

* The author would like to thank the support of grant UNAM IN 401294

1 As J. Sebestik says "Since Descartes up to the beginning of 19th century, modern science has lived under the regime of analytical theories". (Sebestik 1992, 25).

2 In her profound work, Hourya Sinaceur (1991) points out the differences between Lagrange and Fourier, with regard to the question of the resolution of algebraic equations, starting from their different conceptions of what the analytic methods ought to be.

3 "The simple factors of an entire function Z of z are found by equating the function to zero and by looking for the roots of this equation; since they give one a simple factor for the function Z." (Euler 1748, 17)

4 In 1746 Jean le Rond d'Alembert (1746) proved that any imaginary quantity is of the form $a+b\sqrt{-1}$. In 1749 Euler gave a proof of the same fact in his "Recherches sur les racines imaginaires des équations" (1749), although he had presented a previous version of his memoir in 1746. Concerning this proof given by Euler and d'Alembert cf. Gilain (1991).

5 Clearly if the imaginary quantities are supposed to be complex quantities of the form $a+b\sqrt{-1}$, the conclusion comes out immediately: if $a+b\sqrt{-1}$ is a root of the equation, then $a-b\sqrt{-1}$ is also a root, and the product of the two imaginary factors $\left(a+b\sqrt{-1}\right)\left(a-b\sqrt{-1}\right)$ is a real double factor.

6 "Quod quamvis non summo rigore sit demonstratum, tamen eius veritas in sequentibus magis corroborabitur".

7 The proposition that any entire function is equal to the product of double or simple real factors implies both properties: that any equation of odd degree has a real root, and that imaginary roots are always "complex" quantities.

8 In chapter IX of his *Introductio* (1748, 108), Euler says that

> "It is sometimes difficult to find the imaginary factors [...] but if the nature of imaginary factors is such that the product of two of them is real, it is then possible to find all of them by looking for the double factors that are real, but whose simple factors are imaginary; since it is clear that ones we know all the double factors of the form $p-qz+rz^2$ included in the function $a+bz+gz^2+dz^3+\ldots$, we will have then all the imaginary factors".

9 This is one of the main differences between algebraic analysis in the scope of Euler's *Introductio* and that of Cauchy's *Cours d'Analyse*. Euler is certain that his definition of continuity is "analytic", and Cauchy thinks exactly the same about his definition.

10 This last condition towards the property of "continuity" of functions, which will be clearly given by Bolzano and Cauchy, cannot be stated in the algebraic frame for mathematical analysis given by Euler.

11 Euler's attempts to prove Newton's formula in the case of a non integer exponent are given later. Cf. Dhombres (1987).

12 As is clearly stated by Amy Dahan (1992, 186):

> "Ce que Lagrange veut accomplir dans la *Mécanique Analytique* [...] c'est un mouvement de double réduction: de la mécanique à l'analyse et de l'analyse à l'algèbre. Si la première partie du programme y est réalisée grâce au calcul des variations, la deuxième réduction est à l'œuvre dans la *Théorie des Fonctions Analytiques*".

13 Obtained, as it is well known, from the idea that when substituting the variable x for the variable $x+i$, $f(x+i)$ takes the place of $f(x)$, with the obvious condition that they must be equal whenever $i=0$. In the expression for $f(x+i)$, it should be possible to separate those terms that do not depend on i, from those that are equal to zero when $i=0$. That means that it is possible to write $f(x+i)=f(x)+iP$, where $P=P(x,i)$ is an expression depending on both x and i. By repeating his reasoning Lagrange states that also for the function $P(x,i)$ it is possible to separate that part which depends only on the variable x from another part which also depends on i and must be equal to zero when $i=0$, that means $P(x,i)=p(x)+iQ$, so $f(x+i)=f(x)+ip(x)+i^2Q$. Continuing in this way a development of the form $f(x+i)=f(x)+ip(x)+i^2q(x)+i^3r(x)+\ldots$ is obtained.

14 Where each "derived function" $f'(x), f''(x), \ldots$ is obtained from the previous one and coincides with a differential quotient: $f'(x)=\dfrac{df(x)}{dx}$, $f''(x)=\dfrac{df'(x)}{dx}$.

15 Clearly Lagrange takes for granted that if $f'(x)=mx^{m-1}$, when $f(x)=x^m$, then the algorithm will give for the second derived function $f''(x)=m(m-1)x^{m-2}$; for the third derived function $f'''(x)=m(m-1)(m-2)x^{m-3}$, and so on.

16 Two assumptions that become explicit and clear in Cauchy's proof for Newton's binomial formula.

17 If $x+i=a^{f(x)+o}=a^{f(x)}\cdot a^o$, by writing $o=if'(x)+\dfrac{i^2}{2}f''(x)+\dfrac{i^3}{3\cdot 2}f'''(x)+\ldots$ and substituting this value in (5), $x+i=a^{f(x)}\cdot a^o=a^{f(x)}\left(1+Ao+A^2\dfrac{o^2}{2}+A^3\dfrac{o^3}{3.2}+\ldots\right)$. Dividing by x he gets $\dfrac{i}{x}=Ao+A^2\dfrac{o^2}{2}+\dfrac{o^3}{3\cdot 2}+\ldots$. Dividing then by i, replacing then the value of o, and rearranging according to the increasing powers of i, leads to the expression:

$$\frac{1}{x}=Af'(x)+\frac{i}{2}\left[Af''(x)+A^2f'^2(x)\right]+\ldots$$

All the terms that are multiplied by i disappear, since i is an indeterminate value which does not appear in the quotient $\dfrac{1}{x}$, and so $\dfrac{1}{x}=Af'(x)$.

18 In his *Leçons sur le Calcul des Fonctions*, he goes one step further and states that no matter how big the number y might be, a number r can be found so that the value of $L(y)$ lies between two values:

$$\frac{r}{ln(a)}\left(1-\frac{1}{\sqrt[r]{y}}\right)<L(y)<\frac{r}{ln(a)}\left(\sqrt[r]{y}-1\right).$$

19 This fact would give the prove of the continuity of $f(x)$.

20 This development takes the form $f(x)=A+Bx+Cx^2+Dx^3+\ldots$ already known from the general theory of equations and, given in particular by Euler. Lagrange says that on the basis of the theory of derived functions from the development $f(x)=A+Bx+Cx^2+Dx^3+\ldots$ it is easy to say that $f(0)=A$, $f'(0)=B$, $f''(0)=2C, \ldots$

21 Lagrange uses the theorem as a main tool stating that

> "Si une fonction prime de x telle que $f'(x)$ est toujours positive pour toutes les valeurs de x depuis $x=a$ jusqu'à $x=b$, b étant $>a$, la différence des fonctions primitives qui répodent à ces deux valeurs de x, savoir $f(b)-f(a)$, sera nécessairement une quantité positive." (Lagrange 1797, 45)

(This theorem says that a function $f(x)$ such that $f'(x)>0$ is always increasing).

22 This problem, the converse of the first one, is treated by Cauchy in relation with the "interpolation" problem, the problem to determine completely an entire function once a certain numbers of values are given.

23 Since whenever $x+y\sqrt{-1}$ is a root of an equation, then so does the quantity $x-y\sqrt{-1}$.

24 This theorems affirms that a sequence of numbers having the so called "Cauchy property" is convergent.

25 These two propositions are, as it is well known, equivalent and they both characterize the continuity property for the set of real numbers.

26 Here we agree with Philip Kitcher (1975) when he assures that for Bolzano the hypothesis stating the existence of the limit for a Cauchy sequence is completely compatible with the "fundamental laws" of analytical quantities.

27 "An infinitely small quantity", according to the sense given to this notion in his *Cours d'Analyse*.

28 The only possibility that the continuous function $F(u,v)$ not reach its lower limit would be that this lower limit be reached "at infinity", *i.e.*, that whenever u or $v\to\infty$, $F(u,v)\to A$.

JEAN DHOMBRES

THE ANALYSIS OF THE SYNTHESIS OF THE ANALYSIS... TWO MOMENTS OF A CHIASMUS: VIÈTE AND FOURIER

I Introduction

Old as it is, the debate over analysis versus synthesis is not a foundational one in mathematics. By indistinctly referring to Plato and Theon or more precisely to book VII of the *Mathematical Collections* of Pappus—a text dating from the 4th century AD—most commentators assign a secondary position to the debate, even if they only do so in a rhetorical way[1]. Such a position mainly proves that the conscious surge of analysis, either as a rival to synthesis or a complement to it, is first of all a criticism of mathematical reasoning and its practice. In other words, it is as a historical move that the couple analysis/synthesis finds its way in epistemology and no further explanation is necessary. Yet very little would have been said, had we not simultaneously stated the strong evolution through centuries of the very acceptance of the two words. They even switch their parts, in a similar fashion to mask-plays in Elizabethan theatre. Paradoxically, in the same way as in this theatre Oberon acts in a timeless world, assigning the debate there is a risk of putting aside time. And therefore there is a risk of excluding history under the pretext that the opposition analysis/synthesis would just be a form taken by the eternal problem of what logically comes first and what comes second, but could arguably come first as well. Unfortunately this circuit is made all too easily by restricting this opposition to a philosophical one between induction and deduction, or even between empiricism and rationalism. The timeless nature of this opposition may therefore be due to the intellectual question of equivalences or, to use a less anachronistic expression, to the mathematical back and forth motion[2]. If this motion will be my principal object here, I do not wish to forget its historical insertion, precisely in order to reach its scientific meaning.

At least one should easily recognise, like Titiana under the influence of the philtre that generated the transformations, that the opposition between analysis and synthesis also depends on the tradition of teaching mathematics. Therefore, it depends on the way mathematics takes its grasp on societies, each one organizing the transmission of knowledge in its own way and therefore according a meaningful logic to the teaching of a science for which an added value is provided for what

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 147–176.

could remain a pure technique (as was, for example, the case in classical Chinese culture). Is not mathematics the oldest object of teaching in the Western world? From Boethius proposing the first book of Euclid's *Elements* as a model for school exercises to Antoine Arnauld's ruling through a *Géométrie* the *Petites Écoles* of Port-Royal[3]; from the Jesuit fathers' great expectations for the exemplary Collegio Romano[4] to the enthusiastic adepts of modern mathematics during the sixties[5] of our century, how many personalities have neglected mathematics for the sole benefit of its presupposed effects? If didactics at a given period is scarcely read as serving the description and the structure of a science, it unavoidably serves a culture. Then it makes history run. And, as a consequence, looking for history in our search concerning the analysis/synthesis debate, we may be tempted to restrict ourselves to text-books and to teaching methods. When a study of analysis and synthesis is intended to be historical, not one but many projectors must be used in order for it to be efficient; many questions have then to be selected and pursued. It may even form a structure. Then one must be aware that this structural multiplicity *ipso facto* overthrows the historical localization; each cause having its own particular historical rhythm. The teaching of mathematics does not have the same historical rhythm as mathematics! This is the reason why I decided to reduce observations strictly to two mathematical texts only.

Indeed, I do think that historians of mathematics—and sometimes mathematicians may play that role—contributed more to keeping alive the opposition between analysis and synthesis than to the individual meanings successively attributed to the two terms. It could be more interesting to shed light on the stability of the opposition built by an "historical" line of thought than to follow the commentaries of mathematicians themselves or of philosophers. One way would be to deconstruct some classical histories of mathematics. We only quote certain names to recall a long line of thought; Etienne Montucla, Abraham Gotthelf Kästner, Charles Bossut, Maximilien Marie, Moritz Cantor or Gino Loria, etc.[6] We do not intend to proceed in this analytical manner through historiography here, but at least we may recognize that the mobility of meanings of the two terms in the analysis/synthesis couple is the other side of the historical stability of the opposition. The paradox does not lie in the fact that the "mathematical" back and forth motion generates a "historical" back and forth explanation in mathematics, but that in the long term only one antagonistic couple was fixed by historians. I would like to argue that this perennial opposition finds its mathematical value via the inversions it generates. As this is the value I am looking for, the times of inversion must be privileged.

In spite of the different meanings, determinations and causalities linked with various historical and social contexts, and transient as it may be, the pure epistemological question of analysis and synthesis does not lose any of its dialectical interest. It can easily be seen in a universal way, with many historical concretizations. Without yielding to a facile *mise en abîme*—the analysis of the synthesis of the analysis...—we may suppose some depth to the couple in its game of transformations. And hence in its efficiency as a representation. The philosopher Maurice Blondel, who remarkably perceived the general role played by analysis and synthesis in the sciences—an abstract generality and a linking by way of necessity in one instance and for the other one a synthetic and quantitative individualized intuition—explains that this duality cannot be solved. At least, it cannot be solved through the sciences alone:

> "In their continuous work of integration, [the sciences] constantly appeal to a synthetic process; it is the only one able to provide a material which could be said to be a formal one. But even this initiative of the thought escapes the sciences; they are alien to themselves [...]. As for what they know, they do not *know* it the way they know *it*."[7] (Blondel 1893, 61)

By deciding to illuminate some moments precisely where meanings turn up, that is when analysis becomes synthesis and when synthesis constructs analysis as well, we try to specify the back and forth motion of mathematics; we reach the crossings of what we metaphorically call a chiasmus. Thus we may localize the strong thought of Maurice Blondel in order to show it is just an artefact.

In order to act on the analysis/synthesis opposition within the conditions of a historical view I tried to circumscribe in the preamble, my display of the moments of a chiasmus requires a temporal determination of at least two periods. But two moments already require a lot. Thus, I will speak of the end of the 16th century using François Viète's work, and of the early 19th century using Joseph Fourier's contribution. Two names, but as already stated two texts only and each treating quite different subjects: we look at a style and at a method, and less at specific objects. In order to examine two cases when analysis and synthesis exchange their meanings, the comparison is none too pleasant, as two different languages are at work. There is the pompous Latin of a Renaissance already influenced by the baroque, and there is the severe French style of mathematical physics looking for a style somewhere between the analytical description derived from the Enlightenment and the rigorous style of convergent series of the positivist period. We have to win over the heterogeneity of the two texts in order to build a meaning: its validity and its soundness should be measured by a critical appraisal which may give back their own fragrances to the two periods.

II Viète or Analysis Seen as an Appeal for a Constructive Synthesis

In a printed text of 1593, Viète works out the sum of all terms of an infinite geometric progression (1593, ch. XVII). Even though it is the first occurrence of such a formula, Viète wishes his explanation to be a very short one:

"The whole science of geometric progression almost reduces to one theorem only, for which four relations among the datas are naturally deduced." (*ibid.*, 28)

He then abruptly asserts:

"When magnitudes are in a continued proportion, the largest term of the ratio is to the smallest as the sum of all terms is to this sum to which the largest term has been subtracted." [8] (*ibid.*)

A proposition which, as Viète is its author, we immediately have to try to read using notations. By setting a first term as D, which is necessarily "the largest" of the progression[9], then its second term B, and the sum F, we write

$$\frac{F}{F-D}=\frac{D}{B}$$

It therefore comes as a surprise that in the specificative transcription of the theorem in letters, Viète introduces a supplementary notation, some X which is a somewhat restive "smallest term" of the progression as a whole. Its presence has the advantage to build a well-balanced proportion which can be visualized in a modern way by a formula and was appreciated by Viète's contemporary readers from the rhetorical expression:

$$\frac{F-X}{F-D}=\frac{D}{B}$$

A quite simple interpretation can be given, at least if we restrict ourselves to a progression with only a finite number of terms. In fact, in more modern terms, choosing an integer n (≥ 1) and letting the general term be $x_n = x_1 r^{n-1}$ (D then corresponds to $n = 1$, or to x_1, and B to x_2) the sum $F_n = \sum_{k=1}^{k=n} x_k$ for a geometric progression of ratio $r = \frac{x_1}{x_2}$ (in the modern sense) can be written as[10]

$$\frac{F_n - x_n}{F_n - x_1} = \frac{x_1}{x_2}$$

And it is easy to go to infinity by replacing F_n by F and therefore x_n by x_∞:

$$\frac{F - x_\infty}{F - x_1} = \frac{x_1}{x_2}$$

What is simple for us was as simple to Viète's readers in their time because they had read Euclid[11]. In this respect, the emphasis of this author writing the "smallest term" is surprising. In other words, it solicits some reflexion, for as he does not even provide proof of the theorem–in the expression of which we have to recall that the term X does not appear. Here lies our major observation. From a literary form to a literal one, something more is made apparent which is something less in terms of mathematical efficiency.

Unfolding a beautiful analytical process, Viète deduces some other formulations in his form using X, admitted for the duly accepted theorem. Precisely four formulations as there are four quantities being displayed, F, D, B and X. The last X from which we cannot escape is set up at the same level as the others. Four ways of expressing any one of the quantities in terms of the three other quantities. It is a display of analysis first referred to by means of a classification but Viète explicitely refers to analysis at the end: "*Vt hæc in Analyticis abunde demonstrata, & exemplificata sunt*"[12] (*ibid.*, 29). He organizes his material according to an algebraical script[13] and, moreover, he introduces the required formula by the word "δεδόμενον" each time. In the literary play of Renaissance texts, this is an allusion to Euclid's *Data* (Δεδόμενα); a typical text of analysis, for which some elements of a drawing are determined from other elements which are postulated as given. In short, Viète clearly proclaims analysis, and for our purpose we have no need to examine it in more detail.

The text does not stop here. Surprisingly—and the effect is deliberate—here there is a question in Viète's exposition: "Shouldn't we say that X will go down to nothing when magnitudes are in a continued proportion to infinity" (*ibid.*). If this is the first time that infinity is mentioned in the text, it was present ineluctably from the early lines. It was hidden in the literary expression used for the theorem: as it only mentions three things, the theorem cannot make any sense to any reader if conceived for a progression with a finite number of terms[14]. On the opposite side, using the game played by X from which infinity is revealed ("smallest term"), the literal transcription makes sense in both finite and infinite cases. Finally, with the notation X, a name is given to what provides an additional meaning to the literary form of the theorem. Then, abruptly, there is a change in the stylistic register of Viète's text. An opinion is given, as in any good scholastic text: "And Mechanists[15] will assure us that it vanishes as the smallest quantity subsides in the intellect only" (*ibid.*). In short, the reader is aware of what is suggested. In its literary form, the theorem sounds true for the reason that is suffices to make the smallest term of the literal form equal to zero. A form which can be said to be the indefinite writing of the sum of a geometric progression (n as a integer, the number of terms, is not specified and might as well be infinite). Isn't this the added value of algebra?

Viète's analysis could therefore end here with only the well regulated game of a computation: reduce to 0 an infinitely small quantity and obtain a formula quite close to the one we usually adopt when we reach for the sum of a convergent geometric progression,[16]

$$\frac{x_1}{F} = \frac{x_1 - x_2}{x_1}$$

The scholastic parenthesis might then just have been a stylistic effect. And analysis will have remained the main tool.

Indeed, the text proceeds further and from now on analysis recedes to give place to synthesis. A synthesis in the sense that there is a construction which answers the question: shouldn't we say that... The question is really about the maintenance of analysis. Synthesis symptomatically begins by a definition; in this case an original definition of an increment (*cremento*): "what the difference of [any] term of the ratio is to the [immediately] inferior term of the ratio, the smallest [magnitude] is to the increment" (*ibid.*, 29). For a progression with a finite number of terms, the increment Δ possesses a unequivocal definition $\frac{x_1 - x_2}{x_2} = \frac{x_n}{\Delta}$. But it obviously depends on the integer n, a parameter in a way too talkative in the literal form, and excluded by the literary one. We could better denote Δ_n, and write F_n as well, for the finite sum with n terms. In the case of an infinite progression, the definition of the increment can be read as $\frac{x_1 - x_2}{x_2} = \frac{x_\infty}{\Delta}$, or better said in the manner of proportions using then Δ_∞. Unfortunately, the second ratio is a quotient of two quantities, each one equal to zero (according to the "Mechanist" opinion); the quotient is therefore a non-assignable quantity. Equipped with such a definition, the result of a synthesis may however appear:

> "As the difference of [any] term of the ratio is to the [immediately] superior term of the ratio, so is the largest magnitude to the one composed of all terms plus the increment." (*ibid.*)

In algebraic notation,

$$\frac{x_1 - x_2}{x_1} = \frac{x_1}{F + \Delta}$$

To see this better, it is possible to rewrite it as:

$$F = \frac{x_1^2}{x_1 - x_2} - \Delta$$

The increment Δ corresponds to a failure; it measures what fails to an infinite sum when one stops after a finite number of terms. Nothing will fail once the infinite is reached. From a finite n to an infinite, from the literary meaning to the literal one, a continuity of meaning is restored, by means of a synthesis.

Proceeding further in this line of reasoning consists in establishing the need to put the so-defined increment to zero. At this step however, Viète is no longer looking for a complete reasoning: it seems enough for him to refer to a result which Archimedes splendidly and synthetically explained—"and there is a fact"—in the *Quadrature of the Parabola* (proposition XXIII; Archimedes OO, II, 310):

> "Let there be continuously proportional magnitudes to infinity[17], with an under-quadruple ratio, and let 3 be the largest of all. The composed magnitude will be 4. And there is a fact[18]; to these in continuous under-quadruple ratio magnitudes, the largest being 3, nothing as small as possible can be added without the composed magnitude being larger than 4." (Viète 1593, 29)

The allusive style is unequivocal: it is by a double *reductio ad absurdum* typical of the method of exhaustion that the increment can be verified to be zero. The only short way is to use the particular case of the Archimedean progression as if it were the general case. Continuity is restored on an historical order as well.

Viète still does not stop here. He went from analysis to synthesis; but he raised a question rather than having solved one. The reference to the tradition of the method of exhaustion of which Archimedes is the most celebrated artist, is in no way an authoritative argument. Viète does not even criticise this tradition; he merely states that it contains a type of satisfactory proof for which no sequence can be provided. Moreover, it seems impossible to follow an algebraical path, or rather, a filiation to the tradition would denature the algebraical way. Indeed, using an algebraical relation, Viète associates the smallest term of a progression to the increment. But there is no link with the double reasoning by contradiction alluded to, which would be enough to validate the theorem on the sum of an infinite progression. Then Viète essentially shows the requirement of a "new algebra". This algebra does not appear as a natural one. It has to deal with indefinite quantities like Δ or x_∞, for which a correct writing is available only in the case of a finite term progression.The new quantities can be combined in some algebraical way as their possible ratio is equal to a well defined ratio of finite quantities. And equating these quantities to zero according to the formula of likelihood, something true is obtained. Viète's is a testimony of this essential experience.

He then concluded by refusing an end and this is undeniably an appeal for a sequel. Viète explicitly says of the reduction to zero: "But Platonicians will agree with difficulty, as the whole of Geometry essentially lies in the intellect" (*ibid.*). Will the sequel be an analysis or a synthesis? Wavering has the value of erasing the differences. For our purpose, it is enough to have shown that in Viète's case the passage from one style to another in the direction of a necessary future, served to make us aware of the uselessness of a motion back and therefore helped to suspend the back and forth move. We recognize a suspended analysis in this text.

III Fourier or the Synthesis Appearing as an Analytical Necessity

With the appearance of the *Théorie analytique de la chaleur* (1822), the localization in analysis seems indisputable. Fourier at least displays the banner of an analysis, by using the specific adjective in the title of his book. Therefore, as there is no apparent ambiguity, we are compelled to present our study in a manner different from the one used for Viète's text. We first have to question the validity of the analytical reference. Using this title, couldn't Fourier mainly be displaying a stylistic filiation to Lagrange's *Mécanique Analytique* (1788). Published in 1811-1815, the second edition of this book, corrected by the famous author, was considered as the example of a mathematization of the real world. In fact, classifying the content of Fourier's book at an epistemological level, the analogy with Lagrange appears less deep than the title may at first suggest. It was Auguste Comte, a thorough reader of Fourier whom he was persistently inviting to attend his first course in positive philosophy during the year 1829, who understood that Fourier was competing with Newton's *Principia* (1687). For even if there are some traces of analysis, Newton's book openly maintains the genre of a synthetic composition which resulted in some stylistic obscurity as has so often been observed[19]. By endowing heat theory with its phenomenological and mathematical concept, the flux[20] (which is the analogous concept to velocity in mechanics, and even its exact mathematical counterpart as a derivative) and by using the technique of a thermal balance implying an invariance, Fourier succeeded in establishing a partial differential equation governing temperature. Thus is the so-called heat equation to which commentators usually reduce the Fourier's achievement from the point of view of physics[21]. In his turn and for the specific physics of heat, he thus realized the Newtonian program which had been exemplified by the derivation of differential equations of motion from universal laws of attraction.

> "I do not fear to pronounce, as if I were ten centuries from now, that since gravitation theory, no mathematical creation was more valuable than this one for the general progress of natural philosophy."[22] (Comte 1830-1842, l. 31, II, 592)

Thus Auguste Comte speaks of Fourier's achievements. And to increase the weight of this judgement, he adds something which is not far from the important distinction between a metaphysical era—Newton—and a positivist one:

> "even so, by seriously scrutinizing the history of those two great thoughts, we could find that the foundation of mathematical thermology by Fourier was less made ready than the foundation of celestial mechanics by Newton." (*ibid.*)

Such a judgement *ipso facto* states that Fourier's theory composes a synthesis: apparently it comes from nowhere and it is totally built and "positively" explained; it has therefore definitively acquired the status of a scientific and perennial work:

> "The new theories which are explained in our work are for ever united to the mathematical sciences and, like them, they rest on invariable foundations; they will preserve all the elements which they now possess, and will continuously grow in extention." (Fourier OD, I, xxviii)[23]

Thus Fourier did not hesitate to proclaim his achievements and he was taking advantage of a language which had been dominant for centuries, namely the language surrounding Euclid's *Elements*, always an admired model for synthetic presentation of the science of magnitudes[24].

Let us then give up the reference to Lagrange. The analytical way is perhaps not yet Analysis! This latter would then appear in the text of Fourier, not as a style subordinate to the explanation, but far better as a whole new branch of Mathematics. It is clearly during the 19th century that any specific denomination for Analysis was abandoned[25]: it is no longer *in Analysin infinitorum* as it used to be with Euler (1748), but forcibly without any adjective in Cauchy's *Cours d'Analyse* (1821). And this is more visible as the first part of the course accounts only for algebraical analysis. A contemporary of Cauchy, could not Fourier be the instigator of Analysis as well? For more than fifteen years, he had been refining the various aspects of his Theory: it is sufficient to read any page of the *Théorie analytique* at random to notice his chiselled wordings. A consultation of the long table of contents at the end of the book, where classification in the finest detail takes care of the very connections of the reasoning itself[26], would convince any reader that the literary structure of the text was deliberately chosen to adapt as close as possible both to the reasoning and to the part of the real which is investigated. "Looked from this point of view, mathematical analysis has an extension as large as Nature herself" (Fourier OD, I, xxiii), so he claims in his preliminary discourse to the *Theory*. If the word Analysis receives then a privilege, it stays in the book without any further definition. Darboux, later editing the *Théorie analytique* for the *Complete Works* of Fourier, will find himself obliged, in printing this sentence, to add a capital "A" to Analysis.

However, the organization of our quest would be upset if we were to pursue the building of Analysis on this path. We had far better go to the conclusion to his work provided by Fourier himself. There, he feels the need to explain that even though it is the main object of his *Théorie*, he has not chosen to derive in a unique form the various integrals found for the heat equation belonging to the various situations met within different kinds of solids subject to heat propagation. He claims that such "transformations require long computation and they suppose almost every time that the form of the results is known in advance" (Fourier OD, I, 525, n° 428). He thus affirms that he could not have purely followed an analysis, even in the sense Pappus acknowledged where analysis has to start from what has to be reached.

If we were to adopt the qualification of "historical" for Fourier's presentation we might avoid choosing between analysis and synthesis and reach some kind of equilibrium. This seems to be a valid statement to start with[27]. Using the word "historical" requires us to play with the double meaning this word usually takes in the sciences. It certainly means a narration, with its chronological and critical unrolling of a thought concerning an object of science, but it also means the account of a systematic look at the real world. This last meaning is precisely the one in "natural history", a familiar expression used throughout during the 18th century and early 19th century. Fourier is first of all an original thinker (or scientist) because while allowing to read history of his thought, he turns it into a history of Nature herself[28]. Individually neither an analysis nor a synthesis, but a history of the real to which reason belongs as well.

A history of thinking and a history of objects; this double function is an old one in the construction of science. The swinging implied by these meanings is certainly one of the major ambiguities of history of science as such, at least as an intellectual mode. And this explains why we are aiming at the stylistic swinging of a chiasmus. The *Théorie analytique* appears to be accomplished in the same way as any historical account which is always told using a past time; as any synthesis, the *Théorie* keeps no trace of a past and bears no error before a future. If the *Théorie* has to be an analytical discourse, it is because so is Nature herself; not only in the interpretations given of the efforts made to analyse it, but in the very way those natural effects are produced. At the end of a section "the object of which almost entirely belongs to Analysis", when he evokes the structure of a differential equation, Fourier aptly qualifies it as the equation of the phenomenon, because this equation represents "in the most distinct manner the natural effect. This is the principal condition we always had in view"[29] (*ibid.*, I, 525, n° 428). The equation is not a model, or a reduction. For Fourier, there exists no middle locus between a mathematical thought and the real; fiction is not a resource which, even through the assumed risk of a logical fault, might account for the adequation of a thought.

Could we say then that we have a synthesis of the analysis! Such a genitive case is used too rashly. In order that the expression might have a meaning which convenes to Fourier's work, we should have to consider, as at any cross-road with no sight-of-crossing sign, that the order of the two words, analysis and synthesis, is indifferent. If Fourier calls the motion which animates his Theory 'analysis', *in fine* he summarizes what explicitly is a synthesis[30]. He turns up the older definitions of the two names; he locates himself at the crossing of the chiasmus.

As a question, the adequation of the analytical style to the synthetic content makes the purpose of our inquiry. We have to understand why analysis only, by sheer accumulation of deductive signs, could not have been sufficient in Fourier's eyes to build the *Théorie*. It could have achieved the status of synthesis only once it was entirely accomplished, that is once ended. Synthesis would have been the result of the unrolling of analysis. However, Fourier himself prevents us from adopting such a compromise which would provide an orientation for the branches of the crossing by explicitly naming each one. His exposition of facts, so he claims, coincides with the discovery of the facts; it is an invention as such and therefore his account cannot be smelt into a synthesis, the unrolling of which necessarily requires some axiomatic method. Even a man like David Hilbert would never state that the axioms precede thought in an inquisitive mind: they have to become the frame for intuition as a construction of the mind. Nevertheless, it is a history of the inquisitive mind of a natural philosopher which is the true account of Fourier, and he claims that it is the account of Nature herself. Analysis and synthesis are unequivocally mixed.

Analysis and synthesis are combined in the fate of Fourier's work. Those two words intervene directly in his intellectual and objectal filiation, and they are to be simultaneously written. They are endowed with a precise meaning, and fortunately there is no questioning about it: it is simply decomposition and recomposition. It is after Fourier, in a way rather a long time after him but in an explicit reference to his work, that everybody spoke of the harmonic analysis of a function and of its synthesis[31]. In the same manner as for the adjectivation of Analysis, even the word function had to disappear when a branch of mathematics was finally organized—Harmonic analysis—but this is no restriction but a metonymy as this branch contains harmonic synthesis as well. The maintenance of the expression "Harmonic analysis" is a rare phenomenon in mathematics, a science which is generally chary of distinctions among its various enterprises; the expression of Fourier Analysis is less common, but with the same metonymy that implies synthesis as well. The last expression follows from the fact that elementary functions are necessarily associated with the very idea of a periodic function: they can be called "simple" modes[32], the obtaining of which for a given function comes from a computation of integral coefficients, the so-called Fourier coefficients[33]. Such is analysis. Once the coefficients associated to the modes are known, according to an infinite addition naturally induced by a numbering by integers—this is the numbering of simple modes—a function is entirely found again or reconstructed: this is its synthesis[34].

Even if we positively follow the mathematical practice of the domain launched by Fourier, we have not yet reached a clear and distinct explanation concerning an analysis which should be followed by a synthesis, in the sense that, historically and epistemologically, we cannot use for long the apparently nice but "frivolous" distinction made by Condillac who places a "before" and an "after" in order to point up the link between the two operations of decomposition and recomposition. Fourier's operations show this clearly. First obtained from a laborious algebraical technique proceeding through the elimination of variables, a computation of Fourier's coefficients acquires a rational transparency only once orthogonal relations intervene[35]. These orthogonality relations exhibit such properties of simple modes that each one may reach an independent existence; each one is taking advantage of the freedom and therefore of the status of a dimension in geometry. These relations provide analysis with its own legitimacy and shape analysis as an independant moment of the reasoning, *i.e.* of the proof. However, as efficient operations, such orthogonality relations are available at the very moment of the synthesis of a function only; and practically as well as formally they can be omitted from what could be seen as the pure moment of the analysis. In short, orthogonality relations cannot be metaphorically viewed as the knuckle-joint linking in this order analysis and synthesis. But curiously we have to ascertain that analysis does offer an explanation in its own right only once synthesis is concluded[36]. Contrary to what has so often been said with good reason by classical epistemologists for whom roads without crossings are the best warrant for a scientific construction—it is the no noise syndrome—, synthesis is not the justification for analysis. Synthesis is certainly not the occurrence of a formalization according to an accepted mathematical canon, from which we can absolve those scientists who are not looking for rigour[37]. In fact, it happens as a crucial experience, and possibly as the main mathematical activity, that the computation yielding Fourier's coefficients works correctly even if, at the moment of synthesis, we were to "forget" certain simple modes[38]. As the conclusive example requires some technical preparation, it will be given somewhat later. Fourier proceeds in the same way, giving it at the very end of his book (this is a supplementary proof, if such is required, that his display is not a linear one; we already used the qualification of enveloping display). Before proving, we go to the consequences. Analysis has its own independence; but it is not automatically conducive to truth. Synthesis is not a conclusion which functions as a validation; it is an interpretation of an earlier analysis which, in this very process, changes for a new meaning: a cycle begins.

Fourier has not underestimated the aporetic conclusion which confuses the order for intellectual operations, analysis/synthesis. He even cancels the opposition. An aporia, which etymologically is what prevents an idea from providing a

path, conspicuously gave him the possibility of creating a theory: he had to erase the opposition, or the parallelism and lack of a meeting point of analysis and synthesis. This seemed necessary to turn "simple" modes into "proper" modes. The adjective has a value of reality. The path followed by Fourier is a thought in itself. The chiasmus analysis/synthesis is no longer the algebraical effect of a presentation: it is part of the work of science.

Such modes, so Fourier explains, are intrinsically linked with a periodic function conceived as a mathematical object; the reason being that Nature so constructs them. The study is that of heat propagation in solids. Ineluctably, at least from an analytical study, periodic functions do appear in the case of heat.There exist "waves" of heat. Mathematically, a "wave" is a mixing of a periodic oscillation and of a decreasing exponential in the variable describing the distance from the heating source. Therefore, analysis reveals a phenomenal property in its own right. Proper modes make their appearance from physics analytically pursued, and they go far beyond periodic functions; they are appearing under the inventive pen of Fourier in many other circumstances, for example with the so-called Bessel functions if we wish to point out only one other example[39]. We find the essential fact which instaures a generality: proper modes are present in all phenomena of heat propagation, and this is why the word "proper" is physically valid. But they "properly" too happen with the harmonics in sound propagation or in the explanation of tides. Both are quite distinct physical phenomenoms. If Harmonic Analysis becomes a mathematical theory, it is because of its universality. But this brings no loss of a "proper" property: the simple character of a mode is not changed into proper by the technical play of the mathematical game which is unable to confer such a quality to its objects. Even by folding analysis into synthesis. Fourier has eliminated any "middle", even mathematics, between a thought—his thought—and the world.

The nature of these modes has to be the object of a proof, for which we are at the active cross between analysis and synthesis. However, if the chiasmus is not yet discernable, it is because we have not sufficiently enveloped it with mathematics. Fourier is not providing a rhetorical discourse; he intends to speak like Nature herself.

In which sense, in fact, could one prove the "proper" property of an object which is deduced or built from an analysis? As a form has been exhibited, there can be no doubt about the very existence of proper modes; synthesis does not play the somewhat restrictive part of an ontology. By the way, in the case of periodic functions, such modes are reduced to the brave functions sine and cosine for integral multiples of the variable and are quite elementary functions. Clearly, by leading to a reconstruction of a function from its proper modes, synthesis gives credit to modes in their status of proper modes. It is not sufficient enough as a proof. Here synthesis appears for what it is etymologically, just an addition. It is not sufficient for a good reason: in its own proof, synthesis shares the defect of analysis. It works, but it does not help the understanding. A scandalous situation, a contradiction indeed to the purpose of providing a proof of a "proper" property.

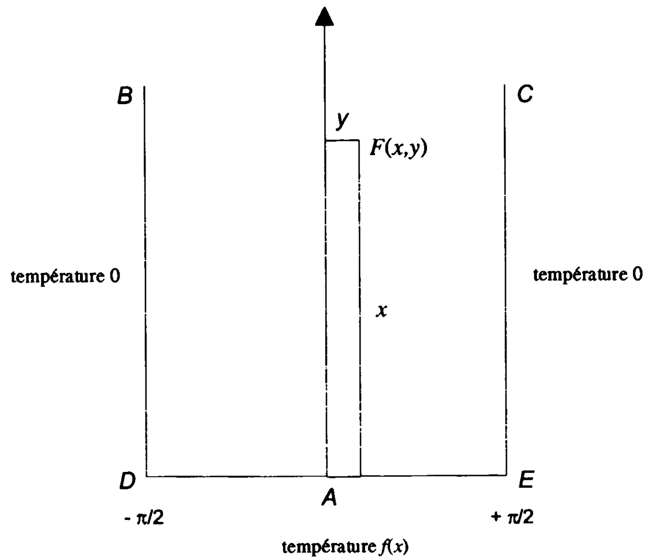
To get rid of the contradiction, the first way chosen by Fourier is just a bias in order to prove synthesis, *i.e.* the sum of a function developed into a trigonometric series (Fourier's series). It relies on the development of the function in power series. He then uses what was more or less called Taylor series, manipulated all through the 18th century, but certainly not rigorously proved, and eventually made the very basis of Analysis by Lagrange in his *Théorie des fonctions analytiques* (1797). Long as it is, with even a strange formal play on a typical constant like π used as a variable for differentiation—a game no longer authorized by acceptable science during the early nineteenth century—Fourier's proof sufficiently shows that he conferred on his manipulation no more value than a linking one. Fourier just helps to join his new mathematics with already known mathematics[40]. His bearing is a normal one for someone introducing an invention when one does not locate it as a revolution. The intention of this proof is not to mathematically fix what "proper" means; but this is the purpose of the theory!

There is no difficulty in proving or ascertaining the adjective "simple" for a mode. For the partial differential equation which governs heat propagation, a simple mode appears as a solution whose variables are separated: it has to be the product of a function of one of the variable by a function of another variable. This is, by the way, how from a computational point of view, such modes are obtained. It is a pleasant and efficient analytical characterization which the first year students usually are compelled to undertake. However, this characterization is a formal one; it cannot "prove" anything "proper"; it is a trick to reach such modes. Guile cannot provide a proof of what "proper" is!

There is another way which tempted Fourier, but it led him to nowhere. This failure is rather surprising to our modern eyes, in that the way is the one which will lead to proper vectors and proper values. Here the usual language adopted in English is unfortunately improper, and we have to think of the original German meaning of Eigen in Eigen-vectors or Eigen values. At least in French or in German, the maintenance of the adjective 'proper' or 'eigen' in linear algebra as well as in linear analysis, has a historical meaning. Fourier, effectively, shows some stability, and this stability is no longer a formal situation like the one where 'simplicity' just meant separation of variables. To explain this, we have now to enter some mathematics and at least a drawing, even if Fourier, as a presumed analyst, is rather parsimonious of such graphic representations.

We consider an infinite rectangular lamina: thus we have a two-dimension problem, with two space variables x, y and a physical mind may fancy that the lamina has an indeterminate depth. The two long lateral sides of the lamina are at a fixed temperature, melting water being a good choice in order to suggest the

idea of a muffler isolating the lamina at the sides, isolating it to the point of suppressing even the unavoidable dilatation which the lamina has to undergo. At the bottom of the lamina freedom reigns for the fancy of the experimentalist mathematician. He may impose a constant temperature—and this is how first Fourier began an analytic computation[41]—or he may impose any function. That is, he may decide any ordering of values of temperature along the internal DE, but only on this real interval where a real variable y is running: in other words, a free function $f(y)$ is available (variable x runs on the oriented median edge of the lamina). As we are at an intermediate moment of the analysis, time is no longer playing a role.



It is presupposed that the regime of heat is a permanent one, temperature is stationary as an equilibrium has been achieved between the lateral muffler and the given and generous source of heat at the base. Temperature at every point of the lamina is a function F of the space variables x and y only.

Fourier establishes a connection between the two functions, $f(y)$ at the bottom of the lamina—the given function—and $F(x,y)$ which is the sought for temperature in the lamina. Physically speaking, the connection seems obvious: only one regime of temperature is obtained. Fourier takes the opportunity to prove this uniqueness from the physics of the flux he has launched. Mathematically speaking, there is also a connection, and this is original as well. Function F is altogether a solution of a partial differential equation of the second order: the Laplacian of F is equal to zero

$$\frac{\partial^2 F}{\partial x^2}+\frac{\partial^2 F}{\partial y^2}=0$$

and it satisfies three more conditions:

$$F\left(x,-\frac{\pi}{2}\right)=F\left(x,+\frac{\pi}{2}\right)=0$$

$$F(0,y)=f(y)$$

$$\text{Lim } F(x,y)=0 \text{ for all } y \text{ in } \left[-\frac{\pi}{2},+\frac{\pi}{2}\right] \text{ where Lim } x=\infty$$

The indissoluble association of boundary conditions to the very partial differential equation is an innovation due to Fourier: it helped him to understand the correspondence between f and F, even at a moment when the concept of function was the prey of transformations to which the work of Fourier was to contribute[42]. It happens that proper modes are such that, if such a mode is an input at the bottom of the lamina, in the form of some function f, any trace of F at any horizontal segment of the lamina is equal to the given f (up to a constant multiplying factor). As an example[43], if $f(y) = \cos(11y)$, then $F(x,y) = \lambda f(y) = \lambda\cos(11y)$, where $\lambda = \frac{4}{11\pi}e^{-11x}$. From this remarkable stability, which we call to-day a proper property in a mathematical sense, Fourier deduces no mathematical action; he let it stay as a physical determination. In other words, he does not try to characterize "proper" modes functionnally as the invariants of the correspondance from f to F (up to a multiplying factor which we learned nowadays to call an eigen-value). The lamina remains as an intermediate object of the correspondance: it has not been identified through a relation. For Fourier, the proper character is not yet proven.

In a sense, we have not to regret Fourier's failure to detect the "proper" mathematical character in the invariance of a direction in a functional space. The irrepressible need of the determination led him to where what he brought is formidable: he affirms that synthesis of a function from the addition of its proper modes covers all thinkable functions. What prevails is the "arbitrary" character of the function; the adjective is thoroughly used by Fourier and associated with the expression

fonction générale. Sure enough, a combined mathematical and historical criticism may eventually say that this character was brought about by the pure analytical computation of Fourier's coefficients, in the sense that, for this computation, just the integral of a function operates, if we multiply the function by a proper mode[44]. In Fourier's time an integral was conceived as an area, therefore any "arbitrary" function possessed an area. However, our account of Fourier's display would not be sufficient if we were to restrict ourselves indicating a necessity due to the form of the computation; or, as could be said using an other description, we are too sensitive to the architecture of Analysis as it becomes independant of Geometry. Historically we think in terms of the building of Analysis. The possibility of the arbitrariness of a function, independently of the computational technique, is precisely for Fourier where the foundation of a mode as a proper mode lies.

We should less emphatically say that Fourier had the capacity to link two concepts, the one of proper mode and the one of arbitrary function. But this is not the knot of the whole situation.

In order finally to justify our description, the proof (which we consider now in order to show from what defect synthesis is suffering), is more remarkable because it plays with oblivion. Let us suppose that a "proper" mode, or better "simple" mode has been forgotten, for instance some $\sin(n_k x)$ for a certain integer n_k. Nothing would have been changed concerning the analytical computation of all other coefficients: we already said that the first part of analysis was independent of any synthesis. Strong as he is thanks to the orthogonal relations, Fourier however takes notice that any function synthetized with all the other proper modes would at least be orthogonal to this, willingly forgotten, mode. Forgotten, but still perpetuated by a sign

$$\int_0^{2\pi} f(x)\sin(n_k x)\,dx=0$$

The fact that an integral is zero is really a condition imposed on the function f. Therefore f is in no way an arbitrary function. Synthesis forgetting a mode is then a false synthesis. To give warrant to the arbitrariness of the temperature function at the bottom of the lamina is the way to offer to modes their "proper" property. "Proper" properly means an unavoidable property and thus it is an intrinsic property. Nature, which governs heat, cannot avoid proper modes: it is Nature who compels the mathematician, or better the natural philosopher, to think the abstraction of an arbitrary function, a function upon which no condition can be imposed. Obtained *via* analysis, the nullity of an integral helps to understand why forgetting some mode makes synthesis wrong: but this understanding comes only once synthesis is viewed as working for an arbitrary function. This condition of the arbitrariness—I dare call it that way—*ipso facto* intervenes for the practise of analysis itself. We were to eager to find a knuckle-joint between two styles and in fact we have found arbitrary functions as a general condition for both styles; we have acknowledged the shift from one style to the other. This is precisely what orders the Theory as constructed by Fourier; and it is the localization of a chiasmus.

In this move, the whole construction of the *Théorie Analytique* is at stake. To ensure the arbitrary character of the functions used or to avoid using just a name, Fourier has to exhaust all possible cases. He undertakes a systematical journey through different cases of heat propagation in quite different solids. Totality of the journey is necessary to fill the freedom provided by the arbitrariness of functions. From to-day, the word "total" precisely refers to the concept ruling mathematically proper modes, at least once some functional spaces are specified. A system of modes is total when there exists no function outside the zero function which may be orthogonal to all modes. Fourier did not have this ingredient at his disposal and was therefore obliged to verify the exhaustivity of proper modes by totalizing all possible cases. Analysis could provide a convincing proof of the proper character of a mode, only once all cases are synthetized. Each case, individually, is then a renewed analysis, and not simply a reproduced one. The risk of a chiasmus is not a unique risk in the theory: its very moment is therefore a scientific creation. With each case the theory can be falsified; the synthesis of one case helps the analysis of its successor. It also renews the analysis of the previous ones.

No redundancy at all[45]! Fourier organises its presentation according to an ordering of successive solid forms where heat propagates—lamina, prismatic beams, cylinders, armillas, or cubes—and each case provides, not only a confirmation, but its contribution to an understanding of propagation. This is an unavoidable proof that analysis alone is insufficient. Here is the answer to our original question. By specifying for each body a particular form, heat draws its proper geometry. This is this "reality", which has to be drawn for each case, and analyzed to each occurrence, from which at the end a structure —thermogeometry—is found. Each case has to be recomposed and informs the analysis of the previous case, thus modifying the meaning of analysis already made. Solved case by case, Fourier's thermogeometry is not the result of a synthesis: it is, in its ordered multiplicity, a direction for an analysis always reformed by synthesis.

As in any analysis properly done, there is the problem of the end of the theory, that is the moment where the back and forth move has to be stopped. It is here signalled by pure repetition, when any new case only brings computations but no renewed analysis. Fourier does not theorize, perhaps because he judges repetition

as not being sufficiently objective. And he was right, as his intellectually richest experience came long after he had thought his Theory ended.

IV Fourier's Transform: an Erasing of Synthesis

The most remarkable example of the efficiency of this style is provided by Fourier's transform, for which we first of all have to recall the extraordinary success in contemporary sciences, from solid-state physics to pseudo-differential operators, from wavelets and magnetic nuclear resonance, to a spectacular spread out in chemistry or medicine. It is the last case considered by Fourier in his quest for mere heat propagation[46], a case which he considered only in his text of 1822 almost without manuscript preparation. It is moreover a case for which the geometry is the flattest, just presenting an indescernible diffusion of "heat motion in an homogenous solid mass whose dimensions are all infinite" (*ibid.*, I, 387, n° 342)[47]. A case which would not be the possible focus of an analysis had not previous results shown the role of proper modes. The indiscernible geometry of the space can now be structured into a thermogeometry and therefore made analyzable: by a feed-back, in this process the mirror effect from the apparently dull geometry helps in turn to better "see" previous analyses of more particular cases.

By separation of variables, proper modes are easily found for the general "spatial" case which can be summarized by a partial differential equation (for which there exist a constant k, obviously a positive one which reflects physical parameters). This equation rules temperature allocation $T(x,t)$ where x runs through all real values—this is spatial freedom—and time t runs through real positive values only[48].

$$\frac{\partial T}{\partial t} = k\frac{\partial^2 T}{\partial x^2}$$

Right away, the case is a functional one as Fourier allocates an initial distribution of temperature—he writes $F(x)$—and makes clear, in his rigorous manner, that this function has to be an arbitrary one, under the specification that the function is defined over an (arbitrary) segment. A purely mathematical analogy is thus prepared with the case of the lamina for which the bottom temperature—involving a repartition on another segment—was also thought of as an arbitrary function on a given segment. Such a situation gave place to Fourier series (developed in a cosine series). Strong as he is from this result, Fourier may now begin by imposing a symmetry property to function F: it will be an even function ($F(x) = F(-x)$) as is the cosine function and the definition segment will have the origin as its middle point. But this is pure commodity.

Proper modes are many, $e^{-kq^2t}\cos qx$, with a positive real parameter q, and the trick for the computation is just to look for "simple" modes: Fourier no longer tries to prove their "property"; it has been seen in the lamina case, in the armilla case, etc. The passage from the discrete situation—that is all previous cases with a enumerable numbering of proper modes—to the continuous situation of the new geometry imposed by the freedom offered to parameter q, presents no difficulty; neither to Fourier nor to any mathematician of his time[49]. All have learned how to manage the passage by precisely using Calculus and by replacing a discrete sum by an integral. Without batting an eye, and by sheer analogy with the formula obtained in the lamina case, Fourier writes for the temperature T at point x and time t

$$T(x,t)=\int_0^\infty Q(q)e^{-kq^2t}\cos qx\,dq\,,$$

where Q is a function of the only variable q, the integral being extended to the whole domain of q, that is from 0 to ∞. This domain is not a fiction invented by the mathematician: it really is the space of what is "proper" and it does not depend upon the nature of function F or of the segment where it is defined. In the same way as with the lamina where one was compelled to suitably compute coefficients relative to the discrete family of proper modes, here "the difficulty lies in suitably determining function Q" (*ibid.*, I, 390, n° 345). The initial condition ($t = 0$) indeed yields a functional equation for Q.

$$F(x)=\int_0^\infty Q(q)\cos qx\,dq$$

In this equation, function F is known and function Q is the unknown. In other words, analysis has its object. But this is not the last aspect. In its turn, synthesis will change the object in order to present a new object to analysis: this will be the Fourier transform. But everything in its own order. In a suggestive fashion, Fourier speaks of an "inverse problem" as he is confronted to what, after I. Fredholm and D. Hilbert, we call an integral equation of the first class. He is conscious of the novelty and the interest of this "singular problem" (*ibid.*, I, 391, n° 346). In order to solve it, he reinterprets the result obtained in the lamina case: such a back and forth move is the main component of his method. For the lamina, the n-th order Fourier coefficient of the even function is obtained through an integration by summing the product of the temperature allocation by function cos nx. Then, multiplying this computed coefficient once more by function cos nx, and summing this time over all integers n, the original allocation f is found once again. Such is the lesson given by an investigation of the formula for even and 2π-periodic function. In order to avoid the exception of the coefficient of zero order, and precisely to avoid putting the analogy to come at a disadvantage, Fourier uses all integers, positive and negative, to exhibit a formula for the lamina case:

$$a_n=\frac{1}{2\pi}\int_0^{2\pi} f(x)\cos nx\,dx$$

and

$$f(x)=\sum_{n=-\infty}^{n=+\infty} a_n\cos nx$$

Thus, in the new case Q where the "proper" domain for q is no longer the set of integers but the interval of all real numbers from 0 to ∞, Q has to be obtained by an inversion

$$Q(q)=\frac{1}{\pi}\int_{-\infty}^{+\infty} F(x)\cos qx\,dq$$

Symmetry of the roles played by F and Q is now apparent: up to a constant, the same formula links the two. Judiciously, Cauchy (1817) speaks of "reciprocal function". An explicit involutive relation is available. This is equation (E) as Fourier calls it (OD, I, 408, n° 36) in order to magnify its importance[51].

$$F(x)=\frac{1}{\pi}\int_{-\infty}^{+\infty} F(\alpha)\,d\alpha\int_0^\infty \cos q(x-\alpha)\,dq \tag{E}$$

The straightforward meaning of (E) is an absurd one: an interpretation. But this task appears to Fourier more as the duty of his posterity than his own[51]. To award the merit of the invention of (E) possibly to Cauchy does not in fact modify Fourier's office. Not only was his part to provide a unique meaning to the word "sum" appearing in two occurrences in the lamina case—integration and discrete summation—but also to show that the two opposite functional operations of harmonic analysis and of harmonic synthesis were the same operation of a "sum" after a multiplication by a proper mode. Summation in the sense of integration in one occurence, summation in the sense of series in the other: the difference is a technical one, not a basic difference. This is what function Q brought to attention, and what the "spatial" case of heat propagation brought back to all other cases: Q is obtained from F by an "inverse" operation of the one which yields F from Q. An inverse operation, but as well a similar operation. Analysis and synthesis in this sense are formally identical operations. We already underlined the back and forth motion from analysis to synthesis; their formal identification, in some way, is the final result of the philosophical quest of Fourier.

He knows that the process he followed cannot replace a satisfactory mathematical proof: an analogy is no proof. But nevertheless the formula gives the general allocation of temperature. Fourier is eager to give an integral which, due to an exponential term, obviously converges:

$$T(x,t) = \frac{2}{\pi}\int_0^\infty F(\alpha)d\alpha\int_0^\infty e^{-kq^2t}(\cos qx)(\cos q\alpha)\, dq$$

Such a representation, without any doubt, is the aim of the *Théorie*, as the concrete numerical computation is never forgotten: it is the only way to get a verification. However, this concretization does not hid the main idea, a functional one, which is the "equivalence" between functions F and Q. This very idea moulds a second one, the idea of a transformation: so occurs the Fourier transform[52]. A transform for which, after what may be called experimental computations for special and elementary functions[53], Fourier individualizes a property. It is the transfer of a derivation or an integration operating on a function into a multiplication of the transformed function by a power of the variable, either positive or negative. This transfer is directly linked to the arbitrariness of the functions in order to fix a regulating principle:

> "By this transform, a function in some way acquires all the properties of trigonometric quantities; differentiations, integrations, summations of series are as well performed on general functions in the same way as they apply to trigonometric or exponential functions." (*ibid.*, I, 505, n° 419)

This is the use of such a principle which gives its value to distribution theory, a large and powerful generalization of the concept of function which was organized in the 20th century by Sobolev and Laurent Schwartz. The direction which has to be taken by posterity appears therefore as obvious for Fourier: "the use of such a proposition gives at once solutions of partial differential equations with constant coefficients" (*ibid.*). The solutions are precisely obtained using the method of "proper modes"; in the instance of these equations they are exponentials on which it is now possible to work inasmuch as "theorems of which we speak give to general and arbitrary functions the qualities of exponentials" (*ibid.*). "Representation" is thus an extraordinary tool for the "expression of complete solutions". Nowadays, it makes the kernel of pseudo-differential operators, an expression which wonderfully adheres to the idea of Fourier "representing" as well differentiation and generalizing it[54].

If technically speaking, for trigonometric series as well as for integrals, Fourier has shown that the analogy between analysis and synthesis lies in their being reciprocal, at the same time he justified the necessity of the back and forth motion followed in the *Théorie analytique de la chaleur*. His theory is altogether an analysis and a synthesis.

V The Scientific Sufficiency of a Chiasmus

In the two historical cases we investigated—Viète, Fourier—the passage from analysis to synthesis is no stylish pride of the author: it seems a required one, due to the nature of the mathematical objects and to the project of the inventor. Therefore it may be appraised as a scientific style. Moreover, in both cases, *de facto* there is a calling into question of what analysis is. But in both cases we find no soothing substitutions through synthesis. A synthesis may certainly be sought for by Viète, but he has not achieved it, which is an acknowledgement in itself. For Fourier, synthesis is viewed as impossible, or better not useful. In both cases, a criticism is dispatched in the mathematical way, that is on the edge of a problem, and not for itself. This is precisely the *in concreto* which Kant judiciously assigned to mathematics.

As such a mathematics is a culture, the question immediately arises of the relation between such criticism and more general thought. At the time when Fourier wrote, simultaneously a particularly severe criticism of the analytical way had been made by Kant and the scientific world itself was questioning its efficiency[55]. Kant invented the synthetic judgment *a priori* in order to maintain the idea of a progress, a progress which professionals themselves were no longer seeing as an inexorable chase[56]. One might think that this was the end of an era, and this was thought by contemporary thinkers[57]. In the time of Viète, the questioning was no less active; but it was in a context of analysis perceived as a new way, a way which may then stumble over tradition.

A suspensed analysis with Viète, a synthesis by analytical exhaustion by Fourier, the dissolution of differences between analysis and synthesis is striking in the two texts we have chosen. And the dissolution is independant of the particular meanings the concepts of analysis and synthesis may have had. What makes history then, is that in order to solve a problem—and I take the word in its general epistemological meaning—no appeal was made in either cases to some other intellectual resource. It thus ascertained that science is self-sufficient. The judgement which Blondel gave about the impossibility of science to know itself is not always justified by the history of mathematics. It may be a valid judgment of the value of science in some times of restlessness, but not of all times of restlessness.

Centre F. Viète of the History and Philosophy of Sciences,
University of Nantes

Notes

1 The constant reference to Pappus as an origin for the meaning of the analysis/synthesis opposition, is certainly fascinating. We may think that François Viète has some responsibility for this reference in modern times (using perhaps the recent Latin edition of Pappus by F. Commandino (1588)):

> "Il y a une voye aux Mathématiques pour enquerir et rechercher la verité, laquelle est dite avoir ésté premièrement trouvée par Platon, et par Theon appellée Analyse; et d'icelles définies l'Assumption du requis comme concedé, par les consequences au vray concedé." (Viète IV, 13)

However, one should not neglect the following, also historical, fact: Viète explicitly refers to the Ancients in his *Isagoge in artem analyticam* (1591*a*) in order to offer a new kind of analysis of epistemological thought. He coins a specific name for this new analysis (exegetics). Therefore, Viète interprets past mathematics in order to justify the advent of a new approach. *Mutatis mutandis*, we could say the same for Pappus: by exploring analysis he was obliged to locate it opposite to synthesis and he also claims his novelty. Isn't it true that mathematics is an action?

2 To qualify the opposition between analysis and synthesis as part of a back and forth motion seems a natural conclusion once the usual reference to Pappus has been stated. We use a translation from the French version of Ver Eecke in order to emphasize Pappus' choice ("that is called the domain of analysis, as I conceive it..."):

> "Now analysis is the path from what one is seeking, as if it were admitted, through its consequences to something that is admitted in synthesis. That is to say, in analysis we suppose what is sought as if it had been achieved, we look for the thing from which it follows and again from what comes before that, until by regressing in this way we come upon some of the things that are already known, or that occupy the rank of a first principle; and we call this kind of method 'analysis', as if to say a reduction backwards." (Pappus VE, II, 477)

3 More Cartesian than it was possible to be, in his *Elémens de Géométrie* (1667) Antoine Arnauld imposes a "natural order" to the display for the various objects of mathematics; he was, paradoxically, aiming at shaping a "natural" thought. Cf. Gardies (1984, ch. 4) and Dhombres (fc *a*).

4 A general feature of mathematics as it was fervently taught in the first Jesuit colleges was to develop reasoning according to Euclidean synthesis. But no effort was made to render synthesis as an objective of the teaching. Cf. Dhombres (1996*a*).

5 In his thesis, P. Trabal (1995) tries to describe the move around modern mathematics using a sociological approach. He gives perhaps too much credit to the novelty of an event without inserting it into the long history of teaching mathematics.

6 By contrast, one could underline the weak part played by analysis/synthesis opposition in histories of mathematics which emphasize technical aspects. An example is provided by the *Eléments d'histoire des mathématiques*, according to Nicolas Bourbaki (1974). Cf. Dhombres (fc *b*).

7 It may be useful here to add a quotation from I. Kant, which Blondel certainly refers to, but he refutes the idea it implies:

> "[...] all the steps that Newton had to take from the first elements of geometry to his greatest and most profound discoveries were such as he could make intuitively evident and plain to follow, not only for himself but for every one else." (Kant 1790, § 47, quoted from Kant (CJM))

8 "Si fuerint magnitudines continuè proportionales, Erit vt terminus rationis maior ad terminum rationis minorem, ita composita ex omnibus ad differentiam compositæ ex omnibus & maximæ".

As I do not intend to enter here upon philological explanations, I will not explain why the word 'ratio' does not denote here the quotient of two successive terms of the progression, but, by metonymy, the progression itself.

9 That the progression is convergent to provide a sum is guaranteed by the decrease of the successive terms.

10 For a mind of the Renaissance, the intervention of F_n in a proportion is the equivalent of an exact equality providing F_n.

11 A possible reference is proposition VII, 12 of Euclid's *Elements*.

12 Viète's bibliographical reference is unfortunately obscure to us inasmuch as we find no identical algebraical computation in an earlier book of Viète (1591*a*). But some works of Viète are lost; cf. Grisard (w. d.).

13 In his use of letters, at least in geometry, Viète makes a distinction between vowels used for known quantities and consonants used for the unknown ones. In the text under scrutiny, only consonants appear. It must be understood that each quantity, in its own turn, is an unknown to be computed from the three others. One of the relations fixes the value of X and states "On the contrary if, D, B, F are given, X will be given. In fact it is certain

$$\frac{\begin{array}{l} B \text{ times } F \\ +D \text{ square} \\ -D \text{ times } F \end{array}}{\text{B}}$$

will be equal to X" (1593, 29).

In modern notation, this reduces to $x_\infty = \dfrac{x_2F + x_1^2 - x_1F}{x_2}$.

14 If there is such a sophisticated literary composition, it means that Viète's reader is considered by him as his equal. Such a reader cannot fail to notice that in its litteral form the theorem uses only three imputs and this is contradicted by its transcription through four relations.

15 That is the way we chose to translate "*mechanici*". ("Et euanescere afferent Mechanici...")

16 This is the usual form of this result during the 17th century which is equivalent to our modern formula $\sum_{n=1}^{n=\infty} ax^{n-1} = \dfrac{a}{1-x}$. Apparently three traditions exist for the proof and in each one it is proved that something goes to 0. One tradition, a logistic one, is Viète's way which will be used by Fermat; a second one, a geometrical approach which inscribes computation in a drawing was founded by Gregory of Saint-Vincent; the last one, using a mechanical device, is chosen by Isaac Barrow (Dhombres 1995).

17 In Greek in the original (εως ἄπειρον). Archimedes' sums $3+\frac{3}{4}+\frac{3}{4^2}+\ ...$, and for this he establishes the formula for the remainder $3+\frac{3}{4}+\frac{3}{4^2}+...+\frac{3}{4^n}+\frac{1}{3}\frac{3}{4^n}=3\left(\frac{3}{4}\right)$. A double reasoning by contradiction yields $3+\frac{3}{4}+\frac{3}{4^2}+...+\frac{3}{4^n}+...=4$. On this example, Viète's notations can be interpreted with $F=4$ and

$\Delta_n=\frac{1}{4^n}$.

18 "*Composita ex omnibus fiet 4—Neque enim magnitudinibus* [...]."

19 Roger Cotes is explicit in his preface to the second edition of the *Principia* (1687; 2nd ed. 1713), when he describes the third class among those who cultivate natural philosophy:

> "They proceed therefore in a twofold method, synthetical and analytical. From some select phenomena they deduce by analysis the forces of Nature and the more simple laws of forces; and from thence by synthesis show the constitution of the rest." (quoted from Motte-Cajori translation)

20 The name with its meaning is due to Fourier.

21 With K, C and D being constants having a physicical meaning, heat equation in written in the form:

$$\frac{\partial T}{\partial t}=\frac{K}{CD}\left(\frac{\partial^2 T}{\partial x^2}+\frac{\partial^2 T}{\partial y^2}+\frac{\partial^2 T}{\partial z^2}\right)$$

where $T(x,y,z,t)$ is the temperature at point (x,y,z) and at time t. This equation is the kernel of the Theory. Analysis can then be described as all that has to be developped in order to make use of this equation. In the *Mécanique analytique*, Lagrange was putting to test a different ambition: he tried to interpret the whole science of motion from a unique abstract theorem, the so-called principe of virtual velocities. For sure, he found both Newton's law and velocity in its mathematical acception, but these two notions were not coming first. There *is* therefore a great temptation to attribute to Lagrange the organization of the analytical way, which has to be distinguished from Analysis.

22 An edition of Comte's *Cours*, unfortunately a critical one, was prepared by M. Serres, F. Dagognet, H. Sinaceur (Comte SDS).

23 References to the *Théorie* will be quoted from the edition of *Œuvres de Fourier* (OD), edited by G. Darboux. We add a numbering due to Fourier himself, in order to help references to the original book or to the English translation by A. Freeman.

24 To make a comparison with the perennial quality of the *Elements* does not imply that Fourier adopted an axiomatic method. In the *Théorie analytique de la chaleur*, we have no unfolding from propositions to propositions and from common notions to definitions. The construction is of a very different kind, for which the qualification of an enveloping movement is far better. We can but evoke this construction here, at least in the aspect which may concern analysis and synthesis.

25 To answer such a question, or rather to see its meaning, we should have to go back to the old debate on mathematical rigor. It is historically and mathematically well known that the qualification of rigor was given to Cauchy, for his *Analysis* (1821), but refused to Fourier for his *Théorie* (1822). Is it possible to conceive any kind of rigor if no construction project is at stake?

26 The table of contents takes twenty one pages of the *Théorie* in the edition of the Complete works of Fourier, for a text totalling five hundred and sixty three pages. Sometimes, this table shows more than what is explicitly proved in the corresponding article; as if Fourier had written his table in the manner of a programme to be completed and, later, would have had to reduce his ambitions. One would, at least, admit that such an ambivalence leads us to the trail of an analysis corrected by some kind of synthesis. But we will have to take a far longer path in our study of analysis and synthesis as organized by Fourier.

27 We cannot properly justify here such a description of the work done by Fourier. Many authors have thoroughly described the *Théorie analytique de la chaleur*, first of all Auguste Comte whom we already quoted. There is also Gaston Bachelard (1928). Among historians, we may quote I. Grattan-Guinness (1972) and J. Herivel (1975) and, with the ambition to deal simultaneously with the biography and the scientific work, J. Dhombres and J.B. Robert (1996).

28 Properly speaking, history of science, *i.e.* history of what was done before Fourier, almost never intervenes in his *Théorie*. Probably this refusal of a past is based on the fear that it may bring a kind of contingency to the construction; it may generate unjustified images contradicting the objective of unrolling a proof which pretends to be as close to Nature as possible. In other words, anything concerning a past history will appear under Fourier's pen as a counterpoint. It thus has two purposes; one is to measure the progress made by Fourier himself and the second is to make past errors conspicuous, in order to avoid them. In a very concrete way, we find here the attitude of Auguste Comte about the positive interest of history of science. And this is precisely where he mentions analysis and synthesis:

> "Various sects of metaphysical philosophers so abused, for a century, of those two expressions, using such a variety of logical and deeply different acceptions, that any righteous mind to-day should loath to introduce them in the discourse, at least when the circumstances of their use do not specify in a natural way their positive meaning." (1830-1842, l. 35, vol. III, 33)

29 Perhaps we should linked this with an expression which Newton used, "the nature of things".

30 Although commentators frequently overlook its meaning, the synthetic aspect is very strong in the remarkable *Remarques générales sur la méthode qui a servi à résoudre les questions analytiques de la chaleur* (*General remarks on the method which has been used in order to solve the analytical questions of heat*; Fourier OD, I, 524-531, n° 428). We cannot avoid noticing that the method itself is not stated as being an analytical one: the qualification is only used for the questions which the *Théorie* arouses.

31 The history of the expression "harmonic analysis" is a curious one: it started from the domain of mathematical instrumentation during the 19th century (*Harmonische Analysatoren*) to the theory during the 20th century (as in the title *Harmonic Analysis* used by Norbert Wiener (1930 and 1938)).

32 For a 2π-periodic function, if we add the unit function, those simple modes are $\cos nx$ and $\sin nx$ where the integer n runs from unity.

33 To do the harmonic analysis of a 2π-periodic function is to associate to this function its Fourier coefficients

$$a_n=\frac{1}{\pi}\int_0^{2\pi}f(x)\cos nx\,dx \text{ and } b_n=\frac{1}{\pi}\int_0^{2\pi}f(x)\sin nx\,dx \text{ for } n\geq 1 \text{ and } a_0=\frac{1}{2\pi}\int_0^{2\pi}f(x)\,dx$$

Fourier was obliged to explicitly state the boundaries of a definite integral: his notation is so instrumentalized that the integral becomes an operator. In order to explain Fourier's integrals, he later will use a_n for negative integers n.

34 The synthesis of a 2π-periodic function is, using its Fourier coefficients, to reconstruct f from the infinite sum $\sum_{n=0}^{\infty}(a_n \cos nx + b_n \sin nx)$.

35 Such orthogonality relations are of the form $\int_0^{2\pi} \cos nx . \cos mx dx = 0$ for $n \neq m$. J. B. Pécot (1992) provides an excellent historical and epistemological presentation of these relations over two centuries.

36 I am not pretending to reconstruct the genesis of invention in the case of Fourier in a few lines; I am not trying to confirm or to refute what he himself claims. I already said that the genesis he describes is presented by Fourier as a part of his *Théorie*; both as a tale and as an account: therefore I mainly keep the order he has given. Whatever is the computation leading to Fourier's coefficients, in the precise case of orthogonality relations obtaining them is always a second move. Even if such relations were unconsciously copied by Fourier from Euler, Fourier first presented analytical computation for the coefficients, both in his early manuscripts as well as after he has had time to synthetically polish his *Théorie analytique de la chaleur*. The book issued in 1822 is the last form of many earlier manuscripts, a first and complete one finished in 1807, a second in 1811, part of which was published by the Academy of sciences (Fourier 1819-1820) later after obtaining a "Grand Prix" in January 1812.

37 If I willingly omitted to stipulate as a preamble that Fourier's work was inscribed in physics, it was to avoid, at least for a modern mind, the anachronistic opposition between pure and applied mathematics. I wanted to avoid a too easily thought prejudive of a weaker kind of rigor for a mathematician working on real objects and on the real world, for whom the distinction between analysis and synthesis could have been minimal, distinctions seemingly relevant to the pure world of mathematics only.

38 The example of the so-called Bessel's function is an important one for Fourier. The reason of the emphasis is that it helps him universalizing his method by removing it from the too restrictive category of trigonometric series. Orthogonality of the Bessel functions, which is certainly not an obvious result as in the case of trigonometric functions, becomes therefore both a tool and an explanation. This orthogonality interprets the orthogonality of trigonometric functions: it is not only viewed as a generalization but, as an understanding.

39 Once more, we have to rely on what the reader knows of Fourier's mathematics (see bibliographical list); we are in no way attempting to describe the originality of his treatment of the so-called Fourier series, Bessel functions or of the Fourier integrals.

40 Both in physics and in mathematics, Fourier's theory is literally unchanged; it has been the subject of a considerable formalization by the practise of teaching. Therefore, the objective of the proof for a "proper" character no longer appears as essential: it seems already known. This is often the result of the conjugate weight of history and objectivity: this is also the main difficulty in any history of objectivity.

41 With a function $f(y) = 1$, Fourier was compelled to express 1 as a trigonometric expansion:

$$1 = \frac{4}{\pi}\sum_{n=0}^{\infty}(-1)^n \frac{1}{2n+1}\cos(2n+1)y$$

It gave him the way to express temperature $F(x, y)$ at any point (x, y) of the lamina.

$$F(x,y) = \frac{4}{\pi}\sum_{n=0}^{\infty}(-1)^n \frac{1}{2n+1} e^{-(2n+1)x}\cos(2n+1)y$$

42 This transformation of the function concept is certainly one important part of the constitution of Analysis as a domain. The fact that Fourier is linked with it is not just a chance. It is part of his project: the *Discours préliminaire* of his Theory is explicit.

43 In general, for $f(y) = \cos(2n+1)y$, $F(x, y) = \lambda f(y)$ with $\lambda = \frac{4}{\pi}(-1)^n\left(\frac{1}{2n+1}\right)e^{-(2n+1)x}$.

44 In the twenties of the 19th century, Cauchy has ended this conception by defining a definite integral from "Riemann's sums". In the process, area becomes a property but not a universal one. Thus, a continuous function possesses an area, but not necessarily an arbitrary function. Fourier took no notice of this change.

45 Contrary to what has been claimed by some positivist commentators, even like G. Bachelard: they regret that Fourier renews his analysis in each case, and therefore forget the "proof" by exhaustion provided by Fourier. In other words, they take for granted the claim of Fourier's adequation to the world, whereas the author makes efforts to prove it. In this sense, scientific positivism is not a defect of Fourier!

46 Sumptuously entitled "On diffusion of heat", the last chapter of the *Théorie analytique* signals that no particular geometrical body overtightens the spread of heat.

47 The ordering of cases where heat propagation is to be studied is an important part of the construction of the theory; it is neither an organization issued directly from the empirical world; nor an organization ruled by the criterium of Cartesian simplicity as the simplest case, the purely spatial one, is the last. The ordering has as its objective to let analysis and synthesis interact.

48 For reasons of symmetry, the three space variables are reduced to one only. As usual with Fourier, even with a final case, a first step begins by an analysis and therefore by a reduction of the problem. This simplified model has many possible interpretations: one is the diffusion of heat in the space when the temperature is known in a band (portion between parallel planes) and constant on each intermediate plane.

49 Is it necessary to recall here that, concerning sizes, there is no difference made during the time of Fourier, between an enumerable infinite and a continuous one. Cantor will exhibit the difference in the 1870's, opening a new era for mathematics as a whole, and for analysis in particular.

50 Equation (E) is written in the general case and F is no longer required to be an even function; this explains only $\cos qx \cos qd$'s replaced by $\cos q(x-a)$

51 Posterity will work as Fourier predicted: it only took far more years than we expected and in the process the memory of Fourier as a decent mathematician will suffer. We have attempted to "tell the story" in the last chapter of Dhombres and Robert (1996).

52 Let us give a standard definition of Fourier's transform.

53 Thus, he computes the Fourier transform for power functions and is led to

$$\int_0^{\infty}\frac{\sin u}{\sqrt{u}}du = \int_0^{\infty}\frac{\cos u}{\sqrt{u}}du = \sqrt{\frac{\pi}{2}}$$

Many other formulae are given, a sort of first dictionary for Fourier transform.

54 The main difference between to-day's attitude and Fourier's way is that he realizes the transform as describing the operations duly made by Nature. On the contrary, the modern point of view is a formalist one: it is just the adaptation of a theory, using an analytical form subjected to algebraical handlings, in order to find solutions to partial differential equations.

55 B. Timmermans (1995) remarkably pointed this philosophical inquiry, and doubt, about analysis at the end of the 18th century.

56 To recall the existence of a restlessness, it is enough to mention some sentences of Evariste Galois. He, around 1830, proposed to jump over computations, as analytical deductions were no longer inventive tools.

57 In a collective way, as it represents the opinion of the members of the First Class of the Institute, the impression of having to create the conditions of a new era can be seen in Delambre (1810).

MORITZ EPPLE

STYLES OF ARGUMENTATION IN LATE 19TH CENTURY GEOMETRY AND THE STRUCTURE OF MATHEMATICAL MODERNITY

I Introduction

In this paper, the distinction between analysis and synthesis in mathematics will be related to a second distinction, that between concrete and abstract forms of mathematical argumentation or, more generally, of mathematical practice.

As discussed in other contributions to this volume, the distinction between analysis and synthesis in mathematics has a long history, involving topics of a rather different nature. There is the proof-theoretical aspect, which appeared first in the ancient Greek uses of the term. There is the aspect of epistemology, which played a central role in Descartes' *Discours de la méthode* and Kant's *Kritik der reinen Vernunft*, bearing on central issues in the philosophy of mathematics; and there is the aspect of two different research styles in geometry, made possible by the merging of geometry and algebra in early modern times and which evolved into a great controversy in 19th century projective geometry.

The situation with regard to the distinction between concrete and abstract concepts, knowledge, or argumentations is similar. Again, this distinction has a long history, including its connections with mathematics. Suffice it here to say that Aristotle used the Greek counterparts of abstraction (ἀφαίρεσις and χωρισμός) to describe the ontological status of the objects of mathematical knowledge as well as the epistemic perspective which mathematicians make their own in looking at real (that is for him: concrete) objects as mathematicians[1]. And even more than is the case with the terms 'analytic' and 'synthetic,' the expressions 'concrete' and 'abstract' have often been used in a rather intuitive way, without explicitly introducing them as notions with a clear meaning. (Even though there is at least one technical sense to which one could refer: namely the technique of defining mathematical terms "by abstraction", *i.e.*, by means of invariance under an equivalence relation[2].)

Here I will not try to give a comprehensive history or philosophy of the role of this distinction in mathematics or even in modern mathematics. Instead, I want to

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 177–198.

begin my discussion with a rather limited historical question, namely: what became of the controversy between the analytic and the synthetic style of geometry towards the end of the 19th century? If one uses the term 'mathematical modernity' for the period *after* the great changes in 19th century mathematics (as I shall do), then the controversy about analytic and synthetic geometry seems to be a *premodern* affair. Later there arose a new, *modern* difference in geometrical style, exemplified by the geometric writings of Felix Klein on the one hand, and David Hilbert on the other. It is a difference of this latter type which I want to describe in the following, using the distinction between a concrete and an abstract style of mathematical reasoning.

After a few remarks on the historical developments in question, I will try to make my use of the terms 'concrete' and 'abstract' a little more precise philosophically. It will turn out that, as in the case of the analysis-synthesis distinction, the difference between an abstract and a concrete mathematical argumentation is not confined to geometry, but represents a rather general difference in the style of mathematical reasoning. Finally, I want to relate this difference to the historical reconstruction of mathematical modernity due to Herbert Mehrtens. My proposal will be *to use the distinction between abstract and concrete mathematical styles as an internal criterion to judge the modernity of a piece of mathematical research.* In the course of the discussion, a historical example—the invention of the braid group—will be discussed in some detail in order to bring out how this criterion could work in historiographical practice.

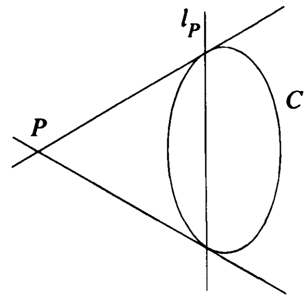
II From Synthesis and Analysis to Concrete and Abstract Styles of Mathematical Argumentation

II.1 Concerning the development of geometric argumentation during the 19th century, I shall restrict myself to some rather general remarks, most of which are due to the historical writings of Felix Klein. Certainly, they do not really capture the complexity of the historical development. However, they may serve the purpose of setting the stage for the discussion that follows. Let me begin by recalling some aspects of the controversy between synthetic and analytic geometers in the early 19th century.

It is well known that a revival of a "pure" approach to geometry was advocated by important pupils of the French mathematician Gaspard Monge[3]. This approach avoided the algebraic formulation of geometric relations which had proved so successful since the appearance of Descartes' *Géométrie* (1637). Instead, a research program gradually evolved which aimed at finding and using purely geometrical techniques to investigate properties of various geometrical objects in the plane or in space. A typical example was Poncelet's use of the machinery of the polar correspondence between points and lines with respect to a given conic section in order to translate theorems about point configurations into theorems about lines and vice versa. This research program, which eventually also found supporters in Germany, was particularly successful in the investigation of projective properties of geometric figures. For instance, Jacob Steiner had shown in 1832 how to generate conic sections and certain surfaces by means of projective correspondences between pencils of lines or planes[4].

On the other hand, some French and German mathematicians immediately realized that the projective properties which had become the focus of geometrical research could equally well be treated by means of algebraic equations. The main step in this direction was the introduction of adequate systems of coordinates by Möbius and Plücker in the late twenties of the last century. The relation between pole and polar with respect to a given conic thus appeared, for instance, as a simple consequence of a bilinear equation in homogeneous coordinates. It did not take long before mathematicians like Plücker and Hesse handled the formulas of projective geometry quite masterfully and could use them to establish astonishing facts like the configuration of inflection points of a general curve of third order. Their achievements contributed essentially to the rise of the new field of algebraic geometry.

synthetic *analytic*



$$C: f(x,y,z)=0$$
$$P: (x',y',z')$$
$$l_p : \frac{\partial f}{\partial x}x' + \frac{\partial f}{\partial y}y' + \frac{\partial f}{\partial z}z' = 0$$

Figure 1: Pole-polar correspondence

II.2 It soon became clear that most parts of projective geometry could be formulated either synthetically or analytically, and both parties competed in re-proving results of the other party in their respective idioms. Thus it is obvious that these were not two different branches of mathematical knowledge but rather two different modes of presenting, acquiring and justifying this knowledge. Modern theories of scientific knowledge have furnished us with a number of concepts to describe such differences. Ludwig Fleck's notion of a thought style or Gerald Holton's notion of a pair of methodological themata would apply here (Fleck 1980 and Holton 1978).

This view was expressed already by Klein in his *Elementary mathematics from a higher standpoint* of 1908:

> "Synthetic geometry is that which studies figures as such, without recourse to formulas, whereas analytic geometry consistently makes use of such formulas as can be written down after the adoption of an appropriate system of coordinates. Rightly understood, there exists only a *difference of gradation* between these two kinds of geometry, according as one gives *more prominence to the figures or to the formulas*. [...] In mathematics, however, as everywhere else, men are inclined to form parties, so that there arose *schools of pure synthesists and schools of pure analysts*, who placed chief emphasis upon absolute 'purity of method.'" (Klein 1908-1909, II, 55)

To show that the controversy lay in fact on this level, we may look at the critical objections of the synthetic geometers against analytical arguments. One such objection ran as follows: In a sequence of algebraic manipulations of a formula, it may be impossible to keep track of a sequence of geometric steps to which the formal manipulations should correspond. Therefore, we arrive in the end at a geometrical statement without knowing what its place in the system of geometric truths is. As Chasles put this argument: "Is it then sufficient in a philosophic and basic study of a science to know that something is true if one does not know why it is so and what place it should take in the series of truths to which it belongs?"[5] Obviously, Chasles refused to consider an analytic derivation as a *adequate justification* of geometric knowledge, even though he allowed for the *correctness* of the result of such a derivation.

Synthetic geometry thus appeared as a form of methodological purism. A particular argumentative context was specified—for example, the geometry of systems of projection rays[6]—and criteria were given which singled out the accepted types of questions and arguments relative to that context. The same was true for geometers with strong analytic commitments: here the argumentative context was the manipulation of algebraic equations in the space of homogeneous coordinates[7].

II.3 In the second half of the 19th century, the most fruitful lines of geometrical research were no longer structured by the research programs of analytic and synthetic geometry. These lines were, first, the one leading to the development of algebraic and differential geometry, and, second, the line leading to a strictly axiomatic approach to geometry. Klein's later geometrical writings were intended to convey to the reader some main ideas of the first line, ideas which were due to people like Clebsch, Riemann, or Lie. To Moritz Pasch and David Hilbert we owe the classics of the second line[8]. Let me briefly illustrate this reorientation with

some remarks pertaining to Klein's *Vorlesungen über höhere Geometrie* (1893) and Pasch's *Vorlesungen über neuere Geometrie* (1882).

Felix Klein had been one of the first to make clear that the opposition between analytic and synthetic geometry had lost its importance. In a note to his *Erlanger Programm* he had written in 1872: "The difference between recent synthesis and recent analytic geometry has no longer to be considered as an essential one, since the ways of reasoning on both sides have gradually evolved into quite similar forms." (Klein 1872, 74) Later he spoke of a "certain petrifaction" in geometry, due to the exaggeration of purist orientations[9].

Klein himself avoided a commitment to one of the sides. Early in his *Vorlesungen über höhere Geometrie* he said: "We pronounce it already here as a principle that we shall always combine the analytic and the geometric treatment of our problems and will not take a one-sided point of view." (1893, 26) In fact, Klein himself built both aspects simultaneously into his own unifying conception of geometry. If he proposed to study geometric properties in terms of invariants under a group of transformations, he also combined new algebraic notions with typical synthetic questions. For the topics presented in his *Lectures on Higher Geometry*, he favoured the name "algebraic geometry," making explicit his interest in the geometric properties of algebraic objects, from zero sets of polynomials to differential equations. The list of topics mentioned is—as with most of his writings—impressive. It includes, besides traditional material of analytic and synthetic geometry, multilinear equations and determinants, quadratic forms, rational and algebraic functions, algebraic curves and surfaces, Gaussian differential geometry, differential equations, invariant theory, group theory, Riemann surfaces, and some of Lie's ideas. But also he hinted at subjects like graphical statics or the theory of cogwheel profiles.

II.4 Like Klein, Moritz Pasch acknowledged the importance of synthetic as well as analytic points of view. In the Preface to Pasch's *Lectures* of 1882, we find the remark: "Analytic geometry has learned from synthetic geometry, and in case of a further fusion, there may emerge a higher geometry of a unified nature." (1882, 2) Perhaps, Pasch would have accepted Lie's or Klein's geometrical writings as a candidate for that higher, unified geometry. However, his own conception of geometry was directed at different aims. As is well known, he strove for a "pure," axiomatic development of elementary geometry, making it a rigorous mathematical theory by establishing its theorems on the basis of the smallest possible set of "core notions" and "core propositions" (*ibid.*, 4 and 15). His basic notions and propositions are synthetic notions like points, planes, and incidence, and Pasch even placed his work in the tradition of synthetic geometry (*ibid.*, 1). Only at the end of the book do we find a discussion of coordinates and of the continuum of real numbers, by which, as he says, analytic geometry is made available for the field of projective geometry (*ibid.*, 179). However, it is quite clear that Pasch's central aim was not intuitive, but conceptual, logical clearity. This comes out in his extension of the use of the basic notions, *e.g.* the use of "point" for a "bundle of rays" which reduces the number of necessary basic propositions, or his famous criticism of the logical gaps in Euclid's *Elements.*

In Pasch's book, we find again a consciously cultivated purity of method. We do not, on the other hand, find the wealth of connections to other mathematical disciplines present in Klein's lectures. Neither do we see Pasch switching constantly between algebraic, geometric or even intuitive arguments. He remains strictly within the conceptual framework set out at the beginning of his presentation.

In this methodological respect, there is but a small step to Hilbert's *Grundlagen der Geometrie* (1899)[10]. Certainly, in Hilbert's text the interpretation of the axiomatic method is rather different from Pasch's view. (For the latter, geometry is still to be considered as part of "natural science" (*ibid.*, 3); the basic notions and propositions encode empirical evidence (*ibid.*, 16).) Moreover, the mathematical treatment is complete in a quite different sense. But the style of Hilbert's text, the strict adherence to a well-defined argumentative context and method, is quite close to Pasch's and indeed very far from Klein's.

II.5 The difference between the two lines of geometrical thinking connected to the names of Klein (or Riemann or Lie) on the one hand and Pasch or Hilbert on the other is not merely a difference in style but also a difference in the topics investigated. The inquiry into the relations between curves, surfaces and algebraic function theory leads to different mathematical questions than those concerned with the relations between the different groups of geometrical axioms. However, it is obvious that there is still an important difference in style between Klein's *Lectures on Higher Geometry* and Pasch's *Lectures on Recent Geometry.* It is a difference in style of this kind which may be understood as replacing the issue of a synthetic or an analytic treatment of geometry in the context of mathematical modernity[11]. In order to mark this shift, let me propose to use the distinction between a "concrete" and an "abstract" style of geometrical argumentation. For the moment these are but two names. I want to explain my choice in the following, making the notions of a concrete and an abstract argumentative style more precise at the same time.

Let me begin by noting two rather obvious features of the shift from the analysis-synthesis opposition to that of the concrete and the abstract. *i)* While the beginning of the century had seen a controversy between two competing, more or less purist methodologies, the interesting opposition by the end of the century is better described as one about methodological purity *vs.* methodological diversity. Pasch and Hilbert made a deliberate choice of methodological purism. Klein, on the other hand, explicitly favoured the use of different methods, and most of his

mathematical achievements are closely related to this diversity of methods. *ii)* The second feature is a very different view of the generality of a piece of mathematics. The axiomatic style of Pasch and Hilbert sought to guarantee the general applicability of its results by reducing the argumentative context to its uttermost minimum. (It is only implicitly encoded in the axiomatic basis of a mathematical theory.) In Klein's style, on the contrary, it was precisely the density of the argumentative context, the rich variety of topics and points of view discussed, which was intended to show the general relevance of the ideas presented.

III A Philosophical Analysis of Concrete and Abstract Arguments

III.1 At this point I would like to sketch a philosophical analysis of the relationship and differences between an abstract and a concrete style of argumentation. Thus I leave history aside for a moment and make a digression into the philosophy of mathematics.

It seems that a more precise description of abstract and concrete arguments can start from two premises. The first is that mathematical arguments are pieces of mathematical practice, *i.e.*, we have to deal with a question of the pragmatics of mathematics. The second premise is that one should begin with a consideration of the relation in question from a local point of view. That is to say, one should look at a small piece of argumentative practice and try to explain the difference there.

I take the practice of mathematical argumentation to be a complex of actions, such as defining, conjecturing, proving, etc.[12] (These *mathematical actions* are immersed in communicative and *social actions* like publishing, giving talks, applying for positions, organizing meetings, and the like.) Argumentative practice is organized in smaller units, which I shall call '*mathematical games*', using a notion for complexes of actions going back to Wittgenstein[13]. In the first half of the 19th century, synthetic geometry was guided by a set of methodological constraints that defined a certain mathematical game, and similarly, analytic geometry may be viewed as another, though related, argumentation game. Such games may be described by specifying the possible situations belonging to the game and the rules guiding possible actions in these situations. A part of the rules is determined by, or rather, determines the mathematical subject of the game (*e.g.* geometrical objects), and another part fixes the techniques, types of arguments etc. considered legitimate. Thus the games of analytic and synthetic geometry show a partial, but not a complete correspondence of action-rules. For instance, the pole-polar correspondence could be used in both games to derive dual theorems. (A closer look shows, however, that we have in fact two rules here: a purely geometric construction, on the one hand, and a correspondence determined by a bilinear equation, on the other[14].)

We can immediately translate the two features of an abstract and a concrete mathematical style noted above into this language. A domain of mathematical argumentation is methodologically pure if it belongs to a single, well-defined argumentation game. Diversity on the other hand means playing more than one game at a time, or switching frequently between different argumentative contexts[15]. Whether a context of argumentation is (relatively) "poor" or "rich" may be judged by the degree of detail of the descriptions of situations and rules of the game(s) in question. Still, this may not be clear enough. Let me thus turn to my example, by means of which I can complete the local description of the distinction between an abstract and a concrete argument.

III.2 The example was included by Wilhelm Blaschke in the third edition of Klein's *Lectures on Higher Geometry*, published posthumously in 1926, as one of five topics under the heading "Examples of geometric research of the last decades" (Klein 1893). In fact, it is a topological example, namely Artin's *Theory of braids*, which had appeared in 1925 in the *Hamburger Abhandlungen* (Artin 1925-1926). The inclusion of this example into Klein's book is revealing for several reasons. First, it shows how broad the conception of geometry was which Blaschke ascribed to Klein, and in fact I think he was essentially correct. Second, Artin's work on braids was rooted in Klein's favourite subject, the geometric theory of algebraic functions. (For details concerning the history, of the next §IV.6.) Third, it was one of the few topological problems which could in some sense be solved completely by group-theoretic methods at the time. This last feature makes the example particularly suited for my purposes.

Artin defined his braids as follows:

> "By a braid Z of n-th order we understand the following topological object: Let a rectangle with opposite sides g_1, g_2 and h_1, h_2 (the 'frame' of Z) be given in space. Let n points $A_1, A_2, \ldots, A_n$ and $B_1, B_2, \ldots, B_n$ be given on each of the sides g_1 and g_2, counting from h_1 to h_2. With every point A_i we associate uniquely a point $B_{r(i)}$ with which it is connected by a curve m_i without double points and without intersections with any other curve m_k. Let the curve m_i be oriented from A_i to $B_{r(i)}$." (*ibid.*, 47; see fig. 2.)

In addition, Artin required that every curve cuts a plane orthogonal to h_1 and h_2 at most once.

Two such braids are considered "equal" (says Artin), if they can be deformed into each other without self-intersection. Obviously, Artin introduces here an equivalence relation between braids without being too explicit about that, as was still common practice at this time. (In fact, definitions by abstraction had been analyzed logically only some 20 years earlier, by Peano (1901) and Weyl (1910 and 1913)[16].) Further on, he sometimes speaks of the topological objects as braids, and sometimes of the equivalence classes under isotopy. Only in his second, more

rigorous attempt to deal with braids in the late 1940's Artin did draw a clear distinction between "weaving patterns" and "braids," which are equivalence classes

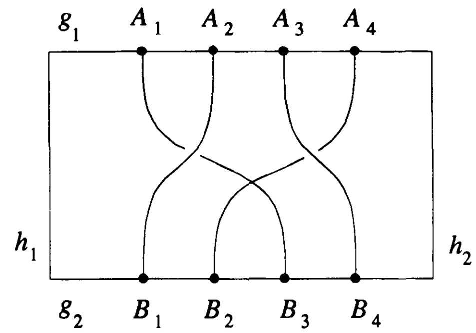


Figure 2: A braid of 4th order

of weaving patterns (Artin 1947, 101-126 and Artin 1950, 112-119). Let me call the weaving patterns "concrete" braids, and equivalence classes of weaving patterns "abstract" braids.

By joining two concrete braids and removing the joining line, we get a third braid (cf. fig. 3).

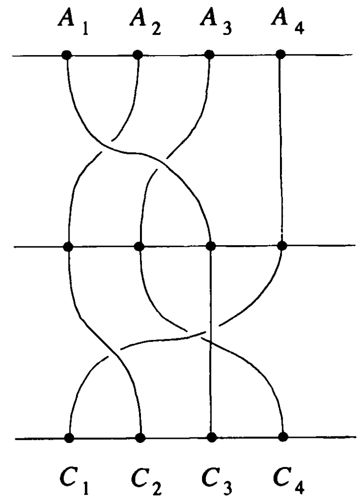
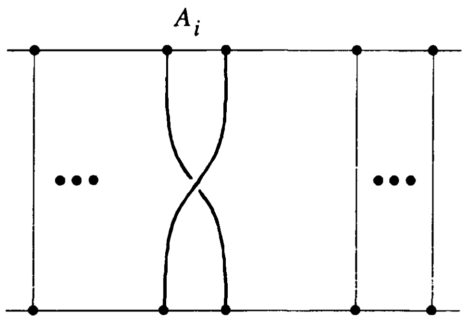


Figure 3: Joining braids

This makes abstract braids into a *group*. Artin's first step is to "arithmetize" braids, *i.e.* to give a symbolic presentation of the group of abstract braids. This is achieved by looking at the elementary braids in which only the i-th curve crosses the $(i+1)$-th (cf. fig. 4 below). These braids generate the whole group. In this way, Artin finds a new definition of the group in question (in fact it would be more precise to say: of an isomorphic group): It is the group with symbolic generators $\sigma_1, \sigma_2, \dots \sigma_{n-1}$ and relations



$$\sigma_i\sigma_j = \sigma_j\sigma_i \text{ , for } |i-j| \geq 2,$$
$$\sigma_i\sigma_{i+1}\sigma_i = \sigma_{i+1}\sigma_i\sigma_{i+1}.$$

Figure 4: The elementary braid σ_i

By this argumentative move, Artin had related the topological problem of classifying isotopy classes of concrete braids to problems of combinatorial group theory. In fact it turned out that the topological problem is equivalent to the word problem in the braid group, and Artin's main theorem presents a solution of the latter.

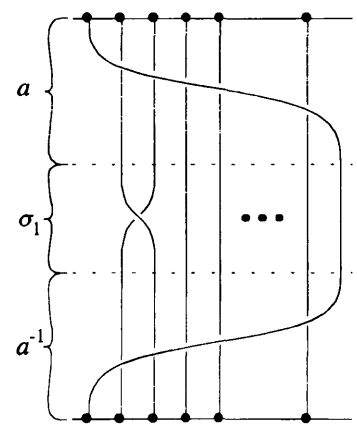
III.3 Now what is really going on here (and in the wealth of similar examples)? At first sight, we have a situation very much similar to the situation in early 19th century projective geometry. We may compare the topological point of view to the synthetic approach, and the group-theoretical standpoint to the analytic approach. In the language introduced above, we have two mathematical games, the game of weaving patterns, and the game of the symbolically defined group. However, what really matters for a description of Artin's argumentative practice is not the difference between these two mathematical games but *the way they are related.* What Artin showed is that the group-theoretical game may be *embedded*, as I shall say, into the topological one. *I.e.*, we can redescribe certain situations, rules and moves of the topological argumentation game in such a way that they appear as situations, rules and moves of the group-theoretical game. (This I take as a definition of the notion of embedding of games[17].) This embedding of group theory into topology allows Artin to change his perspective during his arguments from one to the other. In particular, and this seems to me the essential point, he has two ways

at his disposal to deal with the braid group. Either he can deal with it as a purely symbolically defined object, disregarding its topological interpretation. Or he can look at the group elements as equivalence classes of concrete braids and use the whole topological context to make arguments (provided he does not violate the necessary invariance under isotopy)[18].

Now there is clearly a significant difference between the two possibilities. The first involves only a single game. In this sense, arguments restricted to it are (relatively) abstract: they are methodologically pure, and their argumentative context is (relatively) poor. Arguments of the second alternative, however, are (relatively) concrete: they use the methods of two mathematical games, and thus also the argumentative context is (relatively) rich.

Let me give you examples of an abstract and a concrete argument about the braid group.

a) By a sequence of symbolic calculations, we may deduce that the braid group is generated by the two elements σ_1 and $a := \sigma_1\sigma_2 \dots \sigma_{n-1}$.



$= \sigma_2$

Figure 5: A braid equation

b) The same fact may be learned from the fig. 5. Iterating the idea of this figure we understand that $a^k\sigma_1a^{-k} = \sigma_{k+1}$ holds. Therefore, a and σ generate the braid group. (Here we face a typical situation: The concrete arguments seem to be intuitive. This is interesting from the pragmatical point of view, but not logically essential: Imagine the argument formulated in a rigorous language, say of piecewise linear topology. Hence it is more adequate to say: By the game change from combinatorial group theory to topology, more intuitions are made accessible which may eventually be turned into rigorous arguments.)

III.4 The situation which we encountered in the example (the embedding of an abstract game into a concrete one) is the elementary, local situation in which the difference between an abstract and a concrete argument, as I propose to use it, may be illustrated. Before I proceed to extend my description to a more global level, let me add some remarks concerning the explanations given so far.

i) It is now possible to relate the names chosen for my distinction to the formal notion of abstraction. The embedding of the example depended on a definition by abstraction in the technical sense of the term. In fact, definitions by abstraction always lead to an embedding of an abstract mathematical game into a concrete one, so that the distinction introduced above may be applied. However, this situation is only a special case of the relation between mathematical games which I called "embedding".

ii) Certainly, the above example is mathematically rather simple. Nevertheless, modern mathematical experience tells us that similar examples abound – on the elementary as well as on more advanced levels. The possibility of embeddings of mathematical games has, in fact, itself become a subject of modern mathematical research. This shows that there is no difference in rigor between an abstract and a concrete argument insofar as my present analysis is concerned. Thus it is clear that the question of using abstract or concrete arguments may again be (as in the case of an analytic or synthetic treatment of geometry) a question of style, of methodology, and not a question of substantively different mathematics.

iii) Finally, it should be emphasized that the distinction introduced above turns out to be a *relative* one. In the elementary situation of the example, concreteness comes about by means of a relation between two games. Only relative to these two games (or a more complex interrelation of mathematical games) is it reasonable to distinguish between an abstract and a concrete approach *to the same questions*.

III.5 I am now in a position to sketch a reconstruction of the global difference between an abstract and a concrete argumentative style. It is clear that modern mathematics consists of a whole network of mathematical games. The mutual embeddings provide, so to speak, the links between these games. An author like Klein seeks systematically to exhibit such embeddings, and he does not hesitate to change the game continually in order to form a convincing argument (like in the concrete argument of the example). A text like Pasch's or Hilbert's, on the other hand, restricts itself as far as possible to a single, mathematically well-defined game (in the extreme case: a single axiomatic system) and argues strictly within the context thus defined. On this level, an abstract orientation produces with great probability theorems of a rather different type than those that arise from a concrete orientation. The search for algebraic invariants of topological objects was motivated by the wish to enrich the argumentative context available for the treatment of topological (and algebraical) problems. The proof of the independence of a specific axiom with respect to a given system of axioms, on the other hand, is motivated by the intention to clarify the logical structure of a single, restricted mathematical game.

One may even go one step further towards a global picture of the abstract and the concrete mathematical styles. The games of mathematical argumentations are not only linked by internal embeddings. They are also embedded into external, non-mathematical domains of scientific and social practice. In the light of such embeddings, there is also a scale of concreteness ranging from the pure to the applied. (Think of Klein's discussion of graphical statics and of cogwheel profiles.)

III.6 Are there other mathematical disciplines in which the distinction between an abstract and a concrete argumentative style played a role in late nineteenth and early twentieth century mathematics? I think there are. I have discussed a topological example above. In fact, the development of algebraic topology provides a wealth of examples which could be analyzed in terms of abstract and concrete argumentative styles. Another field of mathematics where the distinction seems to have been relevant is number theory. Dirichlet and Riemann had shown how to embed number theory into complex analysis (by means of Dirichlet series and Riemann's ζ-function). Thus the argumentative context of number theory became richer, and Hadamard's and de la Vallée-Poussin's success in proving Gauss's conjecture on the asymptotic distribution of primes motivated a whole generation of number theorists to employ the concrete style of analytic number theory. On the other hand, an elementary, abstract approach finally succeeded in proving the prime number theorem, too (Erdös and Selberg). A revival of elementary number theory was the consequence (Echeverria 1992, 249ff.). As in the case of geometry it seems to be the analytical side which tends to methodological diversity, while the synthetical, elementary side is committed to methodological purism.

It is an interesting question whether the shift which I described in the development of geometry could be related to the shift in the philosophical conceptions of mathematics from Kant to the end of the 19th century. Whereas Kant's philosophy of mathematics was centered on the analysis-synthesis distinction, two of the most important thinkers in philosophy of mathematics of the end of the century, namely Frege and Husserl, tried hard to make clear the second distinction as applied to mathematics.

IV The Role of Concrete and Abstract Argumentative Styles in Mathematical Modernity

IV.1 It seems that the shift from the controversy about analytic and synthetic geometry to that between a concrete and an abstract style of geometrical argumentation described in the first part of this paper is related to the formation of what has been called "mathematical modernity". Let me now turn to explaining briefly how the distinction introduced above could contribute to a better understanding of the modernity of modern mathematics.

Herbert Mehrtens has drawn an impressive and detailed picture of the process of mathematical modernization in his book, *Moderne–Sprache–Mathematik* (1990). Mehrtens tries to show that there are two fundamentally different types of reactions to the changes in 19th century mathematics. The first, in an emphatic sense modern reaction, was to fully accept the new autonomy and to pursue mathematics as a free, creative enterprise, with no bounds on mathematical production other than internal coherence and success. Among the modernists, Mehrtens points to pure mathematicians like Cantor, Hausdorff, and Hilbert as the "general director." On the other hand, there is a second type of reaction which tries to re-establish the threatened ontological basis and epistemic certainty of mathematical knowledge and the links of mathematics to science under the new conditions. A typical representative of this counter-modern type of reaction is Felix Klein, who was engaged in reforming mathematics at technical universities, and who favoured applied mathematics while constantly emphasizing the role of intuition as a basic pre-requisite for doing mathematics.

Mehrtens' thesis is that the modern and the counter-modern attitudes together provided a framework for mathematicians' sense of self-identity at the beginning of the twentieth century. These attitudes helped to justify mathematical research, and played a role in the fight for positions and prestige. The professional politics of the two Göttingen leaders, Hilbert and Klein, was determined by the difference between modern and counter-modern attitudes as well as the later *Grundlagenkrise* between "formalists" and "intuitionists." While in the case of Hilbert and Klein, their different attitudes did not preclude the possibility of "forging of an intellectual alliance" between the two in the fight for Göttingen mathematics (Rowe 1989, 195 ff.), after the take-over by the German National "Socialists," there appeared, according to Mehrtens, a fatal connection between radical counter-modernists and the fascist ideology.

IV.2 In order to draw his picture, Mehrtens needs criteria which allow him to place his actors on the modern/counter-modern scale. In fact, his historical narrative tries to exhibit such criteria along the way. The autonomy of modern mathematics is best described, so he claims, by viewing mathematics as the production

of a language, the meaning and uses of which are not determined beforehand. ("That, by which the discipline of mathematics identifies itself, is the self-referential language Mathematics in the products of the mathematicians, *i.e.* the texts." (Mehrtens 1990, 404)) Consequently, the difference between the modern and the counter-modern attitude must be expressible in terms of the attitude towards mathematical language. Mehrtens uses the linguistic distinction between "signifying" and "signified" to describe this difference. He writes: "The modern and the counter-modern conception give rise to different conceptions of the realm of mathematical language. Modernity is oriented in the Hilbertian formalism at the signifiers which it interprets as the empirically treatable signs on the paper. Counter-modernity resorts to an *a-priori* psychology by postulating a unifying subjectivity with the gift of an original intuition, in which all mathematicians partake" (*ibid.*, 414). And due to this *Ur*-intuition, there is a guarantee of access to that which is "signified."

The main criterion for being a modern is thus, in Mehrtens' view, whether one is prepared to dispense with an explanation of what the meaning of mathematical language is, be it the meaning of mathematical expressions like "point", "line", "field" etc., or even the cultural meaning of mathematical discourse as a whole. A counter-modern, on the contrary, would insist on precisely that. Mehrtens illustrates this criterion with Hilbert's *Foundations of Geometry*, which in fact does without an explanation of the meaning of the basic notions like point, line, etc. From this standpoint, Frege's critique of Hilbert's axiomatic definitions may be the philosophically most self-conscious counter-modern attack on modernism. It revealed that not only questions of the semantics of mathematical language are concerned but also questions of mathematical truth and questions pertaining to what mathematics is really *about.*

IV.3 Mehrtens' book is an example of a very elaborated kind of external historiography. His sources are mainly the programmatic declarations of the mathematicians involved and the documents of their institutional activities. Mehrtens does not attempt to analyze some of the more advanced productions of modernist or counter-modernist mathematicians, and, in fact, he makes no claims about the internal construction of modern mathematics. Thus we are left in a somewhat unclear position if we accept his narrative. Was the struggle between moderns and counter-moderns only a meta-mathematical drama, staged for reasons of self-interpretation and disciplinary politics? Or does the conflict also manifest itself in the "regular discourse of mathematics," as Mehrtens described it, *i.e.*, in the research activities and programs, in the mathematical writings of the period under consideration? Apart from some rather general remarks on the semiotic structure of modern mathematical texts (*ibid.*, ch. 6.3), Mehrtens leaves this question entirely open.

In any case, Mehrtens' thesis would lose much of its attractiveness, if it could not be complemented by an analysis of the modernity or counter-modernity of pieces of mathematical research. Thus we may ask: is there a difference between Hilbert's and Klein's, or between Landau's and Bieberbach's mathematics? That is, between the styles of their mathematical texts, the mathematical games they played? As Mehrtens is silent on this point, we are free to look for our own answers to these questions.

IV.4 Evidently, there is a difference between a text such as Klein's *Lectures on Higher Geometry* and Hilbert's *Foundations of Geometry.* I have tried to describe this difference in the second part of this article and I ventured at a philosophical analysis of its core in the third. Thus the question arises whether we could reasonably use the distinction between an abstract and a concrete argumentative style as an internal criterion for the degree of modernity of a mathematical text. A typical modern piece of mathematics should then argue in a strictly abstract fashion, while counter-modern texts should be written with a concrete style of argumentation. For the two texts of Klein and Hilbert, the statement holds.

In fact there is some evidence in favour of such a proposal. The form of mathematical texts and the type of mathematical questions discussed in the first decades of the twentieth century show strong variations on the scale concrete/abstract. To mention two other names: Henri Poincaré, a counter-modern according to Mehrtens' classification, introduced the fundamental group and the homology groups of a manifold. In this way, he established a far-reaching embedding of the games of group theory into those of geometry, or rather, topology. Felix Hausdorff, placed among the moderns, became famous for his axiomatization of the game of set-theoretical topology.

Let me add immediately that a schematic thesis of the type: "Moderns only wrote abstract texts, counter-moderns only concrete ones" seems very problematic. Counterexamples are too obvious. Frege's *Fundamental Laws of Arithmetics* (1893-1903) are evidently abstract in the sense introduced here, and hence should be called a modern text according to my criterion. On the opposite side, one could mention Hausdorff's very concrete proof that there exist non-measurable subsets of the circle and the sphere (Hausdorff 1914, 428-433), not to speak of much of Hilbert's mathematical work. Rather, the use of this criterion to judge the modernity of a piece of mathematics will lead to modifications of Mehrtens' picture. A grey scale will appear between the white moderns and the black counter-moderns. And I think it will also become clear that (and how) concrete and abstract argumentative styles stimulated each other.

IV.5 Nevertheless, differences in mathematical style existed, and often they corresponded to the metamathematical views of the authors. This correlation would

find a partial explanation if we could relate Mehrtens' semantic criterion for being a modernist to the internal criterion of an abstract argumentative style.

In order to establish such a relation we have to ask whether the use of abstract or of concrete arguments leads, or may lead, to different attitudes toward the meaning of mathematical language. Let us go back to the example of the braid group. In fact the difference between viewing the group elements *a*) as words in the symbolic generators σ_i, or *b*) as isotopy classes of weaving patterns, can be described as a difference in semantics. Disregarding the topological game means considering braid words as uninterpreted strings of symbols. The only possibility of ascribing meaning to them is to explain the rules governing their use in the argumentation game we play. If we connect the group theoretical game to the topological game, we open up the possibility of an interpretation of the group symbols: we may call the "isotopy class of concrete braids with one positive twist between the first two threads" the ***meaning*** of the symbol σ_1[19]. Thus the passage from an abstract to a concrete perspective on a mathematical game creates meaning, while the converse passage suspends it.

In this way, we have found, on the local level, a counterpart to Mehrtens' criterion of meaning. The language of abstract arguments is, relative to the given embedding of mathematical games, devoid of that element of meaning which a concrete argument exploits to enable game changes. It seems quite probable that mathematicians who strove for axiomatizations developed a distaste for the varieties of meaning alluded to in concrete argumentations. These meanings occupied the mathematical mind, tending to obscure the logical structure of an argument or a theory. Authors like Klein or Weyl, on the other hand, must have been fond of every new facet of meaning which they could exhibit in mathematical language.

The relativization of Mehrtens' criterion of meaning to an embedding of mathematical games even allows one to reconstruct some of Mehrtens' statements about the attitude of mathematicians towards the cultural meaning of mathematical discourse. If mathematical argumentation moves in a complex network of mathematical games, the outer ends of which are embedded into non-mathematical practice, then a concrete argumentative style in the outer parts of the net creates meaning *outside* the cultural system called 'mathematics'. Klein's love for concrete arguments goes a long way toward embeddings of mathematical argumentations into non-mathematical contexts. (Again I come back to the cogwheels.) The least one can say is that this corresponds to his conviction that mathematics had a meaning for physicists, or for engineers.

IV.6 To finish, I want to discuss once again Artin's braids, but now from a historical point of view. This is meant to illustrate the use of the concrete-abstract distinction as a criterion for the modernity of mathematical argumentations in historiographical practice.

The original context for the topological objects called 'braids' by Artin was the theory of Riemann surfaces, viewed as branched coverings of the complex plane. After some earlier results on two-sheeted surfaces, Hurwitz investigated in 1891 n-sheeted Riemann surfaces with a finite number k of branch points. In particular, he counted the number of inequivalent surfaces for low n and k which had only simple branch points, *i.e.* points where exactly two sheets of the branched covering meet. Hurwitz' text is certainly concrete in my sense: he defined the surfaces by the then usual cutting and pasting techniques, thus aiming at a *topological* definition of Riemann surfaces (without explicit reference to complex function theory). In the next step, he translated the problem of classifying these surfaces into a *group-theoretical* problem. (To every surface, there corresponds a transitive subgroup of permutations of the sheets, generated by the permutations arising at branch points. The associated presentation of this group determines the surface.) Thus he established an embedding of mathematical games.

In the course of his arguments, he came to consider the following situation: Suppose that, for a given surface, we move the branch points in the basis of the covering in such a way that they never meet, but reach a permutation of the original point configuration in the end. By continuously deforming the surface along the way, we arrive at a new surface with the same number of branch points and sheets in the end. Viewing time as a third dimension, we see that the movement of the branch points in the base plane forms a braid! (Imagine the branch points originally on a line; cf. fig. 6.) In fact, Hurwitz showed that (isotopy classes of) these movements form a group, and that they induce a transitive action of this group (to be called braid group only later) on the set of Riemann surfaces with n sheets and k simple branch points.

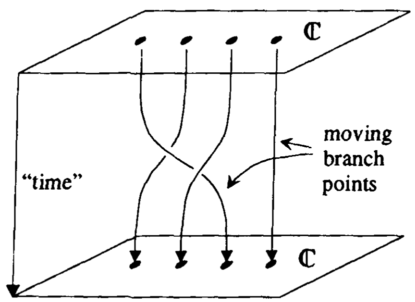


Figure 6: Moving branch points of Riemann surfaces

Thus, the original context of the study of braids is a typical, rich context of argumentation, involving geometric, complex analytic, and group-theoretic ide-

as[20]. This rich argumentative context almost completely disappeared in Artin's definition of the braid group in 1925. There are few doubts that Artin was aware of Hurwitz's work and that it was his deliberate decision not to mention it. (Rather, he placed his braids into a more recent context, namely the problem of classifying knots.) Artin's move even led to a quite common opinion that he had actually invented the braid group.

Artin not only restricted his argumentative context by cutting off the connection to Riemann surfaces and function theory, but originally he even aimed at a purely abstract argumentation on the local level described in III.2. He hoped to solve the classification problem for braids by solving the word problem of the braid group using only methods of the group-theoretical game. This hope is documented in his acknowledgements to his colleague Schreier: "My special thanks are due to Mr. Otto Schreier, who forcefully supported me in the writing of this paper, in particular with the complicated calculations by means of which we first hoped to get through" (Artin 1925-1926, 47). Thus the argumentative strategy seemed clear enough: *i*) define braids topologically, *ii*) "arithmetize" braids, *i.e.* introduce the argumentation game of group theory, and then *iii*) solve the classification problem exclusively in the latter. It is even possible that the main intention of the paper was not to contribute to knot theory by classifying topological braids, but rather to find an interesting example of a group presentation with a non-trivial, but solvable word problem[21].

The tendency toward abstract argumentation makes Artin's paper on braids a modern piece of mathematics. This is in agreement with his general position in German mathematics in the twenties. His lectures on algebra were one of the sources of the strictly axiomatic approach of van der Waerden's *Modern Algebra* (1930-1931); the other being Emmy Noether's work, of course. From 1926 to 1937, when he was dismissed by the Nazis, he held one of the chairs at the Mathematical Seminar at the University of Hamburg, which certainly was one of the liveliest centers of mathematical modernity in Germany during the decade before 33. (The other chairs were held by Blaschke and Hecke. The activities of the seminar are documented in the very successful journal of the seminar, the *Hamburger Abhandlungen.*)

However, the abstract strategy of solving the word problem of the braid group did not quite work. The symbolic calculations which Artin and Schreier undertook turned out to be tedious, and the solution which Artin gave in the paper rests essentially on topological arguments and on frequent changes between the group-theoretic and the topological argumentation game. A close analysis of the proof even shows a certain "return of the repressed": the topological methods employed have a strong connection to the methods which had been used earlier in the context of complex function theory. (In particular, this holds for the method of calculating the fundamental group of a closed braid, which was essential for Artin's argument. The method was due to Wilhelm Wirtinger, a Viennese mathematician who specialized in algebraic function theory.) Hence, counter to his original intention, Artin was forced into a concrete argumentative style.

In later years, Artin was completely dissatisfied with the argument he gave in 1925. He felt it was too intuitive, and the proof of the main theorem was, as he said, "not even convincing" (Artin 1947, 101). Again we see the abstract impulse of modernity. Nevertheless, even the second attack on the braid group did not achieve a purely group-theoretical treatment. Instead, concrete braids were defined more cautiously in order to make rigorous topological arguments available. When Artin wrote a popular article on braids in 1950, he emphasized that "the theory of braids shows the interplay of two disciplines of pure mathematics—topology, used in the definition of braids, and the theory of groups, used in their treatment" (Artin 1950, 112).

Perhaps these remarks mirror some general features of the fate of the abstract style in mathematical modernity. The tendency towards abstract reasoning probably revealed more about the hopes of committed modernists than it did the structure of the actual arguments at the cutting edge of mathematical research. The rigorous axiomatization of mathematical theories even made it possible to clarify the relations between different mathematical games in such a way that concrete arguments lost the flavour of being intuitive and imprecise, as was the case in the braid example. Some of the deepest research of modern mathematics concerned the relations between different mathematical games (or structures, if you wish), but there are few examples where a single mathematical game was carried on for a long time *without* being related to other ones.

Of the two modern lines of geometry at least, the strictly abstract approach of Pasch and Hilbert seems soon to have lost its fertility, while the branches of differential and algebraic geometry lead to exciting results and open questions up to the present day. Not only the strict adherence to the methodological purism of analytic or synthetic geometry, but also the adherence to the methodological purism of abstract argumentations led, as Klein had said, to a "certain petrifaction".

University of Mainz
Department of Mathematics

Notes

1 Cf. *e.g.* Aristotle, *Metaphysics*, 1029*a*, 1061*b*; *Second Analytics*, 92*b*.

2 Cf. the survey by Thiel (1988). See also below, III.4.

3 On Monge, compare Glas (1985).

4 For general historical information about the development of projective geometry, see *e.g.* Kline, (1972, ch 35). A wealth of information is contained in Felix Klein's *Vorlesungen über die Entwicklung der Mathematik in 19. Jahrhundert* (1926-1927).

5 Cited after Kline (1972, 836).

6 This characterization eventually evolved in a modern mathematical notion of projective space: the projective space of a vector space is the set of its one-dimensional linear subspaces.

7 Two "purist" classics of 19th century geometry are mentioned in Klein's *Lectures on Higher Geometry* of 1893 (to be discussed below): "Hesse (1861) purely analytic; Reye (1866-1867) (purely synthetic). Both methodically one-sided, but in their treatment very elegant." (Klein 1893, 5).

8 Certainly, the discovery of the non-Euclidean geometries contributed essentially to the need for a clarification of the logical foundations of geometry. However this can hardly be the "only" reason for axiomatic thinking in geometry (which was then a common trend in other parts of mathematics as well).

9 Klein (1908-1909, II, 55 f.):

> "The analytic geometricians often lost themselves in blind calculations, devoid of any geometric representation. The synthesists, on the other hand, saw salvation in an artificial avoidance of all formulas, and thus they accomplished nothing more, finally, than to develop their own peculiar language formulas, different from ordinary formulas. Such exaggeration of the essential fundamental principles into scientific schools leads to a *certain petrifaction*; when this occurs, stimulation to renewed progress in the science comes principally from 'outsiders'."

10 For the relations between Pasch's and Hilbert's work, see Toepell (1986, in particular 51 ff.).

11 Certainly, it may be objected that Klein's *Lectures on Higher Geometry* represented a rather singular way of treating geometry. However, I hope it will become clear in the following that the stylistic differences on which I focus here are characteristic not only for texts like Klein's and Pasch's.

12 Unfortunately, questions of mathematical pragmatics are still rather unexplored in recent philosophy of mathematics. This is partly due to the fact that the Fregean tradition has focused on parts of mathematics which are far from actual mathematical practice (such as elementary arithmetic). With the revival of methodological and epistemological questions (Lakatos, Benacerraf, Kitcher), the situation has changed to some extent. There seem to be quite a number of valuable ideas still waiting to be unearthed in the non-logicist classics of twentieth century philosophy of mathematics as, *e.g.*, Husserl or Wittgenstein.

13 For an account of the history of the comparison between mathematical practice and games, see my (1994). David Bloor has developed an "anthropological" perspective on mathematics as a system of language games in his (1983). Although I doubt that my view of mathematical games coincides with his notion of language games, some of the remarks below might contribute to his perspective.

14 It seems possible to formalize the notion of a mathematical game: one would then be led to a pragmatic interpretation of the formal systems which Hilbert introduced in his metamathematical work. However, a rigid notion of mathematical games certainly would restrict the range of phenomena in mathematical practice to which it could be applied in an instructive way.

15 Bloor speaks of a "superposition of language games" (1983, 110ff).

16 Cf. Thiel (1988).

17 Or of "superposition", cf. note 15. Whether or how this definition applies to the embedding of mathematical into social games—the situation which interests Bloor most—will be left open here. Also Lakatos stresses the importance of embeddings of contexts of argumentation into each other, cf. *e.g.* (1976, ch. 1, section 2).

[18] In fact, we have three ways to play a braid game (that is, to investigate braids): 1) We may only look at the topological definitions, disregarding the embedded group structure. This we could call the "synthetic braid game". 2) We may only look at the group presentation, disregarding the topological context. The "analytic braid game". 3) We may interpret the group sometimes topologically, sometimes symbollically, using both methods as it suits in studying braids. The "mixed braid game." Only in the last two cases, the object of argumentation is really the braid *group*. Thus the alternative above.

[19] We are not compelled to interpret this type of meaning as reference. We may equally view it from the standpoint of a "use" theory of meaning: by embedding the group-theoretical game into topology, we can make a different use of the symbol σ_1 than without. It is this extension of its possible use which gives a new "meaning" to the symbol, not necessarily its connection to an object.

[20] To Hurwitz' ideas, one must still add the connection between braids and the mapping class group of the complex plane with n points removed, which appeared in Fricke and Klein (1897-1912, I). Cf. Magnus (1974).

[21] Combinatorial group theory was still in its beginnings, and there was considerable need for good examples. Cf. Magnus (1974), and Chandler and Magnus (1982).

II. Philosophy

PETRI MÄENPÄÄ

FROM BACKWARD REDUCTION TO CONFIGURATIONAL ANALYSIS

I Introduction

Ancient Greek geometers devised the method of analysis and synthesis for solving construction problems. According to Pappus (*ca.* 300 *AD*), it was also used for proving theorems, the other class of propositions conceived by the Greeks. He gave the only extensive ancient methodological account of analysis that survives. The term "analysis" has a variety of usages, but only this mathematical one is studied here.

Pappus described analysis as the reduction of a proposition to be solved or proved successively backward to its antecedents until arriving at a proposition whose solution or proof is known (Section II). This is the "directional interpretation" of analysis.

Modern studies of analysis in terms of the directional interpretation have focused on its logical character. The question has been whether the analysis of the ancients is deduction or reduction, which is not deductive in general. Hintikka and Remes (1974), notably, try to read the latter interpretation into Pappus's description. This is forced, because almost all examples of analysis in the Greek mathematical corpus are in fact deductions. Of course, these deductions are also reductions, because they are to be convertible into syntheses, but there is little evidence of non-deductively reductive analyses. I shall call such analyses "purely reductive".

The few examples of Greek purely reductive analyses were devised by commentators rather than mathematicians with original contributions (Knorr 1986, ch. 8). The first purely reductive directional interpretation of analysis in a methodological description that I know of is by Duhamel (1865, ch. X and XI). He goes so far as to regard the deductive analysis of the ancients as defective, because it ignores concerns of convertibility of an analysis into a synthesis. He says further that modern analysis, which is (purely) reductive, does not suffer from this defect. It is trivially convertible.

But purely reductive analysis appears in mathematical practice much earlier: Galileo's manuscripts on mechanics contain a purely reductive analysis (Mäenpää

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 201–226.

1993, section 7.2). Nevertheless, it doesn't seem to appear in the methodological discussions of the 1600's. Analysis was discussed extensively then, notably by Descartes and Newton. Their conception of analysis is deductive in methodological accounts as well as in mathematical practice.

Mathematical language and method changed decisively around 1600 in the hands of Viète and Descartes. They introduced a new kind of algebra, explicitly based on the ancient Greek method of analysis (Section III). The main innovation of their algebraic language was the introduction of variable symbols for all given and unknown quantities. The Greeks used no variable symbols before Diophantus introduced one in his *Arithmetic* in *ca* 250 AD. (The present account deals with Descartes only, see Mäenpää 1993, ch. 5-7 for Diophantus, Viète, and Newton.)

At the same time, Descartes's methodological description of his algebraic method of analysis introduced an important novelty with respect to Pappus's description. Descartes said that analysis serves to determine how the unknown quantities of a problem depend on the given ones. Instead of seeking a deductive connection between the proposition to be solved or proved and propositions whose solution or proof was known, Descartes sought to determine the dependencies of the unknown quantities on the given ones. This is the "configurational interpretation" of analysis.

On the face of it, the configurational interpretation is a simple specification of the directional one. The analyst works backwards by reduction from the sought conclusion to given premisses (Pappus). More specifically, he thereby establishes a dependency of the sought quantities on the given ones (Descartes).

But this specification has deeper methodological and logical significance. It shifts the focus of the analytical method from the analysis of a deductive connection to the analysis of what is in more modern terms a "functional" connection. Analysis is, according to the configurational interpretation, a study of the functional dependencies in a mathematical configuration with known as well as unknown constituents.

In the Greeks' twin method of analysis and synthesis, synthesis served to put together the sought objects from the given ones, making use of their functional dependencies uncovered in analysis. This concerns problems. In the case of theorems, the task of synthesis was to convert the analysis into a demonstration of the proposition to be proved from ones known to be true.

This informal description gives the impression that the configurational interpretation suits problem solving better, while the directional interpretation suits theorem proving. To get a more precise and deeper understanding of the situation, we shall describe the analytical method in formal terms. This is intended as a theoretical explanation of the configurational and directional interpretations. It aims at finding a theoretical structure behind the phenomena, so to say, of the examples of analysis in the mathematical literature and of informal methodological accounts. Devising such a theoretical explanation is quite clearly a task that calls for a logical formalism as a conceptual tool. The resulting reconciliation of the two interpretations of analysis serves to spell out in relevant theoretical (logical) terms what they are and how they relate to one another.

The question of how the analysis of problems relates to the analysis of theorems is also a logical question. This is why it is answered in the most satisfactory way, from the systematic point of view, in terms of a logical formalism. In particular, the formalism must describe adequately functional dependencies between configurations (=constructions) as well as deductive connections between propositions.

Descartes's algebraic analysis has had a remarkable success due to its problem-solving power. It soon became the *lingua franca* of the exact sciences, and that it remains today. What is more, it has served as a standard system of forms of understanding ancient historical materials in mathematics beginning from Zeuthen in the late 1800's (cf. *e.g.* Zeuthen 1893). Yet the reduction of ancient historical materials to Cartesian algebra does not preserve mathematical content. One possibility of dealing with this difficulty is to refrain from using anachronistic concepts as forms of historical understanding. Another possibility, which is made use of here, is to employ a system of concepts that is general enough to preserve mathematical content in full.

We shall then be in a position to see, for instance, the precise difference in meaning between the informal expressions

"deduction of a construction",
"deduction of a proposition"

current in modern studies of ancient mathematics. This has not been possible before, because there has been no conceptual system for relating the notion of construction to the notions of deduction and proposition in a satisfactory way before constructive type theory (from now on: type theory), which we shall employ here (Section IV). Type theory (Martin-Löf 1984) is one of the main current approaches to the foundations of mathematics and computing science.

This formal system of concepts helps us to understand the systematic source of the heuristic usefulness or problem-solving power of analysis. Furthermore, it lets us see new things in historical and informal mathematical materials, using the new forms of understanding.

Hintikka and Remes (1974 and 1976) brought the configurational interpretation into recent methodological discussion, and coined the names of the two interpretations. They described analysis in terms of predicate logic, both the configurational and the directional interpretation. They also refuted conclusively

Mahoney's (1968) claim that the analysis of problems is not a method that can be described in logical terms, in contrast to the analysis of theorems.

Analysis is, in their configurational interpretation, a study of the functional dependencies among the constituents of a definite mathematical configuration. In the case of geometry, for example, the configuration is a geometrical figure. They also introduced the term "constructional interpretation" as a synonym for configurational interpretation, and "propositional interpretation" as a synonym for directional interpretation.

Besides bringing the modern methodological study of analysis to a new, theoretical level of precision, by employing modern logical concepts, they identified the crucial heuristic role of "auxiliary constructions" in analysis. Taking apart a definite configuration into its constituents is routine compared to inventing the auxiliary constructions that are needed to amplify the configuration in order to find the solution to nontrivial problems (Section V). Auxiliary constructions are in fact indispensable also in finding the proof of nontrivial theorems, and this is one important logical connection between the analysis of problems and of theorems. Hintikka and Remes describe also auxiliary constructions in terms of predicate logic.

Now it has turned out that the logical tools used by Hintikka and Remes do not suffice for a natural logical description of the configurational interpretation and of auxiliary constructions (Mäenpää 1993). The systematic reason for this is that predicate logic does not recognize constructions. In its stead, I use type theory, which enriches predicate logic with a functional hierarchy that exactly captures on the formal level the informal notion of synthesis as functional composition of constructions of various types, like points, circles, and line segments in geometry, and of analysis as its inverse operation, functional decomposition of a construction into its constituents.

Despite its introduction of quantifiers and individuals, predicate logic is still too close to propositional logic in order to serve as a formal tool for describing the analysis and synthesis of constructions adequately, which the configurational interpretation of analysis requires. Propositional and predicate logic suit the directional interpretation better. It turns out that predicate logic fails, for instance, to describe geometrical construction postulates, which are used in solving geometrical problems.

Auxiliary constructions receive a logical description that is eminently natural in view of the informal way of understanding them as constructions that are not constituents of the configuration originally subjected to analysis (Section VI). That is, auxiliary constructions are constructions that are constituents of neither the given nor the sought objects.

II The Directional Interpretation of Analysis: Pappus's Description

The directional interpretation of analysis runs as follows in Pappus's classical description in the seventh book of his *Mathematical Collection* (the English translation is from Jones's edition of Pappus (CJ, 82-85); I have added the Greek terms in square brackets). Part of the description may originate in older sources, probably in Euclid (Knorr 1986, 354-360).

> "That which is called the Domain of Analysis, my son Hermodorus, is, taken as a whole, a special resource that was prepared, after the composition of the Common Elements, for those who want to acquire a power in geometry that is capable of solving problems set to them; and it is useful for this alone. It was written by three men: Euclid the Elementarist, Apollonius of Perge, and Aristaeus the elder, and its approach is by analysis and synthesis.
>
> Now analysis is the path from what one is seeking [*zetoumenon*], as if it were established, by way of its consequences [*akoloutha*], to something that is established by synthesis. That is to say, in analysis we assume what is sought [*zetoumenon*] as if it has been achieved, and look for the thing from which it follows, and again what comes before that, until by regressing in this way we come upon some one of the things that are already known, or that occupy the rank of a first principle. We call this kind of method 'analysis', as if to say *anapalin lysis* (reduction backward).
>
> In synthesis, by reversal, we assume what was obtained last in analysis to have been achieved already, and, setting now in natural order, as precedents, what before were following, and fitting them to each other, we attain the end of the construction of what was sought [*zetoumenon*]. This is what we call 'synthesis'.
>
> There are two kinds of analysis: one of them seeks after the truth, and is called 'theorematic'; while the other tries to find what was demanded, and is called 'problematic'. In the case of the theorematic kind, we assume what is sought [*zetoumenon*] as a fact and true, then, advancing through its consequences [*akoloutha*], as if they are true facts according to the hypothesis, to something established, if this thing that has been established is a truth, then that which was sought [*zetoumenon*] will also be true, and its proof [*apodeixis*] the reverse of the analysis; but if we should meet with something established to be false, then the thing that was sought [*zetoumenon*] too will be false. In the case of the problematic kind, we assume the proposition as something we know, then, proceeding through its consequences [*akoloutha*], as if true, to something established, if the established thing is possible and obtainable, which is what mathematicians call 'given', the required thing [*protathen*] will also be possible, and again the proof [*apodeixis*] will be the reverse of analysis; but should we meet with something established to be impossible, then the problem too will be impossible. Diorism is the preliminary distinction of when, how, and in how many ways the problem will be possible. So much, then, concerning analysis and synthesis."

The translation of certain Greek terms deserves comment. Issues of translation depend on how the logical character of analysis is understood.

Pappus calls analysis as applied to theorem proving "theorematic' and as applied to problem solving "problematic" in Jones's translation. I use the terms "theoretical" and "problematical" instead, because they have become standard, although Jones's terms avoid the ambiguity inherent in "theoretical" between the terms "theorem" and "theory".

Jones translates "*anapalin lysis*" as "reduction backward", whereas Heath (in his translation of Euclid's *Elements*, I, 138-139) translates it as "backward solution". Hintikka and Remes (1974, 8-10) follow Heath. I find Jones's translation preferable, because Pappus describes analysis as a method that applies also to theorem proving, not only to problem solving. Note however that Pappus does not use the technical term "*apagoge*" for reduction here. The term "*lysis*" is nontechnical (Knorr 1986, ch. 8). Knorr translates "*lysis*" as "resolution", but I prefer not to do so, because I shall use resolution as a technical term for the second part to be distinguished in analysis.

In sum, Pappus says that if the end-point of analysis is an impossible problem (or absurd theorem), then synthesis is not needed, and the original problem is also impossible (or the original theorem absurd). That is, analysis constitutes a *reductio ad absurdum*. This is quite conclusive evidence for the interpretation that Pappus conceives analysis as deductive, because a purely reductive analysis could not constitute a *reductio ad absurdum*.

If analysis leads to a problem whose solution is known (or a theorem whose proof is known), a synthesis is needed. The synthesis reverses the analysis and yields a solution to the original problem (or a proof of the original theorem). Pappus's description of synthesis as complementing analysis would be pointless if he regarded analysis as purely reductive, because this would make synthesis trivial and superfluous.

Strangely enough, Pappus does not have anything to say about the nontriviality of this reversal. He does mention that the analyst must in general determine the conditions of solvability of a problem, the "diorisms [*diorismos*]". They are part of establishing reversibility, because they are conditions under which an analysis is reversible.

Hintikka and Remes translate "*akoloutha*" as "concomitants" in order to leave room for their interpretation of analysis as a purely reductive procedure. Previously "*akoloutha*" had been translated as "consequences". The evidence provided by the Greek mathematical corpus renders Hintikka and Remes's translation implausible, because the extant Greek analyses are deductive, with the few exceptions devised by commentators. It is hardly conceivable that Pappus, in describing the analytical works of the corpus, should have described analysis in a way that is not consistent with those works.

On the other hand, an important precursor of analysis was the method of reduction (*apagoge*), which was not deductive (Knorr 1986, 23-24). A well-known application of *apagoge* is Hippocrates's (pre-Euclidean) reduction of the problem of duplicating a cube to the problem of finding two mean proportionals between two given line segments. Proclus, who flourished in the fifth centrury *AD*, says in his commentary of Euclid's *Elements* (PEEL, 212-213) that

> "'Reduction [*apagoge*]' is a transition from a problem or a theorem to another which, if known or constructed will make the original proposition evident. For example, to solve the problem of doubling the cube geometers shifted [*metethesan*] their inquiry to another on which this depends, namely, the finding of two mean proportionals; and thenceforth they devoted their efforts to discovering how to find two means in continuous proportion between two given straight lines. They say that the first to effect reduction of difficult constructions was Hippocrates of Chios, who also squared the lune and made many other discoveries in geometry, being a man of genius when it came to constructions, if there ever was one."

(I have inserted some Greek terms in square brackets from Friedlein's edition.) Neither Pappus nor anyone else of the ancients seems to relate analysis to *apagoge* methodologically. The only testimony we have is their mathematical practice. Judging from that, analysis and *apagoge* seem to have been distinct methods, and the modern purely reductive interpretation of analysis applies to *apagoge* rather than to analysis in Greek mathematics.

III The Configurational Interpretation of Analysis: Descartes's Description

Descartes introduced his algebra as a new tool for solving mathematical problems. It turned out so powerful that those problem domains that it applies to were studied in great depth, while those falling outside its scope received less attention after Descartes. Its application in geometry, in particular, required that geometry, as practised in the tradition established by Euclid and his contemporaries, be abstracted to algebra.

The non-algebraic aspects of geometry gradually fell out of the scope of what is now known as analytic geometry. A case in point is an elementary construction problem like the first proposition in Euclid's *Elements*, to construct an equilateral triangle on a given line segment. This is why Descartes's method of algebraic analysis is not a general mathematical method. To study analysis in all its generality requires a system of concepts that does not reduce mathematical content. This requirement concerns the systematic as well as the historical point of view. Cartesian algebra has been widely used as a system of concepts for studying ancient geometry historically, but this approach falls short of describing the historical materials in full, because it abstracts the geometrical materials to algebraic forms.

Here is how Descartes describes his analytical algebraic method. Rule Seventeen of his *Rules for the Direction of the Mind* (ROP) reads as follows (quoted from Descartes PW, I, 70-71).

> "We should make a direct survey of the problem to be solved, disregarding the fact that some of its terms are known and others unknown, and intuiting, through a train of sound reasoning, the dependence of one term on another.
> [...] the trick here is to treat the unknown ones as if they were known. This may enable us to adopt the easy and direct method of inquiry even in the most complicated of problems. There is no reason why we should not always do this, since from the outset of this part of the treatise our assumption has been that we know that the unknown terms in the problem are so dependent on the

known ones that they are wholly determined by them. Accordingly, we shall be carrying out everything this Rule prescribes if, recognizing that the unknown is determined by the known, we reflect on the terms which occur to us first and count the unknown ones among the known, so that by reasoning soundly step by step we may deduce from these all the rest, even the known terms as if they were unknown."

And in his *Geometry* (1637), Descartes says in his classical description of analytical algebraic problem solving (quoted from Descartes GSL, 6-9) that:

"If, then, we wish to solve any problem, we first suppose the solution already effected, and give names to all the lines that seem needful for its construction,—to those that are unknown as well as to those that are known. Then, making no distinction between known and unknown lines, we must unravel the difficulty in any way that shows most naturally the relations between these lines, until we find it possible to express a single quantity in two ways. This will constitute an equation, since the terms of one of these two expressions are together equal to the terms of the other. We must find as many such equations as there are supposed to be unknown lines; but if, after considering everything involved, so many cannot be found, it is evident that the question is not entirely determined. In such a case we may choose arbitrarily lines of known length for each unknown line to which there corresponds no equation. If there are several equations, we must use each in order, either considering it alone or comparing with the others, so as to obtain a value for each of the unknown lines; and so we must combine them until there remains a single unknown line which is equal to some known line [...]."

Descartes shifts the focus of analysis from the deductive connection between propositions known to be true and the proposition to be proved to the dependencies, that is, the functional connections, between the known and unknown terms of a problem. Notice also the shift in terminology: where Pappus connects "something established" or "known" or "given" to "what is sought", Descartes connects "known terms" to "unknown terms", the "terms" now obviously referring to quantities, not propositions.

Descartes is concerned with problem solving exclusively. Pappus, on the other hand, uses the word "*zetoumenon*" for what is sought neutrally with respect to theoretical and problematical analysis.

In introducing the configurational interpretation into modern methodological discussion, Hintikka and Remes don't seem to have been aware that it was introduced by Descartes. They even say that "Descartes insists on discussing methodological matters in propositional terms or at least in terms of sequences of steps of thought" (Hintikka and Remes 1974, 103).

IV Logical Form in Analysis

Consider the elementary geometric construction of a circle from a point and a line segment, by a compass as it were, using the point as the centre of the circle and the line segment as its radius. This is the third construction postulate of Euclid's

Elements, with the slight generalization that Euclid uses one end-point of the given line segment as the centre of the circle rather than any given point.

In the formalism of type theory, this can be represented as the rule

$$\frac{a : Point \qquad b : LineSegment}{c(a,b) : Circle}$$

Progressing here from premisses to conclusion by deduction, we synthesize the circle $c(a,b)$ in the conclusion from the point a and the line segment b given in the premisses. This rule establishes at the same time a deductive connection from the premisses to the conclusion and a functional dependency from the constructions in the premisses to the construction in the conclusion.

Suppose we seek the construction of a circle. We thus have a variable y: *Circle*. Now if we match this with the conclusion of the type-theoretical rule, and reduce the conclusion to the premisses, we get to know that the unknown circle y can be composed from a point a and a line segment b, that is, that

$$y = c(a,b) : Circle$$

in the formal terms of type theory.

In predicate logic, the same construction postulate could be represented as the rule

$$\frac{\vdash Point(a) \qquad \vdash LineSegment(b)}{\vdash Circle(c(a,b))}$$

This does codify the same informal step of construction, but the forms of expression of predicate logic do not allow systematizing rules of construction in a natural way, in contrast to type theory. A type-theoretical rule like the one above simply composes a sought construction functionally in synthesis or decomposes it functionally in analysis. Thus a circle $c(a,b)$ decomposes into a point a and a line segment b. This reconciles the configurational and the directional interpretations of analysis on the level of a single step of construction.

The predicate-logical rule, on the other hand, infers properties of individuals from other properties in a way that lacks this natural compositionality, which is at the heart of the informal conception of analysis. There is no natural way to analyze the predication $Circle(c(a,b))$ into the predications $Point(a)$ and $LineSegment(b)$.

Another approach in terms of predicate logic is presented by Mueller (1981, 1-3) in his formal rendering of Hilbert's axioms for Euclidean geometry. He denotes lines by upper case letters and points by lower case ones instead of distiguishing them by predicates.

There is no way to formalize geometric construction postulates like the one above for circles. Predicate logic thus reduces geometry to theorem proving, because problem solving requires construction postulates.

The only way to reason about constructions in this predicate-logical approach is to lay down existence axioms and then infer existence theorems from them. In the tradition of geometry established by ancient Greeks, on the other hand, constructions are more primitive than existence propositions. Constructions can be used for proving existence propositions, but the former do not reduce to the latter, as in this predicate-logical codification. Thus, it is not adequate.

Indeed, Hilbert's notion of abstract axiomatization, as exemplified in his *Grundlagen der Geometrie* (1899), reduced geometry to theorem proving by reducing the existence of mathematical objects to the consistency of the axiomatic system that defines them implicitly. In the tradition of the Greeks, in contrast, mathematical objects were defined explicitly by construction postulates, as in type theory. Hilbert's model has spread throughout mathematics in this century, reducing it to theorem proving. Problem solving, which was the primary concern of Greek mathematicians (Knorr 1986, ch. 8), has been ruled out.

One can conclude, then, that predicate logic is a logic of theorem proving. It serves to describe the directional interpretation of analysis but not the configurational one. To describe the configurational interpretation and problem solving adequately requires a richer system of logical concepts.

Already Kolmogorov (1932) proposed developing a logic of problem solving and applying it to geometric construction problems. He saw that this requires constructive logic, but no one seemed to have taken up the task before my (1993). This is surely because an expressive enough logical language, type theory, was conceived only in the 1970's. Kolmogorov gave a problem interpretation for constructive propositional logic.

In natural deduction terms, the above rule of type theory is an introduction rule for the set of circles. So introduction rules of natural deduction in type theory serve to analyze a sought construction into its immediate constituents, by regressing from conclusion to premisses. Elimination rules, correlatively, serve to analyze a given construction into its immediate constituents. Introduction rules are used for defining a set by telling how its elements are constructed. They represent formally the construction postulates of Greek mathematicians.

We have employed the two type-theoretical forms of judgement

$$a : A$$

$$a = b : A$$

that represent, respectively, the informal judgements

a is an element of the set *A*

a and *b* are equal elements of the set *A*

Let us now consider judgements and inferences where constructions have no intrinsic interest. In case the construction a has no intrinsic interest, the form of judgement

$$a : A$$

can be abbreviated to the form

$$\vdash A$$

The distinction between these two forms of judgement is already present in ancient Greek mathematics in the distinction between problems and theorems. The solution to a problem consists of a construction of the sought objects from the given ones and a proof that the construction satisfies the condition of the problem. The proof of a theorem, on the other hand, is just a proof that the given objects satisfy the condition of the theorem. Solutions to problems thus contain a construction with intrinsic interest and a proof that has no intrinsic interest. A proof of a theorem is just the latter, thus without intrinsic interest as a construction (cf. Mäenpää 1993, ch. 3 for further information).

Zeuthen (1896) identified problems in ancient Greek geometry with existence propositions, proved by constructions. Knorr (1983 and 1986, ch. 8) refutes this by displaying and discussing Greek theorems that have explicit existential form. In them, existence was not proved by construction, and on the other hand, problems were understood quite simply as tasks of construction rather than as existential propositions. Hintikka and Remes (1974 and 1976), in the same vein as Zeuthen, distinguish between problems and theorems in terms of existential form. Problems are for them propositions that have existential form.

Type theory allows us to distinguish problems formally from theorems in a more satisfactory way. This requires enriching the forms of judgement of predicate logic. Recall that the form of judgement $a : A$ can be used to express that a is an element of the set A. More generally, it expresses that a is a construction of type A.

Another particular case of a construction besides an element of a set is a proof of a proposition. The above form of judgment can be used to express also that a is a proof of the proposition A. In case the proof a has no intrinsic interest as a construction, the form of judgement can be abbreviated to the form $\vdash A$ by suppressing a. This abbreviated form expresses that the proposition A is true. It can be used when we are not intrinsically interested in how A is proved, that is, in what construction proves it, but only in its truth.

The form of judgement $a : A$ can now be used to formalize the "deduction of the construction" a, and the form $\vdash A$ to formalize the "deduction of the proposition" A.

Thus, the two forms of judgement $a : A$ and $a = b : A$ can also represent the informal judgements

a is a proof of the proposition A,

a and b are equal proofs of the proposition A.

Proof here is to be understood in the sense of construction, as employed in constructive logic. For example, the conjunction introduction rule

$$\frac{\vdash A \qquad \vdash B}{\vdash A\&B}$$

of propositional logic in natural deduction formulation is seen in type theory as an abbreviation of the rule

$$\frac{a : A \qquad b : B}{(a,b) : A\&B}$$

where the proof (a,b) of the conjunction proposition $A\&B$ in the conclusion is a pair composed of the the proofs a and b of the propositions A and B, respectively, in the premisses.

From the point of view of the traditional distinction between problems and theorems, we can now see the abbreviated rule as a rule for theorems and the full type-theoretical rule as a rule for problems, because the latter rule displays constructions.

Corresponding to the usual natural deduction rules of conjunction elimination

$$\frac{\vdash A\&B}{\vdash A} \qquad \frac{\vdash A\&B}{\vdash B}$$

type theory has the rules

$$\frac{c : A\&B}{p(c) : A} \qquad \frac{c : A\&B}{q(c) : B}$$

that take apart the proof c of the conjunction in the premiss into the left projection $p(c)$ and the right projection $q(c)$, which prove the left conjunct A and the right conjunct B, respectively.

The values of expressions obtained by applying elimination rules are determined by "computation rules" of type theory. Each proposition and set has its rules of computation. There is nothing corresponding to them in predicate logic. Conjunction, for instance, has the following computation rules that determine how to evaluate effectively left and right projections:

$$\frac{a : A \qquad b : B}{p((a,b)) = a : A} \qquad \frac{a : A \qquad b : B}{q((a,b)) = b : B}$$

Constructions may thus have intrinsic interest already on the level of propositional logic. Representing them as individuals of predicate logic is an artificial codification. This strengthens the conception that predicate logic is suitable for a logic of theorem proving but not for a logic of problem solving.

Proofs in this type-theoretical sense of constructions are formal functional representations of proof trees. They are brought into the formal language as objects that can be reasoned about like any other objects. This is why they are also called "proof objects".

Each step of constructing a proof tree is at the same time a step of constructing a proof object. There is nothing restrictive from the point of view of classical logic in this formal procedure, because classical logic uses proof trees just like constructive logic. Proof trees of classical and constructive logic have representations on a par as proof objects in type theory. Type theory just enriches predicate logic by bringing proofs into the formalism as objects. Trees that form elements of sets, generated by rules like the introduction rule for circles above, are treated on a par with proof trees.

Type theory and predicate logic differ in the formalization of sets, like those of points, line segments, and circles. The type theorist represents them directly as sets, whereas the predicate logician represents them indirectly by codifying them as predicates over the single domain of individuals. Correlatively, the terms a, b, and $c(a,b)$ are formalized as individuals of the single domain in predicate logic, but as elements of the sets *Point*, *LineSegment*, and *Circle*, respectively, in type theory. This is because type theory enriches predicate logic so that instead of the one domain of individuals, each set is a domain of individuals in type theory.

This account of sets in predicate logic followed our first formalization above. In the second formalization above, that presented by Mueller, sets are distinguished from each other only by representing their elements by different variable symbols. There is thus no real distinction, on the formal level, between a point and a line, for instance. Nothing prevents forming meaningless predications like *Intersects*(A,a), where A is a point and a is a line.

To conclude, predicate logic does not recognize constructions and cannot formalize them naturally, although they can be artificially codified in terms of predicates over the single domain of individuals. It is not an adequate system of concepts for relating the configurational to the directional interpretation of analysis in formal terms. Hintikka and Remes (1974 and 1976) understand the analysis of a configuration formally as taking apart propositions into their constituents (by making use of the subformula property of natural deduction systems), although they describe it informally as taking apart a construction into its constituents.

Now let us consider the parts of a proposition in the Greek sense. A problem has "given" objects, "sought" objects, and a "condition" that relates them. A theorem, on the other hand, has only given objects and a condition on them. There are no things sought. A theorem is thus the limiting case of a problem with no sought objects. This distinction of the parts of a proposition is introduced by me (Mäenpää 1993, ch. 3) in order to discuss analysis in precise logical terms—it was not made explicitly by the Greeks. Their *zetoumenon* was the combination of what is here called the sought for objects and the condition. Thus for theorems it was just what is here called the condition.

An example of a problem is the first proposition of Euclid's *Elements*, to construct an equilateral triangle on a given line segment. Here the given object is a line segment, the sought object is a triangle, and the condition is that the triangle must be equilateral and constructed on the line segment.

An example of a theorem is proposition 32 of the first book of Euclid's *Elements*. It states that the angle sum of a triangle equals two right angles. Here the given object is a triangle, and the condition is that the sum of its angles is equal to two right angles.

In type-theoretical terms, we can represent the given objects, the sought objects, and the condition schematically as

$$x : A$$

$$y : B(x)$$

$$\vdash C(x,y)$$

respectively. Here x and y are (possibly empty) vectors of objects, because there may be any number of given and sought objects. For theorems the vector y is empty, so the scheme reduces to

$$x : A$$

$$\vdash C(x)$$

A problem or a theorem need not even have a condition, as in propositional logic, where predicates cannot be used. The existential form of proposition $(\exists y : B(x))C(x,y)$ in the context $x : A$ can be used to represent a problem that has a condition. However, problems are not to be identified with existential propositions, because problems that lack a condition are not be represented as existential propositions. And on the other hand, a theorem may be just as well be an existential proposition. This is the case when the condition of the theorem is an existential proposition.

Now analysis can be conceived of as a succession of two parts, "transformation" and "resolution", following Hankel (1874). Using our distinction between the given objects, the sought objects, and the condition, we can refine Hankel's proposal with type-theoretical form. First, transformation reduces the condition $C(x,y)$ to a transformed condition $T(x,y)$ that the analyst knows how to satisfy. The transformed condition of a problem must also determine some constituent of the sought objects $y : B(x)$ in terms of the given objects $x : A$. Then, resolution determines all of the sought objects in terms of the given ones. As theorems have no sought objects, their analysis has no resolution.

Synthesis has two corresponding successive parts, already distinguished by the Greeks, "construction [*kataskeue*]" and "demonstration [*apodeixis*]". Construction corresponds to resolution, because it constructs the sought objects $f(x) : B(x)$ from the given ones $x : A$. Demonstration corresponds to transformation, as it deduces the condition $C(x,f(x))$ from the transformed condition $T(x,f(x))$. The synthesis of a theorem has no construction, because there are no sought objects. Schematically, analysis and synthesis have the following form.

Analysis

$$\begin{array}{cc}
\text{resolution} & \text{transformation} \\
x : A & \vdash T(x,y) \\
 & \uparrow \\
\vdots & \vdots \\
 & \uparrow \\
y : B(x) & \vdash C(x,y)
\end{array}$$

Synthesis

$$\begin{array}{cc}
\text{construction} & \text{demonstration} \\
x : A & \vdash T(x,f(x)) \\
\downarrow & \downarrow \\
\vdots & \vdots \\
\downarrow & \downarrow \\
f(x) : B(x) & \vdash C(x,f(x))
\end{array}$$

In case there is no condition, analysis reduces to resolution and synthesis to construction. Analysis uncovers the functional dependency

$$y = f(x) : B(x)$$

of the sought objects on the given ones, and synthesis then constructs the sought objects from the given ones, using this knowledge.

Downward arrows in the scheme indicate deduction, and upward ones reduction. No direction is indicated for resolution, because it does not have any fixed direction. It may proceed either deductively from the given objects to the sought ones or reductively in the converse direction (cf. Mäenpää 1993 for further information).

Ancient Greek mathematicians deduced the sought objects from the given ones in resolution, beginning with the dependency of a constituent of a sought object on the given objects that was uncovered in the transformed condition. Thus Pappus's account only describes the transformation part of analysis.

In Greek mathematics transformation was a deductive reduction, that is, a chain of equivalent conditions. In synthesis, the transformation was converted into a demonstration.

No ancient methodological account seems to exist that discusses the restrictions that ensue from the limitation to deductive transformations in analysis. Quite evidently a large class of propositions admit only a successful analysis whose transformation is not deductive. This may be one reason why some Greek mathematical works were exposed only synthetically. If analysis is restricted to deductive transformations, its universality as a mathematical method of discovery is considerably restricted.

In the case of theorems, the scheme for problems reduces to the following special case.

$$\begin{array}{cc}
\text{Analysis} & \text{Synthesis} \\
\text{transformation} & \text{demonstration} \\
\vdash T(x) & \vdash T(x) \\
\uparrow & \downarrow \\
\vdots & \vdots \\
\uparrow & \downarrow \\
\vdash C(x) & \vdash C(x)
\end{array}$$

V The Heuristic Role of Auxiliary Constructions

The configurational interpretation construes analysis as a study of the functional dependencies in a definite configuration. This configuration consists of the given and the sought objects, assumed to relate to one another as specified by the condition.

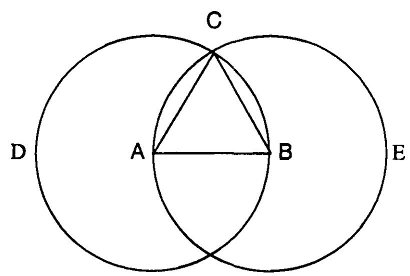
There is one proviso to this description. Determining the sought objects in terms of the given ones will not in general succeed by analysing just this definite configuration. It must be amplified by auxiliary constructions in the course of analysis. This is the heuristically crucial and unpredictable factor of analysis, as Hintikka and Remes (1974 and 1976) pointed out forcefully.

Descartes ignores auxiliary constructions in the account of analysis in his *Geometry* cited above. All he has to say is that in solving a problem the analyst is to "give names to all the lines that seem needful for its construction,—to those that are unknown as well as to those that are known."

To understand the systematic role of auxiliary constructions in analysis, Hintikka and Remes described their introduction in terms of quantifier instantiation rules of predicate logic. They contrast such instantiation steps to other steps of analysis, which take apart a proposition into its constituents as prescribed by the subformula principle of natural deduction systems of predicate logic.

However, as we have seen, analysing a configuration is more naturally formalized as the functional decomposition of a construction in type theory. Introduction rules decompose sought objects into their constituents, and elimination rules decompose given objects.

Let us look informally at a few examples of how auxiliary constructions function in solving problems and proving theorems of Euclidean elementary geometry. First, consider the proposition I, 1 of the *Elements*, which is the problem of constructing a sought triangle on a given line segment satisfying the condition that the triangle is equilateral and constructed on the line segment.



Euclid gives only the synthesis of the solution. In its construction part, he constructs two circles on the given line segment *AB*, one centered on point *A* and the other on point *B*, using *AB* as the radius. These steps apply his third construction postulate, for circles. Then he connects the points *A* and *B* to *C*, which is one of the two intersection points of the circles. These steps apply his first construction postulate, which allows constructing a line segment connecting two given points.

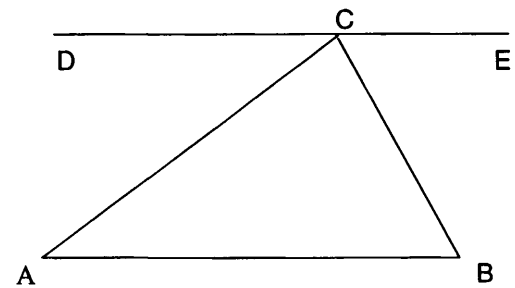
A well-known deficiency of the *Elements* from the point of view of modern standards of axiomatic systems is that Euclid does not justify the construction of a

point by means of two intersecting lines, like point *C* here, by any construction postulate. Rather, he takes it for granted that one may construct a point by letting two lines intersect, whether two straight lines or two circles or one of each.

In the demonstration part of his synthesis, Euclid shows that the condition of the problem holds by appealing to the definition of a circle. As *AC* and *AB* are radii of the same circle, they are equal in length. The same goes for *BC* and *BA*. By his axiom that "things that are equal to the same thing are also equal to one another", the first "common notion" of the *Elements*, *CA* is equal to *CB* and hence the sought triangle *ABC* is equilateral. It is also constructed on the given line segment *AB*, as required.

This solution required the auxiliary constructions of the two circles, carried out in the construction part of the solution. Without them, Euclid could not have determined the sought triangle in terms of the given line segment, because the vertex *C* of the triangle was constructed from the line segment *AB* by intersecting the circles.

Now consider the proof of a theorem, proposition I, 32 of Euclid's *Elements*, which states that the angle sum of a given triangle equals two right angles (this proof is in fact a version handed down by Eudemus).



First Euclid draws a straight line *DCE* through the vertex *C* of the given triangle *ABC* parallel to its base *AB*. Then he argues that $\angle ACD$ is equal to its alternate angle $\angle A$, and likewise $\angle BCE$ equal to $\angle B$, so the angle sum of the given triangle *ABC* equals the sum of $\angle ACD$, $\angle ACB$, and $\angle BCE$, that is, two right angles.

The auxiliary construction of the line *DCE* is the heuristically crucial part of this proof. Without it, the proof would not succeed. This shows that even though theorems have no construction part in their synthesis, auxiliary constructions are in general needed in order for their proofs to succeed.

Auxiliary constructions serve to bring forth new relations among the constituents of the configuration that is analysed, so that new propositions can be applied in the proof or solution. In the synthesis of theorems, auxiliary constructions are performed in the demonstration part, because there is no construction part.

In the present case, the auxiliary construction *DCE* allows mobilizing the theorem that a line intersecting two parallels makes the ensuing alternate angles equal to one another. This is proposition I, 29 of the first book of Euclid's *Elements*.

Now to gain a more general understanding of the significance of auxiliary constructions in mathematics, consider the elementary algebraic problem

$$a^2x^4 + abx^2 = c$$

for reals, assuming we know the standard solution to a quadratic equation. Algebraic equations are equality propositions, to be distinguished from definitional equalities, which are represented in type theory as judgements of the form $a = b : a$. This problem has the following parts.

given	$a,b,c : R$
sought	$x : R$
condition	$\vdash a^2x^4 + abx^2 = c$

The solution by analysis starts with a transformation. First, substitute the fresh variable y for ax^2 in the condition. This reduces the condition to the equivalent one

$$\vdash y^2 + by = c$$

Then transform this into the equivalent condition

$$\vdash y^2 + by - c = 0$$

We have now hit upon the transformed condition, because this equation is solved by the known general solution to a quadratic equation.

The resolution first applies this known solution, which yields the value

$$y = \frac{-b}{2} \pm \sqrt{\frac{b^2}{4} + c} : R.$$

There is a condition of solvability for y, a diorism in Greek terms, that

$$\vdash \frac{b^2}{4} + c \geq 0$$

In the second step of resolution, we determine the original thing sought x in terms of y as

$$x = \pm\sqrt{\frac{\frac{-b}{2} \pm \sqrt{\frac{b^2}{4} + c}}{a}} : R$$

by means of the known solution for x in terms of y from the equation $y = ax^2 : R$ corresponding to the substitution. (Any value of x is a solution if $a = c = 0 : R$.) Here, too, we have diorisms,

$$\vdash \frac{\frac{-b}{2} \pm \sqrt{\frac{b^2}{4} + c}}{a} \geq 0 \qquad \vdash a \neq 0$$

As algebraic equations of this Cartesian kind are more formal than the above propositions of geometry, this example shows more clearly in formal terms how auxiliary constructions enter into the a solution or a proof by analysis. They are introduced by substitution. We substituted the fresh variable y for the expression ax^2 in order to find a solution for x in terms of a, b, and c.

VI The Logical Role of Auxiliary Constructions

As auxiliary constructions are so central heuristically in analysis, let us discern their role in logical terms. Hintikka and Remes characterize them in terms of quantifier instantiation, but type theory allows us to represent them logically in a way that preserves their informal character faithfully.

In informal mathematics, auxiliary constructions are brought into analytical proofs and solutions by substitution. Our algebraic example, for instance, showed no trace of quantifier instantiations in bringing in the auxiliary construction. Recall their other informal characterization, as constructions that are constituents of neither the given nor the sought objects.

The substitution rule of type theory

$$\frac{\begin{array}{cc} (x:A) & \\ b:B & a:A \end{array}}{b(a/x):B(a/x)}$$

can be seen reductively as a generalization of the usual cut rule of natural deduction systems (Mäenpää 1993, ch. 2). Its first premise means that $b:B$ in the context $x:A$, that is, the premise $b:B$ may depend on the hypothesis $x:A$. Proceeding deductively from premisses to conclusion, the rule allows substituting a for x in b and B.

In analysis, this rule can be used reductively for introducing the auxiliary construction x of type A into the problem of proving some specified proposition C. The point is, heuristically, to see C as a substitution instance $B(a/x)$ of a more general proposition B that is defined in terms of the auxiliary construction $x:A$. Proving the proposition C, that is $B(a/x)$, reduces then to proving the proposition B in terms of $x:A$ and to constructing an object a of type A.

In our algebraic example the proposition C was the equation $a^2x^4 + abx^2 = c$ whose solution, the value of x in terms of a, b, and c, we sought. The heuristically crucial step in the analysis was the first step of transformation, where we saw this problem as a substitution instance of the problem $y^2 + by = c$ by applying the above cut rule reductively. This introduced the auxiliary construction y, which matches x in the rule. Furthermore, the object a in the rule matches the expression ax^2 in our example.

This rule of cut or reductive substitution has a special role in our logical description of configurational analysis. As introduction rules serve to analyze sought objects and elimination rules given objects, there must be some rule for introducing auxiliary constructions, because they do not arise from analysing the given and sought objects. This is what the cut rule is for.

In Hintikka and Remes's logical characterization of analysis in terms of predicate logic, the cut rule violates the rules of analysis. They forbid its use altogether, because it does not enjoy the subformula property of predicate logic. Instead, they see auxiliary constructions as entering by quantifier instantiation. This conception is the only reasonable one in predicate logic, where the cut rule is less general than in type theory. It has nothing to do with substitution, because it does not deal with individuals at all. In fact, predicate logic has no rule of substitution. Substitution operations of informal mathematics are artificially codified by means of quantifier rules. This is why predicate logic does not allow representing the

introduction of auxiliary constructions in a way that is faithful to informal mathematical practice.

Let us now enrich the schemes for analysis and synthesis by taking auxiliary constructions into account.

Analysis

$$\begin{array}{cc}
\text{resolution} & \text{transformation} \\
x:A & \vdash T(x,y,z) \\
 & \uparrow \\
\vdots & \vdots \\
z:G(x) & (g(x,y)/z) \\
\vdots & \vdots \\
 & \uparrow \\
y:B(x) & \vdash C(x,y)
\end{array}$$

Synthesis

$$\begin{array}{cc}
\text{construction} & \text{demonstration} \\
x:A & \vdash T(x,f(x,h(x)),h(x)) \\
\downarrow & \downarrow \\
\vdots & \vdots \\
h(x):G(x) & \vdots \\
\vdots & \vdots \\
\downarrow & \downarrow \\
f(x,h(x)):B(x) & \vdash C(x,f(x,h(x)))
\end{array}$$

Analysis introduces the auxiliary constructions z of type $G(x)$ by substituting $g(x,y)$ for them reductively in the transformation (here z and $g(x,y)$ denote, again, vectors of objects). The original configuration that consists of x and y is thereby amplified by z.

Resolution determines the auxiliary constructions z in terms of the given objects x alone. This is why the type $G(x)$ must not depend on the given objects x, in contrast to the expression $g(x,y)$ substituted for z reductively in transformation (for the logical role of auxiliary constructions in analyses where the proposition has no condition, cf. Mäenpää 1993, ch. 3; in this case they have to be both introduced and determined in terms of the given objects in resolution).

Analysis uncovers the functional dependencies

$$z = h(x) : G(x)$$

$$y = f(x,z) = f(x,h(x)) : B(x)$$

that are constructed in synthesis. The determination of the things sought y in terms of the auxiliary constructions z and the given objects x in resolution must respect the equation

$$z = g(x,y) : G(x)$$

that corresponds to the substitutions in transformation.

For instance, in our algebraic example we introduced the auxiliary construction y by substituting the expression ax^2 reductively for it. This expression depends on the given object a as well as the sought object x. Yet in resolution, we determined y in terms of the given objects alone. Then we determined the thing sought x in terms of the given objects and the auxiliary construction y so that the equation $y = ax^2 : R$ corresponding to the substitution was respected.

Notice that substitution doesn't figure in synthesis. This is why we did not discern it in the geometric examples of employing auxiliary constructions from Euclid's *Elements* (for a discussion of the analyses corresponding to these syntheses, cf. Mäenpää 1993, ch. 5).

In the case of theorems, we have the following special case of the above scheme.

$$\begin{array}{cc}
\text{Analysis} & \text{Synthesis} \\
\text{transformation} & \text{demonstration} \\
\vdash T(x,z) & \vdash T(x,g(x)) \\
\uparrow & \downarrow \\
\cdot & \cdot \\
\cdot\ (g(x)/z) & \cdot \\
\cdot & \cdot \\
\uparrow & \downarrow \\
\vdash C(x) & \vdash C(x)
\end{array}$$

Here the auxiliary constructions do not have to be determined in terms of the given objects alone, as in the analysis of problems, because there are no sought objects that the auxiliary constructions could be made to depend on in transformation. We can thus directly use the same expression $g(x)$ in demonstration that was used in transformation.

Nevertheless, this scheme shows how auxiliary constructions figure in the analysis of theorems, and not only in the solution of problems. They do not arise by taking apart the given constructions. Rather, the given configuration x must be amplified by z.

The source of the heuristic usefulness of analysis is that it gives the possibility of making systematic use of the things sought and the condition as well as of the given objects. Plain synthesis, without an antecedent analysis, has to proceed from the given objects to the sought ones and then demonstrate the condition blindly, so to say, without making systematic use of the sought objects and the condition.

Auxiliary constructions, in particular, function in a subtle way. They can be based on the sought as well as the given objects in the substitutions performed in transformation. In resolution they are then determined in terms of the given objects alone. This functioning is spelled out in precise formal terms in the above schemes.

As theorems have no sought objects, analysis is less useful heuristically for proving them than for solving problems. In particular, auxiliary constructions function in a less subtle way. They are introduced outright in transformation in a way that need not be justified in resolution or in synthesis in another way, because they can depend only on the given objects. This explains in part why analysis was above all a method for solving problems for ancient Greek geometers (compare to the account of Knorr 1986, ch. 8).

Let us reconsider the question whether predicate logic is adequte for describing theoretical analysis. As proving theorems requires auxiliary constructions in general, it depends on solving construction problems. Auxiliary constructions are, after all, solutions to problems. Therefore predicate logic suffices for describing theorem proving adequately only in those scarce trivial situations where auxiliary constructions are not needed.

Moreover, auxiliary constructions and constructions in general require a constructive logic for their logical description, because constructions are formed constructively by definition. In the mathematical tradition established by the Greeks, the construction part of synthesis was constructive in terms of modern constructivist standards, whereas indirect proofs were allowed in the demonstration to prove properties of constructions. As mathematics has reduced to theorem proving during this century, classical predicate logic suffices to describe it, if auxiliary constructions are not taken into account.

In contemporary mathematics, indirect proofs of existence are allowed everywhere instead of constructions, which are carried out by means of construction postulates. In particular, auxiliary constructions are replaced by objects whose existence is proved indirectly. Then classical predicate logic suffices to describe them and theorem proving in general.

In another direction, the constructivists of this century have gone further than the Greeks—they require constructive reasoning even in demonstrations of the properties of constructions, not only in constructions.

As a surprising recapitulation of ancient mathematical history, computing scientists have in recent years started to solve problems in exactly the same sense as Greek mathematicians. They carry out constructions (computer programs) and demonstrate that they satisfy specified conditions. As programs are formal by definition, the need for an adequate formalism has been crucial in computing science, in contrast to mathematics.

Type theory has become one of the main theoretical approaches in computing science, because it is a programming language as well as a logical system (Martin-Löf 1982, Nordström, Petersson and Smith 1990). Programs are constructed by rules of introduction and elimination and evaluated by rules of computation. This allows constructing programs and demonstrating their properties in one formalism, which is a considerable advantage over traditional programming languages. Predicate logic does not suffice for this, nor does classical logic, because they don't recognize constructions.

Programming was until the 1970's in a pre-theoretical stage in the way mathematics was before the Greeks made it a science. In the 1970's the need arose to prove that programs satisfy their specified conditions, that is, do what they are supposed to do. This is how programming evolved into a science in the sense established by the Greeks. Characteristic of mathematics before the Greeks as well as programming before the 1970's was a stage of algorithmic constructions with no specifications or demonstrations of conditions of correctness imposed on the constructions. The Greek method of geometrical analysis can be generalized into a method of solving all kinds of mathematical problems in type theory by taking into account inductively defined problems, which are characteristic of programming. The method known as top-down programming turns out to be a special case of analysis (Mäenpää 1993, chs. 3 and 8).

University of Helsinki
Department of Philosophy

JEAN-MICHEL SALANSKIS

ANALYSIS, HERMENEUTICS, MATHEMATICS

I Introduction

In this article, we would like to study the importance of the concept of analysis for mathematics from three points of view:

– Firstly, we will be attentive to what, since the Greeks, since Pappus if what I have learned is correct, is called "analysis", and which, as a characteristic procedure of geometric reasoning, is put forward by Platonic and Aristotelian philosophy as a universal model of thought.

– Secondly, we would like to understand analysis as the regressive method of all transcendental inquiry, following Kant's suggestion, and to reinterpret this transcendental inquiry as necessarily hermeneutical.

– But finally, we will aim to elucidate, starting from these sorts of considerations, the unity of meaning of analysis, that contemporary branch of mathematics whose prodigious development in modern times is well known.

There is little doubt that a certain degree of failure in such an undertaking is likely. The method which will be followed to attain some results despite the scope of the questions raised and the unsettling character of the comparisons we wish to establish will consist in a straightforwardly personal reconstruction of certain elements of the tradition.

Let us therefore begin with Greek analysis.

II Greek Analytical Suspension: Hermeneutics and deliberation

In the *Republic* (510 *b-d*), Plato clearly opposes mathematical and philosophic approaches: he considers the latter as essentially regressive, consisting in an upward move from any given to the "non-hypothetical principle(s)" belonging to the purely intelligible realm. The former is essentially suspensive and progressive, laying down certain hypotheses, passing through their consequences while breaking once and for all with any questioning of them. But on the other hand, the method of geometers is readily called up as an argumentative model for philosophy. Notably in *Meno* (86*b*-87*d*), when a provisional phase of the research into the essence of virtue must be justified, Plato cites a relatively obscure (in the

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 227–241.

words of Plato's French editor and translator Léon Robin[1]) example which does seem to be a case of classical "analysis".

We therefore wish to reflect on Greek analysis to know if it is regressive or progressive, suspensive or interrogative, philosophical or mathematical.

The discussion may commence not with Greek sources, but with what, in the mathematical tradition, has been defined as the method of analysis. In my case, analysis was taught to me, more than two thousand years after the Greeks, as the first phase in the treatment of a problem of geometric construction. Faced with the problem of constructing a figure, a straight line or a point with such and such a property with respect to geometric givens—which are in turn simple figures (point, straight line, triangle, circle, etc.)—we are advised to begin by "assuming that the problem is solved": by tracing in a tentative and approximate manner a figure in which what must be constructed is present and whose construction we assume to be correct (generally speaking, moreover, we know how to adjust distances and angles intuitively so as to actually experience the construction as correct or slightly incorrect). We may then, on inspection of the figure, begin the work of deduction, whose premises are acquired through considering the properties of the entity under construction as satisfied. The process of deduction naturally gives up a series of properties, certain of which will be the relations of the entity under construction—or more generally the constituents of this entity—to the given entities. At a certain point, these relations may be able to indicate and prescribe in transparent fashion a possible construction. There then remains, in the phase called "synthesis", the task of demonstrating that what has been constructed in the discovered procedure indeed satisfies the stipulated properties of the "problem of construction".

Thus, one assumes that the relations to be satisfied are satisfied (the relations of the entity to be constructed to the given entities) so as to deduce other relations out of which a construction is possible and recommendable.

How must this procedure be described?

First of all it is suspensive, for it consists in a hypothesis; but the hypothesis is the elimination of what is at stake, of the aim, of the problematic originary orientation. Thus, there is indeed suspension, at least apparently, suspension at a certain level of the drive toward the goal.

The method is obviously progressive as well: one derives conclusions from the hypothesis that the problem is solved, instead of working down from the hypothesis to its unquestionable sufficient reasons, or attempting such a philosophic regression from the encompassing conditions of the problem. Yet these eventualities of a philosophic treatment of the geometric problem have a false ring to them, because the context of the problem is immediately non-philosophic: the regression to nonhypothetical principles referred to by Plato clearly deals with lexical indicators of conceptual signification, rather than with those configurations, leading to a decision, that the problems are. Therefore analysis does appear suspensive and mathematical, but suspensive in the sense that it is less the evaluation of a thesis that is suspended as the tension of a problem, that is to say a sort of strategic meta-thesis.

Let us proceed now to the purely logical plane. The procedure of analysis begins by laying down a phrase such as:

$\exists x P(a,x)$ [there exists an entity x such that it has a relation P to the given objects a].

We now move on the logical deduction, finishing with a phrase such as the following:

$\exists x Q(a,x)$ [there exists an entity x such that it has a relation Q to the given objects a].

One imagines that there must be a way to attest this new phrase "effectively", to construct the entity(ies) mentioned in the phrase existentially. And one imagines as well that this construction is in fact, through certain simple mediations, the *ipso facto* construction of entities x such that $P(a,x)$, which is to say one imagines this to be the solution to the original problem.

This procedure of analysis seems circular: one assumes the existence of an x satisfying P to be able to demonstrate the existence of an x satisfying P. This circularity has nothing to do with a vicious circle, because the presupposition is logico-existential, and because what is achieved at the end is an effective construction. Such a construction can be achieved because the existential description *à la* Russell of the object to be constructed has been transformed into that of another object, with the property that a constructive counterpart to it is immediately given, and because the passage from the construction of this new object to that of the original can be accomplished.

I am led to conclude that analysis, seen in this angle, is a thoughtful elaboration allowing for the transition from the logico-predicative precomprehension of an entity to practical comprehension. The underlying presupposition is that certain logico-predicative precomprehensions have always contained their practical counterparts: this is but to name and to grasp the traditional idea of a "guiding" geometric intuition. The geometric intuition consists in there being practical correspondents of the constructive order to certain simple, defined descriptions, providing that the constant parameters of these descriptions themselves be given in intuition.

In any case, the drift of my argument is now clear: the procedure of analysis in the classical, technical sense of the term that it has acquired since the Greeks in the field of geometry is closely related to hermeneutics. It must be pointed out in passing that this hermeneutics is opposed to the hermeneutics Heidegger adumbrates in section 63 of *Sein und Zeit* (1927): for Heidegger, the precomprehension of being is practical, ante-predicative, and hermeneutical elucidation consists in a bringing into view through predicative speech, whereas here, the passage is from a saying that articulates the object to be constructed in such and such a way to a realisation of the geometric construction which exhibits the object with the same determinations.

This line of thought on Greek analysis can be completed with an account of the following passage from the *Nicomachean Ethics* in which Aristotle conceives of the reasoning of the practical understanding as stemming from the model of analysis:

> "We deliberate not about ends but about means. For a doctor does not deliberate whether he shall heal, nor an orator whether he shall persuade, nor a statesman whether he shall produce law and order, nor does anyone else deliberate about his end. They assume the end and consider how and by what means it is to be attained; and if it seems to be produced by several means they consider by which it is most easily and best produced, while if it is achieved by one only they consider how it will be achieved by this and by what means this will be achieved, till they come to the first cause, which in the order of discovery is last. For the person who deliberates seems to investigate and analyse in the way described as though he were analysing a geometric construction (not all investigation appears to be deliberation—for instance mathematical investigations—but all deliberation is investigation), and what is last in the order of analysis seems to be first in the order of becoming."[2] (1112*b*, 12-25)

Analysis seems here to be characterised by regressive reasoning, which, *prima facie*, is in total contradiction with Plato's divide between mathematics and philosophy. The connection to the traditional notion of analysis in geometry mentioned earlier is easy to establish; Aristotle perceives that in practical deliberation, the problem is assumed solved as in problems of construction. But the deliberation is not analogous to progressive research into the conditions of construction, for it is in fact regressive: the regression it enacts is at one and the same time purely logical and empirical, conditions are introduced as perfectly regular logical premises of the previously considered condition, and the mind remains constantly watchful over the possibility of adjusting practically the world to the present condition.

A type of extremely simple mathematical reasoning conforming to this model can be cited. Moreover, this type of reasoning is of the greatest importance in contemporary mathematical analysis, be it real or complex. I refer here to processes of reasoning adapting α to ε, to attest a property of continuity or limit following the definition prevailing since Weierstrass: let us say, for example, that I wish to establish the continuity in 1 of the function $x \to x^2$; $\varepsilon > 0$ is given, and I will seek $\alpha > 0$ such that the condition $|x-1| \leq \alpha$ implies $|x^2-1| \leq \varepsilon$; what is to be obtained is in fact $|x-1||x+1| \leq \varepsilon$, which follows from $|x-1| \leq \varepsilon/2$ and $|x+1| \leq 2$, this last condition resulting from $|x-1| \leq 1$, so that $\alpha = \mathrm{Min}(1,\varepsilon/2)$ agrees. It is clear that the "deliberation" involved in this proof requires that a "means" be found of a prior (double) "means", therefore the deliberation already possesses a certain depth. Those familiar with contemporary real and complex analysis may witness that

this sort of procedure, with its essential estimative aspect, is omnipresent therein, not necessarily as a global scheme of what is accomplished (modern technicity having introduced other general modes of mathematical reflection), but quite often as the decisive and necessary local manipulation.

The question here is whether this deliberative regression *à la* Aristotle makes it "philosophic" in the Platonic sense. Once again, it seems that the distinction is marked in Platonic regression being semantic and lexical, aiming for the nonhypothetical principle, while Aristotelian regression is logical and phrastic, aiming at the effectuation of the hypothesis. In the case of ethico-practical deliberation, this is the pure and simple concrete faculty instituting a state of affairs in the world. In the case of Weierstrassian "deliberation", the effectuation comes about in the mediate discovery of a condition of a type set down in advance, ultimately implying the condition taken as final theme.

This other type of analysis can no longer be attached to the hermeneutical model, as was suggested above in bringing to light the procedure of analysis in the solution of a problem of geometric construction. The two relevant orders, that of the logical phrase and its implication on the one hand, that of its effectuation on the other, are no longer related in such a way that what takes place in one order can be considered as satisfying what is anticipated in the other. Moreover, must the hermeneutical path not be an uncertain progression, a drift? Is there in fact elucidation if one simply strives through accumulative stages toward a point of resolution and actuality? Aristotelian analysis has something in common with problem-solving , and nothing of the sort with the hermeneutical circle: the "problem is assumed solved", but this is not to make it a premise, nor to acquire it as a pregiven, but quite simply to make it one's goal at the end of a logico-rationally polarised interval. It will become clear by the end of this paper that this logically regressive analysis may however be considered, and doubly so, as a hermeneutics. But for the moment we lack the means of grasping this possibility.

At this point of our presentation it is difficult not to want to deal with that other historically claimed form of analysis: Kantian transcendental regression.

III Transcendental Analysis

In the "Methodology of pure reason", Kant sets up a famous demarcation between mathematics and philosophy, the procedure of philosophy being that of knowledge gained through concepts, and that of mathematics as knowledge gained through the construction of concepts. His essential aim is to explain how the deduction of the principles of pure understanding, which appears *a posteriori* as the philosophic result of the *Critique of Pure Reason*, is not and could not be a part of mathematics. The motive of this divide lies in the nature of the concepts worked through in the transcendental inquiry: they are strictly discursive concepts, thus with no generic instance in intuition (the procedure of "concept construction", so typical a move in mathematics, is in their case impossible). In a logic of auto-justification which is one of the essential stakes of this passage, Kant explains that what may be learned about them is limited to their function as "rules" for the synthesis of sensible manifolds in excess of them and under the extraneous legislation of the pure forms of intuition. But he also says something else, apparently gratuitous and intrinsic, about these concepts: that they are present in ordinary human usage, in such a form however that their content is not delimited. And he names "analysis" the procedure explicating a norm of correct signification for such concepts, opposing this procedure to that of mathematical "definition".

Once again we then meet with the collusion between the mathematico-philosophic divide and the figure of analysis, and that between the latter and the idea of regression, as will be seen more clearly below. Husserl, reading these passages, retained the idea that the regressive method was characteristic of the transcendental spirit *à la* Kant. In order to refute the Kantian transcendental, he retains as its positive principle a partially Cartesian formulation: the transcendental thesis consists in saying that all knowledge is knowledge of a subject and is only valid as knowledge following the certification of the subject—there can be no meaning to the idea of knowledge dictated and validated by the object. Husserl attributes this thesis to Kant as a major insight and progress for thought, but he parts ways with him over how to describe these subjective formations governing all knowledge. According to Husserl, Kant obtains his transcendental invariants, the categories, space and time with their own constraints "by regressing from *de facto* discourse", from *de facto* thought of the subject in general and of the subject of science in particular. But his judgement is that Kant's method issues in opaqueness of the resulting transcendental factors. In Husserl's view, what is discovered by regression, what is identified as the condition of possibility of a *de facto* exercise, even if it never be present in the exercise, has on principle the right not to have either intuitive grounds or evidence for its subject, and ultimately it is likely not to have any sense. Whereas, for Husserl, what we name the transcendental character of what affects our knowledge must appear as such to us in an examination of our subjective performance "on the path" of knowledge. The transcendental factors must not be merely linked in a logical relation to the experience of knowledge, but must themselves be able to be experimented with their functions within that experience. Husserl's position interests us for its negative lesson on what could be called "conceptual analysis", the regression from a fact not toward Platonic non-hypothetical principles, but to guiding notions, conditions of possibilities, a regression that always thinks a logico-significant link: this analysis does not conquer evidence, but rather leads us to contents whose strangeness is maintained at the very moment their guiding quality is acknowledged.

But let us listen to the expression of such a conceptual analysis in Kant:

> "In the second place, it is also true that no concept given *a priori*, such as substance, cause, right, equity, etc., can strictly speaking, be defined. For I can never be certain that the clear representation of a concept, which as given may still be confused, has been completely effected, unless I know that it is adequate to its object. But since the concept of it may, as given, include many obscure representations, which we overlook in our analysis, although we are constantly making use of them in our application of the concept, the completeness of the analysis of my concept is always in doubt, and a multiplicity of suitable examples suffices only to make the completeness probable, never to make it *apodeictically* certain. Instead of the term, definition, I prefer to use the term, *exposition*, as being more guarded term, which the critic can accept as being up to a certain point valid, though still entertaining doubts as to the completeness of the analysis."[3] (Kant A, 729; B, 757)

It is thus clear that the philosophical procedure of analysis starts with a concept given in usage, then attempts to decompose it at the level of signification, without however being certain of ever having a complete semantic portrait of the concept. This procedure is opposed to that of the definition, characterised in the following terms:

> "There remain, therefore, no concepts which allow of definition, except only those which contain an arbitrary synthesis that admits of *a priori* construction. Consequently, mathematics is the only science that has definitions. For the object which it thinks it exhibits *a priori* in intuition, and this object certainly cannot contain either more or less than the concept, since it is through the definition that the concept of the object is given—and given originally, that is, without its being necessary to derive the definition from any other source." (*ibid.* A, 729-730; B, 757-758)

It is then essential to the notion of analysis that it imply the relationship to a given, whereas the definition "gives" itself:

> "We shall confine ourselves simply to remarking that while philosophical definitions are never more than expositions of given concepts, mathematical definitions are constructions of concepts, originally framed by the mind itself [...]." (*ibid.* A, 758; B, 730)

Kant insists strongly on the provisional, perfectible character of analysis. Thus, in a footnote:

> "Philosophy is full of faulty definitions, especially of definitions which, while indeed containing some of the elements required, are yet not complete. If we could make no use of a concept till we had defined it, all philosophy would be in a pitiable plight. But since a good and safe use can still be made of the elements obtained by analysis so far as they go, defective definitions, that is, propositions which are properly not definitions, but are yet true, and are therefore approximations to definitions, can be employed with great advantage. In mathematics definition belongs *ad esse*, in philosophy *ad melius esse*. It is desirable to attain an adequate definition, but often very difficult. The jurists are still without a definition of their concept of right." (*ibid.* A, 731; B, 759)

Therefore I would like to know and ask to what point this figure of analysis is a figure of hermeneutics. The word "exposition" appears for the first time in the *Critique of Pure Reason* in the transcendental aesthetic, where Kant presents a "metaphysical exposition", clear though not detailed, of space. In this case as in that above, the exposition sets forth a content in ignorance of that completeness which is its aim, to the point of despair of ever being able to reach such a goal. With the problem of space, this impossibility has something principled about it, since the very infinity of space, revealed by the exposition, is opposed to its completeness. But this is only one of its aspects: the incompleteness is related as well to what the exposition sets forth of what is "anticipated" of space, to what of space is "prejudged", to what geometry will systematise, but which is not yet in itself formal or exact, thus displaying an essential incompleteness of determination, calling for diverse elucidations. The investigation here called "analysis" has common characteristics with the metaphysical exposition. The principle difference being that it is nevertheless a "decomposition": it works on a word of the language, a word corresponding to a concept, and attempts to elucidate it in what would appear to be the only possible way, *i.e.* through a list purporting to be complete of the semantic contents in which the concept exhausts its meaning. But this work is open and incomplete, consisting in a dialogue with the given which is at one and the same time a way of prescribing this given, as in the case of the metaphysical exposition. In that case, the donation is the celebrated intuitive donation, that of the pure forms of the sensibility to the subject, a donation supposed to precede *de jure* all experience, and which is called pure intuition. While in the case of analysis of a concept such as "substance", the given is that of a semanticism already shared by the circle of the thinking community. Hermeneutics in its most classic concept can only apply to this sort of given, which is easily conceived as equally "not given". This is the structure of the "envelopment of meaning", a sort of *a priori* structure governing the region of meaning, according to which everything having meaning withholds additional meaning that, in one way or another, has to be explicated or activated. On the other hand it is not self evident to conceive of the mathematical theorisation of space, for example, as a hermeneutic: this is nevertheless what I wished to propose as the best epistemological scheme of mathematical activity in my *L'herméneutique formelle* (1991), whose point of departure was indeed the presentation of the relation to space as a relation at once of familiarity and of dispossession, a relation to a given-not given of the same sort as that to a lexical unit in which meaning is enveloped. My complete thesis, whose main argument I have just in part reproduced, is that the relation named by Kant "intuition" is a relation of this sort.

But, as for the usage of the word analysis, there is an important distinction to be made. Analysis as a procedure of finite and controlled decomposition is the hermeneutical method when it has as its object the natural opaqueness of lexical meaning. On the other hand, the mathematical interpretation of space does not follow the path of analysis, but rather proceeds by axiomatic enunciation, "synthetically", the exact inversion of hermeneutics. Judgements prescribing space are made, inscribed and aligned; they are supposedly inspired by our familiarity with space but, whatever the case, they set and delimit that space, enabling a regulated logical usage of the representations that will implement the knowledge of space. The synthetic character resides in the fact that these judgements predicate subjects of determinations that do not figure in their concept, in conformity to the Kantian definition, but we could take a step further in considering modern axiomatic experience, and conclude that axiomatisation is synthetic insofar as it establishes, prejudgementally, a world of objects in its coherence and universality. Whereas conceptual analysis limits itself to deploying problematically the wealth of possibilities of a locus of meaning, of a condensation of thought.

In any case, the mere consideration of analysis as the characteristic method of transcendental investigation and of the metaphysical exposition of the transcendental aesthetic as both belonging to the hermeneutical attitude suffices to show that each factor of the Kantian transcendental structure in fact receives its identity as a hermeneutical conquest: space, time, and the categories constrain knowledge *a priori* only as figures of themselves to which access is given in a dispossessive familiarity. These figures have the status and the composition of non-given givens, objects allowing analytic work in the case of conceptual elements, and, as for intuitive elements, permitting mathematical synthesis which is nonetheless hermeneutical.

Can this preliminary two-headed reflection afford insight into the project of expressing the essence of contemporary mathematical analysis?

IV The Identity of the Branch Analysis of Contemporary Mathematics

How is analysis to be identified today? There is of course J. Dieudonné's *Elements of Analysis* (1963-1982), which gives us a sketch of the complex tree of the subdisciplines of analysis, claiming to expound them one after the other, volume after volume. General topology, theory of topological spaces, theory of analytical functions, functional analysis, algebraic topology, differential geometry, theory of dynamical systems, differential topology: all these headings, of different implicit or explicit levels, coming together and crossing each other in various ways, compose the figure of analysis. At a glance, the unity of these procedures is in the dependence of the objects treated on the $\mathbb{R}$ and the $\mathbb{C}$ of the Cantorian construction, together with the play of the topological element of these structures. Having said that, there are certain cases in which the disciplines of analysis confine with algebra, for various reasons: in the case of analytic geometry, it is because this branch makes use of constructions generally given as algebraic in a geometry itself known as algebraic; for the case of differential equations, the motive would be more strategic, because the solution of equations is an algebraic heuristic and, consequently, despite the topological nature of its objects and situations, many aspects of the theory come from algebra.

The discussion undertaken here revolves naturally around the opposition between "analysis" and "algebra". But this is not the only possible discussion: another one is oriented towards the distinction between "analysis" and "geometry". It seems self-evident to me that the theory of topological vectorial spaces should belong to analysis, but I would much less spontaneously call this theory "geometric". Dieudonné seems to classify in analysis everything in which topology plays a decisive role, thus evincing a particular conception of the branch. But in the diffuse sentiment of contemporary mathematicians, there is also a more restrictive idea of analysis, according to which it would be defined as the study of set-theoretical complexity—that is, above all, functional complexity—developed on the basis of $\mathbb{R}$ and $\mathbb{C}$, indeed from a topological viewpoint, without ever attaining a geometric perspective on these entities. From this point of view, differential geometry would contain numerous aspects outside the field of analysis strictly speaking.

As for the concept of "geometry", it is in a problematic inter-definitional state with that of topology: not all study of topological structure is geometric—there is another diffuse sentiment according to which geometry begins only when the topological structures studied are sufficiently affinitive to classical Euclidean structures. One possible criterion is the presence of a sheaf, that is, that readily operational entities be given above the localisations offered up by the topological space.

Lastly, the concept of "algebra" is difficult to distinguish from that of "arithmetic": the classic "algebraic structures"—group, ring, field—have for their simplest examples the objects $\mathbb{N}$, $\mathbb{Z}$, and $\mathbb{Q}$, which proceed immediately from $\mathbb{N}$, the presumed theme of all mathematics from the constructive point of view. "Arithmetic" may be a word for the designation of the intuitive-constructive base that all mathematics ultimately refers to, and from this viewpoint the notion of the algorithm becomes the decisive notion of arithmetic. Or else arithmetic concerns an interest in the qualitative distribution of integers and for their related operational configurations, which generally ushers us into algebra. Arithmetic thus appears to be linked in two ways: on one side to discrete constructive mathematics, on the other to modern algebra. Research on Fermat's theorem brilliantly underscores the second link. And I recall my teacher Claude Chevalley saying that algebra as a whole was a lemma for proving Fermat.

A few words are in order here in response to the characteristic aggravation with which mathematicians react to these sorts of considerations. They state that it is of no importance to reach an agreement on problems of classification and definition of the major "branch names". One of these mathematicians once said to me: here I am considered a geometer, there a topologist, elsewhere an analyst, but

as this has no incidence on my work it is unimportant. It may in fact be the case that these labels are devoid of operational value. It is notably certain that mathematicians may put any instrument to work from out of the laboratory of any subdiscipline, and do not in fact hesitate to do so in the context of Bourbakian inter-theoreticity. Is that tantamount to concluding that branch identities are no longer subject to questioning? I have serious doubts. The enlargement of the meaning that geometry has experienced since the nineteenth century has for instance clearly functioned as a conquest from which mathematicians have profited: none have scruples over introducing, each time they wish to, their procedures as "the introduction of geometric considerations", referring to the new identity of geometry, to one or another aspect of what today is classed as geometry but which never would have "before". Mathematicians themselves use branch classification in order to measure what is happening in their field, as an instrument of evaluation of research events. This can be done providing that the identities which stand behind branch names are important, that is "can be called into question". Conversely, it may be held that one of the stakes of mathematical development is the increasingly in-depth understanding of branch identities. This is moreover one of the titles under which my work published in 1991 established mathematics as a thinking discipline, as "hermeneutics", concerned with enigmas of various levels.

To return to mathematical analysis, we would also like to see what light historical knowledge might shed on what has been understood as "analysis" throughout history. From this vantage, it does seem that the word "analysis" and its corresponding adjective "analytic" first meant something quite closely attached to what today is understood as "algebra", unless these words designated literal calculus in general. Viète's *ars analytica* is algebraic calculus, literal symbolism with its procedures. When "analytic geometry" becomes the standard designation for coordinate geometry *à la* Descartes, the adjective once again denotes the symbolic level of numeric-literal calculuses, here opposed to that of spatial intuitions. This notion of analysis seems to me closely connected philosophically to the sememe decomposition. Literal calculus is based on the discrete character of the units of language, and the forms gathered within it are gathered on the basis of this presupposed analysis which offers the simple constituents. The numeric coding of geometry likewise appears as a reduction of the spatial-continuous synthetic nature of figures, to those perfectly individualised and mutually distinct determinations that numbers are. Even if $\mathbb{R}^n$ is, following modern discourse, an interpretation of the continuum, the critical vantage sees in this construction a set of ideally distinguishable points, which can be manipulated as independent particulars. This is an insult to the profound intuition of the solidarity of the continuum with itself, ruling out any autonomization.

Thus would we naturally retain the idea that analysis is the theory of the local, a theory whose intention aims at nameable and separable identities in a place.

This description would go to explain the large acceptation which assigns to analysis everything essentially turning on topological structures, and also the limited acceptation, which only assigns to analysis that which deals with the study of numericity and its functional complications in the framework of a topological questioning. This description would also be coherent with the originary given of an analysis whose meaning is equivalent to that of algebra, and with the specialization of the adjective "analytic" to the evocation of the coding of space, or more generally with the use of "analytic" to designate any numerico-formulary explication.

And therefore contemporary mathematical analysis would have no relationship to the hermeneutical part of analysis, if I may use this expression: I am referring to the part I began to situate in my commentary on the Greek method of analysis, or of the activity of analysis identified by Kant as proper to philosophic procedure. There would be no relationship between the fact that analysis—considered from the vantage of that branch of contemporary mathematics—accomplishes and/or presupposes the hermeneutics of the continuum and the fact that a particular logical type of analysis as method—explication, regression, or other types—be affinitive to the hermeneutical spirit.

Unless an attempt was made to think the homology of everything that has been said to this point, necessarily at a more radical level. We will begin by stating that there is a relationship between the theme of the continuum and, for instance, the regressive nature of reasoning attesting the property of a limit, of which an example was given above. Why is this reasoning regressive in exactly the way it is? Because we are in a problematic of "control": the continuum, here carried to the power of itself through the taking into account of a function, calls into play an excessively infinite profusion of information; thought then adapts itself to this excessive situation by concentrating on regions and by reflecting on how one aspect of the local information allows it to be controlled by another. To know what is in excess is to assign determinations to it, is to analyse it and to understand the analysing determinations themselves in their mutual relations. Regression responds to the metaphysical pragmatics of willing: an analysing determination of the continuum is a willing, my knowledge of what is in excess is will, to such an extent that the systematic thought of these determinations is no longer the progressive thought of consequences—a thought that would be adequate to the idea that the determinations reflect what is, and that what is "has" consequences, to be taken up in turn in new determinations derived from the originals—but rather the regressive thought through which the excess becomes known as I acquire the understanding of what I wanted in it, in terms of what I should have or could have desired implying the already desired or what is assumed desired.

At the very least such an image of the mathematics of the continuum makes sense, providing that it be corrected and relativised as is required: of course entire segments of the reasoning in what is normally called "analysis" today is of another type, taxonomic, algebraic, calculative, etc., of course excess is in fact involved in practically all mathematical procedure, at least in the figure of the so-called potential infinite of indefinite enumeration. Therefore the trope of analysis here can justifiably intervene. But is that a reason for denying that the branch of analysis has a privilege with respect to logical procedures inspired by the idea of control? Is this not what J. Dieudonné suggests in formulating his famous adage "increase, decrease, approach" in the preface to his treatise on *Infinitesimal calculus* (1969, 9) (thus in the form of a "maxim for mathematical analysis")?

But with this we have still to reach the hermeneutical element itself. Is there a profound link between thought that decomposes and regresses and the project of interpretation of what is the "stance of the question" presented as such by the tradition? We would like to succeed in thinking this technique as already interpretative in a minimal but radical sense of the term. Analysing what needs to be analysed, that is what is itself enveloped, strictly speaking I am not calculating or thematising. I am not calculating, for calculus presupposes the dis-implication of the individuals that it acts upon, and thus cannot be the operation accomplishing this dis-implication. Neither do I thematise, for thematisation presupposes the subject of enunciation, whereas the situation requiring analysis is not a situation wherein such a subject is available, and it is rather the result of the analytical act to have themes appearing: analysis operates on an envelopment but otherwise than on a predicate. Likewise, something like the procedure of logical regression eliminates the notion of calculus: on the one hand, the simple fact of being on the logical plane keeps us under the dependence of phrases as concerns truth, whereas calculus is originally and once and for all a manipulation of the etymological elements of calculus, that is "pebbles", thus a treatment of objects (and that, in the modern context, phrastic connecting can be considered as calculus or algorithmic modalities as texts in logical theories does not seem in my view to change anything of importance in this difference, which is principled, and moreover these "transgressive" interpretations rely on it); on the other hand, calculus re-elaborates the objective material that it works upon in an essentially progressive fashion; in principle it is a question of reaching another arrangement and not to reach behind the arrangement facing the mathematician (although this intention is possible, it is yet symptomatic of the type of relation to symbolic objects that we are here calling analysis). Logical regression does not mesh well either with the apophantic declaration of the object's determinations: this declaration is presupposed by all logics, there would be no logical connections, thinkable or to be thought, if determinations had not already been assigned to objects, in order to generate phrases. Logical regression is moreover associated—at the onset (Kant A, 331; B, 387-388) of the transcendental dialectic—with the movement that Kant calls "prosyllogistic", consisting in the search for an attribution of the determinations that condition the one already given, the new attribution remaining suspended as for its truth, known only as the condition of the first attribution: this is as much as saying that regression denounces the apophantic act by linking it to a suspensive condition.

To decompose and to regress are however actual operations belonging to the field and to the traditional method of interpretation: as was set out above, to decompose into a number of sememes is the most classic of acts in the explication of lexical contents—the interpretation of texts consists notably in this explication, which in truth is the fundamental operation therein. The interpretative tension results from the fact that on the one hand the analysis depends on the situation and the context, on the other that it is never complete and certain, for "in fact" meaning is not additive, but rather enveloped or affecting, it has its being in a restraint or a transition which is repugnant to the analytical hunt. Logical regression is also an operation of interpretation: the envelopment of meaning, if it is thought at phrase level, is restored as the complete group of phrases implying the given phrase. The field of consequences of a phrase is readily considered the attestation of the opening of its meaning. But this development is in fact the incremental effectuation of meaning, as is well known despite the logical aporia in which deduction would be either tautology or loss of information. On the "textual" plane—unquestionably the pertinent plane for all questions of meaning—the list of axioms of ZFC, for example, does not mean the opened infinite totality of mathematical theorems. However, all elucidation of the logical preconditions of a logico-linguistic situation is always valid as the explication of its meaning. The theories of presupposition in linguistics have highlighted this point.

The conclusion may then be drawn that the modern unity of analysis may be understood in light of the congruence between the hermeneutical situation of analysis—as a theory of the continuum it is linked to the ageless question "what is the continuum?"—and a certain discursive technique that could be called "analysis" which Greek methodological reflection and Kantian thought of a demarcation between mathematics and philosophy have differently described and defined. "Analysis" would essentially be the name of the relation to what is in general enveloped in itself, and this relation is necessarily, in the same stroke, one of decomposition and interpretative explication. The strange doubling produced in the case of mathematical analysis is that the "stance of the question", that which is enveloped in itself, is but the presentative concept of, as it were, envelopment as such (the continuum). Thus the analysis of the continuum is so to speak a double analysis: it is the analysis of the envelopment of the meaning of the enigma of the continuum, but also of the continuum itself as a presented coherence. This doubling also means that the analysis is part and parcel of an interpretation of the continuum and simultaneously, its representative display. In other words, the move of analysis

explicates the continuum while at the same time symbolically repeating its presentation.

At this point of our reflection, we may return to the Greek geometric analysis that was characterised, at the beginning of this paper, as a procedure moving from the logical precomprehension of an object to its practical comprehension, its constructive effectuation. Analysis in this sense is clearly the name of a hermeneutical rhythm lodged in the totality of contemporary mathematics, which is throughout the anticipation of objects such that their structure is given through logical stipulations. This anticipation furnishes a relation to what I have called elsewhere "correlative objectivity". But it is always assumed that within this objectivity there will be realisable, presentable objects, participating in what I have called, rightfully so, "constructive objectivity". Present day mathematics never ceases, repeating the way of Greek analysis, to determine, in the objectivity obtained on the correlative way, the constructive objectivity that may be recovered, or to think the excess of correlative objectivity over constructive objectivity, by any and all technical means. This is the level at which mathematics as a whole becomes hermeneutical as analysis, and this level must be distinguished from the position and the task of analysis according to Dieudonné, which the preceding paragraph was an attempt to examine and comprehend.

University of Lille III
Department of Philosophy

Notes

1 Cf. the note on page 1322 of Robin's French edition of Plato's dialogues: Plato (OC).

2 I quote from Aristotle (WMK).

3 I quote from Kant (CS).

RICHARD TIESZEN

SCIENCE WITHIN REASON: IS THERE A CRISIS OF THE MODERN SCIENCES?*

I Introduction

In this paper I shall discuss and defend a position on the nature of scientific reason with a view to shedding light on the question of whether there are fundamental crises in the modern sciences. I shall argue that, broadly speaking, it is possible to distinguish science within reason from science without reason. I claim that one source of the view that there is a crisis of the modern sciences stems from the historically recent possibility of practicing science without reason. The phenomena I discuss can be found across the spectrum of the sciences, from mathematics to social science. I invite the reader to think about the argument in connection with his or her favorite science. I will not attempt to discuss details about specific sciences but I will make several remarks about how the argument should be understood in connection with mathematics.

As I see it, my concern here is related to the analytic-synthetic distinction in the following way. According to a central tradition in philosophy, analytic truths are truths of (pure) reason. According to this same tradition, reason is distinct from intuition or observation. I would like to align this view with the idea that analytic truths are true by virtue of meaning alone (which is not, for example, to say that they are true by virtue of form alone). On the other hand, synthetic scientific truths involve reason but they are not truths of pure reason. They are instead to be viewed as truths with respect to which reason is conditioned by experience or intuition. I will also say that they are truths in which the "meaning" (*Sinn*) under which we think objects is conditioned by evidence. It is natural to require, in particular, that there be evidence for existence claims in order to say that it is "known" that those existence claims are true. One might hold, under this condition, that knowledge of the truth of existence claims is synthetic. If we can keep the analytic-synthetic distinction at all, then perhaps it can be kept in this guise. I shall suggest below how this view can be developed, and I will link it to the broader issues with which I shall be concerned throughout the paper.

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 243–259.

II Rationality, Intentionality and Everyday Experience

I will not attempt to present a theory of reason in this paper. I only wish to note that the idea that human inquiry can be informed to a greater or lesser extent by reason has a long tradition in philosophy. In Aristotle's *Posterior Analytics*, for example, one finds the idea that mere observation does not suffice for scientific knowledge, for it gives us mere collections of "facts", without any order, coherence, or purpose. Philosophers like Aristotle and Kant hold that what reason brings to the data of sense experience is unity, a kind of universality, and purpose. I will to some extent follow this classical line, but first I will focus on what I think is a key feature of human reason: intentionality. It is difficult to deny that human reason exhibits intentionality. I will explain this claim, and then draw some consequences from it.

Some basic structural features of the intentionality of human reason can be captured in figure 1.

We can say that a person is directed toward a particular domain of investigation consisting of objects and/or states of affairs by virtue of the contents or "meanings" of her acts of reason. These acts of reason may be of different types, *e.g.*, believing, knowing, remembering, etc. What they have in common is their "directedness" by way of their content. The notion of content can also be thought of as the "meaning" associated with the act, in that we simply take it to be the meaning of the expression that is substituted for *S* in the diagram. Once a particular expression is substituted for *S*, a person will automatically be directed toward a particular domain of investigation in a more or less determinate way. Content plays an important role in the objective, non-arbitrary categorization and identification of objects, and in the description and explanation of change. It should be noted that the diagram picks out structural features of the intentionality of reason. The actual contents substituted for *S* may to some extent be bound to particular times, places or cultures.

The object or state of affairs toward which one is directed in an act of reason is placed in brackets in the diagram because it is essential to the notion of intentionality that human subjects may be "directed" toward objects even if those objects fail to exist, or if they are not completely or properly understood. The logical counterpart to the possibility of nonexistence of the object is found in the failure of existential generalization in the context of verbs of propositional attitude.

Consider an example of everyday experience of the type that motivates the idea of "bracketing" the object. Suppose it is your intention to clean the attic of a house. To reach the attic, you must crawl through a small trap door in the ceiling. As you begin to do this, you find yourself face to face with a large, furry, dangerous-looking spider. As a consequence, you back out of the trap door in order to consider your next move. After some time has elapsed, you again approach the

$$\text{Person } P\ \overbrace{\text{believes}}^{\text{act type}}\ \overbrace{\text{that } S}^{\text{act content or } meaning}$$

$$\Big|$$

refers to
(is directed toward)

$$\downarrow$$

[object or state of affairs]

Figure 1

trap door, this time with a small net to snag the spider should it be necessary to do so. This time, however, you see that it was in reality not a spider that you saw, but a tangle of dark thread, shaped in a way that looks like a spider.

At the earlier stage of your experience, say t_1, you saw a spider and acted accordingly. At some later stage, t_k, you saw that what you took to be a spider is actually a tangle of thread. At t_1 you saw the object under the content or meaning of "spider", but at t_k the meaning under which you see the object shifts to "tangle of dark thread", and this shift is brought on by your further experience with the environment in which you are situated. You make a correction or adjustment of your belief in light of further experience.

What you take to be the object of your belief will be the thing that stabilizes in your experience, that is, the thing that remains invariant through your different experiences with it. It will be the thing to which you (and others) can return over and over again, and which remains the same through these different acts. What the object is taken to be, however, will always be a function of the sequences of acts carried out thus far. The future could bring further adjustments or even surprises. In the worst case, there just might not be an object. The "bracketing" in the diagram is meant to indicate this conditional nature of knowledge of the object.

Suppose that at some further stage of your experience, t_n, you come to see that it was actually not a tangle of thread you were experiencing but a small, shredded piece of black cloth. It is possible that this could happen, but the phenomenon of persistent misperception is atypical. Our experience usually settles down into a stable state in various ways. We do not persistently misperceive objects. Or, to put it another way, consider the conditions under which we persistently misperceived objects. This is the kind of situation in which we might begin to raise questions about our sanity, especially if it should constantly turn out that there are no objects (*i.e.*, that objects are hallucinated).[1] What kinds of expectations could we have in circumstances like this? What could we hope to predict or control in our experience? It is precisely in these situations that we begin to apply the notion of irrationality. This shows us how a form of irrationality (or the absence of reason) is associated with the absence of stabilized objects in our experience. Reason and a form of "objectivity" appear to be mutual conditions for one another. It appears that there can be unity on the side of the subject if and only if there is unity on the side of the object. To be more precise, we might say that reason and the possibility of objectivity mutually condition one another. The example suggests that objectivity, at least as a regulative ideal (in a Kantian sense), is a condition for rationality.

The description in this simple example has the following consequences. First, it shows that we need to be fallibilists about knowledge, or at least about knowledge that depends on sensory input. The picture we have presented precludes certain "absolutist" or "foundationalist" claims about our knowledge of objects of experience. We might find, in further experience, that we were under illusions at earlier stages of our experience. Fallibilism, however, need not imply complete skepticism. It is common for our experience in many domains to settle into a fixed state which serves us well for practical purposes. In any case, we do not doubt everything we believe in situations like this, as is shown by actual experience.

Second, the example shows that there is a kind of continuity through the stages in our experience of objects. The different stages are not radically discontinuous with one another, as if there were no connections between the stages. Indeed, if this were so there would be no possibility of making corrections in the experience we portrayed. A particular kind of continuity is, in other words, a condition for the possibility of identifying misperceptions and illusions.

This point is closely related to a third consequence we can draw from the example: there is a progressive character to the experience. It can be claimed that some progress has been made as the person proceeds through the stages of experience envisioned in the example, in contradistinction to the claim that there is merely change from one stage to the next. "Mere change" suggests discontinuity or incommensurability between the stages, as if at the various stages we had discrete, independent, atomic units of information. It would be as if there were no memory (or history) from one stage to the next, as if nothing about a past stage could be contained in the present stage. The example shows, on the contrary, that there is a kind of cumulativity to the experience. At least some of the content that was present at the earlier stage must be present at the later stage if there is to be a correction in the experience.

I am not arguing that later stages in the temporal sequence always represent progress over earlier stages. The point is rather that there has to be an ongoing stability of the object and a development of further sequences of acts with respect to the particular domain. This is what makes future-oriented thinking possible, and helps to fix our expectations. It leads to the possibility of prediction and control that would otherwise not be present. The notions of "progress" and "correction" here are not absolute. What is judged to be progress or correction is itself relative to what is given in the sequences up to a particular stage in time. In other words, the example shows that it is possible to avoid commitment to an absolutist notion of progress without redounding to the view that there is mere change from stage to stage.

These considerations have direct implications for issues about relativism. Let us say that, by definition, "evidence" is acquired in the sequences of acts in time that we pictured. "Strong" relativism may be defined as the view that there is no evidence that will help us to choose between rival (sets of) propositions. Note that in our example "There is a spider behind the trap door", believed at t_1, and "There is a tangle of dark thread behind the trap door", believed at t_k, are rival propositions. Now is it really true, in our example, that there is no evidence that will help us to choose between the propositions? This seems to be patently false. First, it seems that in the kind of case we are considering we do not typically "choose" what we want to believe. We are forced to some extent to change the content of our belief by conditions in the environment. This often happens automatically and without any deliberation of the type associated with choice. We cannot just as readily believe at t_k that the object is a spider as we can that the object is a tangle of dark thread, as if this were like flipping a coin. It would be absurd to think that we could actually do this sort of thing in our experience. Our experience does not work this way, and it is not clear how it could work this way. We would not get on in the world and behave as we do were strong relativism true. The upshot is that by embedding our rival propositions in the kind of intentional contexts that make up our actual experience, we see that strong relativism is baseless. The example suggests that strong relativism is a philosopher's abstraction that has nothing to do with actual experience.

Our position may, however, be compatible with forms of weak relativism. This follows from the fact that what we know at a given stage is "relative" to the sequences we have carried out up to that stage, along with the fact that we typically do not know everything we could know in these sequences. The future could hold surprises, or we may have to make various adjustments and corrections. This kind of epistemic relativity holds at various levels for the individual perceiver, groups of perceivers, cultures, and for historical periods. Following Edmund Husserl, we could say that truth for us at a given stage is always "truth within its horizons" (Husserl 1929, section 105). It is compatible with this view, however, to distinguish truth or objectivity within its horizons from truth or objectivity as it is. Indeed, the latter idea appears to operate as a regulative ideal in the kind of example we have considered. It is by virtue of possessing this ideal that we realize that our knowledge at a particular stage is imperfect and can be improved. We really do think we are coming to know more about the object. I have no objection to the claim that the notion of a perfect identity through difference (in the case of either the object or subject) is ultimately to be understood as a norm. Similarly, the idea of perfect truth can be understood as a norm. If the notion of intentionality is accepted then norms are part of the package. Thus, our weak epistemic relativism is qualified by a kind of objectivism.

As I said earlier, it appears that reason and the phenomenon of reference to "objects" require one another. On my view, we must think of the content or meaning of an act of reasoning as having a regulative function. It directs us toward a domain of objects or states of affairs which we can then proceed to investigate in sequences of acts in an effort to fill in our knowledge. Reason thus carries within itself at least an ideal of "objectivity" in this sense, and this ideal has a regulative function in our experience. In other words, if reason exhibits intentionality then it also exhibits referentiality. As our diagram indicates, we are directed toward or referred to objects in acts of reason. It is not trivial to note this fact about referentiality, for I will later contrast the referentiality of reason with what I will call "relational" views of scientific thinking.

III Scientific Rationality

Scientific rationality, it seems to me, is founded on and has its origins in the kind of everyday use of reason we considered in our example (Husserl 1936). The example provides a sensible description of how some elements of human experience actually work. Scientific reason is just an extension and development of the use of reason that we see in everyday contexts. In this section I would like to briefly indicate some elements of this extension and development.

We can carry the model of the intentionality of reason over directly to the case of scientific reason. Of course scientific reasoning is more systematic, deliberate and reflective, and we may need to distinguish between direct and indirect evidence, and so on. Scientific theories are just sets of propositions that are believed, as in our diagram, except that they are often believed by groups of people. Groups of people come to see problems under the same contents or meanings and pursue their research accordingly. They are directed or referred to domains of investigation in this way. There will just be different acts, contents and objects in different sciences. Scientists are in the business of finding regularities in these domains, of finding identities through difference. Groups of people could be under illusions about what they are doing, and are susceptible to misperception. They may need to make corrections as research proceeds, and so on. In other words, we are simply dealing here with group intentionality.

I am arguing that our experience in science is founded on everyday experience, and that the various consequences we have noted above will therefore also apply in the case of scientific rationality. To deny this is to hold that scientific

rationality and everyday rationality are disanalogous in the relevant respects, but I see no grounds for such a claim. It could not be the case that one exhibits intentionality and the other does not. Suppose both exhibit intentionality. Then it could not be the case that one exhibits continuity and the other does not, that one exhibits cumulativity and the other does not, and so on. We should therefore be able to say that scientific reason exhibits fallibility, continuity, cumulativity, a particular form of progress, and a weak relativism tempered by a kind of regulative objectivism. Much more could be said by way of defending and developing these claims, but it seems that we cannot give up the basic ideas involved in them without also rejecting what appears to be the sensible and innocuous picture presented in our example.

We can also note that it will be all the better to make corrections and to more closely approximate objectivity if as many voices as possible are heard. The perceptions of specific groups of people can be corrected on this basis. Corroboration is generally important in matters of knowledge, but it seems that in the pursuit of objectivity, rationality demands pluralism about who P in our schema could be. True identities will be those that stand out through multiplicities of persons, places and times. They are multi-cultural. They transcend differences in gender. This view of reason and "objectivity" implies that we should maximize difference in order to obtain true identities. To put it another way, it is not reasonable to monopolize reason. This is also not to say that it is always unreasonable to place some constraints on who or what P could be.

Perhaps there are some principles about which we do not have to be weak relativists. For example, the principle of noncontradiction may be a boundary condition on scientific rationality, in the sense that there is no S for which we can have $S \wedge \neg S$ at a given stage of our experience. We might be able to hold that this principle is necessary, relative to our condition on scientific rationality. We can of course have S at one stage and $\neg S$ at another stage. On the other hand, we can have $S \vee \neg S$ at a stage for a particular S. The idea that $S \vee \neg S$ holds for all S at a stage, however, seems to represent the regulative ideal of the decidability of all questions that permit of "yes" or "no" answers. We might take it to represent truth at the limit of our research.

Truth or objectivity, understood as a regulative ideal, is arguably what motivates the rationalistic optimism about problem solving that characterizes the scientific spirit. Consider for a moment the notion of a scientist who is pessimistic about solving any scientific problem. Perhaps no one has expressed this rationalistic optimism better than David Hilbert. As Hilbert puts it, mathematicians are convinced that every mathematical problem is solvable:

> "In fact one of the principal attractions of tackling a mathematical problem is that we always hear this cry within us: there is the problem, find the answer; you can find it just by thinking, for there is no 'ignorabimus' in mathematics."(Hilbert, 1926, 200)

Problems in some sciences certainly cannot be solved by thinking alone, but Hilbert has nonetheless captured something essential to the scientific spirit here.

What I would like to focus on at the moment is the fact that, as a founded structure, science depends upon a variety of additional developments. Everyday reasoning is, for example, typically informal. The content of our everyday acts in the lifeworld does not involve much by way of formal, structural or mathematical elements. We do not spring from the womb thinking in mathematical formulas. We learn these things later, if at all. We can and do separate the formal or structural elements from the content of our acts as we engage in higher cognitive tasks. It is exactly these formal, structural, mathematical and technical elements that are involved in many varieties of scientific thinking.

Some features of formal or mathematical thinking are especially striking. To take a simple example, consider the following possibility. Suppose I give you a particular rule for computing a number, along with some initial values. Here is the rule:

$$P(B|A) = \frac{P(A|B) \cdot P(B)}{P(A)}$$

The values for $P(A|B)$, $P(B)$ and $P(A)$ will be supplied and they will always fall between 1 and 0. It is your task to compute $P(B|A)$. For example, let $P(A|B) = .33$, $P(B) = .75$ and $P(A) = .25$. You will simply plug these values into the formula, compute, and give me the output. It is clear that you can perform this task without knowing anything about what $P(B|A)$ is, what the numbers represent, what the rule is, where it came from, what the purpose of this task is, and so on. I will call this "relational" thinking.[2] This simple procedure might form only a small part of a very large procedure, consisting of many input values and many rules, in which one obtains some output at the end of the procedure.[3] One could operate, or could imagine operating in a vast environment of this type.

There are many different kinds of examples of relational thinking and its use in the modern sciences. What is characteristic of relational thinking in science is that formulas or symbols are related to other formulas or symbols on the basis of sets of rules, and there is no need to reflect on or to understand the meaning of the formulas or symbols.[4] There have been especially striking examples of relational thinking in the sciences since the rise of formalism and its development into the newer forms of mechanism that are part of computer science. The very idea of computation, which is so dominant in our age, is characterized in terms of formal manipulations of finite sign-configurations on the basis of finite sets of rules which take us from input to output. What makes it generally possible to do scientific work in this formal, relational way is the rise of formalization, mechanization, technization and a practical instrumentalism. These trends have been accompanied by a greater division of labor in and professionalization of the sciences.

The formal, mathematical and technical activities that make up what I am calling relational thinking are rigorous, precise, exact. Rigor and exactness are old and venerable goals of science, and with them we obtain a kind of clarity and distinctness we would otherwise not possess in our knowledge. In fact, it is not difficult to see how one might come to believe that only rigorous and exact technical work could count as science, or could count as giving us genuine knowledge. If one begins to take this very seriously, then everything else that seems to be a part of science or scientific knowledge, more broadly construed, will come to be seen as just a prelude to the real thing. That is, it will be a goal of science to bring everything into this rigorous, exact, technical form if it is to count as genuine knowledge. What is informal, in any context, may then come to be viewed as unreliable. One can see this attitude, for example, in the work of Frege, Hilbert and Tarski. One might come to think that informal reasoning must always involve chance-like guesses or "intuition". Here we have the seeds of a particular form of reductionism that may come to be coupled with eliminativism. It might be argued that whatever is not in this form at a particular stage cannot count as knowledge. Eliminativism goes even farther. Once a science is regimented in this form, why not shed the informal, fuzzy reasoning that led to it? For the hard-nosed scientist of this kind, the notion of something like "informal rigor" would be an oxymoron. It would follow, on this view, that to really know anything you must be a technician. I will use the term "scientism" for the view that only the formal, exact, technical part of our relational thinking can count as genuine knowledge.[5]

IV The Analytic-Synthetic Distinction

In a relationalist climate it would be natural for analyticity to be thought of in terms of form (or formal logic) alone, as if we should understand reason itself in purely formal or relationalist terms. The idea, put bluntly, is that there are only symbols and there is no real content or meaning toward which we might be directed. Meaning or content drops away. In particular, one might think this is true in mathematics. Kurt Gödel has noted two different concepts of analyticity that are relevant to this point. Analyticity (of proposition), he says, can be defined in the "purely formal sense":

> "[...] the terms occurring [in an analytic proposition] can be defined (either explicitly or by rules for eliminating them from sentences containing them) in such a way that the axioms and theorems become special cases of the law of identity and disprovable propositions become negations of this law." (Gödel 1944, 150)

In a second sense, a proposition may be called analytic if it

> " [...] holds 'owing to the meaning of the concepts occurring in it', where this meaning may perhaps be undefinable (*i.e.*, irreducible to anything more fundamental)." (*ibid.*, 151)

This second definition of analyticity appears to be much broader than the traditional Kantian definition. Indeed, much of mathematics would appear to be analytic on this definition.[6] It might be possible to explicate this wider notion in terms of our diagram of intentionality. We take meaning to be specified in terms of our notion of the content (or meaning) of our acts. Analytic truths will be truths in which we can proceed from content to content without mediation by experience of the objects the contents are about. Analytic reasoning is reasoning without intuition of these objects. This would, however, require reflection on or intuition of meaning:

believes that T

Person P believes that S

refers to
(is directed toward)

[object or state of affairs]

Figure 2

We are now directed toward meanings, not just meaningless formulas or symbols. This notion of analyticity requires that meaning itself be analyzable. It requires the notion of informal rigor. In attempting to clarify our understanding of meaning in acts of reflection we typically turn to the concepts of S in order to clarify them. We may then need, upon reflection, to further clarify the concepts used in that effort at clarification, and so on. This is arguably how some parts of our knowledge are developed. Gödel, for example, thinks that we need to analyze the meaning of the general concept of set more deeply in order to solve open problems in set theory, like the continuum problem.

It could not be the case, on this view, that only logic is analytic and that all of mathematics is synthetic. Instead, what would now distinguish logic from mathematics is the fact that logic is content-(or topic-)neutral while mathematics is not. We would need to distinguish different meaning categories in mathematics, in addition to recognizing a form-content distinction.

As I am construing this broad notion of analyticity, it will sometimes be possible to hold that content-to-content links are true or false without needing to have evidence for individual objects toward which we may directed by these contents. But it is exactly such an appeal to our "experience of objects" that is needed for synthetic truths. For synthetic truths, the meaning or intention under which we think of an object must be at least partially fulfilled. In other words, the meaning or intention needs to be conditioned by evidence for objects. I have argued elsewhere that we should understand the fulfillment of "mathematical" intentions in terms of the general notion of construction (as in constructive mathematics) (Tieszen 1989, 1995). It is when we possess constructions that we can be said to have evidence for the existence of the objects our mathematical intentions are about. It follows from these remarks that what is analytic in mathematics will be what is believed to be true (owing to the meaning of the terms involved) but which cannot (at least at present) be understood as constructive. Some parts of mathematics will, accordingly, be analytic (but not content-neutral) and some parts will be synthetic. Impredicative set theory, for example, might be construed as analytic in this sense.

I am somewhat skeptical about the idea that the analytic-synthetic distinction will be important in future philosophical and mathematical work. On the other hand, the issue of what the distinction amounts to seems to involve complications that are not yet understood very well. The view I have suggested may be worth exploring. It is obviously quite far from a relationalist understanding of analyticity.

V Crisis?

If only the formal, rigorous and exact scientific work of the relationalist kind is taken to constitute genuine science or knowledge then we approach a crisis state in science. It follows from what we have said about science within reason that scientism is a form of science without reason.[7] Why?

We are viewing scientific knowledge as a founded creation and in this founded creation we may have purely relational thinking. It is inherent in the formal, technical, structural or mathematical thinking we have called relational that we need not know what it is about. Formalism is often portrayed as a viewpoint according to which we are to abstract away from meaning or content. This also means that we can or even should forget about the origins of meaning in the lifeworld. In relation to our earlier model, if we abstract away from meaning and the informal deep background of the meanings in our experience, then the directedness of our acts drops away or shifts. As I said above, it does not need to be denied that we are, in a sense, "directed" in relational thinking. There is, nonetheless, a significant shift, and we are certainly not directed in the same way. The "objects" toward which we are now directed are symbols or formulas. We are not directed toward the objects the formulas are about in a particular context. We are not directed toward the objects to which the formulas are being applied. We are in a different environment. There may still be a regulative function in this context but now the goal or purpose has shifted. For example, the goal may be to simply obtain the output of a procedure given the input, quite independently of what the procedure is about. All of one's energies may then go to this end and it is possible to become submerged in this kind of work. There can be a complete displacement of concern. Consider, for example, how this has actually been used in various top-secret projects, such as the atom bomb project at Los Alamos. In this kind of situation there is a sufficient division of labor so that many people may work on a project while only a few actually know what it is about. Note that it is not the division of labor itself which makes this possible. Specialization does not by itself preclude the referential model. I am describing a particular alignment of specialization and relationalism. There are also other differences. The work of specialists in the humanities, for example, is not likely to have significant consequences for nature or the environment and is therefore unlike the work of specialists in the natural sciences.

The extent to which a referential or relational model is adopted determines the extent to which various skills and abilities are valued. In a worldview dominated by scientism it is more likely to be held that a person does not really know anything unless this knowledge takes a technical form. The skills and knowledge of technicians will be more highly valued, *e.g.*, the expert's knack for application of technique, or the ability to devise or acquire familiarity with relational systems. Understanding and discovery of the type associated with the referential model will be valued less than formal elegance and pragmatic success. Once goals are understood in a relationist way, it would be natural to see the rise of a kind of pragmatic instrumentalism about how to obtain such goals.

In short, there can be a shift to a very different account of reason, meaning, directedness, objects, knowing, and the like.[8] These concepts, at the level of relationalism, may in fact be reducible to mechanism by way of something like the Turing test. It might even be argued that Turing has captured a relationalist notion in his well known analysis of computation.[9] It is not clear at all, however, how we would have thereby captured the referential versions of these notions. On the relationalist view, we lose sight of (non-symbolic) objects or objectivity, and so we also lose the notion of evidence described above. The structure we pictured in our diagram changes considerably if we focus exclusively on the form or structure of S, in abstraction from content. What remains in place of S at a stage is a formula which we can try to relate to other formulas on the basis of existing or discoverable sets of formal rules.

I maintain that the following claims look all the more plausible from the viewpoint of relationalism: strong relativism, the claim that there is no progress in science, and the claim that different scientific theories are discontinuous and incommensurable. It is not difficult to see why. If objects drop out of our earlier diagram, even as regulative ideals, then we arrive at the position of strong relativism. There are only distinct or rival propositions at different stages of experience and we can make no appeal to the notion of evidence to motivate (justify?) the adoption of one over the other. It would be natural to hold that there is no notion of evidence which could motivate the adoption of one proposition over another. On the relationalist model, it will not be claimed that we are constrained or forced in some ways by our experience of objects. Once objects are out of the picture, it is easy to think that there is mere change from one stage to the next, for then we are only entitled to say that there are different propositions or formulas which need not have any apparent relation to one another. Even if we keep the content of the acts at the different stages of our example, we can ask what spiders have to do with tangles of dark thread. These concepts appear to be discontinuous, and the networks of propositions of which they respectively form parts are arguably incommensurable. Different propositions that appear at different stages might now look like (logically or semantically) independent and discrete units of information. On the purely relationalist picture, the propositions that appear at different stages of our experience will simply be different. There is difference without continuity. No connections between the formulas or even the meanings at the stages can be seen because it is not possible to recognize mediation through the experience of objects. On the referential model, there is difference with continuity.

I am not claiming that formal, mathematical and technical work is not important or not needed in science. Quite the contrary. Formal, mathematical and technical work is a necessary condition for science. It should also be apparent from the comments above that I am not claiming that mathematics is without reason. One can hold that formalization is very important without being a formalist. Similarly, it can be held that mathematization and technization are important without reverting to scientism. I do not think that mathematics is purely formal or relational. It does not exist only to serve the other sciences. It is also contentual. I would argue that the model of intentionality described above also applies to the (founded) science of mathematics. Thus, there are acts, contents and objects appropriate to mathematics. We are directed toward mathematical objects through the meanings of our mathematical acts. Mathematical objects are distinct from sensory objects, and there will clearly be some differences in the ways that we come to know about these kinds of objects. The meanings or contents have a regulative function in our mathematical experience. It is possible to become more reflective about these meanings, and more conscious and systematic about our understanding of them. Perhaps the motivation for reflection arises most clearly at the boundaries of the science of mathematics, where there are difficult open problems. Mathematics receives its meaning and direction through its own distinctive content, and not primarily through its applications in or its services to the other sciences. In order to know about the objects toward which we are directed, our acts have to be at least partially fulfilled. We can certainly say that science within reason involves relational thinking, but we do not need to hold that only this kind of thinking counts as knowledge or genuine science. It is a matter of balance.

If there have been excesses in the direction of scientism, then there have also been excesses in the direction of anti-scientism, to the extent of being anti-scientific. It has been suggested, for example, that this is the plight of Heidegger's work, and the suggestion could perhaps be extended to much of the post-modernist theory that has followed in Heidegger's wake.[10] After all, what has happened to the notion of reason in this work? The answer to this question is closely linked to what has happened to the notion of intentionality. The notion of intentionality or directedness has disappeared or been radically reinterpreted. It is supposed to be a virtue of Heidegger's position, for example, that the act-content-object model is undermined and replaced by appeals to practices and skills. There are no objects. (It is even a question whether there are any subjects.) Just as one can speak about propositions without objects on some of the views we have been considering, so one can speak about practices without objects. But if there are no objects, then at different stages we have only a motley of distinct or rival practices and we can make no appeal to the notion of evidence to motivate (justify?) the adoption of one practice over another. It would be natural to be of the opinion that there is no notion of evidence which could motivate the adoption of one practice over another. Once objects are out of the picture, it is easy to think that there is mere change between one stage and the next, for then we are only entitled to say that there are different practices which need not have any apparent relation to one another. These may appear to be discontinuous and incommensurable. Practices that appear at different stages of history or in different cultures will simply be different. There is difference without continuity. We can see no connections between the practices at the stages because, counter to the referential model, there is no mediation through the experience of objects. On the referential model, there is difference with continuity. Some post-modernist authors arguably embrace just such a notion of difference without continuity, or of difference without objectivity. In an interesting parallel with scientific relationalism, some post-modernist writers suggest that everything is symbolic, everything is a text. There are also other variations on this theme: everything is just a "language game", or there are just narratives without objects.[11]

Thus, I am arguing that strong relativistic claims about meaning, objectivity, progress, continuity and the like appear to be more plausible not only from the viewpoint of scientism, but from any viewpoint that rejects the notion of the intentionality of reason described in section II.

VI (Un-) Intentional Knots

In the account I have presented, scientific activity is taken to be founded on basic "lifeworld" activities of human beings. In the founded structure of modern science we create a viewpoint which we then turn upon various phenomena in the world. Suppose it is held that only the formal, rigorous, technical work that is part of relational thinking can count as genuine scientific knowledge. Suppose, in other words, that scientism is true. When we turn this viewpoint back around to ourselves it should come as no surprise that reason, meaning, and indeed consciousness itself disappear. We live in an age in which it is fashionable to talk about the disappearance of these things. We hear this talk everywhere. It is, for example, reflected in work in cognitive science, where the concepts of intelligence, thought, etc. are understood in a formal, mechanical, and computational way. There is nothing more to these phenomena. And to "know" anything about these phenomena one has to be a technician. Everything short of technical knowledge in this domain is relegated to "folk psychology".[12]

I am claiming that in all of this we are interpreting ourselves through a (founded) viewpoint that we have created. This viewpoint is itself an interpretation. It is not some neutral, theory-free, value-free, "correct" viewpoint. It is itself a "content" or "intention" under which groups perceive the world. Some investigators may then try to fulfill this intention. They may, for example, try to fulfill the intention according to which we are machines, or even the intention according to which there are no intentions. But is it possible to fulfill the intention according to which there are no intentions? If the analysis above is correct, then we cannot pretend to eliminate the semantic notion of an interpretation by appealing to the sciences. There are also reasons for believing that it is not necessary to interpret ourselves exclusively in this way. Perhaps there is no point of view prior to or superior to that of natural science, as is sometimes claimed in efforts to naturalize epistemology, but if the argument of this paper is correct then it also does not follow that an uncritical natural science can occupy a privileged position.

In the situation of the modern sciences that we have described there is a particular irony that borders on paradox: the extent to which we apply science without reason to ourselves is the extent to which we come to believe that reason is not intentional and, hence, that science is without reason.

VII Conclusion

The main argument of this paper can be summarized as follows: the use of reason in everyday experience exhibits intentionality. Scientific rationality exhibits intentionality but it is founded on everyday reasoning and is more complex and systematic. Some scientific thinking is relational. Many concepts may come to be thought of in a relationalist way, including the concept of analyticity. Now suppose, as in scientism, that only relational thinking in science can count as genuine science or knowledge, on the grounds that only this kind of thinking is rigorous, reliable and exact. It follows from the claim that reason exhibits intentionality that relational scientific thinking by itself, as in scientism, is without reason. The fact that it is possible to practice science without reason in this sense is one source of the view that there is a crisis of the modern sciences. Science within reason must involve relational thinking, but it cannot be held that only this kind of thinking counts as genuine knowledge or science.

San Jose State University
Department of Philosophy

Notes

* Versions of this paper were presented at a seminar on the nature of science at the University of Oslo and in the philosophy department colloquium at San Jose State University. I thank the members of both audiences for helpful comments, and especially Dagfinn Føllesdal and Alastair Hannay. Some of the work on this paper was supported by N.W.O. (Dutch National Science Foundation) Grant # 22-266. I thank Dirk van Dalen for sponsoring the grant. Finally, I am grateful to Nancy Tieszen for a number of important suggestions and comments.

1 Since we are on the matter of spiders, consider also whether or to what extent you would take the activity of dreaming about spiders to be rational.

2 The points I wish to make about what I call "relational" thinking are similar to some points made by, among others, Husserl (1935-1936), Cassirer (1923-1929), and more recently, O'Neill (1991).

3 Note to those for whom this rule is purely relational: this happens to be a very important rule. It is one of Bayes' rules for computing conditional probabilities.

4 It does not need to be denied that we are "directed" in relational thinking. We can say that we are directed, but it is now toward the formulas involved and toward obtaining the output from the given input. This, is not, however, the same thing as being directed toward the objects the formulas are about in a particular context (cf. section V).

5 I argue in unpublished work that scientism or relationalism is closely related to some viewpoints that Gödel criticizes in Gödel (1961) and other papers. Gödel can therefore be seen as making some similar points about science without reason. In particular, see his comments on the imbalance of "leftward" directions in philosophy and his objections to Hilbert's program and to Carnap's "syntactical" program. In addition, Hao Wang suggests that Gödel sympathizes with Husserl's claim that we must consider the origins of science in everyday experience (Wang 1987, 62, 122 and 239).

6 This idea has been explored to some extent, in relation to Quine and others, in Parsons (1995). It is worth noting that Bolzano also recognizes narrower and broader notions of analyticity (Bolzano W, sect. 148).

7 An immediate corollary is that scientific rationality, as described above, is not itself the source of crisis in the sciences. I note this consequence because there appear to be views on which it would be denied.

8 One fairly clear example of this can be found in Hilbert's conception of metamathematics. The objects toward which we are supposed to be directed in metamathematics are finite sign configurations. What is taken to be meaningful, reliable, and knowable in mathematics is to be understood on this basis. Hilbert then seems to construe properties like decidability in purely formal or mechanical terms, although some of his appeals to Kant's views about reason obscure elements of his conception of metamathematics. On the basis of what we have said above, it is not surprising that Hilbert's program has been interpreted as a form of instrumentalism.

9 See Turing (1936). I discuss some related ideas in section 5 of Tieszen (1994). Could there be a "referential" notion of computability? Such a notion would refer to what intentional systems do when they are computing *and* know what the computation is about.

10 It is on this kind of point that Husserl and Heidegger parted ways.

11 This view about narratives is arguably appropriate to literature and fiction, but it is not clear to me that it extends to other domains. See the section of Tieszen (1995) entitled "Against Fictionalism".

12 Compare, for example, the work of Dennett (1991) and Searle (1992) on consciousness.

MICHAEL OTTE AND MARCO PANZA

MATHEMATICS AS AN ACTIVITY AND THE ANALYTIC-SYNTHETIC DISTINCTION

I Intensional and Extensional Theories

Frequently, in modern discussions in philosophy of sciences, science—that is the object of the discussion—is intended as a class of (scientific) theories and a (scientific) theory is conceived as a linguistic system, or even as a class of propositions. Moreover, scientific theories (in this sense) are intended either as purely "intensional theories" or as purely "extensional theories".

By "intensional theory" (in the previous sense of the term "theory") we understand a theory that, as a set of postulates (or by means of a set of postulates), determines the intensions of its terms and in which (if you accept that there are extensions, in a proper sense) the extension of each term, that is its referential domain, is not only delimitated by its intension, but it is also constitued by it, as a sort of logical counterpart of it. The elements which belong to such an extension are not given independently of the theory, they are nothing but what the terms of the theory denote (if we accept that such terms are denotative terms). As Gödel says, "the existence of a class" depends "on the content or meaning" of the propositional functions (Gödel 1944, 132). Thus, an intensional theory is not really open with respect to the growth of knowledge and to the changes of our understanding of something that is not fully determined by the theory but exists outside of it.

In a proper sense it does not realize, as such, any form of knowledge or objective understanding; it is a closed domain, which provides no more than synoptic tables or something like that. Even if a some people have conceived empirical theories in purely intensional terms, the privileged model of an intensional theory is provided by a mathematical axiomatic theory, intended as a purely formal system. A classical example is provided by the Hilbertian axiomatic reconstruction of Euclidean geometry. Here, if the terms "straight line", "point" or "plane" are intended as denotative terms, they denote nothing but the arguments of the conditions expressed by the axioms. This idea was expressed by G. G. Granger by means of the notion of "formal content": if the terms of an intensional theory denote something, they denote formal contents (Granger 1982).

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 261–271.

Of course a lot of people have denied that the terms of an axiomatic Hilbertian theory (or of a formal theory in general) were denotative terms. They simply are, it is claimed, symbolic characters in a syntactical game or expressions of concepts without objects, as in the conceptualist account of mathematics (for example: Tharp 1989-1991). Even if in such cases the terms "intensional theory" could be misleading, we propose to maintain it, providing the term "intension" with a more general meaning than would be required in order to be able to speak of intensions as we have done up to now. We will come back to this point later. Let us pass now to the notion of "extensional theory".

By an "extensional theory" (in the previous sense of the term "theory") we understand a theory that speaks about something that is already given otherwise. The terms of such a theory have an intension as well as an extension, but neither the term "meaning" nor the terms "intension" and "extension" are understood in a way that would necessarily depend on the particular theory. Rather, the extensions are given by a sort of reality, intended as a system of things (acting upon the subject), and intensions are nothing but the means by which such things are introduced in the theory. Intensions seem to relate to extensions by grasping their "essential" characteristics in an unspecified manner.

The privileged model of an extensional theory is provided by a physical theory conceived as a realistic account of the external world. Nevertheless, a lot of people—the Platonists, as they are generally called—have advanced the idea of also interpreting mathematical theories as extensional theories. But in order to do so—without abandoning the idea that a mathematical theory is a formal theory—we have to accept something like an ideal reality that, in principle, is describable by means of a convenient set of definitions or axioms, expressing the "essential" characteristics of a domain of things (even if, purely formal things).

II Analytical and Synthetical Judgments

If we understand mathematics as a class of theories and these theories either as intensional theories or extensional theories, we are confronted with a number of difficulties when trying to make sense of the classical Kantian analytic-synthetic distinction. Let us consider this point in some detail.

By considering mathematics as a class of theories in the previous sense, many people have understood this distinction as primarily concerning the (logical) properties of mathematical propositions or the (logical) nature of their justification. As a consequence of such an understanding, the hard core of the Kantian thesis has been located in the assertion of the syntheticity and apriority of mathematical judgments, as explained according to the criterion advanced by Kant in the *Introduction* to the first *Critique*: a subject-predicate judgment is analytical if and only if the predicate does not assign to the subject any properties other than those that it has to have in order to be just that subject, otherwise it is synthetic (Kant, A, 6; B, 10).

In order to apply such a criterion to the judgment "q is P", we have to understand the subject as something that is q and not simply as something that we call "q" : "q" is not a name here, but is already a way of specifying the nature of the subject itself. This is the reason why the examples that are generally presented to illustrate the Kantian criterion are not of the previous form, "x is F", but of the form "all G's are F", or, better, in the usual Fregean translation, "for all x, if x is G, then x is F". In this way, the subject-predicate judgment is interpreted as a way of connecting not really a subject to a predicate, but a predicate (that is G) to another predicate (that is F). Predicates play two different roles here. The first (that is G) specifies the domain to which a generic subject belongs (and in this way it specifies the subject, completely or partially) the second assigns to such a subject a certain property. It is only if a subject-predicate judgment is intended in such a way, that we can apply Kant's criterion: such a judgment will be analytic if and only if F expresses a sub-specification of the property expressed by G. The judgment "all congruent triangles are similar" is analytic—we could say—because the predicate "to be congruent" is a sub-specification of the predicate "to be similar" (for a triangle). But, here another presupposition is required. The properties expressed by our predicates have to realize a partial order with respect to a meta-relation of inclusion. And, in order to say that a certain judgment "is" analytical or synthetical, we have to assume that the configuration of such a partially ordered space of properties is fixed.

From such a point of view, to be something means (or has to be intended as) to satisfy a certain property and to satisfy a certain property implies that a certain set of other properties is met or fulfilled. Thus, the problem of analyticity or syntheticity of a judgment is the problem of connection between different properties : a mathematical judgment, as "all Q's are P", or "for all x, if x is Q, then x is P" would be synthetic if and only if it was logically possible to satisfy the property Q, without fulfilling the property P. But a mathematical judgment has to be proved in a mathematical theory (except if it is an axiom or a definition). So, a mathematical judgment would be synthetic only if it was possible to prove that to satisfy the property Q means to satisfy (among other) the property P, even if it is logically possible to meet the property Q, without meeting the property P.

The problem concerns, of course, the notion of "logically possible". In the previous context this notion refers to the partially ordered space of properties to which the properties P and Q belong. Such a possibility takes place if and only if the property Q does not include the property P. But how is the configuration of such a space fixed?

II.1 Mathematical Theories as Intensional Theories

If a mathematical theory is intended as an intensional theory, such a configuration can not be fixed outside or independently from the theory itself. Outside the theory there is properly speaking nothing concerning the theory itself. Therefore, if a judgment is a theorem (an axiom or a proposition) of the theory (that is, if it is a proposition of the theory and, thus, a mathematical judgment), it cannot be but analytic.

This seems immediately obvious, but we prefer to insist a little bit more on this point. In our characterization of an intensional theory we have not really specified what kind of things intensions are and this could cause problems in order to understand the point.

What then are intensions? With respect to our problem of deciding whether a judgment is analytic or synthetic, we need only answer this question up to relations of difference and equality of intensions (and in fact we can only answer it so). Using an informal language of sets and in particular interpreting equality as mutual inclusion of sets (and the latter in turn as logical implication) we realize immediately that, whatever intensions might be, in an intensional theory all statements are analytic, because they just state relationships of inclusion between intensions (interpreted as sets here). Therefore, the analytic-synthetic distinction makes no sense with respect to a mathematical theory intended as an intensional theory.

Perhaps it makes sense as a correlative distinction with respect to the other distinction between a mathematical judgment and an empirical one: all mathematical judgments being analytic, it could be possible that all empirical judgments are synthetic, because empirical theories are not intensional theories, as the terms of the theory cannot be complete descriptions of their referents. Otherwise for such a theory to have referents would equal its being true and *vice versa*. Now, in conceiving of (mathematical or empirical) theories as intensional theories, one negates a fundamental insight of Kant's *Critique*, namely that "no general description of existence is possible, which is perhaps the most valuable proposition that the *Critique* contains" (Peirce CP, 1.35). Thus this view amounts to denying the essential Kantian idea, namely that synthetic *a priori* judgments are possible and they take place in mathematics (even if not only there). Therefore, even if we could make sense of the Kantian distinction, with respect to mathematics (although not "within" mathematics[1]) it would fail its essential aim.

II.2 Mathematical Theories as Extensional Theories

It might appear that the situation changes essentially if we conceive mathematics as an extensional theory, but this is not really the case. For a long time it has

generally been accepted that predicates bearing on empirical extensions could be connected analytically—providing logical truths, rather than genuine empirical judgments—as well as synthetically—providing genuine empirical judgment. But—as Quine has shown, in *Two dogmas of empiricism* (Quine 1953, 20-37)—even if this distinction can be maintained, from the point of view of an extensional theory, it does not express anything but our decisions on the internal organization of our language.

The arguments and conclusions of Quine are well-known and it is not necessary to repeat them. We would only like to insist on one point that seems to be connected with our problem concerning mathematical extensional theories. If we accept the Kantian criterion of the *Introduction* to the first *Critique*, as Quine does, essentially, we are compelled to assert, as we have seen, that a (true) subject-predicate judgment—let us say "all Q's are P"—is synthetic if it is not necessary to be P, in order to be Q, even if, contingently, all Q's are just P. Even though, it would seem to be a very natural situation from an extensional point of view, it is not.

Let us consider an example. We can argue, it is not necessary to weigh less that 200 pounds in order to be a swan, even if, contingently, all swans weigh less than 200 pounds. But, how are we sure that it is not necessary to weigh less than 200 pounds in order to be a swan? This is possible only if we have in our hands a precise and objective definition of what a swan is and if such a definition does not include that a swan weighs less than 200 pounds. Nevertheless, if we intend a swan as a "real external object", that is how it is independently from all possible definitions that we could give, it is possible only if our definition grasps what is "essential" in a swan, without specifying all properties of a "real swan", so that we can imagine genuine swans different from real ones, for example swans weighing 300 pounds, or even 30.000 pounds. But how do we know what is "essential" in a swan? has some God given the required definition? Certainly, in a proper sense, we cannot know it, we can only decide it. Thus, it is clear that the the analytic-synthetic distinction makes sense for an extensional theory (according to the criterion of the *Introduction* to the first *Critique*) only if the predicates are introduced into the theory by means of a definition which determines their logical range, according to a certain decision. This shows that an extensional theory—as well as an intensional one—depends on the choice of a perspective. A judgment like "all swans weigh less than 200 (or even 30.000) pounds" is then either analytic or synthetic, according not to the "objective extension" of the predicate "to be a swan", but to the perspective that has been chosen in fixing the logical range of such a predicate.

In order to make this point clearer, let us assume, provisionally, that properties are nothing but (names of) classes of objects. This is exactly the content of what is called generally the "axiom of extensionality" (Gödel 1944, 137):

$$\forall Q, P\{[Q = P] \Leftrightarrow \forall x[Q(x) \Leftrightarrow P(x)]\} \tag{1}$$

Once this axiom is given, let us consider two predicates G and F, such that $\neg(G \subseteq F)$. As G is then distinct from F, according to (1) these predicates satisfy the condition:

$$\exists x\{[G(x) \wedge \neg F(x)] \vee [F(x) \wedge \neg G(x)]\} \tag{2}$$

Let us consider now the domain of G and determine the range of the free variable x relatively to it, such that:

$$\forall x G(x) \tag{3}$$

As from (2) and (3) it follows

$$\neg\forall x[G(x) \Rightarrow F(x)] \tag{4}$$

we have,

$$\neg\{\neg[G \subseteq F] \wedge \forall x[G(x) \Rightarrow F(x)]\} \tag{5}$$

Thus, the judgment "all G's are F" cannot be synthetic, according to the criterion of the *Introduction* to the first *Critique*: the distinction between analytic and synthetic judgements like "all Q's are P" makes sense in an extensional theory, according to such a criterion, only if the space of the predicates occurring in it is partially ordered, independently from the partial order of the classes which constitute the extension of these predicates.

But, if so, how the partial order of the predicates is fixed? From an extensional point of view—different from an intensional one—we can try to answer in a number of ways, all of which do not provide however meaning for the Kantian distinction (according to the criterion of the *Introduction* to the first *Critique*).

First, we can imagine that it is an aim of our theory (or of a part of it, for example of the "meaning postulates", as Carnap proposed (Carnap 1952)) to provide the configuration of such a space. But if this is the case the distinction between analytical and synthetical judgments is nothing but an expression of the organization of the theory itself. Second, we can imagine that such a configuration is fixed once and for all, as if it were the configuration of the mind of God. In such a case, the "real" distinction between analytical and synthetical judgments rests on foundations unknown to us and our distinction is nothing but a conjectural representation of it[2]. In the first, as well as in the second case, it seems rather

arbitrary and open to points of view whether a statement is considered analytic or synthetic and the analytic-synthetic distinction does not lead to much.

But there is a third possibility: we can accept the Leibnizian idea according to which things are to be distinguished on basis of the sum of all their actual properties, so that it is not possible to be a Q, without having all the properties that a Q has. The configuration of the space of properties is then imposed by the real nature of things. All true judgments are analytical in this case.

Someone has imagined that, with respect to mathematical extensional theories, we are necessarily in such a case. From such a point of view formal theories, like mathematical theories, are in fact considered as meta-linguistic theories dealing with linguistic extensions, for which the "sum" of their actual properties is finite, and to be a (mathematical object) Q is exactly to have all these properties and only them. We can justify in such a way the neopositivistic thesis, according to which all mathematical judgments are analytic. Thus, the thesis of analyticity of mathematics can be defended by intending mathematical theories as intensional ones as well as by conceiving them as extensional theories. Whatever the choice may be, by accepting such a thesis one denies the essential content of Kant's thesis.

If we deny, in contrast, that mathematical extensional theories are meta-linguistic theories the situation for such theories is not really different from that for empirical extensional theories. Thus Quine's argument can be applied *mutatis mutandis*.

We may suppose that to be a certain formal thing is to satisfy certain properties $\{Q_i\}$ (that is, in a more convenient interpretation, certain conditions), expressed by certain definitions or axioms, in such a way that without any additional axiom it is not possible to prove that the fulfillment of these properties (or conditions) entails the satisfaction of certain other properties (or conditions) $\{P_i\}$. But we can introduce some additional axiom (and passing, for example, from absolute geometry to Euclidean geometry or from finitary arithmetic concerning numbers $\{0, 1, 2, ..., 100\}$ to infinitary usual arithmetic, or from an algebra without associativity for a certain operation to an algebra with associativity for that operation) and then prove that to satisfy the properties (or conditions) $\{Q_i\}$ entails the fulfillment of the properties (or conditions) $\{P_i\}$. We can interpret such a case in different ways and if our reasoning capabilities are strong enough, we may arrive at a justification of the syntheticity of a certain mathematical judgment. We can even interpret in this way the thesis of Poincaré according to which arithmetical infinitary judgments are synthetic (the additional axiom being the fifth axiom of Peano) or Cassirer's claim, according to which all the usual arithmetical judgments, like $n+m = \nu$, are synthetic (the additional axioms being the associative law of addition) (Poincaré 1894 and Cassirer 1907). But it is clear that there is no possibility to show that a certain mathematical judgment is, in such a framework, definitely synthetic. In order to make such a claim, we should justify that the real ideal things, of which the theory is speaking, are completely described by the first axioms only. And, we certainly cannot do that.

III Cassirer and Poincaré

But, of course, neither Poincaré nor Cassirer presented their theses exactly in such terms. Rather it seems that, when they state that arithmetical judgments are synthetic (and *a priori*) they do not refer to the Kantian criterion of the *Introduction* to the first *Critique*. But it is very difficult to say what their criteria for the distinction between the analytic and synthetic really are.

Cassirer (1907, 41) considers the proposition "7+5 = 12", quoted by Kant in the *Critique of Pure Reason*, to be synthetic, because its proof contains "a synthetic assumption", namely "the theorem that $a+(b+1) = (a+b)+1$". But, what Cassirer terms a synthetical assumption here is a special case of the associative law, which functions as a definition of the addition on the basis of the successor operation of ordinal numbers in the normal axiomatic characterization. Thus, even if we accept that the proposition "7+5 = 12" could be intended as a subject-predicate judgment, it would be very difficult to justify that it is possible to intend the subject of this proposition—that is the sum-number 7+5—without characterizing the operation of addition by means of the associative law or in a way that entails such a law. In case we characterize the operation of sum in terms of the cardinality of sets the situation is completely different. The associative law is in fact in such a case a consequence of our definition of addition (and not a part of it), and people could claim that such a consequence does not follow by a formal proof, but is to be observed by experience of concrete sets and their unions ; thus, it is nothing but a (quasi-empirical) generalization. If we accept that, we might conclude that the judgment expressing this law is synthetic, and the related proposition "7+5 = 12" as well. But the question is completely open to points of view. Thus, it is clear that, if the criterion of syntheticity of a judgment is that of the *Introduction* to the first *Critique*, Cassirer fails in asserting that "7+5 = 12" is definitely a synthetical judgment. This conclusion depends on our definition of addition and on our point of view with respect to the way in which the properties of the operations on sets are stated.

The same is true for Poincaré. Poincaré (like Hölder 1924) called (infinitary) arithmetic synthetic (and *a priori*) because arithmetical propositions—being founded on the axiom of recursion—are just expressions of the free activity of the human mind, they represent the structure of the subject itself. According to Poincaré, recursion cannot be reduced to the principle of contradiction: it is "the affirmation of a property of the mind itself" (Poincaré 1894, 12-13). The fifth axiom of Euclid in contrast is nothing but a "definition in disguise" (Poincaré 1891, 50), Poincaré

believes, and it has been chosen only for reasons of our convenience. Thus, according to Poincaré arithmetical propositions are synthetic (and *a priori*) because they are founded on something we can intend as an *a priori* assumption to which the human subject is compelled by its very nature. Let us try to get rid of recursiveness—Poincaré says—and "let us construct a false arithmetic analogous to non-Euclidean geometry. We shall not able to do it" (*ibid.*, 49).

Clearly, Poincaré, as well as Cassirer, refer here to a different criterion for syntheticity than that of Kant's *Introduction* to the first *Critique*. But what is this criterion? This is really not very clear. Perhaps, we have to see in this lack of clarity one of the reasons for the success of the neopositivistic attitude concerning mathematics.

IV Mathematics as an Activity

Thus, we have to conclude that, both from the point of view of an intensional theory and from the point of view of an extensional theory, a logical distinction between analytical and synthetical judgments, founded on the criterion of the *Introduction* to the first *Critique*, makes no real sense. Do we have to conclude from this also, that the Kantian distinction as such, makes no sense logically ? We think not. We believe in fact: *i*) that the Kantian criterion of the *Introduction* to the first *Critique* is nothing but a bad illustration of a deeper idea; *ii*) that, in order to understand such an idea, we have to abandon the presupposition according to which mathematics (and science, in general) is to be understood as a class of theories (a theory in turn being a class of propositions); *iii*) that, by abandoning such a presupposition, we could gain a new perspective on the nature of logic, usually intended as an inquiry into the formal characters of our knowledge; *iv*) that, by assuming such a perspective, we can get rid of the dichotomy between intensional and extensional theories and conceive a scientific theory as something else.

The situation in fact changes radically if a theory is considered in the context of its genesis or application, or, in other words, if the notion of theory is transformed to incorporate activities that represent the epistemic subject-object relation. Extensions and intensions enter into varying and flexible relations with each other and this means that we have to base our considerations on the evolutionary process of cognitive activity, rather than on the idea of a theory as a class of propostions. Bolzano, among others, had accused Kant of having confounded mathematics with its development. Kant was right, although his ideas, with respect to the question of "the objectivity of the subjective" were insufficient and ahistorical.

From our point of view, mathematics (like science in general) has to be understood as a human activity, namely the activity of producing mathematical (or generally scientific) theories (in the previous sense). The aim of logic is not merely to study the internal structure of such theories, or even the formal nature of their propositions (either taken in isolation or jointly). Rather it implies the study of the modalities of human activity that produces them. Such an activity is a concrete and historical phenomenon. It is in terms of this phenomenon only that, we believe, it becomes possible to explain all other phenomena or entities. Nevertheless, logic has not to be confounded in our perspective with psychology. The latter treats the human activity producing our knowledge as a particular activity proper to each singular subject and tries, if it is possible, to isolate some constant features of it. The former treats of the general categories that may be used in describing such an activity and tries to understand the way by which it realizes intersubjectivity and founds the external world with respect to each subject.

Such a perspective is not to be confounded with a solipsistic point of view. Every realism, we believe in fact, has to be a constructive realism. Neither subject nor object exist in isolation and activity marks the essence of the subject-object relation, that is fundamental with respect to both relata. We do not suppose that only the individual subject exists, all the rest being pure appearance, but, to the contrary, we think that the notion of existence, or reality, is not a primitive notion, but has to be intended in terms of the modalities of the subject's activity.

Now to describe or explain the activity itself—and this is the only way for explaining a lot of subjective evidence (for instance the phenomenon of intuition)—one may conceive it as a system of means-objects relations. No activity exists without means and without objects. And neither internal experiences nor objective constraints can be understood but in terms of means and contents of activity. External conditions for the subjective activity or for consciousness are just to be intended as contents of intentional acts. The form they take is thus the form of these acts and the objectivity of such a form—that is the fact that it can be assumed as the same in our communication or along our life—is nothing but the effect of our capacity of connecting evidences in classes of equivalence and of inducing intentional acts in similar subjects. Of course, such a capacity is, once again, an hypothesis we advance in order to explain our evidences, that is: it is part of the intentional acts directed to pose ourselves as a subject or the external subjects as such.

Now, in such a context, science is nothing but a specific way of producing objectivity and the problem of a philosophy of science is essentially the problem of explaining this objectivity in terms of the activity that produces it. A scientific theory is nothing but a way for expressing this objectivity. We can recognize in it an intensional as well as an extensional component. The former is connected with the fact that the objectivity is the result of an activity, that is an act of consciousness. The latter is connected with the fact that this activity is an intentional one: it is just that which produces an objectivity.

But, what new sense can we give, from such a point of view, to the analytic-synthetic distinction, with respect to mathematics? We will try to answer to such a question in our following two papers.

Institute for Didactics of Mathematics,
University of Bielefeld

Centre F. Viète of the History and Philosophy of Sciences,
University of Nantes

Notes

1 For Kant, all mathematical judgments seems to be synthetic, and therefore for him also such a distinction—if it is applied to judgments—takes sense with respect to mathematics, but not in mathematics.

2 If we were realists, we could argue that certain properties are necessarily connected to other properties being particular specifications of them. For example, the property of being red—we could say—is a particular specification of the property of having a color (of reflecting the light). Thus a judgment as "all reds are colored" should certainly be analytic. However, this argument has two main defects: not only does it not prove that objectively synthetic judgements exist, but it is false also. In fact, if we were realists concerning properties, we should make a distinction between real and ascribed properties, an ascribed property being a property compounded by real properties or a generalization of certain real properties. According to such a sense, to be colored is certainly an ascribed property, because a body does not simply reflect the light, but reflects it in a peculiar way. Thus the necessity of the connection between the real property of being red and the ascribed property of being colored is, once again, a question of definition of the second property.

MARCO PANZA

MATHEMATICAL ACTS OF REASONING AS SYNTHETIC *A PRIORI**

I Introduction

My paper pursues two aims. First, I would like to argue that mathematical activity deals with pure objects, or even that mathematics is the human activity dealing with mathematical (that is pure) objects. In my view, this means that mathematical activity essentially consists of synthetic acts of reasoning and, as mathematical objects are pure objects, these acts are also *a priori*. This thesis should not to be confused with the standard thesis generally ascribed to Kant, according to which mathematical judgments are synthetic *a priori*. Nevertheless, I think that my thesis could be presented as a development of some of Kant's views on mathematics: as such, it is not a Kantian thesis, but I believe it is a "quite natural" consequence of Kant's views. Thus, my second aim is to trace a path leading from Kant's premises to my own conclusions.

II Standard Accounts

According to section V of the *Introduction* to Kant's *Critique of Pure Reason*, "All mathematical judgments [*Mathematische Urteile*[1]], without exception, are synthetic [*synthetisch*]" and "mathematical propositions [*Sätze*], strictly so called, are always judgments *a priori*" (Kant B, 14). If we accept that every mathematical judgment is a (mathematical) proposition, we have to conclude that:

(T_1) Every mathematical judgment is synthetic and *a priori*.

Since Kant certainly agreed with the auxiliary premise, (T_1) is certainly a Kantian thesis, and it is advanced by Kant in the section V of the *Introduction* to the first *Critique*. Thus, it is very natural that (T_1) is presented as an important Kantian thesis concerning analysis and synthesis in mathematics. Generally, this thesis is explained by referring to the following passage contained in section IV of the same *Introduction*:

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 273–326.

"In all judgments in which the relation of a subject [*Subjekt*] to the predicate [*Prädikat*] is thought [...], this relation is possible in two different ways. Either the predicate *B* belongs [*gehört*] to the subject *A*, as something which is (covertly) contained [*enthalten*] in this concept [*Begriff*] *A*; or lies outside the concept *A*, although it does indeed stand in connection with it. In the one case I entitle the judgment analytic [*analytisch*], in the other synthetic."(Kant A, 6-7; B, 10)

According to Kant, a judgment is not merely a "representation [*Vorstellung*] of a relation between two concepts" (Kant B, 140), but it is "the manner in which given modes of knowledge [*Erkennntnis*] are brought to the objective unity of apperception" (Kant B, 141). This is a very difficult definition and it is not my task to explain it here. However, it is clear that a judgment according to Kant, does not express any sort of possible association between two (or more) concepts: as long as it expresses a relation between a subject and a predicate, it expresses the appurtenance (*Zugehören*) of what is individuated by means of a certain concept *S*—the concept of the subject—to the sphere (or to the domain) of another concept *P*—the concept-predicate. In other words: as long as it expresses a relation between a subject and a predicate, a judgment says of a certain "representation" that if it is *S*, then it is *P*. Thus, we can reformulate the previous distinction in the following way:

(D_1) A judgment of the form $< \forall x[S(x) \Rightarrow P(x)] >$—where *S* and *P* are concepts and $S(x)$ and $P(x)$ mean that x belongs respectively to the domains of *S* and *P*—is analytic if and only if *P* belongs to *S* , and it is synthetic if and only if *P* does not belong to *S*.

(D_1) supposes that it possibly makes sense to say of two concepts α and β that α belongs to β. Such a possibility depends on a compositional (classic) notion of concepts, according to which concepts—or at least certain sorts of concepts—can be treated as collections of other concepts. Definitely, this seems to be an idea of Kant. Nevertheless, I am far from certain whether (T_1) is the hard core of Kant's philosophy of mathematics, and whether according to Kant, the essential epistemological relevance of the opposition between analysis and synthesis is expressed by (D_1), at least when such a distinction is meant literally. However, before I will give my own interpretation of Kant's views, I would like to present some standard reactions to (T_1) and (D_1). This will help to make my point clear.

In order to use (D_1) for justifying (T_1), we have to state the following lemmas:

(L_1) If *S* and *P* are respectively the concept of the subject and the concept-predicate of a mathematical judgment of the form $< \forall x[S(x) \Rightarrow P(x)] >$ (and it makes sense to say that *P* does not belong to *S*), then *P* does not belong to *S*.

(L_2) Every mathematical judgment is of the form $< \forall x[S(x) \Rightarrow P(x)] >$ (where *S* and *P* are two concepts such that it makes sense to say that *P* does not belong to *S*).

Therefore, it is very easy to refute (T_1) without denying that (D_1) is a good and useful distinction: you can deny (L_1), (L_2) or both. However, it is also possible to deny (L_1), (L_2) or both, without rejecting (T_1): if you want to do that, you have to look for an argument based on a distinction between analytic and synthetic judgments different from (D_1). The history of discussions about Kant's philosophy of mathematics contains a number of different examples for all these points of view.

It is very easy, for example, to refute (L_2) by quoting appropriate counterexamples and then reject Kant's philosophy of mathematics as a whole. This was done by Couturat (Couturat 1893, 84), for instance.

A more interesting position is Frege's (1844, particularly §88). According to Frege, the distinction between analytic and synthetic judgments cannot refer to the logical relation of inclusion between the concept-predicate and the concept of the subject, since this relation does not apply to arithmetical judgments, where the subject is generally a singular object. Moreover, an arithmetical judgment should not to be taken as an isolated one, for it is a consequence of a deductive proof. Thus, it is analytic if and only if it is "deducible solely from purely logical laws" (Frege 1884, §90), and it is synthetic if its proof depends on an appeal to intuition.

Though Frege speaks, like Kant, of mathematical judgments, his position can easily be generalized as one referring to mathematical sentences, to mathematical systems, or even to mathematics as a whole. For that, we only have to replace (D_1) by a more general and explicit distinction:

(D_2) A sentence is analytic if and only if it is part of an analytical system, otherwise it is synthetic; a system (of sentences) is analytic if and only if it is deductively closed with respect to purely logical rules and (eventually) purely logical axioms, otherwise it is synthetic.

Referring to (D_2), many have argued that "mathematics is analytic". (D_2), however, is a very problematic distinction, since it bears on the problematic notions of purely logical rules and axioms.

If we intend these notions in a strict sense, it follows from (D_2) that only propositional and predicative calculus are analytic systems. Hence—since it is obvious that, due to the occurrence of proper axioms in its deduction, no mathematical theorem, as it is generally enunciated, can be intended as a theorem of propositional or predicative calculus—we can assert that a mathematical theory is an analytic system only if we are ready to acknowledge that a mathematical theorem is nothing but an implication, where the antecedent is just formed by a suitable class of proper axioms. This was one of Russell's ideas (Russell 1903*a*), for example.

However, even though it would be possible to intend any mathematical theory as a system of implications of this sort, someone could argue that this is not the point, since these implications have generally to be of the form < if *A*, then *B* >, where *A* and *B* are not yet implications of this sort. Thus, by asserting, according to such an argument, that a mathematical theory is an analytic system, we are just saying that its theorems form a system of logical consequences of the given axioms, which Kant himself would have accepted. Here is what he writes just after the passage I quoted at the beginning:

> "[...] For as it was found that all mathematical inferences [*Schlüsse*] proceed in accordance with the principle of contradiction (which the nature of all apodeictic [*apodiktischen*] certainty requires), it was supposed that the fundamental propositions of the science can be themselves be known to be true through that principle. This is an erroneous view. For though a synthetic proposition can indeed be discerned in accordance with the principle of contradiction, this can only be if another synthetic proposition is presupposed, and if it can then be apprehended as following from this other proposition; it can never be so discerned in and by itself." (Kant B, 14)

Thus, to argue that a mathematical sentence or system (or even mathematics as a whole) is analytic is not enough—according to Kant—to show that mathematical rules of inferences are purely logical; if we want to deny (T_1) on the base of (D_2), we have to argue that mathematical axioms are purely logical too, even though they are proper axioms. There was a time when Russell and Whitehead dreamed to show that just this is the case: that every mathematical theory could be reduced to a system of logical consequences of axioms that we should take as logical, since they express nothing but general properties of sets (Whitehead and Russell 1910-1913)[2].

However, what is a logical axiom in this sense is really a disputed question and it is certainly not in this manner we can hope to decide whether (T_1) has to be accepted or not. If this is the problem, the question of analyticity or syntheticity of mathematics is simply a question of subjective views. According to Cassirer (Cassirer 1907), proper mathematical axioms and definitions are synthetic, for example, and every mathematical theory is then a synthetic system, even though it uses only logical rules of inference.

At first glance, we might believe that Poincaré advanced a similar thesis with respect to arithmetic (Poincaré 1894), but it seems to me that Poincaré's view is essentially different from Cassirer's. Poincaré is interested in the nature of "mathematical reasoning", rather than in the character of mathematical axioms. Thus, when he claims that the mathematical principle of induction is synthetic, he wants to say that mathematicians proceed by non-logical inferences in arithmetic, that is: they "proceed by construction" and "mathematical induction".

A similar point has been made by Hintikka (Hintikka 1973). According to him, Kant's distinction applies to "modes of reasoning", namely, the modes of reasoning "which are now treated in quantificational theory" (*ibid.*, 182). These "modes of reasoning are synthetic if the inferences or arguments that occur in them are synthetic", and an inference (or argument) is synthetic if it does not deal "with general concepts only", but needs "the introduction of an intuition" (*ibid.*, 194). A part of Hintikka's notion of reasoning in its relations to inferences or arguments in quantificational theory, this is exactly the thesis I will ascribe to Kant in the next paragraphs III and IV. However, according to Hintikka, this means that "for Kant the reason why mathematical arguments are synthetic is that they are constructive", that is: they proceed by introducting "new individual mathematical objects" (*ibid.*, 206). In other words:

> "Synthetic steps are those in which new individuals are introduced into the argument; analytic ones are those in which we merely discuss the individual which we have already introduced." (*ibid.*, 210)

Moreover:

> "In a suitable formulation, arguments of the former kind can be boiled down to existential instantiation." (*ibid.*, 210-211)

If the previous thesis is ascribed to Kant, I do not think this is a good explanation of it. I think that for Kant an "analytical argument" (to use Hintikka's terminology) does not "discuss individuals" at all (at least, if the term "individual" means "object"), and a synthetic one does not ask for "existential instantiation" and does not deal properly with "mathematical objects".

III A Provisional Reformulation of Kant's Distinction

By shifting attention from judgments or sentences to inferences or even to reasoning, Poincaré and Hintikka move, as I believe, in the right direction. Moreover, when Hintikka states that, according to Kant, mathematics is a constructive affair, and syntheticity is concerned with intuition, he points to a crucial aspect of Kant's philosophy of mathematics.

Nevertheless, in my opinion, for Kant the syntheticity of mathematics does not depend on the occurrence of constructive, or generally non-deductive, inferences. As a matter of fact, it depends on the role of intuition, but intuition has to be intended neither as a condition of construction (in the usual sense), nor as a sort of psychological capacity: the capacity of "seeing" some hidden relations, or to be convinced by some particular evidence or even to switch on a mental light in the darkness of doubt or ignorance. *A fortiori*, intuition has not to be taken as a (logical or psychological) condition of non-deductive or constructive inferences. Thus, even though I think that for Kant intuition is in a sense the source of syntheticity in mathematics, I do not think that we have to argue, to justify Kant's views, that mathematical proofs or arguments are full of "intuitive" (that is non-deductive, or even "non-logical", or "constructive") steps, as some say.

III.1 Judgments and Propositions

Let me begin with a remark on *Jäsche Logic* (Kant, JL). Here, Kant does not distinguish between analytic and synthetic judgments, but only between analytic and synthetic propositions. According to him, every proposition is a judgment, but not every judgment is a proposition: a proposition is an assertoric [*assertorisch*] judgment (*ibid.*, § 30, note 3), that is a judgment "accompanied with the consciousness [*Bewusstsein*]" of "the reality[3] [*Wirklichkeit*] of the judging" (*ibid.*, §30). Such a definition is not so different from that of the first *Critique*—according to which an assertoric judgment is that in which "affirmation or negation is viewed as real [*wirklich*] (true [*wahr*])" (Kant A, 74; B, 100)—but the reference to the idea of consciousness makes my point clearer.

That between problematic [*problematisch*], assertoric and apodeictic judgment is, according to Kant, a distinction of judgments on the base of their modality, that is "the way in which something is maintained [*behauptet*] or denied [*verneint*] in the judgment" (Kant, JL, § 30, note 1). What is important here is the modality of maintaining or denial and not what it is maintained or denied: a problematic judgment maintains or denies something possibly (*möglicherweise*); an assertoric judgment maintains or denies something really (*wirklich*); an apodeictic judgment maintains or denies something necessarily (*notwendigerweise*). The distinction does not concern the modal form of the judgment itself, but the modality of the act of formulating such a judgment. As Kant writes in the first Critique:

> "The modality [*Modalität*] of judgments is a quite peculiar function. Its distinguishing characteristic is that it contributes nothing to the content [*Inhalt*] of the judgment [...], but concerns only the value of the copula in the relation to thought [*Denken*] in general." (Kant A, 74; B, 99-100)

But, what does it mean that something is maintained or denied possibly, really or necessarily?

If we try to understand such a distinction using the usual conception of modality in terms of truth in a given collection of worlds, we are not able to do it without a criterion founded on the modal form of appropriate statements. It is possible to imagine different ways to construct sets of worlds and to associate any judgment with appropriate statements to be evaluated with regard to these worlds, in order to say if such a judgment is problematic, assertoric or apodeictic. In this way we can justify, for example, that a judgment as < It is possible that A > is not problematic, since it maintains something: particularly, it maintains that A is possible.

However, in that way we reduce any judgment to usual modal statements and we decide about its nature on the basis of the modal form of the associated statements.

I think that Kant's distinction should not be understood this way. In my interpretation, this distinction is rather a question of justification.

A problematic judgment is not a judgment expressed by an appropriate statement that is true in the worlds belonging to a proper non-empty sub-set of a given set of appropriate worlds. It is a judgment, referring to only one world, that has been formulated without any kind of justification. As Kant does not admit any sort of guess, this means that the act of formulating this judgment cannot be an act of stating anything; it is simply an act of expressing a certain connection.

But such a connection, you might notice, has to be a possible one. Hence, we have to explain what a possible connection is. There are two ways for doing that. First, we could say that the logical form of this connection has to be a possible form of a judgment, that is: it has to respect certain logical (or simply syntactical) rules of formation. Second, we could say that it is the content of the connection that has to be possible. If so, we come back to modality, intended in the usual extensional sense, but now we are considering it, not in order to know whether a judgment is problematic or not, but to know whether a certain connection can be a judgment or not. Thus, we would say that the act of formulating a problematic judgment is the act of expressing an arbitrary connection we have ascertained to be possible.

As, according to Kant, the "expression through words" is a necessary condition for the act of judging (Kant JL, § 30, note 3), the first solution leads us to conclude that problematic judgments are sentences (or are expressed by sentences), while the second solution leads us to conclude that problematic judgments are non-contradictory sentences (or are expressed by non-contradictory sentences).

What is important to me is that according to both, the first and the second interpretation, the act of formulating a problematic judgment does not require any justification of the content of the judgment itself. This is not the case for assertoric and apodeictic judgments, since, according to Kant, the act of formulating an assertoric or apodeictic judgment is an act of stating something. The difference between these two sorts of acts lies in the nature of justification. If such a justification merely depends on the "laws of understanding [*Verstand*]" (Kant A, 76; B, 101) the judgment is apodeictic, otherwise it is assertoric.

If I am right, Kant's distinction is asymmetric, since it actually distinguishes between problematic and non-problematic judgments on the one hand, and between non-problematic assertoric judgments and non-problematic apodeictic judgments on the other hand. As long as problematic judgments are sentences (or are expressed by sentences), non-problematic judgments—both assertoric and apodeictic—are statements (or are expressed by statements).

Now, if only propositions can be analytic or synthetic and propositions are assertoric judgments, it follows that Kant's distinction between analytic and synthetic does not apply to sentences, but rather to statements. However, if we would like to stay close to Kant's text, we should argue that the only statements that could really be called "analytic" or "synthetic" are assertoric statements. But if that is so, how could Kant have advanced the view that mathematical judgments (which in his views are apodeictic judgments) are synthetic? The difficulty would be a major one, if Kant had not explained once again his notion of proposition in the following terms:

"Before I have a proposition I must first judge [*urteilen*]; and I judge about much that I cannot make out [*ausmachen*][4], which I must do, however, as soon as I determine a judgment as a proposition." (Kant JL, §30, note 3)

According to such a characterization, a proposition is a judgment associated with an act of making out. Since in my interpretation this is true for any sort of non-problematic judgment, that is any sort of statements, we have to conclude that any sort of non-problematic judgment is a proposition. The point is plaintly this: Kant's distinction between analytic and synthetic judgments lies exactly in the nature of such an act of "making out" and can then be applied to any sort of statement. Thus, I propose to force Kant's text a little bit and to interpret Kant's distinction between analytic and synthetic propositions as referring to any sort of statement.

III.2 Analytic and Synthetic Propositions

According to paragraph 36 of the *Jäsche Logic*, "propositions whose certainty rest on identity [*Identität*] of concepts (of the predicate with the notion of the subject) are called analytic propositions", while "propositions whose truth [*Wahrheit*] is not grounded [*gründet*] on identity of concepts must be called *synthetic*" (*ibid.*, § 36). Even though he speaks of identity, Kant is clearly referring to the identity of the concept-predicate P with a part of the concept of the subject S. The remark 1 about the same paragraph 36 is clear:

"An example of an *analytic* proposition is [...] [:'] To everything x, to which the concept of body $(a + b)$ suits [*zukommt*], suits [*kommt*][5] also *extension* (b) ['].
An example of a *synthetic* proposition is [...] [:'] To everything x, to which the concept of body $(a + b)$ suits, suits also *attraction* (c) [']." (*ibid.*, § 36, note 1)

These examples fit very well with (D_1), but here Kant does not seem to insist on the fact that the concepts-predicate b and c belong or do not belong to the concept of the subject $(a+b)$. What is important here is rather that the act of "making out" the content of the sentence "every body is extended" rests on ascertaining the appurtenance of the concept b to the concept $a+b$, while the act of "making out" the content of the sentence "every body attracts" does not rest and can not rest on ascertaining any logic relation between concept $a+b$ and concept c. The point I want to make is this: the content of analytical judgments "is made out" merely by analyzing concepts; to "make out" the content of a synthetic judgment, we in contrast have to go away from concepts and base ourselves on something else. The following quotation, drawn from the *Introduction* to the first edition of the *Critique of Pure Reason*, seems very clear to me:

"[...] through analytic judgments our knowledge is not in any way extended, and the concept which I already have is merely set forth and made intelligible to me; [...] in synthetic judgments I must have besides the concept of the subject something else (X), upon which the understanding may rely, if it is to know that a predicate, not contained in this concept, nevertheless belongs to it." (Kant A, 7-8)

Even though Kant eliminated this passage in the second edition, the same point is clearly expressed in the sections IV and V of the *Introduction*[6]. The main question Kant faces in these sections, after having presented (D_1), could be presented like this: on what do we ground ourselves for "making out" the content of synthetic judgments, if it is not on analysis of concepts?

For *a posteriori* judgments the answer is very simple and clear: we ground ourselves on our experience of objects, particularly of the objects that fall under the concepts occurring in the judgments themselves. For *a priori* judgments, the question is much more difficult, since here we cannot refer to any sort of experience.

"[...] in *a priori* synthetic judgments—Kant writes—this help is entirely lacking. Upon what, then, am I to rely, when I seek to go beyond the concept A, and to know that another concept B is connected with it? Through what is the synthesis made possible? since[7] I do not here have the advantage of looking around in the field of experience [*Erfahrung*][...]. What is here the unknown = X which gives support to the understanding when it believes that it can discover outside the concept A a predicate B foreign to this concept, which it yet at the same time considers to be connected with it?" (*ibid.* A, 9; B, 12-13)

Even though this is one of the most fundamental questions in the first Critique (since it is equivalent to the famous one: "how are synthetic *a priori* judgments possible?") Kant does not sketch a general answer in the *Introduction*. He prefers to consider mathematics, natural sciences and metaphysics separately (in section V), and even in these cases he does not give a direct answer to the question.

If we abstract from such an answer, we once again limit ourselves to subject-predicate judgments and we assume that, according to Kant, analytic and synthetic statements have to form two complementary classes we may provisionally formulate Kant's distinction as follows:

(D_3) A statement of the form < $\forall x[S(x) \Rightarrow P(x)]$ >—where S and P are concepts and $S(x)$ and $P(x)$ mean respectively that x belongs to the domains of S and P—is analytic if and only if the act of "making out" that every x which is S is also P is grounded on nothing but the ascertainment of the logical relation 'P belongs to S' between the concepts S and P; it is synthetic if and only if this act asks for moving away from the consideration of the logical relations between the concepts S and P.

If we consider judgments as statements and assume that the appurtenance of P to S is a sufficient (and obviously necessary) condition for the act of "making out" that every x that is S is also P is grounded on the ascertainment of such a logical relation, then the first part of (D_3) (the definition of analyticity) is equivalent to the first part of (D_1). Moreover, the second part of (D_1) (the definition of syntheticity) is perfectly complementary to the first part: according to it, a judgment is synthetic if and only if it is not analytic. Thus, if we accept that the second part of (D_3) is also perfectly complementary to the first part, we have to conclude that (D_1) and (D_3) are absolutely equivalent under the previous conditions. Now, in the first *Critique*, Kant is most of all concerned with statements rather than with sentences, thus we can imagine that, for him, (D_1) really deals with statements, rather than with sentences. Moreover, he certainly accepts the appurtenance of S to P as a sufficient condition for the act of "making out" that every x that is S is also P is grounded on the ascertainment of such a logical relation. So, if I am right in asserting that (D_3) is a Kantian distinction, and if we assume that, according to Kant, the second part of such a distinction is purely complementary to the first, we should conclude that in the *Introduction* to the first *Critique*, Kant advanced (D_1) as a simplified version of (D_3). This is just my thesis.

However, (D_3) is a provisional distinction, for at least two reasons. First, it does not specify what enables us to formulate synthetic statements; second, it is restricted to subject-predicate statements. Moreover, it is also not totally satisfactory, since it is grounded on the interpretation of S and P as concepts, while, strictly speaking, they are predicates.

The latter difficulty is obviously connected with my shifting from judgments to statements and can only be solved by presenting an appropriate theory of concepts. Such a theory is also necessary for generalizing (D_3) to any sort of statements and making its second part explicit. Furthermore, these two latter tasks also need an appropriate theory of logical counter-parts of concepts. I doubt that two appropriate theories of this sort are really available in Kant's philosophy. In the next paragraph, I will try to expound Kant's theory of concept and its logical counter-part (that is intuition or object) as briefly as I can, and as I am able to understand it, in order to make Kant's own distinction clearer—even though not satisfactory yet—and to understand Kant's reasons for claiming that "mathematical knowledge" is synthetic.

IV Concept, Object and Intuition: the Final Version of Kant's Distinction

In the *Critique of Pure Reason*, Kant is concerned with conditions of knowledge. For him, judgments are forms or moments of knowledge. Thus, if we intend assertoric and apodeictic judgments as statements, we have to consider statements both as logical (or linguistic) forms and as cognitive acts: intended in the first way, a statement is the logical form of the same statement, intended in the second way. But forms can be classified and so, in the first sense, statements both are forms and have forms of a higher level. These forms of higher level can be expressed by logical formulas as < $\forall x[S(x) \Rightarrow P(x)]$ >, so that these formulas express forms of forms of cognitive acts. The elements occurring in these formulas then have to express elements of a cognitive act, that is elements of an act of knowing.

Now, for Kant, knowledge can be either *a priori* or *a posteriori*. The *a posteriori* knowledge is nothing but experience and generally consists in appropriate representations and judgments connected to the occurrence of a sensation. In this sense, it is knowledge of objects. On the other side, *a priori* knowledge is independent from the occurrence of sensations, but it is neither knowledge of something different than objects, nor is it a form of knowledge alternative to experience. Rather, it consists of representations and judgments that make *a posteriori* knowledge or experience possible. In this sense, it is a condition of *a posteriori* knowledge and it is justified as such. The possibility of *a priori* knowledge is thus the possibility of the conditions of possibility of *a posteriori* knowledge or experience. Hence, as for Kant *a posteriori* knowledge is a fact, *a priori* knowledge is a fact too and the form and nature of the latter depends on the form and nature of the former. To understand the first (*a priori* knowledge), we then have to understand the second (*a posteriori* knowledge).

IV.1 *A Posteriori* Knowledge

A posteriori knowledge is for Kant either "objective perception [*objektive Perzeption*]" or judgment. An objective perception is for Kant a species of the genus of representation:

> "The genus is *representation* in general (*representatio*). Subordinate to it stands representation with consciousness (*perceptio*). A *perception* which relates solely to the subject as the modification of its state is sensation [*Empfindung*] (*sensatio*), an objective perception is *knowledge* (*cognitio*)." (*ibid.* A, 320; B, 376)

In other words: an objective perception is a perception intended as an act of perceiving something, rather than as the event of the subject's modification, which is sensation. The difference between objective perception and sensation does not concern the nature or the direction of consciousness; in the former as well as in the latter, consciousness is directed toward what is perceived. Objective perception and sensation are just two different (and complementary) aspects of perception, that is the act in which a subject represents something to himself, as the cause of modification of its own internal status. Hence, there is no objective perception without sensation, and the reason for this is not merely that objective perception and sensation are necessarily connected facts. They are simply not different facts, but different aspects of the same fact, namely perception. Thus, there is also no perception without objective perception or sensation:

> "Perception [*Wahrnehmung*][8] is empirical [*empirische*] consciousness, that is a consciousness in which sensation is to be found." (*ibid.* B, 207)

Kant's distinction between sensation and objective perception can be expressed in the following terms: if we analyze perception without considering the specificity of consciousness, only insisting on its presence, we speak about sensation; in contrast, if we analyze perception regarding the specific nature of consciousness occurring in it, we speak about objective perception. To analyze objective perception is then the same as analyzing consciousness occurring in perception.

However, Kant's aim is not that of analyzing *a posteriori* knowledge as such, but it is rather that of looking for its conditions. Concerning objective perception, Kant's problem is the following: how is it possible for a subject to represent something to himself as the cause of a certain sensation (the modification of his internal status)?

According to Kant, the cause of a sensation, as the subject of such a sensation represents it to himself, is an object (*Gegenstand, Object*)[9]. If the term "representation" means what is represented, rather than the act of representing it, an object is a conscious representation:

> "Everything, every representation even, in so far as we are conscious of it, may be entitled object." (*ibid.* A, 189; B, 234)

Even though elementary knowledge is always a representation of something, such a something is an object only as long as it is represented in an act of objective perception. Thus, we cannot intend objective perception as a representation of something that is given as an object before the representation itself. Obviously, a subject can represent something to himself that has been already given as an object, but this is not an act of objective perception. It is rather a judgment that makes the subject able to classify objects (which are already given as such), according to their particular characters. Properly speaking, such a representation does not produce objects, but classes of objects, aspects of objects, functions of objects, etc. According to Kant, an object is properly a representation of something that is not an object, and *a posteriori* knowledge just begins when an object is given in this proper sense. Hence, objective perception is representation of something that cannot be known, but only thought as a something that is represented by a certain object (*ibid.* B, xxv)[11].

The problem of the conditions of objective perception is then the problem of the possibility of the represention of something, which is not known as such, but only thought as the cause of a sensation, as a certain object. This way, we have arrived at the crucial point: such a representation is possible for Kant only if the object arises from a "subjective constitution [*subjektive Beschaffenheit*]" (*ibid.* A, 44; B, 62), which is necessarily *a priori*. Thus, even though objective perception, as a perception of a certain object, is a sort of *a posteriori* knowledge, it is possible only as the result of an *a priori* act of subjective constitution of the object itself and, as such, it can be genetically analyzed in two different (and even opposite) aspects:

> "This [Knowledge or objective perception] is either *intuition* [*Anschauung*] or *concept* (*intuitus vel conceptus*). The former relates immediately to the object and is singular[10] [*einzeln*], the latter refers to it mediately by means of a feature which several things may have in common." (*ibid.* A, 320; B, 376-377)

As long as they occur in objective perception, intuition and concept are thus two different aspects of our representation of the cause of a sensation as an object for Kant, that is two aspects of subjective constitution of such an object. These aspects have not to be confounded with two complementary and alternative forms of subjective constitution of an object. The opposition between intuition as "singular representation" and concept as "universal representation" (cf. also Kant JL, §1) is founded for Kant in another and deeper opposition: that between intuition as the aspect of objective perception for which the object is given as such, and concept as the aspect of objective perception for which the same object is thought as such (Kant A, 19; B, 33)[11]. And for Kant an object cannot be given as such if it is not thought, and it cannot be thought as such if it is not given. Thus, regarding objective perception, a singular representation is not a sort of representation different from universal representation: singular and universal representation, *i. e.* intuition and concept, are two aspects of the same representation, that is perception taken as knowledge. They are then two aspects of the same act, the act of subjective constitution of an object.

In their primitive and more fundamental sense, intuition and concept have thus to be intended, with respect to *a posteriori* knowledge, as two opposite cognitive functions we distinguish by analysis in only one cognitive act, the act of subjective constitution of objects. Since this act is nothing but objective perception, an object is by definition an empirical entity. However such an entity has not an external existence, but results from an act of representation that has been analyzed as an act of constitution.

Let us continue. According to Kant, an object is given as such when the subject connects his sensations (the modifications of his status) to a spatio-temporal unity, and it is thought as such when he recognizes such an entity as an example of a certain class of spatio-temporal unities. The former condition is certainly necessary for the latter to take place, but, according to Kant, the latter condition too is, a necessary condition for the former to take place, since spatio-temporal bounds of a certain unity do not depend on the unity itself, but have been imposed on it by the subjective constitution, according to a certain reason that could not be anywhere else but in the concept.

This remark leads us to the final step of Kant's analysis of objective perception: the act of subjective constitution of an object is possible only if we dispose of two connected faculties, or better of one faculty that can be analyzed into two different aspects. According to one of its aspects, this faculty enables us to connect our sensations to certain positions in an order—the spatio-temporal order—that is already given (and can be analyzed in two different aspects, the internal order, or sense—which is time—and the external order or sense—which is space). According to the other of its aspects, this faculty enables us to recognize these positions as particular representations of certain forms that are already given too.

Here a first important shift occurs: these two different aspects are generally taken as opposite faculties (of pure reason) and the first is confounded with intuition itself, the second one being the faculty of understanding. As a faculty, intuition is not opposed by Kant to concept anymore, it is rather opposed to a faculty—namely understanding—that is taken as the faculty of producing and even composing concepts. Thus, while intuition becomes a faculty, the place of concept is taken by a plurality of concepts intended as a sort of entities produced and manipulated by the subject.

If I am correct, Kant's analysis of conditions of objective perception could be summarized as follows: the act of representing as an object which is thought as the cause of our sensation is an act of subjective constitution. In such a constitution, two faculties concur: intuition and understanding. Intuition connects our sensations to positions in spatio-temporal order, that is an order that is already given as such; understanding recognizes these positions as particular representations of certain forms that also are already given. These faculties are necessarily connected, since forms are already given as possibilities of positioning in spatio-temporal order, and intuition realizes such a connection under the guidance of the capacity of recognizing positions in spatio-temporal order as particular representations of these forms.

These forms are not objects, since the subject does not represent them to himself as the cause of his sensations; they are thought as concepts, but they are just given as forms in "pure intuition", or "pure forms". Furthermore, the objects thus constituted are not examples of the concepts of these forms. Instead, these concepts are necessarily *a priori* and universal, while objects are empirical entities connected to particular sensations. The connection between objects and the concepts of pure forms is governed by schematism. I will not discuss this question here. What interests me is rather that the objects which are constituted in objective perception (intended as an act of constitution) are not instances of the concepts of the pure forms which occur in such an act. Properly speaking, there is no object that is an instantiation of these concepts; they are just concepts of pure forms. Thus intuition and understanding, as long as they are intended as faculties which concur in the act of *a priori* constitution of objects, are both certainly *a priori* faculties. But they are not properly pure, since they apply to particular sensations. However, such an application is possible only if the subject disposes of pure forms. In other words: the subjective constitution of an object depends not only on two *a priori* faculties, but also on the disposability of *a priori* entities as pure forms. The first task then of *a priori* knowledge is to provide these entities.

Once objects have been constituted, they are given as particular spatio-temporal unities and they are thought as particular representations of concepts of pure forms, as examples of empirical and individual concepts. Still, this is not the final step of our *a posteriori* knowledge. It is only the final step of the act of exhibiting these objects as such, namely objective perception. To know these objects in their respective relations, we have to be able to pass to judgments. Nevertheless, not every judgment is an act of *a posteriori* knowledge, since what is essential in *a posteriori* knowledge is not the logical form of judgment, but the occurrence of an experience. When it is not merely an act of objective perception, an act of *a posteriori* knowledge is necessarily a judgment only according to its form, or, if you prefer: the logical form of judgment is only a formal or external—even though necessary—condition of experience. We cannot have experience of anything else but objects; moreover a simple succession of acts of objective perception is not an experience yet, it is nothing but a "rhapsody of perceptions" (*ibid.* A, 156; B, 195), that is a rhapsody of different and isolated acts of elementary experience. In his genuine sense, experience asks for a connection between these acts, and judgment is just a logical form of this connection. Still, the occurrence of a connection of this form is only a necessary condition for knowledge, since in order to have knowledge, such a connection must not only be a judgment; it must also be an objective judgment.

But what makes a judgment "objective"? Certainly, a judgment is not objective when it connects objects, since, according to Kant, a judgment always connects concepts. Rather, we should say that a judgment is objective if it connects concepts according to the respective objects. However, in face of such an answer, we could insist: what does it mean exactly, in any particular case, that a judgment connects concepts according to the respective objects? This is a very difficult problem, but in a sense, this is not our problem now. At the present stage of the analysis, what is important is this: whatever such a condition would be, it is certainly impossible to satisfy it, if the concepts we are connecting were all concepts of particular objects. Thus, in order to make *a posteriori* judgments possible, a first condition has to be satisfied: the subject has to dispose of non-elementary empirical concepts, that is of empirical concepts different from distinct concepts of individual and particular objects. These concepts are concepts of forms of particular objects. Hence, when it is not merely an act of objective perception, an act of *a posteriori* knowledge is just a judgment connecting these concepts to each other, or to concepts of particular objects. It is only by the mediation of these concepts that a judgment can (indirectly) connect particular acts of objective perception. As these concepts have to be empirical, they cannot come from any other source than objective perception itself. But since they are non-elementary, they cannot result from a simple succession of acts of objective perception. They have to be produced by a different sort of connection of objective perceptions. Still, this is not the end of the story, since once these non-elementary concepts have been produced, in order to have a judgment, they have to be connected to one another, or to elementary concepts. And this is certainly not possible if they are produced in different and isolated acts.

Furthermore, in order to be an act of knowledge, a judgment must not merely be problematic; it not only must connect concepts, but it must state the content of such a connection. Thus, the possibility of non-elementary *a posteriori* knowledge depends on the possibility of producing non-problematic *a posteriori* judgments.

IV.2 *A Priori* Knowledge

The analysis of *a posteriori* knowledge has led us to distinguish three tasks for *a priori* knowledge: *i)* to provide pure forms and to permit both *ii)* the production of non-elementary concepts and *iii)* the connection of non-elementary or elementary concepts in a judgment that is not merely problematic.

Let us begin with the second task. For Kant, the production of non-elementary concepts is the result of a synthesis of understanding. It is an act of understanding, but it is not as such an *a priori* act, since it operates on empirical concepts given as forms of real (and not only possible) experience. However, this act would be impossible without the unity of internal sense that makes the different acts of objective perception different elements of only one unity of knowledge, and such a unity is for Kant assured by "pure [*reine*] intuition". Here, intuition is no longer a faculty occurring in the act of constitution of objects or an aspect of objective perception, it becomes a guarantee of the possibility of the synthesis of understanding. Thus, the passage from sensible intuition to pure intuition produces a new important shift in Kant's conception of intuition.

Nevertheless unity of internal sense is only a formal condition for the synthesis of understanding applied to elementary empirical concepts. It guarantees the possibility of such a synthesis, but it does not guarantee that something like a genuine concept is produced. In other words: it does not guarantee that the result of the synthesis of understanding is, as such, a component of an act of knowledge. Even though the results of such a synthesis could certainly not be concepts of objects, they have to be able to refer to objects as concepts of objects do, that is, they have to be exemplified by aspects, functions, relations etc., or, in general, forms of possible objects. These results have to be "really possible concepts", they have to "agree with the formal conditions of an experience in general" (*ibid.* A, 220; B, 267). Of course, this could not mean that any result of a synthesis of understanding applied to elementary empirical concepts had to be exemplified in such an indirect way by an actual object. The problem then is this: what is the result of a synthesis of understanding applied to elementary empirical concepts?

The first part of the answer is trivial: the result has at least to be a "logically possible concept". Whatever the synthesis of the understanding is, it must respect the condition of logical possibility, that is the principle of non-contradiction. However, this is not a sufficient condition, yet. Another condition is needed, but it is not so easy to formulate. As I have just said, for Kant any concept has also to be indirectly exemplified by something that we could represent as a possible object. But clearly this is not a criterion, since the subject does not dispose of possible objects as such, and then he cannot classify logically possible concepts by comparing them to possible objects. Thus, if a demarcation is possible between logically possible concepts that are really possible, and logically possible concepts that are not really possible, its criterion can not be based on appealing to a comparison to possible objects. In other words: possible objects should be, by definition, nothing but the objects of really possible concepts, and not *vice versa*. If we want to distinguish between logically possible concepts that are also really possible and logically possible concepts that are not really possible, we have to refer directly to the synthesis of intuition as such. Now, according to Kant, such a discrimination does not depend on a criterion, rather it depends on a faculty. Such a faculty is again pure intuition. In this case, pure intuition is not directly applied to objective perceptions as a condition of unity, it directly applies to the concepts of empirical objects, as a condition of compatibility. For producing new empirical concepts, understanding realises a synthesis by starting from elementary empirical concepts, that are concepts of objects which manifest concepts of pure forms in particular; the result of such a synthesis is a really possible concept if it is produced in a way that is compatible with the conditions of compositions of concepts of particular objects and pure forms.

Let us consider now the third task of *a priori* knowledge: that of making non-problematic *a posteriori* judgments possible. Let us imagine that a subject has non-elementary empirical concepts at his disposal. In order to connect them to another, or with elementary empirical concepts, in a problematic judgment, he has to be able to consider his own concepts together, as part of a unity of consciousness. Such a unity is assured by pure intuition that is now applied to elementary or non-elementary concepts as a condition of unity. Still, this is only the beginning of the story. To obtain *a posteriori* knowledge, the problematic judgment has to be justified and transformed into an assertoric judgment (since it is clear that no *a posteriori* judgment can be apodeictic). To make it, we have to come back to the objects themselves—the objects that directly or indirectly exemplify the concepts occurring in the judgment—and to consider the distinct acts of objective perception corresponding to them as only one experience. Thus, pure intuition has to occur once again as a guarantee of such a unity. In this new role, pure intuition does not work simply as a deaf guarantee for the act of synthesis; according to Kant, it is also the base of a class of synthetic *a priori* judgments that express the conditions of *a posteriori* knowledge discursively. These judgments are the dynamic principle of pure understanding, "analogies of experience", and "postulates of empirical thought in general" which are rules "according to which a unity of experience may arise from perception" (*ibid.* A, 180; B, 222).

Even though we could go on by analyzing the justifications (or deductions) and the function of these synthetic *a priori* judgments (which are obviously apodeictic judgments), I stop here, since I am not directly concerned with this sort of judgments. It is sufficient to have stated that they are grounded on pure intuition as a guarantee of the formal possibility of non-elementary experience, that is the possibility of the necessary form of *a posteriori* judgments.

However, the possibility of the form of *a posteriori* judgments is not yet the possibility of these judgments as such. For this possibility to be insured, we still have to guarantee that these forms can be filled up by connections that express something as a "possible experience" (*ibid.* A, 160; B, 199). This is guaranteed by the fact that objects are necessarily "extensive magnitudes" characterized by "intensive magnitudes". This is the content of the principles of "axioms of intuition" and "anticipations of perception". Such a fact is expressed by these judgments—that are synthetic *a priori* judgments and are, as such, produced by pure understanding—but it depends on the nature of intuition—as it occurs in the subjective constitution of objects—and it is present to the subject thanks to pure intuition.

Thus both axioms of intuition and anticipations of perception are justified, according to Kant, by a sort of application of pure intuition to intuition itself, or at least to the form of intuition. This is a new essential function of pure intuition.

Still, this is not the end of the story, since, up to now, we have only justified the possibility of the realization of a necessary form of experience, and not the possibility of assigning a real content to such a form. Now, in order to realize the latter possibility, we need both pure forms and real judgments connecting the concepts of these forms. Thus, we have arrived at the first task of *a priori* knowledge.

Let us begin with the first point. The act of subjective constitution of objects, as it was just described, asks for pure forms, but it does not give any guarantee that they are possible. Again, such a guarantee is provided by pure intuition that seems to guarantee both the availability of elementary pure forms (as straight lines and circles) and the possibility of composing them in order to produce other forms (as triangles or squares). On the first point, Kant is not really explicit. He seems to reason as if these forms were given as such to pure intuition. In contrast, he does not leave any doubt as to the second point (cf. for example, *ibid.* A, 220-226; B, 267-274). The synthesis of understanding produces new concepts of pure forms that have to be not only logically possible, but really possible too: they have to be forms that can be manifested in particular by possible objects. The problem is thus analogous to the one we just discussed with respect to non-elementary concepts: how can really possible concepts of pure forms be distinguished from really impossible ones? Of course, such a distinction cannot be made *a posteriori* and has to rely on an *a priori* capacity of the subject. Thus, the guarantee of this capacity is once again pure intuition as a guarantee of the possibility of certain sorts of objects. This is, I believe, responsible for Kant's monolithic conception of mathematics, and particularly of geometry[12].

Moreover, even if we accept that really possible concepts of pure forms are given (and distinguished by really impossible ones), we do not have any guarantee of the possibility of judgments connecting them. These judgments do not provide, as such, a condition of possibility of *a posteriori* knowledge in general, but they make possible particular experiences and contribute, in this way, to our knowledge. According to their forms, these judgments are submitted to the conditions of possibility of any sort of judgment. Nevertheless, the question here is not that of the unity of different acts of subjective constitution of objects, it rather refers to the subject's own consciousness. The judgment has to connect concepts of pure forms here, concepts that occur in the act of subjective constitution of objects as something that is already given. Thus, the unity that has to be guaranteed is the unity both of the field of giveness of elementary pure forms—that is also a condition of possibility of their composition—and of the different acts of their composition. Thus, if such a unity is guaranteed by pure intuition as well, a new shift occurs.

But the question is not only that of the formal possibility of judgments connecting concepts of pure forms. These judgments provide a condition of possibility for experience only if they are not merely problematic. Thus, the main question is that of their justification.

Of course, if these judgments connect non elementary concepts of pure forms, they can be logical consequences of the way in which the concepts that occur in them have been produced. They are then analytic judgments. However, according to Kant, this is not the only way by which a judgment connecting concepts of pure forms can be justified.

Even though these judgments speak about pure forms, it is possible to justify them by considering objects (as physical figures or collections of physical objects) which are constructed—according to a fixed procedure (as the constructive procedure given in the Euclidean postulates[13])—in order to be particular representations of these forms. Since such objects are only considered for the forms they represent, the conclusions we draw from considering them necessarily apply to these forms. As pure intuition makes the subject certain of the real possibility of the concepts of pure forms, it simultaneously makes him certain of the possibility of constructing appropriate empirical objects for this task. But, how can the subject be certain that the objects he effectively constructs have the form he wants them to have, that they are particular representations of pure forms? Moreover: how can the subject be sure that, in considering them, he refers only to the properties that make them particular representations of pure forms? For Kant, the guarantee of all that is again pure intuition[14]. Thus, a new shift occurs: intuition now becomes a guarantee of the correspondence of certain objects and procedures to the concepts of pure forms.

If we accept that pure intuition provides the previous guarantees, we have to conclude that it enables a subject to justify judgments about pure concepts by leaving these concepts, but without referring to anything as pure objects. These judgments are then synthetic, but since they concern the concepts of pure forms and use objects only as they are constructed according to the concepts of these forms, they are *a priori* and apodeictic too. Finally, since the pure forms are nothing but possibilities of positioning in spatio-temporal order, they are part of the pure science of space and time, namely mathematics. Hence, they are mathematical judgments[15].

IV.3 Kant's Distinction

If I am not mistaken, Kant's distinction between analytic and synthetic judgments refers only to non-problematic judgments, that is to statements, it concerns judgments as forms of knowledge, and it is, as such, independent of the particular

logical form of these judgments. Thus it can finally be formulated in the following way:

$(D_3)^*$ As long as an act of knowledge consists in the act of "making out" a statement of the form $< A(P, Q, ..., S) >$—where the concepts $P, Q, ..., S$ occur—it is analytic if and only if at least one of these concepts is non-elementary and the act itself is founded on nothing but the ascertainment of the logical relations occurring between the concepts $P, Q, ..., S$, according to the way in which the non-elementary concepts which take place among them has been produced by a synthesis of understanding; it is synthetic, if and only if it asks for an appeal either to the experience of some objects—according to the way in which these objects fall under the concepts $P, Q, .., S$—or to some guarantee provided by pure intuition.

V Kant's Ontologism

According to Kant, an act of knowledge, consisting in an act of "making out" a statement, can only be grounded on: *i*) the ascertainment of the logical relations that take place between certain concepts, according to the way in which the non-elementary ones are produced by a synthesis of understanding, *ii*) the experience of some objects, *iii*) an appeal to some guarantee provided by pure intuition. Hence, every act of knowledge of this sort is either analytic or synthetic, according to $(D_3)^*$.

However, this is no complementarity between analytic and synthetic statements yet. For the domains of analytic and synthetic statements to be complementary, it is also necessary that no statement is analytic and synthetic at the same time. Certainly $(D_3)^*$ only satisfies such a condition if we accept that no concept can be considered as an object. This is definitely Kant's idea, since for Kant, concepts and objects are essentially different entities or forms of representation. For him, the distinction between objects and concepts is not a logical one; it does not concern logical roles, but the intrinsic characters of these entities: a concept could never be intended as the cause of a certain sensation, as the subject represents it to himself. However, it seems that Kant always wants to maintain something as a correlation between concepts and objects: even though understanding is able to realize any sort of synthesis of concepts already given, we cannot say that the result of this synthesis is a genuine concept if we cannot say that it refers in same way to one or more objects.

In order to satisfy both conditions, Kant should provide a non-relational characterization of two sorts of entities (concepts and objects) that he wants to intend as essentially correlative. This is the root of many difficulties in Kant's philosophy, I think. The first condition seems to be satisfied if we understand the object as a sort of specification, of a "reality" that is already given in confused terms, realized by means of concepts. Such a reality—which we have to take as absolute first—provides the object with its intrinsic and irreducible nature (contrary to the mental or, if you like, discursive nature of the concept), without denying its correlative character with respect to the concept. Thus it seems that the second condition also can be satisfied regarding the dependence of the object on the concept. But, what about the dependence of the concept on the object? To guarantee it, we have to assume that a concept is one only if it provides a characterization of an object—intended, as I just said, as something that participates in the primitive reality. If this is not the case, we do not really have a concept, but only an arbitrary synthesis of understanding: understanding—by means of imagination[16]—puts together different concepts without really producing a new concept. But this solution is very weak if we do not think that the primitive reality can act, as such, on imagination, while the latter produces its synthesis. Now, even though we could find the means for expressing such a condition, without denying the apriority of the formal conditions of knowledge, we again have the problem of distinguishing guided imagination, which produces real concepts, from completely arbitrary imagination, which produces nothing but an empty synthesis of concepts. Certainly, we cannot do it by referring to primitive reality itself, since it is inaccessible. Thus the problem arises: how can we do it?

Still, if an object is nothing but the cause of a certain sensation, and knowledge is always concerned with objects (even though it is not necessarily an experience with certain objects), as Kant believes, then knowledge is always concerned with the subject's representation of the causes of his sensations: either knowledge is directly such a representation or a judgment connecting in some direct or indirect way different representations of this sort, or it is something like an expression of the conditions of possibility of these representations or connections. But, if a knowledge of the latter sort is not directly about objects, about what is it directly? To give but one example: about what is, in Kant's views, a judgment concerning triangles as such? In a sense, it is about the objects that are particular manifestations of triangles, but certainly it is not directly about them. The correct answer is certainly not that such a judgment is about pure forms, since then the question arises: what is a pure form, if it is neither an object nor a concept? and, if it is a concept, of which object is it the concept?

I am not able to find any satisfactory (that is not merely metaphoric) answer to these questions in Kant's philosophy.

The difficulty even grows if we consider it given the background of the leading principle of Kant's theory of knowledge, which is not only the (Platonic) idea that there is no knowledge without justification, but the stronger precept, according to which any theory of knowledge is a theory of justification, as a guarantee of the

validity of the knowledge itself. On this background, the previous questions become really essential: what is a possible justification (or what is the form of a possible justification) of *a priori* knowledge? Such a question is so fundamental in Kant's framework that it cannot be evaded. Nevertheless Kant's answer is really *ad hoc*: a possible justification of such a sort of knowledge consists in an appeal to pure intuition. The problem of this answer is not simply that it is not really an answer to the question—being rather an answer to another question, namely: where could a possible justification of *a priori* knowledge be found?—but that it is either circular or metaphoric. In Kant's philosophy, pure intuition is nothing but the guarantee of the possibility of *a priori* knowledge (Frege 1884, § 12)—or even the genus under which any guarantee of this sort falls down.

Now, it seems to me that such a difficulty does not depend on a limit in Kant's elaboration of transcendental philosophy of knowledge. Rather, I think that it depends on the premises of this philosophy themselves: *i*) the idea that knowledge is always concerned with the subject's representations of the cause of his own sensations; *ii*) the conception of a theory of knowledge as a theory of justification, as a guarantee of validity of the knowledge itself.

I suspect that no satisfactory theory of knowledge is possible on grounds of these premises. Moreover, I think that Kant inherited these premises from the ontological tradition of empiricism. According to the first, knowledge is something like a human interpretation and connection of a number of original facts that are sensations, while, according to the second, a theory of knowledge is something like a general scheme of reduction of any act we want to intend as an act of knowledge either to these facts as such, or to the original conditions of possibility of their interpretation or connection.

VI Analytic and Synthetic Acts of Reasoning

In contrast to the above, I think that from a philosophical point of view a certain act is an act of knowledge if and only if it has a certain logical form, and a theory of knowledge is nothing but a theory of this form as such. Briefly speaking, I think that an act of knowledge is either an act of exhibition of an object or an act of attribution of properties or relations to objects. However, I think that not every act of attributing properties or relations to objects is an act of knowledge. In order to be an act of knowledge, such an act has to satisfy two conditions: first, the objects to which properties are attributed must be already exhibited as such (and be present as such to the subject that attributes properties or relations to them); second, such an attribution has to be a consequence of an analysis of the objects themselves. Generally, the act of attributing properties or relations to objects by grounding on an analysis either of the objects themselves or of the concepts of these objects, property or relation is, in my views, an act of reasoning, and an act of reasoning is an act of knowledge if and only if this attribution depends on (is locally justified by) the analysis of the objects themselves. An act of reasoning of this sort is synthetic[17], while an act of reasoning is analytic when the attribution solely depends on the analysis of the concepts of these objects, properties or relations . A non-elementary act of knowledge is then a synthetic act of reasoning.

As long as we accept the idea that any act of attribution of properties or relations to objects is expressed (or could be expressed) by a statement, such a distinction can be formulated as follows:

(D_4) An act of reasoning expressed by the statement $A(P, Q, ..., S, a, b, ..., d)$—where the (monadic or polyadic) predicates $P, Q, ..., S$ and the names of objects $a, b, ..., d$ occur—is analytic if and only if it is grounded on nothing but the analysis of the concepts of these predicates or objects; it is synthetic, if and only if it is grounded on the analysis of at least one of the objects that fall under these concepts.

Since for me an act of reasoning is an act of attributing properties or relations to objects, by analyzing either these objects themselves or the concepts of these objects, properties or relations, any act of reasoning is then either analytic or synthetic, and cannot be both. However, this is not merely a question of definining the term "act of reasoning". I think that a subject can only analyze objects or concepts. Thus an act of attributing properties or relations to objects is either an act of reasoning, or it is not grounded on an act of analysis, or it is finally grounded on the analysis of other objects or concepts. And it seems to me that, if we are speaking about science, we are interested only in the first sort of acts of attributing properties or relations to objects.

Moreover, even though the terms "reasoning" or, *a fortiori*, "act of reasoning" are not, as such, Kantian ones, and must not be confused with the term "inference" used by Kant as referring to logical forms of acts of reasoning in my sense (Kant JL, part I, ch. 3)[18], it seems to me that what is interesting in Kant's distinction between analytic and synthetic judgments or statements is that such a distinction does not deal with statements merely intended as linguistic objects, but with the acts of formulating judgments. In this sense, my distinction between analytic and synthetic acts of reasoning fits perfectly with the spirit of Kant's own distinction.

However, it essentially differs from this distinction for a number of other reasons, the main ones of which are concerned with the notion of object. Since for Kant an object is nothing but the cause of a sensation, as the subject represents it to himself, Kant cannot generally intend a judgment as an act of attributing properties to objects. Moreover, he cannot accept the idea that a synthetic judgment is an act of attributing properties to an object by grounding on the analysis of the

object itself, since, if he accepted this criterion, he would deny the possibility of any sort of synthetic *a priori* judgment.

It is clear that, according to (D_4), an act of reasoning can be both synthetic and *a priori* only if we accept the idea that there are pure objects. In the following parts of my paper, I will try to justify this possibility by basing myself on a radically non-Kantian notion of object, in order to defend the following thesis:

(T_2) What makes an act of reasoning a mathematical one is that it is grounded on the analysis of mathematical objects; thus mathematical acts of reasoning are synthetic in the sense of (D_4). Moreover, as mathematical objects are pure objects, mathematical acts of reasoning are not only synthetic, but they are also *a priori.*

VI.1 Objects

According to Kant, an object enters the subject's horizon when it is properly constituted by the subject himself. Hence, there is no object for Kant which has not entered the horizon of a subject. Furthermore, an object can enter the horizon of a subject only if a sensation occurs. The act of constitution of an object is then an act of interpretation of a fact that necessarily has to be intended as preceding the appearance of such an object within the subject's horizon. However, this fact cannot be described, in Kant's framework, without referring to such an appearance, and it is even thought only as its source. Thus, in order to say what an object is, Kant has to refer to something that he can think and represent to himself only as the original source of the object itself.

In my opinion, such a situation is unsatisfactory. If we want to avoid such a difficulty (without returning to the idea that objects subsist as such, independent of any act of the subject), we have to give up the idea that an object is the result of an act of interpretation of a fact. Elsewhere, I defined an object as the meaning of the argument of a predication, intended as an intentional act (Panza 1995*b*, 116). I believe of course that this is a good definition, but it is in a sense *a posteriori* with respect to the advent of the object itself in the horizon of the subject. If we imagine that the object is already there, we can use such a definition for characterizing it. Here, I would like to advance a genetic definition: an object is the intentional content of an act of exhibition; an object is exactly what a subject is exhibiting when he wants to exhibit something, he does it, and he recognizes his act just as the act of exhibiting this something. An act of predication (intended as an intentional act) is either addressed to an object that has already been exhibited, or it is part of the act of the exhibition itself. An object that has already been exhibited is not continuously present to the consciousness of the subject. Rather, I would say that an object has already been exhibited when the subject is able to represent it to his consciousness—to evoke it—by using any conventional symbol that is generally intended as the name of the object itself. If this is the case, the object is also able to attribute any property to it, without coming back to the act of exhibition as such. Therefore I say that the object is already present in the subject's horizon.

If we accept such a point of view, the particular nature of an object depends on the particular nature of the act of exhibition itself. Moreover, only logical differences are important here. We cannot distinguish empirical from pure objects by saying that the act of exhibiting the first ones is connected in some way with the occurrence of a sensation. If we do that, we fall back on the previous difficulty. If we want to avoid it, and also avoid any heritage of classical ontological empiricism, we have to use the notion of empirical object to explain what a sensation is, and not *vice versa*.

Perhaps we could do that, by referring to the ostensive character of certain acts of exhibition, but then we have to know what an ostensive act is, before knowing what an empirical object—and *a fortiori* a sensation—is, and I am not sure that this is really possible. Thus, I prefer to pursue a different strategy. In order to present it, I have to introduce the notion of concept.

VI.2 Concepts

First of all, we could intend a concept as the subjective function that enables the subject to identify an object as such, to exhibit it to himself. This characterization does not apply to any sort of concept: the concepts to which it applies are concepts of objects, rather than concepts of properties or relations. From a logical point of view, we could say that a subject possesses such a capacity if and only if he disposes of a criterion of identity and he is able to apply it both to the contents of different acts of exhibition and to the objects evoked by a certain name (or in another way). Hence, we could say that a subject possesses a concept of an object if and only if he disposes of such a criterion and he is able to apply it.

A concept of a (monadic) property is the subjective function that makes the subject able to assign an object that has already been exhibited to a certain class of objects. From a logical point of view, we say that a subject possesses such a capacity—and then the corresponding concept—if and only if he disposes of a criterion for that, and is able to apply it to any sort of objects, by concluding either that they have or do not have to be assigned to that class, or that their modalities of exhibition do not enable him to decide if they have or do not have to be assigned to that class. If a subject possesses such a capacity, he possesses the concept.

Finally, a concept of a relation is the subjective function that makes the subject able to assign a certain class of objects that have been already exhibited and assigned to such a class, to a certain class of classes of objects. In this case too, a

subject possesses such a capacity—and the corresponding concept—if he disposes of a criterion for that, and is able to apply it in order to obtain one of the three issues I have just indicated for concepts of properties.

Let us first consider concepts of objects. According to the previous characterizations, a subject cannot exhibit an object to himself, without possessing the concept of this object, since a subject realizes an act of exhibiting something to himself only if he recognizes one of his acts as an act of exhibiting something to himself and he is able to do it, only if he is able to distinguish his act of exhibition from any other act. Moreover, as no object can be exhibited to any subject if this subject does not ultimately exhibit it to himself, we can conclude that no object can be exhibited to any subject if this subject does not possess the concept of this object. Analogously, a subject cannot possess a concept of a certain object without exhibiting such an object to himself, since he cannot possess a criterion of identity if he does not represent such a criterion to himself, and he cannot represent it to himself without representing to himself the content of an act of exhibition that satisfies this criterion itself. The object of this concept then is present in the horizon of the subject himself, that is: it has been exhibited to him. Thus, the important difference is not between empty and full concepts, but between objects that are preceded by their concepts and objects that precede their concepts. In my view, the objects of the first kind are pure, while those of the second kind are empirical. In the first case—the case of pure objects—the act of exhibition consists in the presentation of the concept itself, the effort of formulating the corresponding criterion. In the second case—the case of empirical objects—the corresponding criterion acts before having been formulated; its formulation is only a *post festum* description of a capacity we have already applied. Of course, we can try to transmit to someone—a child, for example—the concept of Venus by showing a certain star to him and ask him to recognize Venus. But the child really exhibits Venus to himself only when he changes his concepts: Venus is not a star such and such; it is exactly the star the child has finally exhibited to himself. Starting from this moment, the name "Venus" has a new meaning for him, it does not evoke a star such and such (what is, according to me, a pure object), it evokes just the star that he has exhibited to himself at such an occasion.

Consider the concepts of properties or relations. Even though these concepts are not concepts of objects, but ask for a previous exhibition of certain objects, we can put them together in order to produce concepts of objects (which are certainly pure). Moreover, according to the previous definition, any concept of a property or a relation corresponds to the class of objects or classes that are formed by using the corresponding criteria. These classes are certainly objects, but their concepts are not the concepts of the property or the relation to which they correspond, according to the previous definition. The concepts of these objects are just the concepts of these classes taken as objects. Thus, it is perfectly possible to possess the concept of a property or a relation without exhibiting the corresponding classes as such, which are generally open classes and could even be empty.

Let us now come back to the concepts of objects. According to my definitions, concepts of object and objects are two logical categories correlated to one another: just as much as an object could be intended as the correlate of a concept of an object, a concept of an object could be intended as the correlate of an object. Now, if we intend objects and concepts of objects in such a way, we must also intend an act of exhibiting an object as a public act, when it consists in a certain subject's effort to transmit the possession of a concept of a certain object to other subjects. Thus, an object has been exhibited publicly when the capacity of identifying it has been transmitted to a number of subjects. Hence, according to the previous definition, an object that has publicly been exhibited cannot be an empirical object.

Of course, not only the possession of concepts of objects can be transmitted in such a way; the same is true for concepts of properties and concepts of relations. However an effort to transmit these concepts is not a public act of exhibition of the classes (of objects or classes) corresponding to these concepts.

To transmit a concept we use language. Thus, when we study public phenomena, like science, we can intend a concept, by extension, as a linguistic characterization of an object, a property or a relation (that is the aspect of an object or a class of objects that make this object or class the members of a certain class). From my point of view, such a characterization has not to be intended as the discursive transposition of the properties of the object, or the conditions of the property or the relation—which is close to the Leibnitzian notion of complete concept. As a matter of fact, complete concept is only an abstract notion, grounded on ontological presuppositions, and it cannot be used as such in a theory of human knowledge. A linguistic characterization of an object, a property or a relation rather has to be intended as a way to fix these entities in our discourse, and in this sense it is only a sort of "concrete" representation of the concept (as a subjective function), an "exposition" or "explication" of it. This representation cannot in general be a complete characterization of the object, the property or the relation, but it is only a means for transmitting certain capacities in a community of subjects.

As a representation, such a characterization is not a representation of an object, a property or a relation, it is just a representation of their concepts. But, if these concepts are linguistically represented, they are *ipso facto* exhibited and thus, they are objects, even though they are certainly not the objects of themselves or the classes corresponding to themselves. So, when it has been presented as such, any concept is an object, even it is certainly a pure object.

VI.3 Analysis of Concepts, Analysis of Objects

I am now able to explain what I mean when speaking of an act of reasoning grounded on the analysis of the concepts of the objects to which such an act is attributing properties or relations, that is an analytic act of reasoning. This is an act of reasoning where such an attribution is a consequence—according to certain rules of inference—of nothing but the concepts of the objects, the properties or the relations, intended as linguistic characterizations of them.

The acts of reasoning expressed respectively by the statement $< P(a) >$ and $< R(b, c) >$ are for example analytic if and only if these statements are consequences of nothing but the concepts of the property P and of the object a and of the relation R and of the objects b and c, respectively.

The situation is a bit more complicated for an act of reasoning expressed by a statement like $< \forall x[S(x) \Rightarrow P(x)] >$[19]. In order to consider such a statement as an expression of an act of reasoning, we have to intend it as the attribution of the property P to some objects. I have said that it is possible to possess the concept of the property S without having exhibited the class of the objects which are S. Thus, either this statement—as long as it is intended as an expression of an act of reasoning—attributes the property P to all the objects which have been already exhibited and are S, or the concept S in it is taken as a concept of an object rather than a property, or finally it attributes properties to potential objects, that is the objects which could be elements of the class connected to the concept S. In the first case, such a statement is only an abbreviation of a conjunction of statements of the form $< P(a) >$, and the corresponding act of reasoning is analytic if and only if all the acts of reasoning corresponding to these statements are analytic. In the second case the statement $< \forall x[S(x) \Rightarrow P(x)] >$ is not universal and is rather equivalent to only one statement of the form $< P(a) >$ and the corresponding act of reasoning is analytic under the conditions of analyticity of the act of reasoning corresponding to such a statement. Finally in the third case, the act of reasoning corresponding to the statement $< \forall x[S(x) \Rightarrow P(x)] >$ is analytic if the attribution of the property P to all the objects, which could be elements of the class connected to the concept S, is a consequence of nothing but the concepts of the properties S and P.

Still, a statement like $< \exists x\,[P(x)] >$ expresses an act of reasoning in my view, only if it is a logical consequence of a statement of the form $< P(a) >$, and such an act is analytic if and only if the act of reasoning expressed by this statement is analytic.

Here the term "consequence" has to be conceived in its general sense: the analyticity of an act of reasoning depends on the rules of inference that characterize what a consequence of a certain linguistic characterization is. What is important obviously is that these rules do not depend on the exhibition of the objects connected to the considered concepts.

An act of reasoning is grounded on an analysis of the objects to which it attributes properties or relations, and it is then synthetic, if such an attribution depends on the exhibition of these objects as such (that is: if it cannot be realized if such an exhibition is not realized). As any exhibition of an object asks for the possession of its concept, a subject which does not possess the concept of certain objects can certainly not realize a synthetic act of reasoning by attributing properties or relations to these objects themselves. However, even though such a condition is necessary, it is not sufficient.

Thus, the acts of reasoning expressed, respectively, by the statements $< P(a) >$ and $< R(b, c) >$ are synthetic if and only if the attribution of the property P and the relation R respectively to the object a and the objects b and c depends on the exhibition of these objects.

An act of reasoning expressed by a statement like $< \forall x[S(x) \Rightarrow P(x)] >$ could then be synthetic only if such a statement is intended in one of the first two ways I just mentioned. In the first case it is synthetic if and only if one of the conjuncts of the statement of which it is an abbreviation is synthetic. In the second case it is synthetic if and only if the equivalent statement of the form $< P(a) >$ is synthetic.

Finally, an act of reasoning expressed by a statement like $< \exists x\ [P(x)] >$ is synthetic if and only if such a statement is a logical consequence of a statement of the form $< P(a) >$, which expresses a synthetic act of reasoning.

In order to apply the previous definitions to particular acts of reasoning, we have to understand both what an act of exhibition of an object occurring in these acts could be, and how an attribution of properties or relations to such an object could depend on the exhibition of this object itself. As my sole aim is to justify thesis (T_2), I may limit myself to the case of mathematical objects. Therefore, I have to explain what a mathematical object is; why it is pure; and how is it possible to formulate a (synthetic) act of reasoning about it, by grounding on its exhibition. This is now my task.

VII Naive Formalism and Conceptualism

There is a first and very radical objection to (T_2) that we can formulate without having any idea of what a mathematical object could be. One could argue that the object-concept dichotomy does not provide suitable categories for speaking about a formal system, as modern mathematics ultimately is: in a formal system there are no concepts at all and if there are objects, they are only symbols (that is terms of a net of rules). Thus, even though (T_2) is possibly right with respect to premodern mathematics, it is certainly wrong for modern mathematics.

I think there is a deep misunderstanding in such an objection. Such a misunderstanding has been denounced a number of times and it is not necessary here to insist on it. In my opinion, it consists in confusing (modern) mathematics as such with a certain collection of axiomatic systems intended as systems of sentences deduced by means of a number of explicit rules of inference starting from a number of axioms, which are in their turn simply intended as the starting point of deduction. I believe this is just a confusion. Even if we accepted the idea that without axiomatic systems intended in this sense, there is no mathematics (which compels us to conclude that a great part of what has historically been and is nowadays indented as mathematics, is not mathematics), we should add that an axiomatic system, intended in such a way, is only a tool for mathematicians. Not only such a tool has to be constructed, and its construction has to be intended as an important part of the work of mathematicians (that is mathematics as an activity), but even when a mathematician disposes of it, he looks for deductions in it only in order to produce proofs of theorems. And theorems as such are not part of this system.

What is then a theorem? Let us take a very simple example. Even though we can exhibit a deductive path that starts from Peano's axioms and usual definitions of Peano's arithmetic and arrives at the sentence $< 7 + 5 = 12 >$, still the theorem a mathematician wants to prove by exhibiting such a path does not consist in such a sentence. It is rather expressed by the statement < the number '12' is the result of addition of the number '7' and the number '5' >: the symbols "7", "5" and "12" are not simply symbols introduced by the usual definitions; for a mathematician they are names of numbers.

Nevertheless, even if I was right on this point, (T_2) could still be wrong. The statement < the number '12' is the result of addition of the number '7' and the number '5' > could be interpreted as a statement of the form $< \forall x[\{7+5\}(x) \Rightarrow 12(x)] >$ or $< \forall x \forall y[(\{7+5\}(x) \wedge 12(y) \Rightarrow =(x, y)]$, attributing the property '(to be) 12' to potential objects which could be elements of the class connected to the concept '7+5' or the relation of equality to the potential objects which could be elements of the classes connected to the concepts '7+5' and '12', respectively. If this has been a correct analysis, and we intended such a statement as the expression of an act of reasoning in my sense, we should conclude, according to my definitions, that such an act is analytic. Thus one could accept all my definitions, agree with me that a mathematical statement is the expression of an act of reasoning and still argue that (T_2) is wrong.

A suitable framework for such a position is implicit in those philosophical conceptions with regard to mathematics that are generally arranged under the term "conceptualism". According to such a point of view, a mathematical theory is nothing but a relational structure of concepts.

A new version of conceptualism has recently been proposed by L. Tharp (Tharp 1989-1991). Tharp wants to avoid all problems of the referential view of mathematics from the very beginning, by putting forward the idea that mathematical assertions should be regarded as expressing relations among concepts instead of among objects. Tharp explains the claims of his conceptualist position by a comparison to fiction. As an illustration of the role of fiction in his argument, Tharp presents a very short story.

> "The only people in our story are Gertrude and Hamlet. Gertrude is a queen. Hamlet is a prince, and Gertrude is Hamlet's mother. [...] Given these two stipulations which constitute our story, various consequences follow from the meanings of the concepts 'prince', 'queen' and 'mother', and are evidently true-in-the-story: for example no princes are queens; Gertrude and Hamlet are distinct; Hamlet is not Gertrude's mother. None of these conclusions follow logically from the given story, however. Also, the first conclusion doesn't even use our stipulations. Although the stipulations are largely arbitrary, once they are fixed, the consequences of those stipulations are thereby fixed." (*ibid.* part I, 168-169)

Now, according to the conceptualist point of view, the previous conclusions can be intended as expressing relations between concepts. These concepts do not subsume objects or relations between objects under them and thus it is not possible to go away from them in order to get out any conclusion. So, the questions to which we can found an answer in the story are previously delimited. We cannot ask for instance, what color Hamlet's eyes have or what his mother Gertrude weighs: Hamlet and Gertrude are not objects to which we have access.

This is Tharp's position. But, if we want to refute (T_2), after having accepted all my definitions, we are not compelled to accept this position. We could deny that there is something like a concept without objects and argue that any concept, even if it is taken as a concept of a property or a relation, corresponds to actual or potential objects. We should simply state that all the concepts that occur in mathematical acts of reasoning are concepts of properties or relations that do not correspond to objects that have already been exhibited, but only to potential objects that will never be exhibited in mathematics. I am not sure whereas this position is tenable, but since we can always retreat to Tharp's more solid position, there is no need for me to criticize it as such.

In order to make my point clear, I will consider an example different from the "discursive" example of Tharp. I am taking it from an excellent forthcoming paper by R. Casati (Casati fc) where the author tries to "capture some of the intuitions regarding absolute rest and motion", as they work in Newton's conception of space, by presenting an axiomatic system founded on two axioms:

(A_1) If x and y are at absolute rest, then x is at rest relatively to y $(\forall x, y[(R(x) \wedge R(y)) \Rightarrow \mathsf{R}(x, y)])$.

(A_2) If x is at absolute rest and y moves, then x moves relatively to y $(\forall x, y[(R(x) \wedge \neg R(y)) \Rightarrow \neg\, \mathsf{R}(x, y)])$.

From these two very simple axioms and using usual rules of first order predicate calculus, Casati draws a number of "theorems" that describe Newton's point of view.

Here, the variables x and y stand for bodies, but clearly this is not essential for the success of the deduction. In fact x and y could simply stand for any sort of potential objects that are or not R and are or are not between them in the relation R. Moreover, the deduction is also independent of the particular interpretation which Casati advances for the predicates R and R. This interpretation is only responsible for the fact that Casati's axiomatic system captures or does not capture the Newtonian conceptions. Really, (A_1) and (A_2) work simply as Carnap's meaning postulates, fixing non-logical relations between certain predicates (Carnap 1952).

Of course my point is not concerned with the analyticity or syntheticity of (A_1) and (A_2), since I am interested in the analyticity or syntheticity of acts of reasoning and, if they are intended as meaning postulates, these statements do not express acts of reasoning. They are merely stipulations. Moreover, if these stipulations are intended as explicit expressions of the relations that take place between the concepts of the property R and the relation R the acts of reasoning expressed by the theorems deduced from (A_1) and (A_2) are clearly analytic. If these stipulations are not intended in such a way, they have to be taken as arbitrary stipulations and so the theorems deduced from them are consequences of the new Casatian concepts of the property R and the relation R. Once again the acts of reasoning expressed by these theorems are then analytic.

My point should be clear now: if a mathematical theory is expressed by an axiomatic system like the previous one, no mathematical theorem can ever express a synthetic act of reasoning and thus (T_2) is certainly wrong. Hence, in order to justify (T_2), I have to argue that a mathematical theory is not expressed by a system of axioms like the previous one. This is my thesis: in my view, a mathematical theory deals with actual pure objects and not only with concepts of properties or relations.

VIII Madame Bovary as a Pure Object

According to my definitions, the notion of pure object is not problematic as such. We can exhibit a number of pure objects, such as Madame Bovary, the first mover, the concept of analyticity, the property '(to be) red', or the number '3'. Each of them corresponds to a particular way of exhibition, which characterizes its specific nature and which is different in each case. Of course I am not concerned here with these ways as such. My question is a different one: is it possible to ground ourself on a pure object as such (rather than on its concept) for realizing an act of reasoning?

Let us first consider the example of a pure object, which is certainly not a mathematical object, like Madame Bovary. As the corresponding concept (intended as a discursive characterization of such an object) let me take everything Flaubert says about Madame Bovary in his novel. Are we able to derive from it, and from our background knowledge, some other knowledge about Madame Bovary? There are good reasons for answering "yes", as well as "no". I do not intend, here, to discuss these reasons. For my task, it is sufficient to remark that, even if we answer "yes"—by asserting, for example, according to Tharp, that in our background knowledge the concepts occurring in the complex concept of Madame Bovary are connected to other concepts that do not occur directly in it (in such a way that we can surely conclude, for example, that Madame Bovary was not able to write with "Word 5" on a Macintosh PowerBook 145)—we have to maintain that there is no way for grounding our act of reasoning on Madame Bovary as an object. We can derive new knowledge only by considering the concept of Madame Bovary. As an object, Madame Bovary is a purely semantic entity: not only is it the correlate of its concept, but there is no way for having access to it that is not a way for having access to its concept.

You might think that this is the situation with every pure object: the act of exhibiting a pure object really consists in presenting its concept, and you might think that this makes it impossible to have a way of having access to it that is not a way for having access to its concept.

Let us consider this point. According to my definition, a pure object is a semantic entity. In a sense, such an object is only logically different from the corresponding concept: it is only an entity that has a different logical role than the concept. As the act of exhibiting it consists in presenting the concept, and as this presentation is nothing but a linguistic performance, a pure object could be intended as the reference of a linguistic term. The act of exhibition of the concept itself fixes such a reference, and a number of linguistic (and pragmatic) conventions makes a community of subjects able to recognize it as the meaning of the same term in different linguistic contexts. Thus, we have finally to conceive a pure object as the reference of this term, when it is used in certain contexts that a community of subjects is able to recognize.

Marco Santambrogio recently clarified the idea of a non-Fregean (that is non-distributive) notion of reference, and attached a very powerful theory of abstract objects to it (Santambrogio 1992). According to him, we can intend the reference of "certain parts of discourse" as "their contribution to the truth or falseness of the statements in which they appear" (*ibid.*, 144). In this sense, the reference of the terms "Madame Bovary" or "triangle" in the statements < Madame Bovary killed herself by ingesting arsenic > or < the sum of the internal angles of a triangle is π > is that which makes these statements true.

Santambrogio's idea enable us to formulate the problem in a new way: what makes the statements < Madame Bovary killed herself by ingesting arsenic > or < the sum of the internal angles of a the triangle is π > true? Certainly this is a pure object, but: how does this object make these two different statements true? My thesis is that the answer is different in the two cases.

When claiming, in the first case, that the statement < Madame Bovary killed herself by ingesting arsenic > is made true because of the object 'Madame Bovary' rather than because of its concept, we are saying merely that this statement is literally true only if it refers to the object 'Madame Bovary', rather than to its concept: it is really not the concept of Madame Bovary that killed itself in this or in another way; concepts do not kill themselves. However, it is true that Madame Bovary killed herself by ingesting arsenic only because this is said in the presentation of the concept of Madame Bovary and we know it only because we have read this presentation. In other words: here the object 'Madame Bovary', even though it is evoked by its proper name, works ultimately as an object that satisfies the property '(to be) Madame Bovary', when such a property occurs in a meaning postulate like this:

(A3) $\forall x[\text{Madame-Bovary}(x) \Rightarrow \text{killed-herself-by-ingesting-arsenic}(x)]$

Thus, there is no way to realize a synthetic act of reasoning about Madame Bovary.

IX Euclidean Geometry

IX.1 Kant, Once Again

Let us now consider the case of the triangle. Here is what Kant says about the proof of the Euclidean theorem on the internal angles of a triangle:

> "Suppose a philosopher be given the concept of a triangle and he be left to find out, in his own way, what relation the sum of its angles bears to a right angle. He has nothing but the concept of a figure enclosed by three straight lines, and possessing three angles. However long he meditates on this concept, he will never produce anything new. He can analyze and clarify the concept of a straight line or of an angle or of the number three, but he can never arrive at any properties not already lied on[20] these concepts. Now let the geometrician take up these questions. He at once begins by constructing a triangle. Since he knows that the sum of two right angles is exactly equal to the sum of all the adjacent angles which can be constructed from a single point on a straight line, he prolongs one side of his triangle and obtains two adjacent angles, which together are equal to two right angles. He then divides the external angle by drawing a line parallel to the opposite side of the triangle, and observes that he has thus obtained an external adjacent angle which is equal to the internal angle—and so on. In this fashion, through a chain of inferences guided throughout by intuition, he arrives at a fully evident and universally valid solution of the problem." (Kant A, 716-717; B, 744-745)

If we read such a passage in the light of the previous non-Kantian notion of object, it seems to suggest that the Euclidean proof does not deal with the concept of triangle, but with the triangle as an object. Two reactions are quite natural in face of such a thesis. The first is typically philosophical and consists in asking, in a skeptic voice: "what is it, the triangle as an object?" The second consists in recognizing that classical procedures in elementary geometry, as that described by Kant, seem to be concerned with something essentially different from the logical rules of inference we use in performing analytic acts of reasoning started by meaning postulates.

Kant's idea seems to be made clear in the following parts of the first section of chapter one of the *Transcendental Doctrine of Method* (from which I have taken the previous quotation), where the notions of definition, axiom and proof are discussed (*ibid.* A, 727-738; B, 755-766)). According to Kant, definitions are possible only in mathematics:

> "To *define*, as the word itself indicates, really only means to present the detailedly complete [*ausführlichen*], concept of a thing [*Ding*] originally [*ursprünglich*][21] within its limits [*Grenzen*]." (*ibid.* A, 727; B, 755)

The definition is then made precise in a footnote:

> "Detailed completeness[22] means clarity [*Klarheit*] and sufficiency of characteristics [*Zulänglichkeit der Merkmale*]; by limits is meant the precision [*Präzision*] shown in there not being more of these characteristics than belong to the detailed complete concept; by *original* is meant that this determination of these limits is not derived from anything else, and therefore does not require any proof [*Beweis*] [...]." (*ibid.*)

Now, all this is possible only if the concept is "arbitrarily thought [*willkürlich gedacht*]" (*ibid.* A, 729; B, 757)[23], but the presentation of a concept that is "arbitrarily thought" is the definition of a "true object", only if such a concept "contains an arbitrary synthesis that admits of *a priori* construction [*welche a priori konstruiert werden kann*]" (*ibid.* A, 729; B, 758), and this is possible only in mathematics.

Here Kant seems to come very close to the idea that mathematics deals with pure objects. This is what he writes next:

> "[...] mathematics is the only science that has definitions. For the object which it thinks it exhibits [*stellt*] *a priori* in intuition, and this object certainly cannot contain either more or less than the concept, since it is through the definition that the concept of the object is given—and given originally, that is, without its being necessary to derive the definition from any other source." (*ibid.* A, 729-730; B, 757-758)

This passage seems to be quite unambiguous: if the object that "mathematics thinks" and "exhibits in intuition" was the particular empirical figure we trace on a sheet of paper or a blackboard when we repeat the Euclidean proof, Kant should certainly not say about it that it "cannot contain either more or less than the concept".

Nevertheless, after this passage, Kant insists generically on the "construction of the concept", rather than on the presence of a pure (mathematical) object. He distinguishes philosophy from mathematics by saying that the former is only able to "expose" or "explain" given concepts—by performing their definitions, analytically—while the latter "constructs" concepts "originally framed"—and performs their definitions synthetically (*ibid.* A, 730; B, 758).

Of course, what Kant means by "exposition" is not a kind of description of something that is already given as such, and what he means by "construction" is not the act of providing it originally. If it was so, it would be very easy to reply that philosophy constructs its concepts too (where could it take them from, otherwise?) and even advances by constructing further and furher concepts, while mathematics accepts its definitions and merely deduces theorems from them. However, Kant's point is not to deny it. The "exposition of concepts" in Kant's sense is perfectly compatible with their construction in the previous sense, just like their "construction" in Kant's sense is compatible with their exposition in the previous sense. What is important for Kant is that no philosophical construction (in the previous non-Kantian sense) can produce anything but concepts: it is, and it cannot be anything but an exposition of concepts (even though these concepts are new). What is exhibited in such a construction is nothing but concepts, and thus such a construction is nothing but an "exposition of concepts". Mathematical definitions in contrast exhibit objects, and not merely concepts. Thus, for Kant, the construction of concept is just the access to the object, as in the seventeenth century the construction of equations was just the exhibition of the mathematical object expressed by its roots (Bos 1984). This point seems to be made clearly by Kant, not only in the previous passage, but also in his discussion of axioms and proofs in mathematics:

> "Mathematics [...] can have axioms, since by means of the construction [*Konstruktion*] of concepts in the intuition of the object it can combine the predicates of the object both *a priori* and immediately [...]." (*ibid.* A, 732; B, 760)
> "[...] mathematics can consider the universal *in concreto* [*das Allgemeine in concreto*] (in the singular[24] intuition) and yet at the same time through pure *a priori* representation [...][it realizes] *demonstrations* [*Demonstrationen*], which, as the term itself indicates, proceed in and through the intuition of the object." (*ibid.* A, 734-735; B, 762-763)

However, there is no way, in Kant's framework, for making the idea of an object that could be just the object of a mathematical concept (as the concept of triangle) clear and acceptable. This object should be pure, and there is no room for pure objects in Kant's framework. Thus, Kant alternates passages like the the previous one and others much more ambiguous, such as this one:

"Mathematics alone, therefore, contains demonstrations, since it derives its knowledge not from concepts but from the construction of them, that is, from intuition, which can be given *a priori* in accordance with the concepts." (*ibid.* A, 734; B, 762)

Therefore, the only interpretation of the remarks contained in the first section of chapter one of the *Transcendental Doctrine of Method* which seems to be consistent with Kant's philosophy, is the one I have already given in the previous paragraph III.2.: intuition is pure, but objects are not. Pure intuition assures us that usual empirical objects which are manifest in particular pure forms are constructible and that we are able to operate on them in such a way that all the conclusions we draw from such an operation are also true for pure forms. Thus the triangle that the mathematician constructs is a particular empirical figure, but the conclusions he draws by operating on it, as in the Euclidean proof, are about the triangle as pure form, which is not really an object.

The distinction between pure forms and objects is quite impossible to clarify. However, the problem is not solved simply by eliminating such a distinction. Even though we consider pure forms as genuine objects, the situation does not change essentially: if the Euclidean proof deals with a particular empirical object—as in the previous reconstruction of Kant's argument—it can be a proof of a geometrical theorem only if it stays constantly under the control of the concept. But if this is so, the guarantee of the theorem comes just from the concept, and thus such a theorem expresses an analytical act of reasoning, in my sense.

IX.2 Euclid's Proof of The Theorem on Internal Angles of a Triangle

My point should now be clear: in my view, the Euclidean proof uses an empirical figure, but does not deal essentially with it. Such a figure is nothing but a particular notation (an icon, as Peirce says (Peirce 1885, 163)[25]) for the real object of such a proof, that is just the triangle. Like any object, the triangle is particular, but it can be represented by an infinite class of empirical figures. Even though these figures work in any reformulation of the Euclidean proof as a very particular notation, which expresses directly some properties of the triangle itself, this proof runs by applying a number of constructive procedures chosen in a certain domain of permitted procedures to such a notation.

To understand this point, let us reconstruct the Euclidean proof from the very beginning.

Euclid imagines we know what a (finite) straight line is and takes straight lines as elementary objects. He represents them by empirical lines that have two essential properties: they are continuous lines (property of continuity) and they are open lines that separate a region of the surface on which they are traced into two parts we can distinguish (property of separation). These properties are not expounded or defined by Euclid: they are simply two manifest properties of empirical lines we use as notations of straight lines. But they are also the only two properties of these lines that occur as such in the proof of any theorem of Euclid's geometry.

In order to assure such a starting point of his geometry, Euclid certainly has to make an appeal to a certain capacity of his readers: this capacity could be described in my terms as possessing a certain concept. This concept has a very particular nature: it is the concept of a pure object—that is just a straight line—rather than the concept of a property, but it can be used for introducing an open domain of pure objects. These objects are not—as it is the case with every concept of a property—different objects corresponding to different concepts; they are different objects corresponding to the same concept, which is the concept of an object. Simply, these objects are introduced and considered, one after the other, in different positions: a straight line differs from another only by its position. But position is a relative property and it is not possible to characterize the positions of two different straight lines, if we do not intend them as different straight lines beforehand.

Thus, two straight lines are not the objects of two different concepts we arrange in the same class, according to a concept of a property. They are two different objects corresponding to the same concept of an object. But, as these objects are treated in geometry exactly in the same way, they also can be intended as two different manifestations of the same object, too. They merely differ according to an original subjective capacity of differentiation, the capacity which enables us to distinguish different positions in spatio-temporal order. If we generally consider the modalities according to which we can operate on it, we have to speak about a straight line as only one object; if we pass to another level and we consider different applications of certain procedures consistent which these modalities, we have to speak about straight lines as different objects.

If I am right, a straight line is an object exhibited according to a quite complex strategy, appealing to different subjective capacities. However, such an exhibition is not completed since the operative procedures according to which we can operate on a straight line (or on straight lines) are not fixed. This is the task of the Euclidean postulates. These postulates are certainly not simple sentences working as starting points of a deductive game. They are constructive clauses (cf. the paper of Mäenpää in the present volume, who particularly insists on this point) that teach how to compose straight lines, in order to construct non-elementary objects starting from these. First, these objects are constructed, and then they are analyzed just like the objects which are constructed as they are, starting by straight lines.

Now, imagine that three straight lines are given. This means that three empirical lines are traced on a certain surface. If the third of these lines is long enough, relatively to the others, by applying the theorems I.2 of the *Elements*, we can construct the triangle that has these straight lines as its sides, and trace a corresponding figure on the same surface. Then, by applying the theorem I.27, we can construct a straight line parallel to one of the sides of the triangle, passing through the opposite vertex. Furthermore, by applying the second postulate, we can prolong all the sides. If we trace on our surface the lines corresponding to these constructions, we have a new figure. According to the property of separation for empirical straight lines, we can now recognize three angles on the same straight line. By applying the theorem I.29 to the angles formed by a transversal on two parallel straight lines, we can finally prove the theorem.

It is clear that the triangle and the angles we have considered here are the triangle and the angles that have been constructed, according to the previous procedure, starting with the three given straight lines. Thus, they are, like straight lines, pure objects.

As an object, the triangle is exhibited in Euclidean geometry when their modalities of construction starting with straight lines are given, according to the possibilities admitted by the postulates and the properties of continuity and separation of empirical lines. Such an exhibition is in a sense a presentation of a complex concept—that is not the naive and original concept of triangle, as a typical form of empirical objects, but a "mathematical" translation of it. But once this concept has been exhibited in such a way, it does not operate as such in the Euclidean proof; it does not control anything. The proof is properly the result of an analysis of the triangle as an object, that is an account of the properties of it, according to: its particular way of construction; the constructive clauses expressed by the postulates; the properties of continuity and separation of empirical lines; the subjective capacity of multiplication of pure objects in space and time.

X Arithmetical Proofs

If my analysis of the Euclidean proof of the theorem on the internal angles of a triangle is correct, such a theorem expresses, in the Euclidean framework, a synthetic act of reasoning, according to (D_4). The specification "in the Euclidean framework" is essential, since the same theorem could be stated as a consequence of a suitable class of meaning postulates. In this case, it would express an analytical act of reasoning. Thus, in order to justify (T_2) I now have to argue that the Euclidean framework is a typical framework of mathematical acts of reasoning.

The first step in the argument should obviously consist in stating that the situation of the previous theorem is common to every theorem of Euclidean geometry. Since I cannot present a general account of Euclidean geometry here, I am compelled to take for granted that this is the case. I will simply try to argue that the situation I have just described is not typical—with respect to its structural

characters—of such a mathematical theory, but it is general for mathematics as such, that is for classical, as well as for modern mathematics.

With the term "structural characters", I refer of course to the relations between concepts and objects, independent of their particular nature. My point is this: what you make when you conduct a geometrical proof, as the one we have just considered, is not to compare at every step the empirical figure in front of you—the notations of geometrical objects you are dealing with—to your concepts of these objects; simply, you apply with respect to certain figures—which you know to be good notations for these objects—certain standard procedures, you know as being permitted in the context of your theory. Thus, if the concepts of the geometrical objects occur, they occur not in the proof as such, but in an original stage, when the question is that of fixing notations (and identity criteria for them) and legitimate procedures. But if you know that the notations you are using are good notations, and the procedures are accepted, the concept does not occur as such.

Thus, if I am right, the structure of the a Euclidean proof could be described as follows. First, we have a certain number of original and naive concepts of properties, the concepts of spatial forms of extended objects. We associate such concepts with certain empirical figures we learn to reproduce according to certain relations of equivalence. Then, we introduce a number of procedures to transform our figures, and we fix certain rules that allow us to draw certain conclusions from certain figures (by considering the path we have pursued to attain them). Finally we apply these procedures and rules to our figures and we draw our theorems.

Of course, this is not, as such, the structure of every mathematical proof. There is something here that is typical of classical geometry—that is geometry in its original and proper sense. I obviously refer to the empirical figures or notations, which are not merely conventional or uninvolved symbols, but occur as such in the proof itself as bearers of certain properties—the properties of continuity and separation—that are also essential properties of the mathematical objects. Even though such a circumstance seems to entail a more natural development of mathematical acts of reasoning, it obscures its essential character. Empirical figures are essential tools of a geometrical proof, since a geometrical object is essentially a pure object represented by them (certainly it is quite possible to translate a geometrical proof into a purely linguistic deduction, but the result of this translation is not really a geometrical theory, but only a representation of it), but they are not essential tools of a mathematical proof as such.

X.1 < 7 + 5 = 12 >

However, the essential occurrence of empirical figures or notations within a mathematical proof, as bearers of certain properties of mathematical objects is not, as such, proper only to classical geometry. Let us consider another example drawn from classical or constructive arithmetic, which once again is a Kantian example. Here is what Kant writes in section V of the introduction to the second edition of the *Critique of Pure Reason.*

> "We might, indeed, at first suppose that the proposition 7+5 = 12 is a merely analytic proposition, and follows by the principle of contradiction from the concept of a sum of 7 and 5. But if we look more closely we find that the concept of the sum of 7 and 5 contains nothing save the union of the two numbers into one, and in this no thought is being taken as to what that single number may be which combines both. The concept of 12 is by no means already thought in merely thinking the union of 7 and 5, and I may analyze my concept of such a possible sum as long as I please, still I shall never find the 12 in it. We have to go outside these concepts, and call in the aid of the intuition which corresponds to one of them, our five fingers, for instance, or, as Segner does in his *Arithmetic*, five points, adding to the concept of 7, unit by unit, the five given in intuition. For starting with the number 7, and for the concept of 5 calling in the aid of the fingers of my hand as intuition, I now add one by one to the number 7 the units which I previously took together to form the number 5, and with the aid of that figure [the hand] see the number 12 come into being. That 5 should be added to 7, I have indeed already thought in the concept of a sum = 7+5, but not that thus sum is equivalent to the number 12. Arithmetical propositions are therefore always synthetic. This is still more evident if we take larger numbers. For it is then obvious that, however we might turn and twist our concepts, we could never, by the mere analysis of them, and without the aid of intuition, discover what is the sum." (Kant B, 15)

If we analyze the proof described by Kant, as we have done in the case of the Euclidean proof of the theorem on the sum of internal angles of a triangle, we find the following structure. First we have an original and naive concept of number, as a property of any collection of distinct objects: two collections have the same number, if and only if, we can alternatively eliminate or mark their objects one after the other, and finish our work at the same time (or stage). By using this concept, we arrange all the collections of objects we are considering into different classes in such a way that all collections which belong to the same class have the same number. Then, we associate each class of collections we have just formed with a collection of conventional symbols that has the same number as all the collections which belong to such a class. Finally, we fix some procedures for operating on the collections of symbols that have been formed by respecting this condition: all we could do with these collections of symbols, according to these procedures, has to be repeatable when any collection of symbols has been replaced by any other collection with the same number. In other words: we determine these procedures so that they are completely independent of the choice of symbols. Particularly: *i*) we order our collections of symbols in such a way that we can move from each of them to the following one by adding only one symbol, we associate to any collection a conventional name and we order all the names, according to the order of the respective collections; *ii*) we define an operation of composition of two collections of symbols, so that the result of such a composition is exactly the collection of symbols that is formed by putting together the two collections we are composing, and we extend such an operation to the names of our collections. In this way the names '7' and '5' are associated with two collections of symbols, the composition of which gives just the collection of symbols associated to the name '12'.

We can decide that these names are the names of the collections of symbols themselves, of the classes of collections with which these collections of symbols are associated, or of the forms of these collections. This is not important. What is important is that in this way we have proven the statement $< 7 + 5 = 12 >$, by operating—as in the case of a geometrical Euclidean proof—on suitable empirical figures, or notations (the collections of symbols), according to certain procedures. The numbers, intended as objects, are just the objects that are represented by these notations. They are exhibited when the modalities of construction of the corresponding collections are given and the procedures for operating on these collections are fixed. Once again, such an exhibition is, in a sense, a presentation of the concepts of numbers, but these concepts do not occur as such in the proof of our theorem. This proof has the same structure as every proof in Euclidean classical geometry: according to (D_4), the theorem $< 7 + 5 = 12 >$ expresses a synthetic act of reasoning.

X.2 Peano's Arithmetic

If I am right, I have given two arguments for the claim that the two classical Kantian examples of mathematical synthetic *a priori* judgments express, in their natural mathematical framework, synthetic acts of reasoning. However, these are not arguments in favor of (T_2) yet. The objection one could advance is very traditional: even though the previous arguments are correct, they prove nothing but the syntheticity of acts of reasoning proper to classical mathematics, that is Euclidean classical geometry and elementary constructive arithmetic; but these theories are essentially pre-modern mathematical theories and their structural characteristics—particularly the ones I have considered in the previous arguments—are not structural characteristics of modern mathematical theories. Obviously, I think this is wrong. I now have to justify my view.

I have spoken about the mathematical concepts of triangle and of different numbers, but a doubt could arise. We can provide many different characterizations both of the triangle and of number 3. We can say, for example, that a triangle is a region of the plane confined by three non-parallel straight lines, or that it is the region of space that is common to three angles placed in such a manner that every side of one of them is also a side of one (and only one) of the two others. In analogy, we may say that the number 3 is the first odd prime number (if 1 is not prime or not odd), or the only divisor of 9 other than 1 and 9 itself, or that it is the result of the addition of 2 to 1. All of these definitions characterize the triangle and the number 3 as the only objects that satisfy certain conditions, that is: as the only members of the classes associated to certain concepts of properties. But, how can we say that these classes contain the same objects?

Let us imagine that a new novelist writes a modern version of *Madame Bovary*, where Madame Bovary does not kill herself by ingesting arsenic, but by eating poisonous mushrooms. It seems very natural to me to think that we are faced with a new concept of Madame Bovary, and consequently that the new novelist has exhibited a new object, which bears the same name as Flaubert's personage, but is not the same person. Yet, this is clearly not the case with mathematical objects. Even though they are exhibited by means of presenting different concepts, they are not different objects. But, how is this possible if a mathematical object is a pure object?

The answer is not simply that the different concepts of properties we might use for characterizing a mathematical object are equivalent, since this is exactly what we have to explain: how can they be equivalent? It is neither that we dispose of a suitable class of meaning postulates, since these postulates do not take part, as such, in a mathematical theory, and are, at most, a way of expressing the equivalence of different concepts, rather than to guarantee this equivalence.

The different concepts of properties to be used for characterizing a mathematical object are equivalent because their corresponding classes contain only one object that is always the same. And this object is not the object of these concepts, since these concepts are just concepts of properties, rather than concepts of objects. This object corresponds to another concept: the triangle is the geometrical object that is constructed in a certain way starting with straight lines, according to Euclidean constructive clauses; the number 3 is the number represented by the collection of symbols that is constructed in a certain way, starting with only one symbol. Thus, the other concepts I have just presented are equivalent because we can prove in Euclidean classical geometry and in elementary constructive arithmetic that the classes corresponding to these concepts are just composed by the triangle and the number three.

This simple remark clarifies what the essentially structural character of a mathematical theory is: it is just the disposability of a suitable class of concepts of objects that works essentially as such, rather than as concepts of properties. The examples of classical Euclidean geometry and elementary constructive arithmetic show two different modalities for satisfying such a condition. These modalities have an important aspect in common: they are constructive ways grounded on an original cognitive capacity, that is the capacity to fix the elementary objects of constructions—straight line and unity—and to multiply them in space and time. But other, non-constructive modalities are possible.

Using Salanskis' terminology (Salanskis 1995), I oppose these constructive modalities to "correlative" ways. According to a constructive modality, a mathematical object is exhibited when the way for constructing it is exhibited and the

procedures for operating on it are fixed. According to a correlative modality, a domain—generally a set—of mathematical objects is exhibited when the conditions that such a class has to satisfy as such are expounded and the criterion for distinguishing—if necessary—the different objects of such a domain have been given. For example, this is the case of Peano's arithmetic.

Let us also consider such an example. In Peano's arithmetic we assume that we know what a class is and—by means of Peano's axioms—we fix the conditions that a class has to respect in order to be a progression. These conditions refer to the members of the class itself, so that we have to assume too that we can consider these members separately, as different objects, even though we characterize all of them simply as members of a certain class. Thus, we make an appeal, once again, to our original capacity to multiply a certain object—the member of a certain class—in space and time.

The first axiom tells us that it has to be possible to take one element of the class, to nominate it—let us say by the name 'α'—and to evoke it, and only it, by means of this name, in any circumstance. Thus a class is a progression only if it has at least a member. The second axiom tell us that any member x of the class is associated with another (and only an other) one x_Γ by means of a certain monadic operator Γ, that need not to be characterized ulteriorely, even though we have to assume that, for every member x of the class, we can individuate the member x_Γ associated to it by Γ. As this axiom does not specify that x_Γ and x are distinct objects, every singleton could satisfy the first two Peano's axioms. The third axiom tells us that the member α of the class is associated to no other member by the operator Γ. Thus our class could not be a singleton, but it could be, for example, a couple $\{\alpha, \alpha_\Gamma\}$, if $(\alpha_\Gamma)_\Gamma$ is α_Γ itself. The fourth axiom tells us that the member of the class which is associated by Γ with a certain member x cannot be associated with another member y of the class, distinct from x. Hence, the class must be infinite and must be almost a starting point with respect to Γ. But it is possible that it was composed by different Γ-chains (one of which starts with α) independent of each other. Finally the fifth axiom tells us that this cannot be the case, since any property of α (for example the property of participating in the Γ-chain starting with α)—that, if it is a property of a member x of the class, then is also a property of x_Γ—is a property of every member of the class.

Once these axioms are given, we can assume that a class N is a progression, that is: *i*) we use the concept of property '(to be) a progression', or 'to respect the Peano's axioms for exhibiting an open class of classes (the class of progressions), by assuming that classes are, as such, already given objects; *ii*) we assume that such a class is not empty (for example, by asserting that we are able to exhibit a progression, as we have just done for the domain of numbers in elementary constructive arithmetic); *iii*) we assume we are able to choose a member of this class, to give a name to it and and to evoke it, and only it, by means of this name, in any circumstance. If N is a progression, we can pick out one of its member, which is α. We take this member and we rename it "zero" ("0") then we rename the element α_Γ of N "one" ("1"), and so on.

Up to now, we have used a concept of property and applied it to the class of classes in order to pick out the progressions. Then we have assumed that we can take one and only one progression, that is just N. The concept of N is thus a concept of an object, since, in order to be N, a class has not only to be a progression, but also has to be the progression we have chosen. It is not important that we are able to distinguish N from any other progression (certainly, we are not able do to it). What is important is that we take the concept of N as a concept of object: the progression we have chosen as the progression of natural numbers. Once we have done it, the concepts of the different members of N work also as concepts of objects (rather than as concepts of properties), because the concept of the member α of a progression is used as a concept of an object (rather than a concept of a property). Thus, we are in front of an infinite set of objects, which are just Peano's numbers.

To prove the theorem $< 7 + 5 = 12 >$, we now have to introduce the operation of sum to the members of the progression N. For that we state that for every three members x, y and z of N: $i)$ $+(x, 0) =_{df} x$; $ii)$ $+(x, 1) =_{df} x_\Gamma$; $iii)$ $+[x, +(y, z)] =_{df} +[+(x, y), z)]$; $iv)$ $+(x, y) =_{df} +(y, x)$; where "$\nu = \mu$" means: "ν and μ are two notations or names for the same member of N". Once we have done this, we can prove the theorem in the classical Leibnitzian way. However, if I am right, the conducting of such a proof, is an argument for the syntheticity of the act of reasoning expressed by this theorem, rather than from its analyticity. If the mathematical concepts of the numbers 5, 7 and 12 are certainly responsible for the exhibition of these objects, they do not work as such in the proof; rather this proof deals with their objects.

XI Concepts of Objects, Concepts of Properties: the Essential Character of Mathematics

The previous three examples should clarify the essential character of mathematical acts of reasoning, which turns them into synthetic acts of reasoning, according to (D_4). Mathematical objects are not only pure, but they are also exhibited by a very complex act of presenting their concepts. These concepts are generally constructed with the aim of providing a suitable translation of other concepts. Such a translation is successful when we are able to imagine a deductive structure applying to the names or notations of different objects we have introduced, first by multiplying a pure object in space and time, and second by individualizing some of the distinct objects we have created in such a way by a simple act of nomination. Because of the deductive structure and the particular nature of the act of

multiplication, all the objects arising from this sort of act are submitted to the same procedures and correspond to the same concept. Thus, such a concept is in a sense the concept of only one object, let us say a. It is only by a new act of individualization that we can change our level of analysis and pass to consider distinct a's, each of them now being characterized in a particular and suitable way, and submitted as such to the fixed procedures. As the concepts of these objects are not only presentations of the distinctive character of such objects, but integrate both, the act of multiplication in space and time and the act of fixing the possible procedures that can be applied to the objects themselves, they give, in a sense, an autonomous life to their objects, by enabling us to consider them as such. It is just this act of consideration of mathematical objects as such that is typical of mathematical acts of reasoning and makes them synthetic acts of reasoning.

Thus, a mathematical act of reasoning is only possible according to an intentional act which consists in treating a concept that fixes a certain character as a concept of an object, rather than as a concept of a property. Usually, the distinction between a concept of an object and a concept of a property is conceived as the logic correlate of a metaphysical difference between individual substances and their attributes. In contrast, I think that such a difference lies merely in the intentional use of concepts. If our concept of chair is such that to be a chair (or better the chair) means to be a particular object and not to enjoy a particular property referred to a certain class of specified objects, then we have to accept the idea that the chair (and not this or that chair) is an object, a pure object, of course.

Some argue that an object of this sort—like the triangle, or the natural number—is a universal object. However, I cannot understand what a universal object could be, since for me an object is essentially an individual entity. Nevertheless, this does not mean that for me an object is a certain determined substance, but merely that its exhibition (or evocation) exhausts certain exigencies of individuation that a subject could advance: it is possible to treat the pseudo-properties '(to be) a' as a "final characterization" with respect to a certain domain of other properties. Thus a concept of an object is nothing but a final characterization, working with respect to certain exigencies of individuation[26].

As the exigencies of individuation could be very different from one another, the same characterization could work in different context either as a concept of an object (that is a final characterization), or as a concept of a property. This is true for any sort of pure objects. The chair, as an object, is nothing but a particular kind of drawing-room suite: here, the concept of a drawing-room suite is a concept of a property, while the concept of the chair is a concept of an object; likewise the triangle, as an object, is nothing but a particular geometrical figure, a particular polygon: here the concept of triangle is a concept of an object, while the concept of polygon is a concept of a property. However, the same characterization that provides the concept of the chair can be taken to express a property, and, in such a sense, it can be specified: we could have, for example, the Louis XIV chair, or the Louis XIV chair conserved in Versailles, and so on. Analogously for triangles: we could have the isosceles triangle, the isosceles triangle associated with a certain construction, and so on.

Thus, in order to have a domain of objects, we need not individuate a particular substance or a particular content of thought that is intrinsically individual. We simply have to fix the final stage of an exigency of individuation. This is exactly what we do when we expound a mathematical theory as a theory of a certain domain of objects. Hence, that a certain concept is a concept of an object a does not mean that we cannot imagine, and even exhibit a number of different a's. If we do that, we are simply changing our exigencies of individuation, and we are passing to a theory of a strictly different domain of objects.

Since the different exigencies of individuation can often be hierarchically ordered, it is then possible to organize the respective theories, with originally strictly different domains of objects, such that they form only one general theory, the objectual domains of which is hierarchically structured. This is the case with classical Euclidean geometry. Thus, certain concepts of mathematical objects of such a general theory can be specified ulteriorly, when a new exigency of individuation is advanced. However, as these concepts are just concepts of objects, they characterize individual entities on which it is possible to operate according to fixed procedures. I think that it is just this essential character of a mathematical theory that makes it possible, in mathematics, to operate—as Kant said—on the universal *in concreto.*

Moreover, this is also the condition of possibility of analysis as a mathematical method.

Imagine that a mathematical problem asks for the individuation of one or more a's which satisfy certain conditions. If these conditions characterize one or more a's which are still unknown (they do not provide a presentation of the concepts of these objects, but only a presentation of the concept of a property or a relation that they have to satisfy), we can use a suitable notation for expressing these objects and operate on it with respect both to the fixed procedures that apply on the object a and to the conditions which the objects we are looking for have to satisfy.

A very simple case is the following. We are looking for two complex numbers the sum and the product of which are φ and ψ, respectively (where "φ" and "ψ" are names or notations of two objects given as such, two natural numbers). Thus we can express these two numbers by the symbols "x" and "y" and operate on them as if they were common complex numbers. Here, "x" and "y" are the names of two potential objects that satisfy two different, even though reciprocal, properties: for x, the property 'to produce φ and ψ, respectively, when it is added and multiplied to y' and, for y, the property 'to produce φ and ψ, respectively, when it is added and multiplied to x'. Thus the concepts of x and y work as concepts of properties here. However, as the objects that satisfy these properties are certainly complex numbers, we can treat x and y as names of specified complex numbers, operate on them according to the algebraic procedures and solve the problem by exhibiting two couples of complex numbers, let us say, c_1 and c_2 and d_1 and d_2 (which are the objects of suitable concepts of objects) which satisfy the condition of the problem.

In my terminology (Panza fc), x and y are "conditional objects", while c_1, c_2, d_1 and d_2 are "proper objects". This terminology allows us to reformulate the classical Pappus' distinction between analysis and synthesis (as mathematical methods or procedures) in the following terms: analysis consists in operating on conditional objects as if they were proper objects, in order to determinate the proper objects that satisfy a given condition; synthesis is just the act of exhibiting or determinating these objects.

XII Concluding Remarks

If I am right, my notion of mathematical objects as pure objects not only provides a reformulation of the Kantian distinction between analytic and synthetic judgments, as a distinction between analytic and synthetic acts of reasoning, according to which mathematical acts of reasoning are just synthetic, but it also provides a reformulation of Pappus's distinction between analysis and synthesis and makes these two classical distinctions not so extraneous to each other, as it has been usually argued: while both "analysis" and "analytic" refer to our activity on concepts, "synthesis" and "synthetic" refers to our activity on objects. In such a way, the connections between the general question of analysis and synthesis in mathematical knowledge and the classical controversy on Platonism (Panza and Salanskis 1995) also become clear.

It seems to me that such a result is important from a historical point of view, as well. Even though my starting point is essentially a non-Kantian one, my interpretation of Kant's distinction fits very well with some crucial aspects of Kant's interpretation of mathematics.

Firstly, the distinction between "analytic" and "synthetic" bears, in my view, neither on the logical internal form of statements nor on their relations to other statements, nor does it apply to statements as such. Rather, it refers to the logical nature of the act that a statement expresses. It seems to me that, despite the criterion presented by Kant in his *Introduction* to the second edition of the *Critique of Pure Reason*, this is also the case with Kant's distinction itself.

Secondly, in my understanding, an act of reasoning is synthetic because it is grounded on the analysis of the objects to which it is attributing properties or relations, rather than on their concepts. Even though the Kantian thesis asserting the syntheticity of mathematical judgments has frequently been defended by referring to a mentalist conception of intuition—a sort of intellectual light that should originate mathematical principles, axioms or proofs—it seems to me that for Kant a judgment (or better a statement) is synthetic if intuition occurs in its justification, as a modality or even a guarantee of the actual or possible presentation of an object. According to Kant, work on concepts is, in fact, a mark of analyticity, rather than syntheticity. It is only by going away from our concepts, and further away from our mental contents, that we can formulate a synthetic judgment.

Thirdly, my argument for the syntheticity of mathematical acts of reasoning links such a thesis to Kant's thesis, according to which "mathematics can consider the universal *in concreto*". Both this thesis and the other one, which asserts the syntheticity of mathematical knowledge, or judgments, are parts of the hard core of Kant's philosophy of mathematics. Nevertheless, Kant's interpreters frequently fail in showing the link between them. If the second of these theses is reformulated in the manner I have suggested, this link becomes evident.

Still, these three remarks do not eliminate the major differences between my conceptions and those of Kant. I think that these differences are reducible to a fundamental one that I would like to expound, as clearly as possible, at the end of my paper. According to Kant, the distinction between empirical and pure concepts is a primitive one, and it is not ulteriorly explicable. Nevertheless, Kant seems to reason as if empirical intuition could be "prolonged" in the pure one, by providing a guarantee of *a priori* knowledge as a sort of "potentially empirical" or "pre-empirical" knowledge. In such a way, pure intuition has a task to fulfill: it has to found the possibility of *a posteriori* knowledge, by guaranteeing the empirical content of *a priori* knowledge. Particularly, mathematical knowledge is for Kant about the general forms of (empirical) objects, such forms being just the forms that these objects have as such, the forms in which they present themselves to the empirical intuition. A subject, according to him, has intuition of objects only as contents that fill up general pure forms, and the possibility of prolonging empirical intuition in the pure one is nothing but a way to come back to the transcendental origins of empirical intuition itself. Thus, pure intuition often has to work as a criterion of constructibility of mathematical concepts, or of the real possibility of them (Kant A, 220-221; B, 267-268, for example), and it can realize its task only by imposing on these concepts the limits characterizing our empirical intuition. In such a way, mathematics has to respect, as such, certain conditions, or it has to be kept within certain limits, which cannot merely be the limits of thought, and cannot be found other than in the characters of subjective evidence[27].

Such a war-machine is founded on a deep and essential confusion. If objects are filling up general pure forms, it is not possible to refer to them, or their form, in order to distinguish constructible from not constructible concepts. I do not know if this confusion (which is nothing but a circularity) can be avoided when we want to realize the double program of Kant: to found the possibility of *a posteriori* knowledge on the availability of mathematical knowledge and to found mathematical knowledge as such on the conditions of the possibility of *a posteriori* knowledge. I do not know it and I am not interested in it. In my view a philosophy of knowledge has to found nothing: neither the possibility of empirical knowledge, nor mathematics as such. It only has to provide the hermeneutic tools for understanding knowledge as it is, as it has been historically realized by individuals. When I speak about subject, I do not refer, as Kant does, to a transcendental, universal or typical subject. I refer to individual subjects, just like us, I look for a characterization of our cognitive acts, I try to distinguish them among the totality of our acts according to a formal criterion, and finally I aim to describe and understand what sorts of subjective abilities are employed in our cognitive activity.

Thus the task of a philosophy of mathematics is, for me, that of providing valuable categories for characterizing and understanding mathematics, as a typical human activity, and not that of founding its legitimacy on an irrefutable guarantee—even though to understand a mathematical theory is also to come back to its origins and to make clear (and eventually discuss) its reasons. Here, I have suggested that mathematics is both the activity of constituting pure objects on which synthetic acts of reasoning are possible and to realize these acts. Even though it is not only a formal deductive game, it is both the activity of constructing pure objects, according to a certain aim—so that a (quasi-) formal game could be applied to them, for discovering their properties or relations—and the activity of applying this game and realizing this task.

Centre F. Viète of the History and Philosophy of Sciences,
University of Nantes

Notes

* I thank Andreas Etges, Agnese Grieco, Claudio La Rocca, Jean Petitot, Jean-Michel Salanskis, and specially Michael Otte for their linguistic and philosophical suggestions and commentaries on a number of previous versions of my paper.

1 In order to avoid any possible misunderstanding due to the English translation, I add the German original term in brackets after the first occurrence of every English term translating a Kantian key term. If no particular reference is made when the expression is used again, it always refers to the same original German term. A glossary is given at the end of the paper. If not stated otherwise, English translations of Kant's statements are quoted from (CS) and (LY).

2 For a recent version of the logicist program, cf. Wright (1983).

3 For reasons of uniformity with the text of the first *Critique*, I here change the Young's translation and translate "*Wirklichkeit*" with "reality" instead of "actuality".

4 Young translates here "to decide" for "*ausmachen*", but this seems to be a misleading translation, since a decision is a choice between different possibilities that are already given as such, which is strange to the meaning of the German term "*ausmachen*".

5 Once again I changed Young's translation, translating "*kommen*" and "*zukommen*" with "to suit"—instead of with "to belong" as Young does—in order to avoid any possible confusion between the relation of object and concept—which is the case here—and the relation between two concepts, which Kant indicates with the verb "*gehören*", above translated with "to belong".

6 Here is an other excerpt both from the first and the second edition, that is not less clear (cf. *ibid.* A, 154-155; B, 193-194):

> "In the analytic judgment we keep to the given concept, and seek to extract something from it. [...] But in synthetic judgments I have to advance beyond the given concept, viewing as in relation with the concept something altogether different from what was thought in it. This relation is consequently never a relation either of identity or of contradiction; and from the judgment, taken in and by itself, the truth or falsity of the relation can never be discovered."

7 I changed the order of propositions of Smith's translation, in order to stay closer to the original.

8 Following Smith's translation, I translate both the German "*Perzeption*" (which very rarely appears in Kant's texts) and "*Wahrnehmung*" with "perception", by considering these terms equivalent in meaning.

9 According to Eisler's classical *Kant Lexicon* [cf. Eisler [1930], p. 391], there is no appreciable difference between Kant's use of the terms "*Gegenstand*" and "*Objekt*" in the first *Critique*, even if these terms, taking together, cover a large spectrum of different meanings. Here I consider only one of these meanings (actually covered in the first *Critique* by both German terms), namely the one according to which an object is that of which elementary knowledge is just knowledge. What is important to me is not that the same terms could or could not have other meaning in Kant's works (certainly they do), but that for Kant there is no room for something like a pure object (either *Gegenstand* or *Objekt*), intended as that of which *a priori* knowledge could just be knowledge.

10 I prefer the term "singular" to Smith's term "single".

11 The passage, at the very beginning of the *Aesthetics*, is so well known that it is not necessary to quote it here.

12 Cf on this point Panza (1995*a*), where I have tried to justify such a thesis.

13 For Kant's interpretation of postulates as clauses for constructing objects , cf. Kant [JL], § 32 and 38:

> "[...] *practical* propositions [...] are those that state the action whereby, as its necessary condition, an object becomes possible."
> "A *postulate* is a practical, immediately certain proposition, or a principle that determines a possible action, in the case of which it is presupposed that the way of executing it is immediately certain."

14 A. Ferrarin (1995, 137) writes:

> "[According to Kant] the synthesis [...] involves the necessity to go beyond the concept and show its pure a priori determination of spatio-temporal intuition: the guidance for the construction of the object. And a synthetic judgment is not a formal, discursive relation between the subject and its predicate, but the activity of exhibiting in intuition the real belonging of a property of its object."

I completely agree with that.

15 Notice that, if I am correct, a mathematical judgment is for Kant a judgment about (the concepts of) pure forms, but it is justified by means of procedures referred to empirical entities. Thus, truth, or even necessity, of mathematical judgments must as such be independent of their justification or proof. Of course, we can insist on the possibility of imagining the objects occurring in such a justification, but this does not change the situation, since imagination has just to be imagination of objects.

16 On the role of imagination and its relations with understanding in Kant's theory of knowledge cf. Palumbo (1984).

17 It might appear paradoxical that a synthetical act of reasoning depends on an analysis, but I believe it is not. Cf. on this point the concluding chapter of the present book (particularly § VI).

18 In my view a reasoning is a specific and particular event that cannot be repeated as such, even if it can take a general form that logic can study (that is the "inference" in Kant's sense).

19 Analogous considerations could be advanced for universal statements of a conditional form where relational predicates occur.

20 Here Kant uses the verb "*liegen*" which I translate literally as "to lie on"—instead of "to contain", as Smith does—in order to mark the difference with the verb "*enthalten*", just translated "to contain".

21 Smith simply translates "*ausführlichen*" as "complete". Instead I follow the suggestion of Giorgio Colli who in his Italian translation [Einaudi, Torino, 1957] just translates it as "*dettagliatamente completo*". Moreover, he translates "*ursprünglich*" as an adjective referring to "concept", while it works in the German text as an adverb referring to "*Grenzen*". This is the reason for a second change of Smith's translation.

22 Cf. the previous note (21).

23 Smith translates "arbitrarily invented".

24 Cf. the previous footnote (10).

25 Not any notation is for Peirce an icon:

> "I call a sign—he writes (1886, 163)—which stands for something merely because it resembles it, an icon. Icons are so completely substituted for their objects as hardly to be distinguished from them. Such are the diagrams of geometry."

In my (1985*a*), I called this sort of notations "transparent" and I considered their role in classical Euclidean geometry (cf. *ibid.*, § 5, pp. 78-84); I will come back later to some of the arguments I have presented there. In the paragraph X, I will argue that this sort of notation essentially occurs also in classical or constructive arithmetic. Moreover, Peirce (*ibid.*, 165) believes that icons play an essential role in algebraic (or formal) deduction too. I will not discuss this last point here, even though I think that Peirce's argument is far from being completely wrong. On the role of diagrammatic thinking, as founded on iconic notations, in mathematics, according to Peirce, cf. the paper by M. Otte in the present volume.

26 A similar idea has been advanced by M. Otte, in his discussion of the Locke-Berkeley-Kant controversy on the "general triangle" (Otte 1994*b*, 276-284 and 1995, 102). It is evident that a pure object is indeterminate with respect to a very large range of properties. This is the case both with the triangle and with Madame Bovary. To the question "how tall is Madame Bovary?", we have no answer. The reason is not that we do not know how tall Madame Bovary is. The reason is that, for every property like "to be x tall" Madame Bovary does not have such a property without having its negation. Simply, Madame Bovary is a woman, but she does not have a tallness, even if this does not mean that Madame Bovary is a "universal woman". On the consequences of such a character of a pure, and particularly a mathematical object, cf. my (1995*b*), § 6., pp. 122-128.

27 It seems to me that such a question is connected with the one advanced by Parrini, by referring to Herbart, concerning the conditions of possibility of "determinate knowledge" (1990, 60-62).

Glossary

Analytic: *analytisch.*
Apodeictic: *apodiktisch.*
Assertoric: *assertorisch.*
Belong (to): *gehören.*
Clarity: *Klarheit.*
Concept: *Begriff.*
Consciousness: *Bewußtsein.*
Construct (to): *konstruieren.*
Contain (to): *enthalten.*
Content: *Inhalt.*
Construction: *Konstruktion.*
Demonstration: *Demonstration.*
Denied: *verneint.*
Detailedly complete: *ausführlich.*
Empirical: *empirisch.*
Exhibit (to): *vorstellen.*
Experience: *Erfahrung.*
Grounded: *gründet.*
Identity: *Identität.*
Inferences: *Schlüsse.*
Intuition: *Anschauung.*
Judge (to): *urteilen.*
Judgments: *Urteile.*
Knowledge: *Erkennntnis.*
Limit: *Grenze.*
Maintained: *behauptet.*
Make out (to): *ausmachen.*
Mathematical: *mathematisch.*
Modality: *Modalität.*
Object: *Gegenstand* or *Objekt.*
Objective perception: *objektive Perzeption.*
Original: *ursprünglich.*
Perception: *Perzeption* or *Wahrnehmung.*
Precision: *Präzision.*
Predicate: *Prädikat.*
Problematic: *problematisch.*
Propositions: *Sätze.*
Pure: *rein.*
Real: *wirklich.*
Reality: *Wirklichkeit.*
Representation: *Vorstellung.*
Sensation: *Empfindung.*
Singular: *einzeln.*
Subject: *Subjekt.*
Subjective constitution: *subjektive Beschaffenheit.*
Sufficiency of characteristics: *Zulänglichkeit der Merkmale.*
Suit (to): *kommen, zukommen.*
Synthetic: *synthetisch.*
Thing: *Ding.*
Thought: *Denken.*
True: *wahr.*
Truth: *Wahrheit.*
Understanding: *Verstand.*
Universal in concreto: *Allgemeines in concreto.*

MICHAEL OTTE

ANALYSIS AND SYNTHESIS IN MATHEMATICS FROM THE PERSPECTIVE OF CHARLES S. PEIRCE'S PHILOSOPHY*

I Introduction

This paper is particularly concerned with Peirce's conception of mathematics. Taking into account that there exists a great deal of scholarly insight into his philosophy of science, one is surprised to notice how indefinite, uneven and varied opinions are regarding Peirce's conception of mathematics. Peirce has declared mathematics to be paradigmatic for philosophy (CP, 7.80) which leads us to investigate the relationship of Peirce's epistemology to classical German philosophy, to the conceptions of Leibniz, Hegel and above all of Kant. Kantian thought is not only crucial for Peirce's early period but is indispensable to any understanding of Peirce's philosophy and his conception of mathematics. It is true that certain beliefs, common to Kant and Peirce take on a different importance and meaning when passing from one to the other. Intuition, for instance, was an important term for both. Peirce called "intuition" "the one sole method of valuable thought" (*ibid.*, 1.383). Another common idea refers to the linkage between generality and continuity. Continuity, writes Peirce, for instance, is "nothing but a higher type of that which we know as generality. It is relational generality" (*ibid.*, 6.190; with respect to Kant cf., for instance, B 206). As, however, the architecture of Kant's *Kritik* rests exclusively on the idea of the *a priori*, and as Peirce, on the other hand, does not share Kantian aprioricism, these, as well as various other ideas, change in meaning and acquire new roles. For instance, time is such, says Kant, "that every part of it has similar parts,—a proposition very different from merely saying that Time is infinitely divisible, though Kant himself did not perceive the distinction" (*ibid.*, 8.114). However, because of his aprioricism, Kant had no need for that distinction. For Peirce, on the contrary, it was of crucial importance. Continuity, or similarity of parts, making time (as well as space) an individual whole, was absolutely essential, because continuity in this manner serves to introduce a new type of metaphysics, the universe being conceived of as a system of interrelated systems rather than as a set of isolated things.

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 327–362.

> "Nominalism—or at least, modern Nominalisms—is precisely the doctrine that the Universe is a heap of sand whose grains have nothing to do with one another, and to recognize concatenation is to recognize that there is something that is not Individual and has another mode of Real Being than that of an Individual Existent." (Ms 641)

The *a priori* is nothing but that which is universally valid and "whatever is universally true is involved in the conditions of experience" (CP, 2.690). In contrast to his predecessors, Kant considered these general conditions to be subjective rather than objective. "It was the essence of his philosophy to regard [...] the reality as the normal product of mental action, and not as the incognizable cause of it" (*ibid.*, 8.15). Peirce now claims that his new philosophy of synechism (synechism is a regulative principle of logic based on the idea of continuity) allows these general conditions or the *a priori* to be understood as being both subjective and objective by relating them to an evolutionary process which is at the same time constrained and yet not absolutely determined. The resulting relativity of the distinction between the subjective and the objective gives the principle of continuity its prominent place in methodology, because in order to reconcile relativity and objectivity of knowledge, one has to accept that the distinction between the assertion that $A = B$ and its negation cannot be absolute, since "absolute discontinuity cannot be proved to be real" (*ibid.*, 8, 278). "When Synechism has united the two worlds of the subjective and the objective; the belief in the relativity of the subjective and the objective gains new life" (*ibid.*, 6.590).

Kathleen Hull (Hull 1994) claims that Peirce closely follows Kant in his understanding of mathematics and of mathematical reasoning. It therefore seems justified to approach the matter historically and to begin with Kant, with a consideration of Kant's conception of mathematics. As is well-known, Kant characterizes mathematics in terms of the analytic-synthetic distinction, claiming that mathematical propositions, or the judgments represented by them, are synthetic *a priori*. As Peirce abandoned apriorism, his specific answer to the continuum problem and his conception of an evolutionary realism derived from that answer, made his views essentially different from Kant's, rendering the analytic-synthetic distinction somewhat relative, because the distinction between the subjective and the objective became a relative one (with respect to the connection between these two distinctions see part V of this paper, and, *ex negativo*, also Grice and Strawson 1971). Thus, I agree with Kathleen Hull, this being my first premise, that "mathematics, not logic, is the cornerstone of Peirce's architectonic" (Hull 1994, 273). And this is so exactly because of the importance of generality as based on the idea of continuity to which the law of contradiction cannot be applied, and Peirce, as well as Hilbert, therefore had to look for a different logic of reasoning.

My second premise then is that we should take Peirce's views with respect to mathematics and to science in general as being based on one and the same philosophical conception. This implies for instance that mathematical axioms and natural laws are to be classified as ontologically of the same nature, they both employ free variables and are generals in the sense of Peirce's philosophical realism as based on the reality of the continuum. Knowledge of any sort is formal and to take on meaning it has to be applied. As it cannot be itself a theory of its own application, we enter into an infinite regress of meta-levels. Aprioricism (any kind of foundationalism, in fact), on the one side and philosophies of practice (like pragmatism) on the other side meet the challenge of infinite regress differently. With respect to Peirce, one notices that ideas like evolution and continuity become important as substitutes for the idea of foundationalism. In evolution the infinite regress is interrupted as certain possibilities are in fact realized and others not and the continuum represents all that is possible.

This brings me to the third premise of my argument. We should take seriously Peirce's approach to the question of philosophical realism when trying to understand his views regarding mathematics. In contrast to this requirement, investigations of Peirce's realist approach to mathematics sometimes start by asking "can one be a realist without being a platonist" (Engel-Tiercelin 1993), while for Peirce realism and platonism really had nothing in common. In exploring the connections between "Peirce and logicism", Susan Haack places everything into a Fregean framework from the very beginning by asking whether Peirce would ascribe to two theses she presents characterizing logicism in the sense of Frege. Then the impression is conveyed that Peirce was not really consistent in his views, as he seems to accept one of the theses but not the other. He held, it is claimed, that mathematics is reducible to logic and yet staunchly denied another logicist thesis, that the epistemic foundations of mathematics lie in logic, whereas "Frege took it for granted that both theses stand or fall together" (Haack 1993, 36). At this point I do not want to discuss the content of these claims (see however Houser 1993), but would rather oppose the justification of the approach as such. Peirce starts by observing how mathematicians really practice their business and how they accomplish their results, rather than, like Frege, with an idea of how they should perform their activities.

Finally in his excellent and influential book on the development of Peirce's philosophy, M. G. Murphey claims that "the creative or dynamic agent" in this development is Peirce's logic (Murphey 1961, 3), as well as that in spirit "Peirce has more in common with the logicist school than with intuitionism" (*ibid.*, 288). It is correct that Peirce did not accept that "mathematics limits itself to the range of objects it can construct" (*ibid.*). But taking into account the Kantian roots of his philosophy, it is equally correct to say, that he would never believe that the construction and the presentation of mathematical objects could be completely separated. Just this particular problem, how to conceive of the link between the development and substantiation of mathematical knowledge, might already suggest that a framework different from the traditional philosophies of mathematics, formalism, platonism, intuitionism or logicism has to be found in order to understand what Peirce had in mind.

II Analysis and Synthesis from Leibniz to Kant

With respect to the analytic-synthetic distinction Kant states:

> "In all judgments wherein the relation of the subject to the predicate is thought this relation is possible in two different ways. Either the predicate B belongs to the subject A, as something which is contained (covertly) in this concept A; or B lies completely outside of the concept A, although it stands in connection with it. In the first instance, I term the judgment analytical, in the second synthetical." (B, 10)

Synthesis is the opposite of analysis. Now, during the age of classical rationalism the term "analysis" is used in two applications:

1. Empirical theories are analytical, as far as they claim to speak about the essence of reality as such, as far as they seek to find out what is the core of a thing. This is because analysis proceeds from a given unknown which it seeks to investigate. Substances or essences are real and are the real subjects of predication. Kant's reformulation of the analyticity of judgments stays in line with this as long as one assumes that conceptualization captures the essence of some real being or some existing substance. But how is this to be guaranteed? By the structure of the epistemic subject, says Kant. "It was the essence of his philosophy to regard the real object as determined by the mind" (Peirce CP, 8.15).

2. Logical theories are analytical, as far as they deal with the way something which has been said can be said in another way as well, and this is how the law of contradiction, that is the claim that something cannot be simultaneously named p and non-p, obtains its significance. It becomes the basis for the analyticity of formal theories. Algebra, and mediated by it also geometry, are called "analytic" as soon as the unknown variable "x" is introduced into their activities. Equations, taken as $S = P$ expressions, represent not only a method but rather a way of securing true knowledge.

To classical ontologism all true propositions had been analytically true. Classical thought had as its ultimate goal, which was in general only to be accomplished by God because it required an infinite analysis, the determination of individual substances. This view is an outgrowth of the static world view of the classical age but also a result of its optimism that the world is knowable. And the existence of God is a basis of this optimism. In this manner the law of contradiction used formally serves to give proofs of existence. This later became a fundamental idea of Cantorian pure mathematics. Kant does not accept that a non-contradictory is also real (Kant B, 629).

Leibniz has created our modern concept of mathematical proof by understanding that a proof is valid by virtue of its form, not by virtue of its content. This does not imply, however, contrary to a claim made by Russell (Russell 1903*b*, 178) that Leibniz's philosophy rests solely on his logic, because Leibniz assumed a one-one correspondence between concepts and objects. Symbols represent thoughts and collections of thoughts determine or represent objects. As in his view all things in this world are constituted by the concepts corresponding to them in God's mind, proof amounts to an infinite analysis of the respective concept, and all cognition becomes analytical cognition. "Leibniz making proof a matter of ontology not methodology, asserts that all true propositions have an *a priori* proof, although in general human beings cannot make those proofs", because of the infinity of the analysis required. (Hacking 1984, 221). Thus it is due to our limitations that some truths appear to be contingent and not necessary.

Everybody knows analytically that Hamlet's mother cannot have been a man, but nobody can know *a priori* and analytically what was the color of her eyes. Leibniz would consider this due to the fact that we, the human beings, unlike God, do not have the complete concept of "Hamlet's mother" at our disposal. We do not know all the details of her existence, nor the complete story of her life. In mathematics we do, because mathematical concepts are simpler, and thus mathematical truth is based on proof and mathematics is analytical (Hacking 1984). In mathematics the intensions of concepts are just definitions and mathematical concepts can be analyzed. It is therefore easy to see whether a proposition is analytic or synthetic, because we stay completely inside a language system as soon as we reason in mathematics. This does not apply to Kant's views.

The law of contradiction may, according to the above distinction, be interpreted in various ways. Let us consider the example "gold is a yellow metal". According to Kant the law of contradiction comes in because, according to the usual definition of terms, it would be a self-contradiction to say: "gold is not a yellow metal or bodies are not extended". Kant believed that to any substance some predicates inherently and essentially belong while others depend on experience, but the distinctions he draws in some cases of empirical concepts are rather arbitrary.

Now Kant takes great pains to distinguish analytic and synthetic propositions, because his view of the analytic-synthetic distinction depends on the invalidation of the ontological proof of God's existence and represents his own Copernican step. Classical thought rested in the idea of God. The proof of the existence of God warrants Leibniz' foundation of truth on proof as well as the Cartesian *cogito ergo sum*, this final truth which constitutes the foundations of the entire structure of Cartesian rationality. Accordingly a schism was caused in the heritage of the classical age, hence also in the foundations of modern science, by the invalidation of the proofs of God's existence, for God guaranteed a strict correspondence between clear and distinct thought on the one hand and external reality on the other.

Kant claims that "God exists", can never be analytic, as Leibniz believes, because "being is not a real predicate; that is, it is not a concept of something which could be added to the concept of a thing" (Kant B, 626). Thus the proposition "God exists" is not real knowledge. Kant realized "that no general description of existence is possible, which is perhaps the most valuable proposition that the Critic contains" (Peirce CP, 1.35). Kant changes orientation from substances and essential properties to concepts and objects, or functions and arguments as we have already seen when observing his definition of analytic and synthetic. God exists means that the extension of the concept God is not empty. God exists and God, on such a presupposition signify the extension and the intension of one and the same concept, or the factuality and the possibility of the being of God respectively (*ibid.*, 4.583). Extension and intension of concepts appear to be relatively independent of one another and the transition from the possible to the factual cannot be accomplished by means of logic or language and pure thinking. All judgments are conditional. The proposition that a triangle necessarily has three angles does not say "that three angles are absolutely necessary, but that, under the condition that there is a triangle, three angles will necessarily be found in it" (Kant B, 622). Kant, contrary to Cantor or Leibniz, did not consider consistency sufficient for existence even in mathematics.

And Kant shared with Leibniz the foundational concern expressed by aprioricism. Kant's transcendental subject, which takes the role of Leibniz's God, at first sight, shares the problematic nature of the latter, such that not much seems to be gained by this kind of reorientation. On the one hand all knowledge is based on the structure of the transcendental subject. But on the other hand, the transcendental subject is not directly accessible to the individual subject because the proposition "I think", which according to Kant is the supreme foundation of all knowledge (*ibid.*, 132), in itself does not express any knowledge. The proposition "I think" does not imply my existence, although it contains the proposition "I exist". Thus the transcendental subject is in a sense declared to be a thing in itself, "a kind of otherworldly entity" (Lektorsky 1980, 84-86). It guarantees however the constitution of the subject and object of knowledge by means of the process of Synthesis.

"A mind, in which all the manifold should also be given by self-consciousness would be intuitive; our mind can only think, and must look for its intuition to sense" (Kant B, 135). Thus the transcendental subject could become known only after having been externalized by constructive activity, and mathematics plays a special role in this process, as by means of mathematical reasoning we become aware of the forms of all intuition, which mathematics presents as original intuitions themselves. Mathematics therefore presents the general conditions of all knowledge *in concreto* (see, for instance Panza's paper, in this volume).

> "The constructivist project, rooted in Descartes' geometry and exfoliated in Kant's critical enterprise, took its bearings from the desire to master and possess nature, where nature was understood as the locus of apparently ineliminable or intractable otherness. Mind could aspire to master its other [...] by externalizing itself in a construction carrying the clear marks of its inward and deliberate origin." (Lachterman 1989, 23)

However, the idea of the human self producing itself, as well as knowledge as part of it, represents an essential step, because after taking it, the growth and the justification of knowledge become interrelated and intertwined. To understand the reality of knowledge one has to understand the reality of understanding. And in order to accomplish this one has to find a point where understanding is construction, conceived of as a unity of process and result. This can already be guessed from the very special role mathematics seems to play in Kantian epistemology. Mathematics gives the best example of knowledge as active creation. "Approximation to the ideal of a thoroughly free divine or archetypical intellect yields at one and the same time the basic sense of our active existence and the limits or mitigations to which this active existence is inevitably subject" (*ibid.*, 11). Kant, in fact, warns philosophers against trying to imitate mathematical procedures and methods, because in philosophy this unfolding identity of concept and object does not exist.

This warning is based on a very problematic distinction within the area of synthetic judgments *a priori*, classifying them into intuitive ones and discursive ones. The latter refer primarily to the ordering function of general concepts whereas the former are related to the structure of perception. The judgments of pure mathematics belong to the class of intuitive judgments. Kant himself describes the intended distinction as follows:

> "An *a priori* conception contains either a pure intuition, and in this case it can be constructed; or it contains nothing but the synthesis of possible intuitions which are not given *a priori*. In this latter case the concept may help us to form synthetical a priori judgments, but only discursively by means of concepts and never intuitively, by means of the construction of concepts." (Kant B, 749)

And he generally classifies axioms as intuitive principles, adding that philosophy does not possess any axioms and "has no right to impose her a priori principles upon thought, until it has established their authority and validity by a thorough deduction" (*ibid.*, 762; we should take into account at this point that deduction means legitimation rather than logical or mathematical deduction). Thus Kant introduces a separation between intuitive and discursive knowledge, which seems to exclude mathematics from conceptual thinking. Mathematics "does not only construct magnitudes, as in geometry; it also constructs magnitude as such, as in algebra" (*ibid.*, 745). Synthetic *a priori* knowledge in the sense of Kant is most importantly characterized by its constructivity.

Kant believes that mathematics rests on concepts that are given by definitions and that mathematical cognition originates from the construction of the concepts.

> "To construct a concept, however, means: to present the intuition corresponding to it *a priori* [...]. Thus, I construct a triangle by the presentation of the object corresponding to this concept either by mere imagination in pure intuition, or after the latter also on paper, in empirical intuition, in both cases completely *a priori* [...]." (*ibid.*, 741-742)

This distinction depends on the fact that in the world of phenomena "there are two elements—the form of intuition (space and time) [...] and the matter or content, that which is presented in space and time, [...]" (*ibid.*, 751). We are able to construct mathematical concepts *a priori* "in as much as we are ourselves the creators of the objects of the concepts in space and time" (*ibid.*, 752). Mathematical concepts were constituted by definitions (*ibid.*, 756) and had to be reified or applied in intuition according to space and time as the forms of pure intuition. In as much as mathematical reasoning operates on these reifications it is synthetical, otherwise analytical. But no mathematical truth can be acquired by analytical reasoning only, because we have to apply a concept to gain knowledge. We cannot cogitate a straight line without drawing it, Kant says, (*ibid.*, 154). The line drawn is empirical and is therefore no mathematical object, but it is the construction of a mathematical concept. To know means to observe one's own constructive activity and its results.

Kant held a much narrower view with respect to the subject matter of mathematics than say Leibniz, as his contemporaries already noticed. Why is "the form of mathematical knowledge the cause that it is limited exclusively to quantities" (*ibid.*, 743)? Because the construction refers to either geometrical or algebraic algorithms or functions taken in intension, construction of the pair $\{x,f(x)\}$. Thus a judgment is to be presented by a pair or a relation $\{x,f(x)\}$. It is sometimes said that Kant gained his vision of mathematical cognition from the problems of analytical geometry. And his concepts indeed depend on functions insofar as Kant defines functions as the "unity of the act of arranging various representations (*Vorstellungen*) under one common representation" (*ibid.*, 93). The pair or relation $\{x,f(x)\}$ represents a reified function or a judgment. All judgments are functions. Kant took the idea of function from algebraic analysis in the sense of Euler and Lagrange, identifying function with algorithm or formula (Cassirer 1910).

A completed or reified function may be understood as a representation of a representation of an object. This Kant also calls "mediate knowledge of an object". Lachterman claims that Kant took his understanding of the technique of construction from algebra and not from geometry. "Kant's phrase 'construction of a concept' is derived from the expression 'construction of an algebraic equation'". This latter expression refers to "the interpretation of the terms of the equation in ways that lead to the actual exhibition of a particular geometric formation satisfying the general equation" (Lachterman 1989, 11). The construction was meant to yield line segments that corresponded to the roots of the equation together with its application in a particular case. This technique does nothing but exhibit the algorithm by which we arrive at the roots of the equation. The technique gradually disappears around the middle of the eighteenth century. But between the publication of Descartes' *Geometry* and about 1770 it was considered crucial within algebra and analytic geometry and was developed by first-ranking mathematicians (Bos 1984).

Euclidean geometry itself is thoroughly algorithmic. Euclid had founded a geometry that allowed constructions by straight lines and circles. "Descartes had extended this geometry by allowing in principle all algebraic curves as means of construction" (Bos 1984, 360). Newton wanted algebra to be more subservient to geometry and wished "to work out a truly geometrical approach to the construction of problems and equations. Geometrical simplicity, namely the simplicity of tracing, should be the criterion, not algebraic simplicity" (*ibid.*, 362-363). In 1835-1844 a similar motivation led Grassmann, to introduce the direct methods of vector algebra into the geometrical sciences in order to mathematize projective geometry. The point of reference of the construction should be immanent rather then external, was the demand. Grassmann, like Leibniz or Poncelet, wanted to operate on geometrical entities rather than on functions (coordinates) in order to realize a synthesis brought about by the intrinsic properties of space itself. The following statement from Peirce's Cambridge Conference of March 1898 sounds very much like Grassmann indeed:

> "That which already had been called the Elements of geometry long before the day of Euclid is a collection of convenient propositions concerning relations between the lengths of line, the area of surfaces, the volumes of solids and the measures of angles. It concerns itself only incidentally with the intrinsic properties of space." (Peirce CCL, 242-243)

But it is projective geometry or topology (geometrical topic as Peirce called it) "what the philosopher must study who seeks to learn anything about continuity from geometry" (*ibid.*, 246). And continuity is essential to understand synthesis as soon as Aprioricism has been abandoned, as has already been mentioned in the introduction above.

The Greeks, Peirce believes, were acquainted with projective geometry and had already perceived "that it was more fundamental,—more intimately concerned with the intrinsic nature of space,—than metric is" (*ibid.*, 244). Principles of continuity are indispensable when reasoning about infinity, as in calculus or the theory of irrationals, for instance. By means of the notion of similarity or self-similarity Greek diagrams demonstrate the irrationality of the measure of the ratio of the side to the diagonal of certain regular polygons, like, for example, the regular pentagon. One may in fact consider these diagrams in different ways.

Concentrating on the exhibition of self-similarity one might obtain as a result, on the basis of the visual representation, insight into the recursive structure of the Euclidean algorithm for determining a common unit or the greatest common divisor. The true application of the notion of similarity requires us to disregard scales, that means accepting geometric space as a continuum and as an individual whole, in the sense of Peirce. The invariance under geometric similarity then directly demonstrates that the algorithm does not lead to the desired goal, the algorithm itself having been transformed into an object of thought. Side and diagonal are thus incommensurable, *i.e.* $\sqrt{5}$ is an irrational number. Note that I have not proposed to replace the geometrical quantities involved by their numerical measures with respect to a certain unit and therefore I have not obtained the result by an indirect method, based on the law of contradiction, but have "seen" it directly because of the recursive structure of the algorithm.

It is by geometric construction that we notice the concept of say $\sqrt{5}$ not being empty, but such a root is not a number, says Kant, "but only the rule of approximating it" (letter by Kant to Rehberg, quoted after Parsons 1983, 111). But the law of approximating it or the rule can replace the series of values in that approximation, it is the intensional side of this concept of $\sqrt{5}$. Besides Kant, such a view was also held by intuitionists like Kronecker. Kronecker argued that if you have a rule which effectively determines every term of an infinite sequence, then the law itself can replace the sequence. It is obvious that one can only represent the class of computable numbers (in the sense of Church or Turing) in this manner, and therefore the idea of infinity involved here means the countable infinite only.

To the challenge, put forward by Rehberg, that the application of arithmetical truth to sensible items may well be subject to the conditions of time, but not arithmetic as such, Kant replies by letter:

> "As soon as instead of x, the number of which it is the sign $\sqrt{5}$ is given, in order not merely to designate its root as in algebra, but to find it, as in arithmetic, the condition of all generation of numbers, namely time, is unavoidably presupposed." (*ibid.*, 117)

Again we might conclude from this that the procedure or rather its concept, the concept of a particular algorithm is to be exhibited in space or time as the forms of pure intuition. Once again we may observe that it is mathematical constructivity, that means the exhibition of the sense of mathematical concepts which Kant had in mind, when terming mathematics synthetic *a priori* knowledge. Existence is made equal to exhibition in space and time. We construct problems which did not exist prior to our definition of them. And by constructing them and presenting their properties, we construct the construction itself, exhibiting it in the forms of pure intuition.

III Kant and Forster

Peirce agrees with Kant in that it is the idea of the (epistemic) subject on which any conception of knowing is founded. Peirce's concern, however, is not with the unity of ideas (*Vorstellungen*) in a self-consciousness, but rather with the socially effective unity represented by signs, like works of art or of science. "Consciousness is used to denote the I think, the unity of thought; but the unity of thought is nothing but the unity of symbolization", Peirce says (CP 7.585).

Kant assumes that all our knowledge extending cognitions are synthetical. For him, however, this synthesis does not lie in the matter of experience as such, but springs from the function of cognizant consciousness itself which this way becomes aware of itself. The synthetic unity of consciousness, according to Kant, is "an objective condition of all knowledge. [...] For in the absence of this synthesis, the manifold would not be united in one consciousness" (Kant B, 138). Peirce now stresses that this very unity is based on the reality of the continuum. The continuum being that on which the unity of symbolization is based. This unity is not just an *ex post* fact. Representations or interpretations are not arbitrary or just contingent.

> "Thus, the question of nominalism and realism has taken this shape: Are any continua real? Now Kant, like the faithful nominalist [...], says: 'no'. The continuity of Time and Space are merely subjective. There is nothing of the sort in the real thing in itself." (Peirce Ms, 439 and NEM, IV, 343)

That Kant had given epistemology too much of a "subjectivist" turn emerges therefore, first of all, in his conception of the (epistemic) subject, which he conceives primarily in terms of activity, or according to Peircean terminology, as Secondness.

> "Secondness is that in each of two absolutely severed and remote subjects, which pairs it with the other not for my mind nor for, or by, any mediating subject or circumstance whatsoever, but in those two subjects alone. [...] But this pairedness [...] is not mediated or brought about; and consequently it is not of a comprehensible nature, but is absolutely blind. [...] In their essence the two subjects are not paired." (Peirce CCL, 147-148)

Kant had learnt from Hume that relations are "external", that they represent nothing of the essence of the relata, that they are arbitrary. What in the nature of Paul should cause his being taller than Peter? All subjects are isolated like Leibnizian monads. Continuity we find, according to Kant, only in the realm of phenomena as they are synthezised by activity.

Peirce, in contrast, repeatedly emphasized (for instance in his various criticisms of William James, who held views of the continuum similar to Kant's) that action is not the ultimate aim and end of humans (CP, 2.151; 2.763; 5.3; 5.429; 8.115; 8.212). The highest kind of synthesis according to Peirce is represented by

Thirdness. Thirdness replaces Kant's so-called "highest point", that is, synthetic unity of consciousness. Thirdness is what makes representation real. Under the perspective of Thirdness the human subject is to be characterized primarily by its capacity to grow, or to learn and evolve.

Hegel had already put forward a similar criticism of Kantian dualism (cf. Hegel, "Glaube und Wissen", W, I, 1-154). But Hegel neglected the importance of Secondness altogether. Hegel regards the Third as the only true one Category. For in the Hegelian system the other two are only introduced in order to be *aufgehoben*" (Peirce CP, 5.79). Hegel,

> "seeing that the *Begriff* in a sense implies Secondness and Firstness, failed to see that nevertheless they are elements of the phenomenon not to be *aufgehoben*, but as real and able to stand their ground as the *Begriff* itself. The third element of the phenomenon is that we perceive it to be intelligible, that is, to be subject to law, or capable of being represented by a general sign or Symbol." (*ibid.*, 8.268)

Peirce's own position is reflected very clearly in some passages taken from a manuscript written in 1890 under the title *A Guess at the Riddle*:

> "The highest kind of synthesis is what the mind is compelled to make neither by the inward attractions of the feelings or representations themselves, nor by a transcendental force of necessity, but in the interest of intelligibility, that is, in the interest of the synthesizing 'I think' itself; and this it does by introducing an idea not contained in the data, which gives connections which they would not otherwise have had. [...] Kant gives the erroneous view that ideas are presented separated and then thought together by the mind. This is his doctrine that a mental synthesis precedes every analysis. What really happens is that something is presented which in itself has no parts, but which nevertheless is analyzed by the mind, that is to say, its having parts consists in this, that the mind afterward recognizes those parts in it. Those partial ideas are really not in the first idea, in itself, though they are separated out from it. It is a case of destructive distillation. When, having thus separated them, we think over them, we are carried in spite of ourselves from one thought to another, and therein lies the first real synthesis. An earlier synthesis than that is a fiction." (*ibid.*, 1.383-384; this resembles closely Marx's characterization of the dialectical method)

The problematic nature of Kant's conception of the subject, and of his entire epistemology, is nicely reflected in a controversy between Kant and Forster, which took place in 1785. As a boy joining his father, Georg Forster (1754-1794), Alexander von Humboldt's teacher, accompanied James Cook on the latter's second sailing around the world. This voyage took almost three years, and Forster became famous in Europe, still a young man, for his report of it. In an article entitled "Noch etwas über die Menschenrassen [Some Additional Remarks on Human Races]", in which he opposed Kant's considerations concerning "Die Bestimmung des Begriffs einer Menschenrasse und mutmaßlicher Anfang der Menschengeschichte [Determining the Concept of a Human Race and presumptive Beginnings of Human History]", Forster wrote:

> "A large part of the merit Linné earned in botany was incontestably in the precise definitions. [...] After certain assumptions which he abstracted from his own experience, he designed his structure and fitted the creatures of Nature into it. As long as our insight remains limited, however, we would seem far from an infallibility of principles. Will categorizations which are based on limited experience, while possibly useful within these limits, not appear one-sided and half-true once the horizon is expanded, the point of view displaced? [...] Perhaps our present scheme of the sciences will become obsolete and deficient half a century from now, just like the previous ones. Even speculative philosophy would seem to be prone to this fate. Who does not immediately think of the *Critique of Pure Reason* in this context? Even if the theorem that one can only find in experience what one needs if one knows beforehand what to look for, were undisputedly correct [as Kant had written in the *Berliner Monatsschrift* of November 1785 (Kant SA, VII, 107)], a certain care would nevertheless be in order when applying this theorem, to avoid the most common of illusions, namely that in looking for what one needs, one presumes to have found the same even in places where it is really not present." (Forster W, I, 5-6)

Forster's point here is that there can be no transcendental and absolute insight. Otherwise, the new and unexpected would be nothing but a passive case of application of the preestablished categorical frame and the established prejudices. The new would be reduced to things already well familiar, and new insights could never emerge.

In content, the polemic between Forster and Kant is about determining the concept of Human Race and about the question whether Europeans and Africans belong to different genera, or whether they should not better be considered, because of a presumptive common origin, as species of one and the same genus. Both authors depart from their own concept of Nature. For Forster, who traveled the world already as a boy, Nature is the whole, is reality as a continuum, in which all differences and connections can be found.

Forster always points out the systemic character of reality and of Nature in particular.

> "A Negro"—Forster says for instance, is properly speaking—"a true Negro only in his own fatherland. Any creature of Nature is what it should be only in the locality for which it has been created; a truth which is seen confirmed every day in menageries and botanical gardens. A Negro born in Europe is like a greenhouse plant, a modified creature, in all properties subject to change more or less unlike that which would have become of him in his own fatherland." (*ibid.*, 13)

Forster was very familiar with the principle of continuity as it was used by eighteenth-century French authors, like Buffon or Robinet for instance, to emphasize the "Great Chain of Being" (Lovejoy 1936). On the other hand, Forster says, all our categorizations are necessarily arbitrary, a situation which already results from the fact that we are only able to think within fixed differences while the distances between the various genera in Nature fill an entire continuum.

> "The order of Nature does not follow our categorizations, and as soon as one tries to impose them on it, one falls into inconsistencies. Each and every system is meant only to be a guideline for memory by giving sections as Nature itself seems to make them." (Forster W, 22)

Hence, and in contrast to Kantian epistemology, any constructive synthesis is preceded or accompanied by analysis.

What can be said in view of this situation with regard to the question whether "Negro" is a genus or a species of humankind?

Forster, on the basis of his own systemic reasoning, assumes that Nature, like any continuum, forms a complete whole in every locality of the earth and in every climatic zone and that man represents no exception.

> "If every region produced the creatures which were appropriate to it, and moreover in precisely those relations which were indispensable for their safety and upkeep: how is it that the fragile human being should be an exception here? Rather, Nature has given its own character, as Herr Kant himself professes, its special organization, an original relationship to a climate and suitability to the latter to each and every stock and race. Indisputably, this precise relationship between the land and its inhabitants can be most easily and briefly explained by the local emergence of the latter." (*ibid.*, 28)

Kant, in contrast, had claimed that all human races stemmed from only one and the same root.

Forster hesitates to answer the question "whether there are several original races" with certainty, but considers this hypothesis no less plausible than the Kantian one. And to Kant's teleological or functionalist reasoning that in case of bigger differences human beings need to wage war on one another and that it is thus not in the interest of Nature to create such differentiation, Forster objects as follows:

> "In a world where nothing is superfluous, where everything is linked by the finest nuances, where the concept of perfection finally consists in the aggregate and in the harmonic cooperation of all individual parts, the idea of a second genus of humans would be for the supreme mind a forceful means to develop ideas and feelings which are worthy of an earthly creature endowed with reason, thus interweaving this creature himself much more firmly with the plan of the whole."

And he observes that one need only look at the slave trade to see how idealistic and abstract Kant's considerations are. Slavery has not been prevented at all by the belief that all human beings are of one kind only.

Kant published a reply to Forster's objections, "Über den Gebrauch teleologischer Prinzipien in der Philosophie" (Kant SA, VIII, 157-184). In his retort Kant wishes to do more than just maintain his position on the necessity of *a priori* principles: "It is indubitably certain that by mere empirical stumbling about without a guiding principle defining that which is sought after, nothing useful would ever be found" (*ibid.*, 161). Kant accordingly begins with a quite different concept of Nature:

> "If, by Nature, we understand the embodiment of everything which exists determined by laws [...] research into Nature can pursue two paths, either the merely theoretical or the teleological one, while using, however, [...] only such purposes which can become known to us by experience [...] for its intention. [...] Rightfully, reason calls first for theory in every study of Nature, and only later for teleology." (*ibid.*, 159)

Nature is no more than order and uniformity of appearances. We prescribe *a priori* rules to which all possible experiential reality must conform.

For Kant it seems indisputable that "where theoretical sources of knowledge do not suffice, we may make use of the teleological principles, but with such limitations of its use that theoretical-speculative research will always be assured precedence in order to try its best effort on the question at hand" (*ibid.*, 164). From the necessity of this principle, Kant now derives an essential distinction between natural history and a mere description of Nature. Natural history, according to Kant, is exclusively concerned with "pursuing back, only as far as analogy permits, the connection between certain present features of natural things and their causes in former times according to the laws of cause and effect which we do not invent but derive from forces of Nature as they present themselves to us" (*ibid.*, 162). It is evident here that Kant is not concerned with the objects, but with the laws, and further with getting "to know more precisely the limits of these laws lying in reason itself, together with the principles according to which they could best be extended" (*ibid.*, 165).

Kant's intention is to determine

> "how the greatest variety in genesis can be reconciled by reason with the strictest unity of origin [...]. And one sees clearly here that one must be guided by a certain principle to even observe, that is to pay attention to what could give indication of the origin not only of similarity of appearance, because we are concerned here with a task of natural history, not of the description of nature." (*ibid.*, 164)

Kant then introduces such a principle which is intended to demonstrate a difference of origin, that is "the impossibility of obtaining fertile descendants by mixing two genetically different species of humanity" (*ibid.*, 164-165).

According to this concept, Kant writes, "all men on the wide Earth belong to one and the same genus of nature, because they can consistently sire fertile children with one another, no matter how large the differences in their appearances encountered" (Kant SA, II, 430). Kant says that to assume a variety of "local creations" is an opinion "which multiplies the number of causes without necessity" (*ibid.*, 431). "It is the appropriateness in an organization which is the general reason from which we conclude that there is a design originally placed in the nature of a creature" (Kant SA, VII, 103).

Against this criterion, Forster again raises systemic objections by arguing that things in Nature are quite different from those in an experimental situation brought about arbitrarily. Artificial experiments, like breeding experiments "conducted with animals under the constraints of captivity" must not be quoted as genuine scientific explanations of cause. But he does not see this as an absolute counter argument to Kant concerning the hypothesis on the origin of Man, or a counter-argument only insofar as he qualifies Kant's criterion as totally arbitrary, as a matter of mere definition. This resembles Hegel's charge that Kantian reason furnishes only postulates and not knowledge of reality (cf. Hegel, "Glauben und Wissen", *cit.*).

For Kant reasoning is founded on certain teleological principles. Thus, he says:

> "In view of the varieties [*i.e.* of species], nature seems to prevent a fusion, because it runs counter to its purpose, namely manifolds of characters, while it at least permits this (fusion) in case of different races [...] because this makes the creature adapted to several climates while not making it suitable for any of them to the same degree found in the original adaptation to it." (*ibid.*, VIII, 166-167)

That the latter also leads to disadvantages is proved, for Kant, by "the inferior quality" of (American) Indians who exist both in the northern and in the tropical climates. Kant argues against Forster who assumes that every region created its own human race by saying that:

> "If one does not want to add a second to the special creation of the Negro already suggested by Herr Forster, namely that of the American (Indian), no other answer remains but that America is too cold or too new to ever produce the degeneration of the Negroes or of the yellow Indians, or to have produced them in the short period it has been inhabited." (*ibid.*, 176)

Kant thus assumes that men are, on the one had, of one common origin and that on the other hand, a cause lying in themselves, "and not merely in the climate", must have led to the differences between them. For Kant, as is well known, the transcendental principles of the use of reason must serve as a basis to derive everything else in a way coordinated with observation. Laws are verified ex post, since "by mere empirical stumbling around without guiding principles as to what should be sought", nothing useful will be ever found, "for to have experience methodically means solely to observe" (*ibid.*, 161). For Forster, conversely, the principles themselves must also result from observation, even if this cannot be imagined to come by itself and without activity from the cognizant subject.

The excessive mixture of speculations and principles ranging from phlogiston theory to medicine which he draws upon to explain differences in skin color and the like is very remarkable in Kant's argumentation. His contributions are entirely unreadable, while Forster's are still informative today. For instance, Kant takes external features like skin color for mere body paint "which is added to the skin by the sun and which will be taken away again by colder air" (*ibid.*, 105). Everything which cannot be brought in agreement with any kind of experience is mere speculation. In any case, Kant gives the element of the epistemic subject's activity priority over the material element and this is how the principle of synthetic unity of apperception really works. The contrast between Kant and Forster seems essentially to correspond to the two poles in the system-subsystem paradox. This is sometimes presented as follows: "Any given system can be adequately described provided it is regarded as an element of a larger system. The problem of presenting a given system as an element of a larger system can only be solved if this system is described as a system" (Blauberg, Sadovsky and Yudin 1977, 270). It seems obvious that the system paradoxes enforce an evolutionary perspective for their resolution. Kant starts from the necessity of characterizing his own subsystem, Man, as a system before all else, because the (epistemic) subject guarantees the possibility of knowledge, whereas Forster characterizes Man primarily as a subsystem of a more comprehensive system, namely Nature.

One cannot err in assuming that Kantian reasoning is rather more determined by the inner regularities and forces of the mind, that is by mental motive forces, and less by intuition and experience. It seems to be a reasoning based, as Peirce said, on the relation of similarity, for "of the two generally recognized principles of association, contiguity and similarity, the former is a connection due to a power without, the latter a connection due to a power within" (Peirce CP, 6.105). Now Peirce has pointed out that it is exactly analytical reasoning which "depends upon associations of similarity, synthetical reasoning upon associations of contiguity"(*ibid.*, 6.595).

IV Some Issues where Peirce and Kant differ

Peirce writes:

> "Kant divided propositions into Analytic, or Explicatory, and Synthetic, or Ampliative. He defined an analytic proposition as one whose predicate was implied in its subject. This was an objectionable definition due to Kant's total ignorance of the logic of relatives. The distinction is generally condemned by modern writers; and what they have in mind (almost always most confusedly) is just. The only fault that Kant's distinction has is that it is ambiguous, owing to his ignorance of the logic of relatives and consequently of the real nature of mathematical proof. He had his choice of making either one of two distinctions. Let definitions everywhere be substituted for definite in the proposition. Then it was open to him to say that if the proposition could be reduced to an identical one by merely attaching aggregates to its subjects and components to its predicate it was an analytic proposition; but otherwise was synthetic. Or he might have said that if the proposition could be proved to be true by logical necessity without further hypothesis it was an analytic one; but otherwise, was synthetic. These two statements Kant would have supposed to be equivalent. But they are not so." (NEM IV, 58)

The difference Peirce has in mind, I believe is this: Any subject-predicate expression can be transformed by means of a hypostatic abstraction into a logically and empirically equivalent relational statement (CP, 1.551; with respect to the fundamentally important notion of hypostatic abstraction see also: *ibid.*, 4.234, 4.235, 4.463, 4.549, 5.447, 5.534 and NEM IV, 49). Now if the original statement has not just been a logical truth it exhibits its hypothetical character, because the reality of hypostatic abstractions and of relations in general remains a hypothetical one. We have to construct hypostatic abstractions to make possible what Peirce calls theorematic reasoning.

Since Kant's abstract definition is ambiguous, Peirce continues:

> "We naturally look to his examples, in order to determine what he means. Now turning to Rosenkranz and Schubert's edition of his works, Vol. II [the *Kritik*, Kant B, 14] p. 702 we read, *Mathematische Urtheile sind insgesammt synthetisch.* That certainly indicates the former of the two meanings, which in my opinion gives, too, the more important division. The statement, however, is unusually extravagant, to come from Kant. Thus, the 'Urtheile' of Euclid's Elements must be regarded as mathematical; and no less than 132 of them are definitions, which are certainly analytical. Kant maintains, too, that 7+5 = 12 is a synthetical judgment, which he could not have done if he had been acquainted with the logic of relatives. For if we write G for "next greater than," the definition of 7 is 7 = G6 and that of 12 is 12 = G11. Now it is part of the definition of plus, that G*x*+*y* = G(*x*+*y*). That is, that G6+5 = G11 is implied in 6+5 = 11. But the definition of 6 is 6 = G5, and that of 11 is 11 = G10; so that G5+5 = G10 is implied in 5+5 = 10, and so on down to 0+5 = 5. But further it is a part of the definition of plus that *x*+G*y* = G(*x*+*y*) and the definition of 5 is 5 = G4, so that 0+G4 = G4 is implied in 0+4 = 4, and so on down to 0+0 = 0. But this last is part of the definition of plus. There is, in short, no theorematic reasoning required to prove from the definitions that 7+5 = 12. It is not even necessary to take account of the general definition of an integer number. But Kant was quite unaware that there was such a thing as theorematic reasoning, because he had not studied the logic of relatives. Consequently, not being able to account for the richness of mathematics and the mysterious or occult character of its principal theorems by corollarial reasoning, he was led to believe that all mathematical propositions are synthetic." (NEM IV, 58)

Now theorematic reasoning, according to Peirce, essentially depends on hypostatic abstraction. I am able to prove, he writes, "that the most practically important results of mathematics could not in any way be attained without this operation of abstraction" (*ibid.*, 49). We depend on hypostatic abstractions to make relations visible that would otherwise remain hidden.

Kant says that we do not have axioms in arithmetic, because statements like "7+5 = 12" have nothing general to themselves (Kant B, 206). Number symbols seem to be proper names of concepts that have to be applied to gain objectivity. This implies the syntheticity of the statement. But Kant wants it to be *a priori* also. The whole matter, as presented above, therefore rests on a sharp distinction between intuitive and discursive conceptions and procedures.

Peirce ascribes to Kant the merit of having given for the first time in history the distinction between the intuitive and discursive processes of the mind its proper weight. If mathematics is not merely tautological it must contain an intuitive element. But the line between intuition and logic being drawn too firmly, the greatest merit of Kant's doctrine turns itself at the same time into its greatest fault (Peirce CP, 1.35). Kant misses the importance of relations, and "wholly fails to see that even the simplest syllogistic conclusion can only be drawn by observing the relation of the terms in the premises and conclusion" (Peirce W, 5, 258). This is done by means of appropriately constructed diagrams. Peirce believes that mathematics proceeds by diagrammatic reasoning and that a diagram is characterized by the fact that one is able to find out more than was necessary to construct it. Mathematical reasoning is diagrammatic. But diagrams may nowadays contain highly complex conceptual structures. Recall for example the exact sequence defining the notion of a group extension, or the diagrams of homological algebra in general. In any case they do not contain names of definite objects. They are icons and deal with generals only, with hypostatic abstractions, and any individual, "whatever is determinate in every respect must be banished from the logic of mathematics" (Peirce NEM IV, XIII). An icon, like a free variable, does not "profess to represent anything; for if it did, that would be a manner of signifying its object, not consisting in merely resembling it" (Peirce CP, 8.119).

According to Kant, a theorem like "7+5 = 12" is not purely analytical, because

> "the conception of a sum of 7 and 5 contains nothing but the uniting of the two numbers into one, whereby it cannot at all be cogitated what this single number is which embraces both. The conception of twelve is by no means already obtained by merely cogitating the union of 7 and 5; [...] One must go beyond these concepts, and have recourse to an intuition [...]." (Kant B, 15-16)

An intuition of what, the reader might ask. And he might think, what is clearly needed is an intuition or a concept of the relations and algorithms involved, the relation of recurrence, for instance (such were already the views of Bolzano in 1810 and later again Poincaré). Kant continues by saying: "[...] an intuition, which corresponds to one of the two—our five fingers, for example, [...] and so by degrees add the units contained in the five given in the intuition to the conception of seven" (*ibid.*, B 16). Thus it is obvious that the syntheticity derives from my faculty of coping with the algorithm and that this in turn relies on the fact that it is applied onto particular cases. Kant's distinction between purely conceptual argument or deduction on one side and the application of concepts on intuitions (concepts according to Kant can only be applied on *Vorstellungen* of things rather than things themselves (*ibid.* B, 94)) remains artificial, because even in formal deduction a meta-cognitive element is always present. To state that in Peircean terminology: deduction involves Thirdness and is not confined to Firstness and Secondness.

This results in the first point of difference. Peirce even says that the entire Kantian philosophy must fall to the ground, as his logical system of distinctions of propositions is artificial, resting on mere accidents of language. As soon as one formulates the concept of arithmetical sum, for instance, in terms of the cardinality of sets, the concept is obtained as a law, and the arithmetical theorems in question thus become synthetical. As soon as the whole numbers, however, are constructed completely from the concept of ordinal numbers, introducing the concept of sum axiomatically and recursively on the basis of the successor operation of the ordinal numbers, the arithmetical theorems become analytical (Otte 1992, part IV). This situation leaves us with the choice of either negating the analytic-synthetic distinction any objective meaning or claiming that the operations of mathematical deduction and of concrete observation are not as distinct as it might appear. Peirce takes, as has been shown above, the second route. What is missing in Kant, he says, is the logic of relatives as it is developed from an analysis of diagrams, and as it is involved even in a perceptual judgment.

Now the logic of relatives shows

> "that observation and ingenuity are involved in the reasoning process. For it leads us to perceive that purely deductive reasonings involve discovery as truly as does the experimentation of the chemist; only the discovery here is of the secrets of the mind within, instead of those of Nature's mind. Now the distinction between the Inward and the Outward, great and decisive as it is, is, after all, only a matter of degree." (Peirce NEM, IV, 355)

Thus the analytic-synthetic distinction also is only a matter of degree (see also part V of this paper).

The usefulness of mathematics is due to the fact that mathematical relations are to be interpreted and applied in an indeterminate multitude of constellations. They relate possibilities not facts. Kant already had seen that things necessarily remain isolated. Thus laws or axioms do relate generals rather than things. They are conditional counterfactuals. Sets of possibilities is what physicists speak about: the configuration space of a system is the set of its possible instantaneous states. Natural laws and mathematical axioms or propositions thus establish relations between possibilities, which means between free variables or continua. "A true general is a whatever-should-be which will impart its generality to the following would be", as Peirce says (Ms, 641). Peirce thus assumes that a characteristic of mathematical thought is, "that it can have no success where it cannot generalize". Mathematicians strive for the greatest possible generality, often "exchanging a smaller problem that involves exceptions for a larger one free from them" (Peirce CP, 6.236). But generalization in respect to its widest possible scope is continuity or refers to the continuum, because "the continuum is all that is possible" (Peirce CCL, 160). In contrast to Kant Peirce believes that continuity is real and that possibility is not just our present possibility. The idea of possibility is not constrained by the idea of a (transcendental) subject. The human subject is a potentially unlimited being and growth or evolution marks its essence, rather than activity (see part III).

Thus we may understand his second point of disagreement with Kant, which is to be seen in the characterization of continuity. We know about the importance of the principle of continuity from the history of mathematics. To Peirce's realism it is however essential to conceive of the continuum, not as a collective entity, but as strictly general. Peirce uses the idea of continuity to introduce the reality of generality. But the reality of a general is the reality of the possible. Thus continu-

ity is possibility. The possible is however not determined and fixed in every respect. Therefore Peirce refuses the continuum being constructed and built up from particulars, as in Cantorian set theory or arithmetized analysis after the fashion of Cauchy. Possibility is essential to Peirce because to really conceive of epistemology in evolutionary terms, the indeterminate or less determinate and possible must to a certain degree govern evolution. Only the past is factual, whereas thought is directed also to the future and therefore to the possible, rather than factual. Otherwise one could not understand how new objects, new laws and new knowledge in general can arise.

With Peirce's abandonment of aprioricism the relation between generality and continuity becomes prominent. Free variables such as in axiomatic statements or statements like "a triangle has ..." do not imply a definite ontological commitment. A free variable or a "general triangle" does not represent a general that is predicative. It refers to a mere possibility. Therefore the term "general" is used by Peirce to designate a regularity or a law open to an indefinite number of instantiations, which means to something beyond all definite cardinality and this something therefore represents a continuum. Were it a set of distinct individuals and not a continuum, then Cantor's powerset axiom would show that it cannot be beyond all multitude. Is there any sense, asks Peirce, "in saying that something that is not a multitude of distinct individuals is more than every multitude of distinct individuals". Yes, he answers, there is in the following way.

> "That which is possible is in so far general, and as general, it ceases to be individual. Hence, remembering that the word 'potential' means indeterminate yet capable of determination in any special case, there may be a potential aggregate of all possibilities that are consistent with certain general conditions; and this may be such that given any collection of distinct individuals whatsoever, out of that potential aggregate there may be actualized a more multitudinous collection than the given collection." (Peirce CCL, 247)

The particular is at the same time general, and the concept of the general must be related to continuity, because a general relationship is a relationship that is stable under small perturbances. Such a variation does not concern a set of facts but a set of possibilities or hypotheses such that a general is a relation between possibilities, which are dependent on continuity and which have no isolated individual existence. The continuum thereby gains an ontological status independent of synthesizing activity and this certainly implies that any mathematical reasoning contains an analytical element, because of the fact that the continuum is not, as Kant believed, subordinate or secondary to a preceding mental synthesis. This idea thus involves that of a continuum. This new idea of general was expressed by Poncelet and by Peirce in nearly the same words.

We know that an algebraic or a complex analytic function f, such that $f(x) = g(x)$ holds for as small a variation of the argument as you please, is identical with g. Poncelet, on the basis of such observations, and taking into account that analytical geometry consists in coordinating continua, understood that the principle of continuity is at the heart of operating with equations like $x = 5$; and that it is the secret of algebraic generality. We can accept $x = 5$ and operate with it although a variable x and a particular value of that variable are of different logical type. The particular, an ellipse for instance, represents in a certain sense, which cannot universally be specified, the general, the conic; as long as it represents certain essential properties pertinent to that purpose, which are stable under continuous variation. Poncelet aimed at a method that was based on the interaction of general and particular, concept and representation. As in geometry, one can always only represent the general by a particular, the genus by a species, or the category by a prototype, as with the idea of "general triangle", for instance, or like a conic section by a particular exemplar, like a circle or an ellipse, one has to employ the principle of continuity to state in full generality relationships that have been verified for a particular diagram. Poncelet himself described the procedure as follows:

> "Let us consider some geometrical diagram, its actual position being arbitrary and in a way indeterminate with respect to all the possible positions it could assume without violating the conditions which are supposed to hold between its different parts. Suppose now that we discover a property of this figure, whether it be metrical or descriptive, by means of ordinary explicit reasoning—that is, by methods alone regarded as rigorous in certain cases. Is it not clear that if, observing the given conditions, we gradually alter the original diagram by imposing a continuous but arbitrary motion on some of its parts, the discovered properties of the original diagram will still hold throughout the successive stages of the system, always provided that we note certain alterations, such as that certain quantities vanish, etc.—alterations, however, which can easily be recognized a priori and by reliable rules?" (Poncelet AAG, II, 531)

Thus the permanence of relationships rather than the empirical and isolated existence or non-existence of the relata validates the argument. The general is of the character of a relationship or connection, like an idea that spreads among minds. Peirce makes this comparison between natural laws and the effect of words. "It is proper to say that a general principle that is operative in the real world is of the essential nature of a representation and of a symbol because its *modus operandi* is the same as that by which words produce physical effects" (Peirce CP, 5.105).

Third, the belief that mathematics represents absolute and apodictic true knowledge, may be questioned on grounds of two types of arguments, doubting that there is indubitable knowledge at all or questioning that mathematics represents factual knowledge. Peirce voices both kinds of disbelief. We have already dealt with one of them above in the first point of divergence. With respect to the second Peirce writes:

> "Kant regarded mathematical propositions as synthetical judgments *a priori*; wherein there is this much truth, that they are not, for the most part, what he called analytical judgments; that is, the predicate is not, in the sense he intended, contained in the definition of the subject. But if the propositions of arithmetic for example are true cognitions, or even forms of cognitions, this circumstance is quite aside from their mathematical truth." (*ibid.*, 4.232)

Mathematics is not at all concerned with meanings (*ibid.*, 5.567), but rather, as Peirce writes, with the substance of hypotheses. "Mathematics is purely hypothetical: it produces nothing but conditional propositions" (*ibid.*, 4.240). And what is more important: mathematics cannot be applied to reality by first identifying premises in every detail. Observable details do not at all guarantee any real connection and "synthetic inference is founded upon a classification of facts, not according to their characters, but according to the manner of obtaining them" (*ibid.*, 2.692).

This, however, implies that all knowledge is fallible and subject to possible revision. We have seen how Peirce's conception of the subject matter of mathematics is connected with his conception of the continuum and that this conception in turn implied to treat the problem of the evolution of knowledge in mathematics and in the natural sciences on *a par*. It also follows that theories become realities *sui generis* in relation to concrete reality. This means, that they cannot simultaneously be theories of their own application. Interpretation is a meta-operation that leads to a new representation. But theories being also signs (besides being entities in their own right) take part in a continuum of signs. This continuum, again, is not just a collection of particulars, because it incorporates all the meta-meta ...-levels of interpretation.

How then does Peirce the Pragmatist conceive of the interaction of general and particular? This is what Doctor Z, a character in one of Peirce' dialogues, asked the Pragmatist:

> "You say that no collection of individuals could ever be adequate to the extension of a concept in general, [...]. But really I do not quite see how you propose to reconcile that to the proposition that the meaning extends no further than to future embodiments of it." (*ibid.*, 5.526)

The Pragmatist in answering this question illustrates his views "by the consideration of the continuity of space". I shall, he says,

> "adopt the Leibnizian conception of space in place of the Newtonian. In that Leibnizian view, Space is merely a possibility [...] of no matter what affections of bodies (determining their relative positions), together with the impossibility of those affections being actualized otherwise than under certain limitations, expressed in the postulates of topical, graphical and metrical geometry. No collection of points [...] could fill a line so that there would be room for no more points, and in that respect the line is truly general, [...] and yet it is so to say nothing but the way in which actual bodies conduct themselves." (*ibid.*, 5.530)

Fourth, Peirce, as opposed to Kant, does not see the problem in the question: "How are synthetical judgments *a priori* possible ?", but rather in the more general question: "How are any synthetical judgments at all possible? How is it that a man can observe one fact and straightway pronounce judgment concerning another different fact not involved in the first?" (*ibid.*, 2.690). An answer is given which reminds us of the principle of continuity, which is fundamental to Peirce's philosophy. The answer is this: "whatever is universally true is involved in the conditions of experience" (*ibid.*, 2.691); and further: "experiences whose conditions are the same will have the same general character" (*ibid.*, 2.692). The principle of continuity referring to generals cannot be based on a concept of uniformity of Nature (Mill).

> "Mill never made up his mind in what sense he took the phrase uniformity of Nature when he spoke of it as the basis of induction. In some passages [...] Mill holds that it is not the knowledge of the uniformity, but the uniformity itself that supports induction, and furthermore that it is no special uniformity but a general uniformity in nature. Mill's mind was certainly acute and vigorous, but it was not mathematically accurate; and it is by that trait that I am forced to explain his not seeing that this general uniformity could not be so defined as not on the one hand to appear manifestly false or on the other hand to render no support to induction, or both. He says it means that under similar circumstances similar events will occur. But this is vague. Does he mean that objects alike in all respects but one are alike in that one? But plainly no two different real objects are alike in all respects but one. Does he mean that objects sufficiently alike in other respects are alike in any given respect? But that would be but another way of saying that no two different objects are alike in all respects but one. It is obviously true; but it has no bearing on induction, where we deal with objects which we well know are, like all existing things, alike in numberless respects and unlike in numberless other respects." (*ibid.*, 1.92)

The principle of continuity applies here because "whatever is universally true is involved in the conditions of experience" (*ibid.*, 2.691), that is, belongs to the general aspects of that particular event in question, to its law like character. The principle of continuity, according to Peirce is a methodological principle regulating the interaction between general and particular and it is the only such fundamental principle, lending support also to induction.

If we understand Kant in the sense that synthetical judgments a priori just signify conditions of experience (see the introduction), then the difference between Kant and Peirce amounts essentially to the question of the nature and ontological status of generals (or continua) or laws.

> "While uniformity is a character which might be realized, in all its fullness, in a short series of past events, law, on the other hand, is essentially a character of an indefinite future; and while uniformity involves a regularity exact and exceptionless, law only requires an approach to uniformity in a decided majority of cases. [...] The law should be a truth expressible as a conditional proposition whose antecedent and consequent express experiences in a future tense, and further, that, as long as the law retains the character of a law, there should be possible occasions in an indefinite future when events of the kind described in the antecedent may come to pass. Such, then, ought to be our conception of law, whether it has been so or not." (*ibid.*, 8.192)

For Peirce, the reality of the "general" becomes clear from the way we deal with natural laws: natural laws are general because they permit predictions, and not only because they are stated with regard "to many things", as the traditional definitions of the general say. In other words: the Aristotelian concept of the general as something predicative is replaced here by another concept of the general, a cognition or a situation being designated as "general" which permits predictions to a certain degree. If these predictions, however, are not to be held to be acciden-

tally true, the general must be assumed to be an active connection in reality, be it in nature or in history (see for example *ibid.*, 5.103). If the possibility of predictions with regard to future events is given (a stone raised will fall down), this possibility must find a basis in the reality of the connection between things suggested here. And if relations (the laws of falling bodies) and relata (the series of falling stones) thus have the same ontological status, then there exists a genuine, that is in Peirce's sense an inexhaustible continuum between these two entities—between the general law and the particular case—both whose existence is assumed here.

Let us come back once more to the analogy between mathematical axioms and natural laws to illustrate how Peirce's ideas about continuity are linked to his philosophical realism. To explain a statement like $2+2=4$ (*ibid.*, 4.91), or $7+5=12$ if you like, one first argues, as in discourse on ordinary knowledge, that this proposition expresses a simple matter of fact, to be easily verified by means of a calculation (which however is in itself independent of such verification as it seems present in intuition). After a while one goes on, completely as in the case of science, to try and give an explanation of this fact. This endeavor implies a change of perspective, a jump to a level of different logical or categorical type. The law gives a unified account of what is otherwise a mere series (Armstrong 1983). In this endeavor one uses the general and abstract to explain the particular and concrete, or seemingly concrete, in exactly the same manner in which Newton's laws are used to explain simple mechanical phenomena, or Ohm's law is used to explain the facts of electricity. The general, as used in scientific explanations of such kind, in our case for instance the associative law of algebra, is less sure from a concrete empirical point of view and less positive than the individual facts to be founded on it. The less certain is used to explain the more certain, because what could be more certain because the effects of a law can never be certain. Such a strategy makes sense if it is employed exploratively and predictively, even though the predictions made can never be absolutely sure.

Nominalism, denying the existence of universals outside the mind, has no use for the idea that laws are relations between universals and therefore cannot explain the power of prediction inherent in them. Nominalism, or empiricism, perhaps, would speak of an inductive establishment of regularities, in which theoretical concepts and scientific laws lose their independent meaning. The great difference between induction and what is involved here is "that the former infers the existence of phenomena such as we have observed in cases which are similar, while hypothesis supposes something of a different kind from what we have directly observed, and frequently something which it would be impossible for us to observe directly" (Peirce W, III, 335-336).

The nominalists would say that a natural law is a mere representation, "the word mere meaning that to be represented and really to be are two very different things" (Peirce CP, 5.96). Now natural laws, as was said earlier, have some importance to us because of their predictive power. Exactly because of their prognostic function they cannot just be established by empirical verification. The nominalist would say they are free creations of the human mind, making their effectualness a miracle or a matter of pure chance "in order to escape the conclusion that general principles are really operative in nature" (*ibid.*, 5.101). It makes no difference that the laws of nature do produce their effects with a certain probability only. On the contrary probability judgments exhibit much more clearly the general character of synthetic reasoning (*ibid.*, 2.692 and Ms, 107).

For Peirce, the general is thus necessarily of a hypothetical character, as it is seen from the very outset in its potential for development. And this holds in the same vein for the natural laws which in Peirce's view are subject to evolution in the same way as the physical phenomena determined by them. On this basis, the paradigmatic role of mathematics can be seen in the very fact that for Peirce it always had to do with hypotheses alone, so that the mode "in which mathematicians generalize" (CP, 6.26) can be used to study the process of increasing generalization within a "true" continuum of applications. Again this continuum, not being collective, just forms a space of possibilities.

The process of applying a theorem is thus a generalization. Firstly, because collective experience accumulates and is embodied within the system of symbolic means and every application of that means fosters this process, being at the same time dependent on it. Therefore generalization takes place, because the embodiment of experience in the construction of signs suggests new analogies, and generalized hypotheses. Generalization thus is both a social process and an object-related one. Two continua, one linking the sign with its object, and the other established by the successive series of interpretants, appear as if fused into one, because interpretants depend on the relation between sign and signified object.

The idea of sign brings us to the fifth divergence between Kant and Peirce. Kant's refutation of the ontological proof of God's existence, which formed the basis of Leibnizianism, confines us in philosophy to a construction of concepts, without providing the certainty that these concepts are not empty. Hence, the question arises as to how these concepts can be applied to objects. Kant's answer consists in pointing out the role of intuition. In mathematics these objects are only variables, such that mathematical reasoning becomes hypothetical. Peirce introduces the following changes: on the one hand, he eliminates the difference between concept and representation (*Vorstellung*) by means of the notion of "sign". On the other hand, he has a quite different idea of what reasoning or inferring is. Peirce always stressed that the insufficiencies of Kant's epistemology were due to the latter's insufficient logic, to a mere subject-predicate logic, and that this logic, in order to remedy the defect, must be extended to a logic of relations. It is in this very aspect, that diagrammatic reasoning becomes indispensable. Diagrammatic thinking is essentially established by the principle of continuity (*ibid.*, 5.162) and it shows that deduction and induction or analysis and synthesis are not so thoroughly unlike as might be thought (*ibid.*, 5.579).

A sixth aspect is also closely linked with the role of signs and means of representation, namely that mathematics is essentially a kind of social cognition. Mathematical cognition is the art of bridging gaps by inventing analogies and generalizations. Pure mathematics is the child of an explosive growth of mathematical activity that occurred around 1800 and that, in its sources, may be summarily characterized by stating that for the first time in the history of mathematics a great number of connections between apparently very different results and problems was discovered (Scharlau 1979, 277). A complementary presupposition is hidden here, namely that plurality and difference played a fundamental epistemological role. The world was seen as ruled by difference rather than by similarity or equality. In view of the fact that equality and difference are the fundamental subject matter of mathematics, it seems plausible to claim that

> "the chief characteristic of mathematical propositions is the wide variety of equivalent formulations that they possess. [...] In mathematics the number of ways of expressing what is in some sense the same fact while apparently not talking about the same objects is especially striking." (Putnam 1975, 45)

It seems obvious then that mathematics cannot be analytic, as otherwise there should be a universal mechanism that decides for any A whether one should be allowed to call it B thus deciding whether A could also be called B. It seems not surprising at all that Quine in "Two Dogmas" (1953) was not able to define synonymy in logical terms.

V The Analytic-Synthetic Distinction according to Peirce is only relative

Kant's definition of analytic judgments expresses a whole or partial identity between concepts serving as subject and predicate. The predicate essentially belongs to the subject and the subject is presented in its essential properties or relations. What is new about this situation in Kantian philosophy is only the fact that the essence of an object is not given but is constructed. Knowledge, says Kant,

> "consists in the determinate relation of given representations (*Vorstellungen*) to an object; and an object is that in the concept of which the manifold of a given intuition is united. Now, all unification of representations demand unity of consciousness in the synthesis of them. Consequently it is the unity of consciousness that alone constitutes the relation of representations to an object, and therefore their objective validity, [...] and upon it therefore rests the very possibility of the understanding." (Kant B, 137)

Accordingly knowledge and understanding depend on consciousness and the (epistemic) subject becomes the pivotal and crucial point of epistemology. Peirce substitutes the subject's consciousness for the sign. In a sign, like in a work of art for instance, the synthesis of representations is realized in a way similar to the way the very essence of Monet's garden at Giverny has been realized in his paintings.

> "The work of the poet or novelist is not so utterly different from that of the scientific man. The artist introduces a fiction; but it is not an arbitrary one; it exhibits affinities to which the mind accords a certain approval in pronouncing them beautiful, which if it is not exactly the same as saying that the synthesis is true, is something of the same general kind. The geometer draws a diagram, which if not exactly a fiction, is at least a creation, and by means of observation of that diagram he is able to synthesize and show relations between elements which before seemed to have no necessary connection. The realities compel us to put some things into very close relation and others less so, in a highly complicated, and in a sense itself unintelligible manner; but it is the genius of the mind, that takes up all these hints of sense, adds immensely to them, makes them precise, and shows them in intelligible form in the intuitions of space and time." (Peirce CP, 1.383)

The objectivity of a piece of art or of a theory which "compels us to put some things into very close relation and others less so" is due to the fact that works of art or theories, besides being signs, became recognized as realities *sui generis*. They are, in Peirce's words, distinct *quales* or *qualia*.

> "In so far as qualia can be said to have anything in common, that which belongs to one and all is unity; and the various synthetical unities which Kant attributes to the different operations of the mind, as well as the unity of logical consistency, or specific unity, and also the unity of the individual object, all these unities originate, not in the operations of the intellect, but in the quale-consciousness upon which the intellect operates." (*ibid.*, 6.225)

By his "semiotic transformation" of critical philosophy, Peirce was able to take into account that looking from different perspectives on one and the same thing and viewing different objects from one and the same point of view become indistinguishable approaches, as in the fusion of analytical geometry and linear algebra. The semiotic theory attempts to explain cognitive growth as a process in which the stages are indifferently members of a social community or sequential states of a single person. Knowledge and cognition are relative only in that they have to grow and to be generalized. That is their essential nature. Man is a sign himself and the processes of objective and of communicative generalization become unified into one process. Peirce semiotic theory now relies essentially on the logic of continuity and on the reality of the continuum. I cannot extensively deal with this thesis here but take it into account only as far as it concerns my topic.

With respect to this "semiotic transformation" of critical philosophy the reformulation of the definition of analytical judgments—in the sense of Kant as given by Quine in his "Two Dogmas of Empiricism"—seems justified. Quine writes:

> "Kant conceived of an analytic statement as one that attributes to its subject no more than is already conceptually contained in the subject. This formulation has two shortcomings: it limits itself to statements of subject-predicate form, and it appeals to a notion of containment which is left at a metaphorical level. But Kant's intent, evident more from the use he makes of the notion of analyticity than from his definition of it, can be restated thus: a statement is analytic when it is true by virtue of meanings and independently of fact." (Quine 1953, 20-21)

Meanings are generals, they are instances of Thirdness, and that implies that an investigation into meaning relations is a meta-knowledge activity. Any mental activity, in fact, involves the idea of context and this means meta-cognitive elements. For instance, the form which a simple distinction commonly takes is "All things of sort S are either A or B". A simple distinction thus already involves generality (hinted at by the term: " ... of sort S"). Every cognitive activity involves a meta-cognitive element. To give but one more example: human rote learning is an example of a very rudimentary form of cognitive activity. But normally it is accompanied by a second-order phenomenon which we may call "learning to rote learn". For any given subject, there is an improvement in rote learning with successive sessions asymptotically approaching a degree of skill which varies from subject to subject. Meta-cognitive activity making that one has thought about any subject itself a subject of thought creates what Peirce has termed "hypostatic abstractions".

> "In order to get an inkling—though a very slight one—of the importance of this operation in mathematics, it will suffice to remember that a collection is an hypostatic abstraction, or *ens rationis*, that multitude is the hypostatic abstraction derived from a predicate of a collection, and that a cardinal number is an abstraction attached to a multitude." (Peirce CP, 5.534)

Now hypostatic abstractions like the essence of "Two" or like "Blue-ness" are indeterminate in many respects and to varying degrees, they are continua and they are real. Thus they represent Thirdness.

The analytic-synthetic distinction must therefore be liberated from questions about objectivity and objective truth. It is a methodological question. One has, with respect to the purpose at hand, to choose the appropriate level of generality. And taking into account the identity between generality and continuity any investigation into meaning relations should be governed by the principle of continuity rather than the principle of identity of indiscernibles. One would ask then how meanings become connected, that is become species of one kind or type, rather than whether different meanings refer to the same thing or are identical. Now

> "the meanings of words ordinarily depend upon our tendencies to weld together qualities and our aptitudes to see resemblance, or, to use the received phrase, upon associations by similarity; while experience is bound together, and only recognizable, by forces acting upon us, or, to use an even worse chosen technical term, by means of associations by contiguity." (*ibid.*, 3.419)

And

> "analytical reasoning depends upon associations of similarity, synthetical reasoning upon associations of contiguity. The logic of relatives, which justifies these assertions, shows accordingly that deductive reasoning is really quite different from what it was supposed by Kant to be; and this explains how it is that he and others have taken various mathematical propositions to be synthetical which in their ideal sense, as propositions of pure mathematics, are in truth only analytical." (*ibid.*, 6.595)

This error with respect to the character of deduction in pure mathematics is due to the sharp discrimination Kant has drawn between deductive inference and observation or between discursive and intuitive knowledge. Kant

> "saw far more clearly than any predecessor had done the whole philosophical import of this distinction. This was what emancipated him from Leibnizianism, and at the same time turned him against sensationalism. [...] But he drew too hard a line between the operations of observation and of ratiocination." (*ibid.*, 1.35)

Kant shared with Leibniz a foundationalist attitude with respect to knowledge. He, however, conceived of the foundations differently from the God's eyes perspective of Leibnizianism. This different orientation made him emphasize the distinction between discursive and intuitive knowledge, because only God's mind is intuitive, whereas ours is necessarily discursive (Kant B, 135). Peirce does not accept Kantian foundationalism and the sharp separation between the subjective and the objective in Kantian thought and this makes the analytic-synthetic distinction a relative one too.

> "The truth is our ideas about the distinction between analytical and synthetical judgments is much modified by the logic of relatives [...]. Deduction, or analytical reasoning, is [...] a reasoning in which the conclusion follows (necessarily, or probably) from the state of things expressed in the premises, in contradistinction to scientific or synthetical reasoning, which is a reasoning in which the conclusion follows probably and approximately from the premises, owing to the conditions under which the latter have been observed [...]. The two classes of reasoning present, besides, some other contrasts [...] some significant resemblances. Deduction is really a matter of perception and of experimentation, just as induction and hypothetical inference are; only, the perception and experimentation are concerned with imaginary objects instead of with real ones." (Peirce CP, 6.595)

Mathematics, being based on experimentation with diagrams, has deduction as its main method of reasoning (Peirce knows of two other methods, namely induction and abduction) and Thirdness as its main category.

Another explanation of the connection between the analytic-synthetic distinction and the subject-object relation can be furnished via a discussion about the character of relations and in particular via the question whether relations are internal or external (Peirce uses the attributes "relation of reason" *vs.* "real relation" and he parallels analytic knowledge with the former (*ibid.*, 1.365)). The attempts by Russell and Moore to understand relations and to see the implications of the distinction between internal and external relations led to the establishment of analytic philosophy around the turn of the century (Moore 1922, 276-309).

Kant believed that relations are "external" and real knowledge must therefore be synthetical. All objects (substances) are isolated like Leibnizian monads. Continuity we find only in the realm of phenomena as they are synthesized by activity. Kant accordingly based synthesis and continuity on activity. But contrary to Hume he believed in the objective character of the synthesis and the resulting knowledge because the subject's activity is framed by conditions that are *a priori*. Therefrom comes his project of understanding how synthetic knowledge *a priori* is possible.

An analytic proposition implies $S = P$ and this means $S < P$ and $S > P$. $S < P$, in words: the predicate is to be applied to the subject; whereas $S > P$ means the predicate inherently belongs to the subject. This last expression is normally used when explaining what analyticity of judgments means. $S < P$ and $S > P$, however, are equivalent, as we have just seen from the rephrasing (a more formal statement of this equivalence can be found in Peirce (Peirce CCL, 131 ff.). We see from this that if all relations are internal all propositions are analytical. The externality of relations by contrast, leads to synthetic propositions. Instead of using *a priori* intuition to secure the objectivity of synthetic knowledge Peirce uses a theory of the continuum. Objectivity of knowledge namely is an ontological question, according to Peirce. It is the question of the reality and generality (which is the same) of relations, and the latter question depends on continuity (as we have seen when discussing Poncelet's views). On these grounds, the analytic-synthetic distinction becomes relative. We have, in fact, shown that analysis and synthesis are complementary elements in every mental activity (even in formal deduction).

Quine in "Two Dogmas of Empiricism" claims (1953, 37) that Peirce adhered to the verification theory of meaning and held a limit theory of truth. This, however, is a too narrow interpretation of the pragmatic maxim and of Peirce's frequent endorsement that the truth of any proposition is a function of whether or not its being accepted by the epistemic community in the idealized long run. In a controversy with William James and the latter's views on pragmatism, Peirce denied the existence of absolute individuals and stressed the importance of the general, which is a continuum that is not collective. The continuum of space, we recall, served Peirce as an illustration of such a potential aggregate that contains only general conditions "which permit the determination of individuals". The pragmatic maxim in a narrow sense implies a God's eye perspective, as Peirce had explained in a review of Royce's philosophy because the thing which God imagines, and the opinion to which investigation would ultimately lead, in point of fact, coincide. (Peirce CP, 8.41). Thus to hold a verification theory of meaning would amount to falling back on Leibnizianism, which certainly was not on Peirce's mind.

Quine, in "Two Dogmas of Empiricism", linked the analytic-synthetic distinction to the classical view of scientific knowledge, namely to the belief that each meaningful statement is equivalent to some logical construct upon statements that express direct matters of fact. Quine, in fact, defines this type of reductionism more narrowly, but we want here to stick to the classical Aristotelian scheme of a science. Such a science is a system of sentences which satisfies the following postulates: there is a finite number of terms and a finite number of sentences such that the meaning of the terms and the truth of the sentences are so obvious as to require no further explanation and proof. The meaning of any other term as well as the truth of any other sentence is definable or logical inferable starting from the original collections of terms and sentences, which are given by means of intuition and experience.

Kant adhered to this Aristotelian model of rationality, but radicalized it by amplifying the part of the rational mind as a standard, since he learned from Hume that all knowledge presupposes a synthetic constructive element. His views are best illustrated by quoting his characterization of the term "Nature" (Kant A, 125-128). The unity of apperception is the basis of any order and uniformity of Nature (cf. part III of this paper).

Holism in the sense of Kuhn or Feyerabend followed the Kantian route a little further still. On this account theory as a whole or the paradigm becomes the standard which determines fact and rationality. But this standard becomes thoroughly relative. Kant sacrificed truth for objectivity. Now even objectivity is to be understood relative to the theory in question. Kuhn or Feyerabend believe that theory as a whole determines the intensions of its terms and that intensions determine extensions. The theory or the paradigm becomes a way of seeing the world, which is completely incommensurable with other ways. It is clear then that the analytic-synthetic distinction loses all objective meaning because of the thoroughgoing relativism involved. Where do the scientific revolutions and the new rationality standards come from? To answer questions like this we would have to engage in an understanding of the objectivity of the subjective outside aprioricism. The task then is to see how in the evolution of knowledge social and objective factors interact. Quine finally believes that the theoretical system as a whole must be squared with experience but is as such hopelessly underdetermined by experiential fact. Quine's solution of the dilemma of relativism is that "in practice we end the regress of background languages, in discussions on reference, by acquiescing in our mother tongue and taking its words at face value" (Quine 1968, 201). This means we understand scientific objectivity as resting on common sense. But this is, says Chomsky, "no help at all, since every question he had raised can be raised about the mother tongue and the face value of its words" (Chomsky 1976, 186). Common sense convictions themselves have to be taken as variables and have to be related to scientific expertise and inquiry. It is the relationship between science and commonsense knowledge which determines our cultural evolution.

Nevertheless it is common sense where our most stable convictions are borrowed from, even in science. Meaning essentially depends on the fact that all humans ultimately live in a common world, irrespective of the fact that pluralism and diversity are very essential to human life. Peirce always stresses that purposes of "a general description" are intended in the pragmatic maxim, and that

> "upon innumerable questions, we have already reached the final opinion. How do we know that? Do we fancy ourselves infallible? Not at all; but throwing off as probably erroneous a thousandth or even a hundredth of all the beliefs established beyond present doubt, there must remain a vast multitude in which the final opinion has been reached. Every directory, guide-book, dictionary, history, and work of science is crammed with such facts. In the history of science, it has sometimes occurred that a really wise man has said concerning one question or another that there was reason to believe it never would be answered. The proportion of these which have in point of fact been conclusively settled very soon after the prediction has been surprisingly large. Our experience in this direction warrants us in saying with the highest degree of empirical confidence that questions that are either practical or could conceivably become so are susceptible of receiving final solutions provided the existence of the human race be indefinitely prolonged and the particular question excite sufficient interest." (Peirce CP, 8.43)

VI Pure and Applied Mathematics: Some Examples of Non-Kantian Applications of Mathematics

What concerns us here is the complementarity of means and problems, or of methods and objects, which became prominent. This complementarity becomes essentially Thirdness, if one takes into account that activity has to enter as a third into the relation. The fundamental ideas of science or mathematics are of a methodological character, rather then of an objective one. Objects and relations become means and means become objects of scientific activity. Means and objects are fully differentiable by their respective moments on individual cognitive activity, but they play a completely symmetric part in the development of cognition. This complementarity (difference and unity) of objects and means accounts for the emergence and dynamism of pure mathematics in the nineteenth century. It follows from this that there are no absolute foundations nor universal justification processes for mathematics. Looking from different perspectives on one and the same thing and viewing different objects from one and the same point of view become methodologically indistinguishable approaches, as in the fusion of analytical geometry and linear algebra. This equivalence or complementarity is represented in the idea of sign, when taken in the sense of Peirce. Linear algebra or synthetic projective geometry were meant by its inventors as new and more fundamental approaches to geometry, in comparison to the ones espoused by Euclid or Descartes. Still they did not lead as was hoped to the final determination of mathematics. They were in fact first steps towards what later on became called a de-ontologization of mathematics.

Mathematical ontology nowadays can only be conceived of as Peirce's inexhaustible continuum of real possibilities of relations. And which of these possibilities become actualized in a certain context and at a certain point in time depends on our goals and means of knowing. The future determines the past, which is the universe of the factual. The world contains only signs and the continuum of possibilities ahead in the future. A theory of meaning based on concepts abstracted from substances does not permit us to distinguish between analytical or synthetical judgments of cognition. The continuum's meaning serves only as a philosophical hypothesis which enables us to tie meanings to whether a practice of cognition has been verified and to justify generalizations by their ability to predict. The foundation of mathematics cannot be separated from its application. This is the conclusion we draw from what has been said above.

The dialectic of means and objects may briefly be summarized as follows:
A) As in any other cognitive activity, object and means of cognition are linked in mathematical activity as well. Mathematics cannot proceed in an exclusive orientation towards universal, formal methods. This would in the last instance amount to mathematical activity itself being suited to mechanization and formalization. Mathematics, too, forms specific concepts intended to serve in the grasping of mathematical facts.
B) Object and means are not only linked, but also stand in opposition to one another. Objects or facts are resistant to cognition. They represent Secondness, as Peirce says. And problems do not produce the means to their solutions out of themselves. Modern mathematics even obtains its own dynamics in no small part from applying theorems and methods which at first glance have nothing to do with the problems at hand.

In this, we understand by "object" any problem or any kind of resistance of reality against the subject's activity, and by "means" anything which seems appropriate to achieve mediation between the subject and the object of cognition. In this sense not only sign systems but also theories—knowledge of any kind and also intuitions in the Kantian sense—are means of the subject's activity.

This double problematic of means and objects as outlined under (A) and (B) also determines the relationship between analysis and synthesis and the quite controversial evaluations of the latter.

With regard to (A), for instance, the advantage of synthesis is presented as concreteness and genuine objectivity in mathematics (AS), whereas under (B) synthesis is presented as a method too much dependent on the particulars of the situation under consideration that proceeds timidly, conservatively and tentatively, and by chance and error. Synthesis is a method which becomes mired in the particular and is unable to attain genuine generalization (BS).

Under (A), conversely, the restricted character of logic or of algebraic analysis is salient.

> "The objects considered which are mere compositions or compounds of elements do not contain more or less than the elements themselves; as a result, the goal pursued will always be determined by the means applied [...]. The problem is from the very outset cast into the mold of algebraic composition." (Boutroux 1920, 193-194)

In these words Boutroux criticizes Cartesian algebraic science. The means themselves, dominating thought too much, become the only objects considered. Or, to put it differently, knowledge becomes abstract and formal (AA). From perspective (B), on the other hand, algebraic generalization appears as an opportunity for symbolic generality which detaches itself from links too close to referential meanings, and in which true generalization is attained by introducing hypostatic abstractions, whereas in synthetic mathematics the general is always only presented by a particular (BA).

For purposes of illustration, let us consider two examples of mathematical application. The first concerns the so-called "theory of cellular automatons" and the possibilities of using them to describe the developmental dynamics of processes. In an application developed by Bielefeld mathematicians, the matter at hand was to investigate heterogeneous-catalytic reactions on metal surfaces. These are chemical reactions occurring in many processes of detoxification of exhaust fumes, and in particular in exhaust catalyzers for car engines. Complicated oscillation patterns were formed in these reactions, and it was possible to simulate these by cellular automatons. These simulations, however, were not hit upon by analyses of the chemical processes at hand, but rather by observing that a certain function of number theory shows a quite similar oscillatory behavior. And this function in turn was easily represented by a cellular automaton. Only afterwards did it become possible to give a chemically plausible interpretation for this behavior (Jahnke 1992). These relations of similarity led to a computer simulation of the relevant processes, and this in turn led to deeper study and interpretation. Mathematics and computer simulation just furnished a reservoir of forms.

A second, similar example comes from research into the brain and into cognition. First, by investigating the brain, the computer was used to try and find out what thinking really is. This mechanistic or reductionist approach, however, did not bring theory close to the "essence" of cognitions. Later, computers were variously used in trying to identify certain brain activities within an electrical thunderstorm which can be measured on the scalp. The results obtained were then used to build apparatuses which transform certain brain activities into material processes such as controlling an airplane. For this, it is necessary that the individual whose brain is the source of the signals learns to repeatedly produce certain impulses at will, just as I do involuntarily when I raise my right arm. How a certain effect can be produced must be found out by every individual for himself. There is, so to say, no clear-cut material basis for that. "In principle, it doesn't matter what signal is measured as long as one is able to influence it somehow with one's brain", as A. Junka, one of the pioneers in the analysis of biosignals, describes his own working philosophy (*Focus*, No. 28 July 1994, 104 ff.). Together with some friends, he set up a company which developed a so-called biolink system. Three forehead electrodes do not only record electric currents in the brain, but also signals from facial muscles. A calculating method is used which analyzes the signals in real time in ten different frequency ranges. According to the strength of the signal received, the computer can be made to carry out certain actions. "This must not be seen too analytically, the main thing is that it feels good" (*ibid.*), Junka points out to those who want to decide and find out for themselves how they want to coordinate their own will and their brain activity. Particularly for wheelchair patients with severed spinal cords opportunities hitherto unheard of are provided.

The researchers do not approach the matter analytically, but rather play around with various types of brain control devices. What matters therefore is not the question what thinking really is, here and at this point in time, but rather how thinking can influence reality. The computer thus is a machine which establishes relations between the brain and some other entity and confers a certain reality on them. Similarly, the diagram in mathematics is a machine which permits us to confer reality to certain relations. The process is always the same. From a continuum of real possibilities, some of these are being actualized by means of distinctions. In this sense, Peirce guessed

> "that the laws of nature are ideas or resolutions in the mind of some vast consciousness, who, whether supreme or subordinate, is a Deity relative to us. I do not approve of mixing Religion and Philosophy; but as a purely philosophical hypothesis, that has the advantage of being supported by analogy." (Peirce CP, 5.107)

Institute for Didactics of Mathematics,
University of Bielefeld

Note

* I would like to thank Michael Hoffmann, Marco Panza, Andreas Etges, Richard Steigmann and Günter Seib for helpful remarks and linguistic advice on earlier versions of this paper.

III. History and Philosophy

MARCO PANZA

CLASSICAL SOURCES FOR THE CONCEPTS OF ANALYSIS AND SYNTHESIS*

In the introduction to the present book, different meanings of the terms "analysis", "synthesis" and their cognates, variously related to mathematics are taken into account. It appears to me that the papers composing the present volume, exhibit a great variety of meanings of these terms as they occurred in the history of philosophy of mathematics. In such a situation, it is quite natural to wonder if, when speaking of analysis and synthesis in mathematics, we are really speaking of a unitary and well-defined question, or if the title of the present book merely refers to a number of different and unconnected questions. At first glance, one might believe that this is the case; that what is common to the different meanings of "analysis" and "synthesis" consists just in the fact that people happen to use these same words. But if a term is used to refer to different meanings, it is plausible that there is a reason for that. Even though these meanings are really different, it is nevertheless possible, for example, that they are linked by a causal chain which is so long that the ends of it have actually nothing to do with beginnings. If this were the case, our book would finally be concerned with a succession of semantic shifts or stretches rather than with a historical and philosophical question. I do not believe that this is so. The different topics discussed in the various contributions are, I believe, intrinsically connected to each other; besides, I argue that all of them are parts of only one question, and that this question can be addressed both as a historical and as a philosophical one.

I should like to provide two distinct arguments: the first is based on my understanding of the relations between history and philosophy of mathematics, the second one is concerned with my understanding of the different meanings of "analysis" and "synthesis" and their cognates. The main objective of the present essay is to state and unfold the second of these arguments. Thus I will consider the first one only very briefly.

Mathematics is a human activity (here, ch. 11), as is philosophy. Mathematics is concerned with the creation and study of mathematical objects (here, ch. 12, par. IV), while philosophy creates and studies philosophical objects. A philosophical object is nothing but a concept. It is a general category we use in our explanation of certain phenomena, for example, the phenomenon of knowledge. Thus, philosophy takes part in any explanatory activity. Thus, as long as mathematics is

M. Otte and M. Panza (eds.), Analysis and Synthesis in Mathematics, 365–414.

an explanatory activity, it contains philosophy as a part of it. But, as long as it is a human activity, mathematics is also a phenomenon that we would possibly want to explain. Such an explanation is exactly the goal of a different sort of activity which is generally either called "history" (or "historiography") or "philosophy" of mathematics. The use of one or the other of these two distinct names depends on the particular aspect of explanation on which we want to insist. By using the first name, we insist on a local explanation, that is the explanation of a fragment of mathematics, as it has been performed (and according to the results it has produced). By using the second name, we insist on the search for and discussion of the general categories we use in such an explanation. This does not mean, of course, that I intend history (or historiography) of mathematics as a particular application of philosophy of mathematics. As an activity, mathematics is a single and individual phenomenon and it seems to me that it is not possible to intend it as a succession of repetitions of certain patterns or models. Thus, philosophy of mathematics is not the activity of describing patterns or models of mathematics. By speaking of general categories, I do not refer to general patterns for mathematical activity, but to general concepts we use in order to speak about such an activity and to explain it.

From such a point of view, the question of analysis and synthesis in mathematics is the question of legitimacy, nature and use of the general categories of analysis and synthesis for the explanation of (certain fragments of) mathematics, and it is really a unitary question if the terms "analysis" and "synthesis" refer, or could refer, to two general concepts used to speak of mathematics and explain it. It is a matter of fact that these terms have been used both to explain and do mathematics. A number of papers of our book aim to understand and discuss some of these uses. If their conclusions were intended as an evidence for a radical difference between these uses, it would not be possible to assert that they are parts of an answer (or even different partial answers) to only one historical and philosophical question. We would be justified in speaking about the philosophical and historical question of analysis and synthesis in mathematics only if we accepted to specify a particular meaning in which we use the terms "analysis" and "synthesis". This is not my wish, since I do not think that the conclusions of the previous papers are evidence for a radical difference between the admitted uses of these terms. I think, quite to the contrary, that the different concepts of analysis and synthesis discussed in the previous essays are intended as different elements of two classes of equivalence which constitute as such two general concepts; that is, they are different forms of exposition of these concepts. The aim of the present essay is to expound some important aspects of these concepts by discussing some classical source.

I Philology and Literature

Both the terms "analysis" and "synthesis" stem from the Greek. As they are composed of more primitive terms, they could, in a sense, be understood as sorts of descriptions. Thus, at a first glance, we can consider their etymology as a source of suggestions.

The Greek term for "analysis" is "ἀνάλυσις" that is composed by the prefix "ἀνά" and the substantive "λύσις". The prefix "ἀνά" was generally used in Greek to indicate the idea of motion upwards, and could accordingly be translated by expressions like: "upwards", "above", "towards", or even "near" or "close to". However, in composed words it is also used sometimes in the sense of "back" or "backwards". The substantive "λύσις" is used in different senses too, like "solution" or "conclusion", but—as it is derived in turn from the verb "λύω", that means "to free", "to liberate", "to loose", "to unknot", "to dissolve", or even "to break" or "to destroy"—it is also used to indicate the ideas of liberation, loosening, dissolution or even destruction. Thus, tentative transitions of "ἀνάλυσις" could be: "back from solution"—or, as it was common for Latin translations of Greek texts, "resolution [*resolutio*]"—or "back from conclusion", but also "toward the solution", "close to the conclusion" or again "what brings to the solution (or dissolution or even destruction)", "what makes it possible to unknot something", etc.

The situation is simpler for the term "synthesis", that is the English version of "σύνθεσις" or (more seldom) "ξύνθεσις". This is composed by the prefix "σύν" (or "ξύν")—which means "with" or "together"—and the verb "τίθημι"—which means "to put", "to lay (down)", "to set" or even "to state". Thus a synthesis could be etymologically intended as the act of putting (something) together or the act of stating (something) with an accord.

These swift etymological considerations suggest a starting point for our search: etymologically, the Greek terms for "analysis" and "synthesis" do not oppose each other in a direct way. Whatever semantic opposition there is, it is that between the verb "λύω", which vehicles an idea of separation and the prefix "σύν" which transports an idea of composition. However, though the term "σύνθεσις" directly refers to the action of composing, the term "ἀνάλυσις" refers to the action of separating only in a more indirect way, by means of the prefix "ἀνά" and according to the complex idea of "λύσις".

This is confirmed by the occurrence of the terms "ἀνάλυσις", "σύνθεσις" and their cognates in the Greek *corpus*, where they are not generally used to express two opposite ideas. Even though the first one is often used to express an idea close to that of separation, such an idea is generally more complex, and it is not in direct contrast to an idea of composition as transported by the term "σύνθεσις".[1] In the *Odyssey*, Penelope, waiting for Ulysses to return, "analysed [ἀλλύεσκεν]" her web during the night, but she did not synthesize it during the day; she "weaved [ὑφαίνεσκεν]" it (*Odyssey*, β, 104-105 and τ, 149-150). In the tragedy by Sophocles, the chorus snubs Electra because of her inability to "analyze" herself from her males (*Electra*, 142), but Electra never synthesizes herself with them. Again, for the author of *On the Universe* (which was during a long time ascribed to Aristotle), some winds can be formed by "analysis" of clouds' thickness (*On the Universe*, 394*b*, 17), but no cloud is formed by synthesis. In these three examples, "analysis" and its cognates carry respectively the ideas of unraveling, liberation and dissolution, three ideas expressing separation that are not opposed to compositions by "synthesis". A similar exercise is possible starting from the term "synthesis". According to Pindar (*The Pythian Odes*, IV, 168) the agreement between Pelias and Jason, after which the latter leaves for Colchis to seek the Golden Fleece, is just a "synthesis". You can find the same idea of synthesis, as an agreement in Plutarch (*Live of Sulla*, 35, 10), who uses the verb "to synthesize [συντίθημι]" to indicate the act of bargaining over a marriage (namely the marriage of Sulla and Valeria at the end of Sulla's life). Following Isocrates (X. *Helen*, 11), a "synthesis" is then the act of drafting an oration—five centuries later, it will be for Plutarch (*Moralia*, 747*d*) the act of composing a poem—, while for Aeschylus (*Prometheus Bound*, 460) it was, in the same vein but more fundamentally, the science of writing, that is the art of arranging letters in order to form a word. In such a sense, it is one of the gifts from Prometheus to human beings, which make them able to reason and think. Six centuries later, Plutarch associates the idea of synthesis to a different art, namely the art of counting or even to the science of numbers. In his treatise *The Obsolescence of Oracles*, he generalizes an old definition of (natural) numbers as "synthesis of unities", already quoted by Aristotle in *Metaphysics* as a customary one (1039*a* 12), and uses the term "σύνθεσις" to refer both to the composition of (natural) numbers by smaller numbers (*Moralia*, 429*b*, cf. also 744*b*) and to their addition (416*b*). Cognates of the verb "συντίθημι" were besides used in the *Elements* (for example in the definitions VII, 13-14) in a similar sense, to indicate composition of numbers or magnitudes. In these five examples, "synthesis" means something close to composition, but it does not appeal to any sort of analysis, before it, or after it.

Of course these examples have not to be taken too seriously, in particular when two common verbs like "ἀναλύω" and "συντίθημι" are involved. They confirm however that the opposition between analysis and synthesis was not as natural in Greek culture as it is for us. Moreover as long as, in all of these previous examples, analysis and synthesis are particular sorts of separation and composition, they seem to operate on certain objects to change their relational status or obtain other sort of objects of the same logical nature. Neither synthesis, nor analysis entails a passage from the particular to the universal, or from the universal to the particular, or from objects to concepts or *vice versa*.

II Plato

The same seems to be true of the idea of synthesis as it occurs in Plato's dialogues. In the *Cratylus* (431*c*), Plato comes back to the idea of Aeschylus and generalizes it with respect to the structure of language, by saying a proposition is a "synthesis" of verbs and nouns (cf. also *Sophist*, 263*d* and Plutarch, *Moralia*, 1011*e*, which just assigns such a definition to Plato). In the *Republic*, he speaks of "synthesis" as referring to the combination of parts in a certain system. You have not to believe—he argues (611*b*)—that soul consists of distinct parts, since it is difficult that a being is immortal if is composed (σύνθετον) from a number of parts, except when the "synthesis" is perfect. Elsewhere, in the same treatise (533*b*), he speaks about "synthesis" of manufactures (συντιθεμένα) as one of the concerns of τέχναι. And in the *Phaedo* (92*e* - 93*a*) he treats harmony (ἁρμονία) as something produced by an act of synthesis. In these examples, synthesis is something like the process of composing or arranging objects into a structure or system, and it is not, as such, opposed to any sort of analysis. Moreover, in contrast to the term "synthesis", the term "analysis" is not part of Plato's lexicon.

This does not prevent Plato from contrasting the ideas of composition and separation in the core of his philosophy, namely in his presentation of dialectics. In the *Phaedrus* (265*c* - 266*c*) he calls "dialecticians" those, who are able to operate with "division" and "gathering" (διαίρεσις καὶ συναγωγή). By the second of these conducts, scattered ideas are grouped together, while, by the first, one idea is presented according to its natural joints. Plato's choice to use the term "συναγωγή" rather than "σύνθεσις" to indicate the first of these operations could be understood as a symptom of his will to distinguish between two different sorts of compositions: the assemblage of distinct objects in order to form a certain system (we could call "σύνθεσις") and the subsuming of distinct ideas under one of a higher type (we could call "συναγωγή"). As, for Plato, ideas are contrasted to appearances in terms of an opposition between real objects and fictitious ones, this distinction does neither correspond to the distinction between composition of objects and composition of concepts nor does it refer to subsumption of objects under concepts. As long as Plato does not dispose of concepts, both synthesis and συναγωγή operate on objects ("ideas"), but while the result of synthesis is a new object, which operates as such in a certain realm, the result of συναγωγή is the acknowledgment of a certain relation linking different ideas, which produces, as Plato says, "clearness and consistency" of discourse (*ibid.* 265*d*). According to such a conduct, we can say, for example, as Plato says, what is love, but we do not necessarily recognize the different sorts of loves, that is the different ideas which are submitted to the idea of love (but which do not compose it). This is the concern of διαίρεσις, which operates on an idea that has been made clear by συναγωγή and which recognizes its different species.

III Aristotle

III.1 Synthesis

When Aristotle in *Politics* (1294*a*, 30 - 1294*b*, 1) speaks jointly of διαίρεσις and σύνθεσις, he seems not to understand them very differently from Plato's. The διαίρεσις is the distinctive character of certain forms (namely democracy and oligarchy as forms of government), while σύνθεσις is just the composition of these forms by resulting in a new form (of government). Similarily in the *Metaphysics*, where Aristotle contrasts σύνθεσις with διαίρεσις (1027*b*, 19; and 1067*b*, 26), by respectively referring to the composition and separation of subject and predicate, or (1042*b*, 12-18) observing that differences (διαφοραί) between subjects may depend on the manner in which they are "synthesized". Aristotle in these arguments associates the notion of synthesis with an idea of separation, but he does not express the latter by the term "analysis", using respectively the Platonic term "διαίρεσις" and the term "διαφορά". Here, a synthesis is a way to produce objects (either subjects or forms), which can be distinguished (or separated) from one another in terms of the particular character of synthesis itself. Elsewhere, in *Metaphysics*, the term "synthesis" is used to indicate a particular mode of composition—which Aristotle explicitly distinguishes both from mixture (μῖξις; 1043*a*, 13; and 1092*a*, 26) and from communion (συνουσία; 1045*b*, 12)—or composition in general (1113*b*, 22; and 1114*b*, 37).

III.2 Analysis: *Analytics Prior* and *Posterior*

Thus, taken as such, the idea of synthesis seem not to suffer very deep modifications, when passing from Plato to Aristotle: both authors use it to express the composition of objects in order to form new objects. What is new in Aristotle is rather the conception of objects upon which a synthesis may operate. Like Plato, Aristotle believes that knowledge entails, as a necessary condition of it, a fundamental duality. But he substitutes for Plato's duality of real objects (that is ideas) and fictitious objects (that is appearances) the duality of matter and form, or subject and predicate, and finally, object and concept (here, ch. 12, § IV.1 and IV.2). Thus Aristotelian objects are objects of certain concepts, subjects of certain predicates, or pieces of matter with a certain form.

Therefore, in contrast to Plato, a proposition like "Socrates is mortal", for Aristotle, does not mean to say that the idea of Socrates is subsumed, in the hierarchy of ideas, under the idea of mortality, but is to say that the predicate '(to be) mortal' applies to the subject 'Socrates', or that the object 'Socrates' belongs to the extension of the concept 'mortal'. "Socrates" is here the name of an object (which functions as the subject of a predication). However, according to Aristotle, an object is not merely a piece of matter, rather it is a substance; it is a piece of matter with a certain form. And it is this form, which makes this substance just what it is. Thus, the term "Socrates", properly speaking, refers to this form, that is to a predicate or, even to a concept (here, ch. 11, § II). The question thus is the following: to what piece of matter does the form 'Socrates' apply? In different terms: what is the subject of the predicate '(to be) Socrates' or the object which belongs to the concept '(that which is) Socrates'. In answering that this object (subject or piece of matter) is just Socrates, we accept to use a concept (a form or a predicate) to indicate a piece of matter, a subject or an object. No knowledge would be possible if we were not able to do it. But no knowledge would be possible yet, if all forms, predicates or concepts were treated as pieces of matter, subjects or objects. Thus knowledge asks for a distinction between forms, predicate or concepts, which indicate pieces of matter, subjects or objects, and forms, predicate or concepts which do not. Of course, such a distinction is relative to specific acts of knowledge, since we can utter both the sentence "Socrates is mortal" and the other "this man is Socrates". Therefore, new and essentially non-Platonic problems arise at the core of the Aristotelian theory of knowledge: is it possible—in a certain epistemological context—to treat a certain form, predicate or concept as a piece of matter, a subject or an object, or is this impossible? Under which conditions is such a thing possible? What piece of matter, subject or object, is the content of this form, predicate or concept, when it is treated in such a way? In other and simpler terms: is a certain concept able to indicate an object, or a plurality of objects, or it is not?

This is not the same question as asking if one or more objects fall under a certain concept, since the latter may be possible, even though the concept is not able to indicate any objects as such. Take the example of the concept '(to be) red'. Its extension is certainly not empty in the context of our empirical knowledge. Nevertheless, it fails to indicate any empirical object as such. Nor is it the question whether a certain predicate is essential to a certain subject, or not, since it is possible that we agree in considering a certain predicate as essential to a certain subject (for example the predicate '(to be) human' for Socrates), even if we maintain that it does not indicate an objects as such.

Even though he seems to accept the intensional distinction between predicates which can indicate a subject and predicates which are essential for a certain subject, Aristotle seems to believe that no predicate can be essential for a certain subject, if it is not able to indicate an object. By essential predicates of a certain subject P Aristotle means (*Posterior Analytics* 73*a* 34 - 73*b* 3) both the predicates which belong to the essence of this subject (as the predicate '(to be a) man' belongs to the essence of Socrates). And the predicates such that if they are taken as indicating a subject, let us say Q, then the predicate which indicates the subject P

belongs to the essence of this subject Q. Thus a predicate Q is an essential predicate for a subject P if and only if either the predication "P is Q" or the predication "Q is P" are essential predications, that is: they assign to their respective subjects a predicate which belongs to the essence of them[2]. On the base of such a definition, Aristotle argues[3] in chapters I, 19 - I, 22 of *Posterior Analytics* in favor of the following thesis:

> If "P is Q" is an essential predication, and $\{P_j\}$, $\{Q_j\}$ and $\{S_j\}$ are three series of predicates respectively occurring in the series of predications:
>
> (*a*) {"P_1 is P", "P_2 is P_1", "P_3 is P_2", ...}
>
> (*b*) {"Q is Q_1", "Q_1 is Q_2", "Q_2 is Q_3", ...}
>
> (*c*) {"P is S_1", "S_1 is S_2", ..., "S_{w-1} is S_w", "S_w is Q"}
>
> then:
>
> (*i*) if the predications of the series (*a*) and (*b*) are all essential, then the series $\{P_j\}$, and $\{Q_j\}$ are finite;
>
> (*ii*) if the predications of the series (*c*) are all essential, then the series $\{S_j\}$ is finite;
>
> (*iii*) if the negations of the predications of the series (*a*), (*b*) and (*c*) are all essential, then the series $\{P_j\}$, $\{Q_j\}$ and $\{S_j\}$ are finite.

As, according to him, a proof can only contain essential predications, this means both that no proof goes on *ad infinitum*, and that there is no proof of everything (*ibid.*, 82*a*, 6-8). In the chapters I, 20 - I, 21, he argues that if (*i*) is true, then (*ii*) and (*iii*) are also true. Finally in the chapter I, 22 he argues that (*i*) is true.

At the beginning of this chapter, Aristotle states that no subject can be defined and known, if its essential predicates are infinite in number (82*b*, 37 - 83*a*, 1)[4] and that no predicate can be an essential predicate of a certain subject, if it is not able to indicate a subject, namely either the same subject to which it applies or a certain species of it (*ibid*, 83*a*, 24-25). According to the literal reading of the second of these theses, it is not possible that a predication "P is Q" is essential, if the predicate Q does not indicate a subject that is just P (since, if Q is a species of P, it is certainly not essential). However Aristotle seems to think that this predication could also be essential if the predicate Q indicates a subject of which the subject P is just a species. In any case, Aristotle is arguing that if there is no white which is just white, without besides being also something else (*ibid*, 83*a*, 30-32), then the predicate '(to be) white' cannot be essential of any subject. This means, according to Aristotle, that Platonic ideas have to be rejected, or, at least, that they can not occur in a proof (*ibid*, 83*a*, 32-33).

After this, Aristotle advances three different arguments in favor of (*i*), the third of which (*ibid.*, 84*a* 17-28) is called "analytic" (*ibid.*, 84*a* 8), and contrasted to the other two, which are said to be "logical [λόγικος]" or—as someone translate, according to Gerard of Cremona— "dialectic".

Let us look how such an argument runs. If the downward series of predicates P_i is infinite, there will be for every (natural) number j a predicate P_j, such that "P_j, is P_{j-1}" is an essential predication, thus, reascending the series, we should conclude that for every (natural) number j there is a predicate P_j such that "P_j is P" is an essential predication. But this is impossible, because it is not possible that infinitely many things belong to only one thing. Thus the conclusion is proved for the first series. The same argument works for the second series too, because if this series were infinite, there would be for every (natural) number j a predicate Q_j, such that "P is Q_j" is an essential predication, what makes definition impossible.

It is not important here whether this argument is correct or not[5]. What is important for us is that Aristotle calls this argument "analytic". What does he mean by that? Which character of this argument does he want to underline by choosing such a qualification? If we consider two further passages of the *Posterior Analytics*, where the term "analysis" occurs with a clearer meaning, two distinct answers are possible[6]. The first answer appeals to a passage of chapter I, 32 (88*b*, 15-20), where Aristotle argues that, from the obvious premise that every (right) conclusion can be proved starting from all principles, it does not follow that the principles are the same for every science. And, as a counter-example, he mentions the cases of mathematics and analysis. Clearly, the term "analysis" here refers to the science of syllogisms or, generally, the science of proof, in harmony with the title itself of Aristotle's treatises on this topic (cf. also *Metaphysics*, 1005*b*, 4). If we accept such a notion of analysis, we may assert that Aristotle's argument is analytic, because it proceeds by (implicit) syllogisms. The second answer appeals to a passage of chapter I, 12 (78*a* 6-8). There Aristotle says that if it were impossible to derive truth from falsehood, "analysis" would be easy, because it would there be necessarily convertibility (ἀντέστρεφε). Here, the term "analysis" seems to refer to deduction of knowns from unknowns, or (accepted or acceptable) premises from conclusions we are trying to prove (cf. Barnes 1975, p. 147). If we assume this is the meaning of the term "analysis", we can assert that Aristotle's argument is analytic, because it assumes that conclusions are true and deduces something that is known to be false (or accepted as false) that is: it is a *reductio ad absurdum*.

Taken separately, these two answers might be convincing. However, when compared with each other, the problem arises of how to understand their compatibility. Why is the science of proof called "analysis", if analysis is, in a different sense, regressive deduction? We can find an answer to such a question in the first lecture (*Proemium*) of Saint Thomas's commentary on the *Posterior Analytics*. Thomas's argument is the following. At the beginning of the *Metaphysics*, Aristotle says that man lives thanks to art and reason. Art is a certain order of reason, according to which human acts attain certain ends. Reason not only directs the acts of inferior parts of man, but it is an act too. Thus, there is an art of reason which enables us to order the acts of reason without mistake. This art is logic, that is thus both rational (as every art) and is about reason. Therefore, logic is divided into different parts, according to the differences of the acts of reason. There are three kinds of acts of reason. The first one is understanding of the indivisible and simple; this is the matter of Aristotle's *Categories*. The second is the act of composition or division, which produces respectively affirmative and negative judgments; this is the matter of Aristotle's *De interpretatione*. The third finally is "concerned with what is proper to reason [*secundum id quod est proprium rationis*]" and it is just the act of inference (as Thomas says: it is "*discurrere ab uno in aliud, ut per id quod est notum deveniat in cognitionem ignoti*"). Such an act in turn can be performed according to three different modalities, since reason can act with or without necessity (or certainty), and if it acts without necessity, it may attain truth or falsehood. The part of logic which treats the first kind of these modalities of reason is called "*iudicativa*" and it produces judgments which have certainty of science. Now, such a certainty is only possible if these judgments are "resolved" into the first principles (they are brought back to the certainty of the first act of reason, that is the understanding of indivisible and simple). Because of that, this part of logic is called "analysis" and is the matter of Aristotle's *Analytics*.

Generally kept back by this splendid argument is the fact that, according to Aristotle, analysis is concerned with certainty and demonstration (rather than with probability and discovery—which is, according to Thomas, the matter of Aristotle's *Topics* —or false arguments—which is the matter of Aristotle's *Sophistici elenchi*[7]). This is certainly the case: according to Aristotle, analysis is concerned with certainty and demonstration. But, if Thomas is right, as I believe he is, it is not because analysis is demonstrative, but because demonstration is necessarily analytic, that is: it guarantees the truth of the conclusions by reducing them to first principles. This does not mean that a proof of T is necessarily a deduction of (some) first principles from T, since Aristotle knows perfectly well that truth can be deduced from falsehood. The point is different and may be stated as follows. If a proposition T is given and has to be proven (or refuted), the only thing we can do is just look for first principles from which T can be deduced. Thus, if we consider a proof from the point of view of its conclusions, rather than of its principles, it is necessarily preceded by a regressive conduct that reduces these conclusions to their principles. By calling "analysis" the science of proof, Aristotle seems to insist on this aspect of proof (Ross 1949, 400), that is really the most important one, if we are concerned—as Aristotle was—with the truth of conclusions and the conditions of such a truth. Of course, if the regressive conduct consists in deducing the negation of one first principle from T, it is *ipso facto* (at least from classical,or Aristotelian point of view) a proof of $\neg T$. This is exactly the case with the previous argument, but it does not represent the general case.

Thus, when Aristotle states that his previous argument is analytic, he is referring to analysis as a regressive conduct, which brings us from certain statements

to the principles making them true (or provable)[8]. A similar idea is evoked in a short passage of the *Metaphysics* (1063*b*, 15-19), where Aristotle argues that contrary statements cannot both be true. The reason of that, he says, is evident "by analyzing the definitions of contraries into [its] principle [ἐπ' ἀρχὴν τοὺς λόγους ἀναλύουσι τοὺς τῶν ἐναντίων]". Here, analysis is a regressive conduct, which brings us from a definition to the principle that explains it, assigns to it a certain meaning.

This seems to be quite clear, but it is not yet the end of the story, since in the *Prior Analytics* Aristotle often uses (cf., as only an example, 51*a* 18-19) the term "analysis" with in a strictly different meaning (Hintikka and Remes 1974, 31). According to this meaning, analysis is reduction, or more precisely, breaking up of a certain figure of syllogism into another figure (cf. Smith 1983, 161), which enables us to know whether the syllogisms of the former figure are valid or invalid. Thus, the science of proof is concerned with regressive reduction in a twofold way. First, because proof asks for regressive reductions of conclusions to first principles, and second because a necessary condition for the correctness of a certain proof is its reducibility to the accepted figures of syllogism. It is just because the act of this double regressive reduction is an analysis, that proof is concerned with analysis: it is not analysis that is demonstrative for Aristotle—as Timmermans (1995) says, for example—but proof that is necessarily analytic.

Once again, this is not the end of the story. Before leaving the *Analytics*, let us briefly come back to the previous analytic argument. What Aristotle asserts by such an argument[9] is that no proof is possible about a certain subject, indicated by a predicate P, if the regressive series of predicates P_j which specify P, does not terminate in a predicate $P_n = A$ which can not be ulteriorly specified. Aristotle speaks of proof, but he seems to refer to knowledge in general. In our terms, he is thus asserting that no knowledge is possible if there are no concepts which are, as such, concepts of objects, rather than concepts of properties or relations; in different and more Aristotelian terms: no knowledge is possible if there are no forms which are intrinsically substances.

III.3 ANALYSIS: *NICOMACHEAN ETHICS*

Let us keep this result in mind, and consider now the famous argument of the chapters III, 3 - III, 5 of Aristotle's *Nicomachean Ethics* (1111*a*, 21 - 1113*a*, 12; cf. here, ch. 9, par. II). Here Aristotle is discussing the difference between a voluntary act (ἑκούσιον) and a choice (προαίρεσις). While a voluntary act is the act of which the moving principle is in the agent itself (111*a* 22-23), a choice is certainly a voluntary act, but it is not any kind of voluntary act. First of all, choice is neither appetite nor anger, nor wish (βούλησις). Moreover, it is neither opinion (δόξα) in general, nor a particular kind of opinion. There are different reasons for that. Two of them are: first, opinion may concern any kind of object, while choice can only be exercised on things which are in our own power; second, opinion is either true or false, whereas choice is either good or bad. Thus opinion, as such, is different from choice, even though the former either precedes or accompanies the latter. Namely (1112*a*, 15) choice is a voluntary act which has been the object of deliberation (προβεβουλευμένον[10]), the voluntary act which follows (and depend on) an act of deliberation (βούλευσις). By referring to the act of the βουλή[11], Aristotle seems to assert that choice is an act resulting from a plural, or even public or political consideration, aiming at determination of a certain action, that is the choice itself.

Now, according to the previous characterization of a voluntary act, the agent of such a deliberation can be nothing else but the subject, who operates the choice himself. But what is the object of such a deliberation, about what is it? This is the topic of chapter 5. Implicitly, the answer has already been given, since Aristotle has said above that choice can only be exercised on things that are in our own power. However, he tries now to make such an answer explicit, by extending it to any sort of deliberation, and by saying what sorts of things these things are, or are not. First of all, according to Aristotle, eternal (that is necessary[12]) things—like those which mathematics treats—are not objects of deliberation. The same is true for that which changes if it changes always in the same way—like the subject of natural motions—or without any regularity—like rain—or still according to chance—like finding a treasure. This is quite clear, since no human (or political) subject—that is the agent of a deliberation—can intervene on these things. According to Aristotle, the range of deliberation is however even narrower, since each subject only deliberates on things which he is able to modify. For example, Aristotle says, no Lacedæmonian deliberates on the Scythian government. Thus, if we are referring to deliberation, human power has to be intended as practical and political power, that is power fixed by accidental constraints and even social conventions. Moreover, deliberation does not concern ends, but only means, which are necessary to reach already fixed ends. Namely, the objects of deliberation are just two. First, if the same end can be reached by a number of distinct means, deliberation establishes, which of them entails the easier and better realization of this end. Second, if the end can be reached only in one way, it establishes the chain of means which produces this way, by descending from it, up to the actual situation of the subject.

Aristotle continues (1112*b* 20-21) "who is deliberating seems to research and analyze the way described as [it happens with] a (geometrical) figure [ὁ [...] βουλευόμενος ἔοικε ζητεῖν καὶ ἀναλύειν τὸν εἰρημένον τρόπον ὥσπερ διάγραμμα]". Here our translation is literal, but we could interpret the previous passage in this way: "who is deliberating seems to research in the way described like he were analyzing [a] (geometrical) figure". Thus Aristotle seems to intend that what has

been described is just the path of analysis. Deliberation is thus a sort of analysis, or better, analysis is the form of deliberation; it is a form of thinking, namely the form or thinking which deliberation satisfies. But how can this form be characterized in general; what is proper to it, rather than to the particular nature of deliberation? It seems that Aristotle would like to answer such a question, since he immediately remarks (1112*b*, 21-23) that, though every deliberation is a research, not every research is a deliberation—"as [it is the case of] mathematical ones [οἷον αἱ μαθηματικαί]"—and asserts (1112*b*, 23-24) that what is last in the analysis is the first "in generation [ἐν τῇ γενέσει]". The meaning of Aristotle's comparison is not completely clear. Different translations understand it in quite different ways. It seems to me, however, that Aristotle is comparing respectively deliberation with the path that brings us from the definition of a certain figure to the elements from which the construction (or generation) of this figure starts (and, implicitly, choice of the construction itself), and he is asserting that both deliberation and this path are examples of analysis. However, comparison is not identification, since the path that goes from the definition of a certain figure to the elements from which the construction of this figure begins is a mathematical research and mathematical researches are not deliberations (even though every deliberation is a research).

If this is correct, Aristotle thinks that, as long as it is a regressive reduction, analysis can be both the reduction of the definition of a geometrical figure to the elements starting from which the (geometrical) construction of this figure is possible (which I shall call a "geometrical reduction"), and the reduction of a certain end to the actually available means, from which a chain of means, bringing us to such an end, could start. In the first case, analysis brings us from a certain condition (that is not still an actual object, but only a character that a certain object should be eventually satisfy) to the actual objects starting from which another actual object satisfying the given condition will certainly (and always) be produced. In the second case, analysis brings us from the determination of a certain end (that has not been actually reached), to the means that possibly may produce such an end. While in this second sense analysis is deliberation, in the first sense it is not.

As we have just seen, a deliberation, according to Aristotle, is never about eternal (that is necessary) things and therefore, it is not accompanied by the guarantee that the end will be reached by following the chain of means that it is actually indicating. Here, Aristotle seems very close to a Platonic conception, since he seems to argue that deliberation is just a matter of opinion and not of knowledge. According to such a point of view, analysis does necessarily accompany the demonstrative necessity of mathematics. Thus, while the agent of the first sort of analysis is nothing but the mathematician, who actually knows that a certain construction is possible and that it certainly produces an object satisfying certain conditions, the agent of the second sort of analysis is the βουλή, or generally the political community that has to evaluate the risks and chances of a certain choice. I do not say that, as long as analysis is a regressive reduction, it is not necessarily a regressive deduction; still, neither do I say that analysis is necessarily neither a regressive deduction nor a regressive reduction preparing a possible deduction. What I am saying is, that analysis is not necessarily neither a regressive deduction, nor a regressive reduction preparing a successful enterprise (and thus, *a fortiori*, a demonstrative performance, as a geometrical construction is).

This is only one aspect of the question, however, since there is a further important, and, as I believe, deeper aspect both of deliberation (in Aristotle's sense) and of geometrical reduction, according to which they appear logically similar, despite the radical difference between practical reason (to which deliberation seems to belong) and purely speculative reason (to which geometrical construction seems to belong, instead). A deliberation starts with the fixation of an end and is concerned with considering suitable means to reach this end. Now, to fix an end means to present both the concept of a state of things and to state the will to realize it. Thus, in the case of deliberation, analysis terminates with the determination of a possible action that has to be performed in order to produce a certain state of things. Similarly, a geometrical reduction does not start by merely stating a definition, but only when the aim is stated to exhibit an actual object satisfying such a definition. In the case of geometrical construction, the conclusion of analysis is therefore also the determination of a possible action which has to be performed, the difference being that in the first case, the action produces, or should produce, a new state of things, while in the second case, the action permits one to exhibit a geometrical object. In the first, as in the second case, however, the result of such an action is just something which falls under the concept presented in the first stage; it is the object of this concept. Thus in both the cases, analysis is reduction of a certain concept, which is given as such (independently from the corresponding object), to the conditions of actual realization of the corresponding object, that is the conditions that make this realization actually possible (for the agent of the analysis himself).

IV Aristotelian Forms of Analysis

Following Aristotle in his arguments of *Analytics* and *Nicomachean Ethics*, we have thus encountered four examples of what he calls "analysis": the regressive conduct connected to a proof of a given statement *T* (that is its reduction to accepted principles or their negation) or to an explication of a certain definition—which we could call "reduction to principles"—, the reduction of a certain figure of syllogism to a different figure—which we could call "syllogistic reduction"—,

the geometrical reduction, and the deliberation. What do these four examples have in common?

A first answer is already implicitly contained in what I have said: they are all examples of regressive reduction[13]. Differently from the first two, the third and the fourth examples, however, are not examples of regressive reduction, because in them something is reduced to something different which is already given or known as being true or false. These are examples of regressive reduction, because they reduce a concept to certain conditions that can be satisfied in the actual situation of the subject. This observation suggests a possible generalization of the idea of regressive reduction: a reduction is regressive when: *i)* it is finite; *ii)* it is such that its last stage is a conclusive stage, a stage that could not support any further reduction (as long as analysis is always research, as Aristotle says, it is finished only when its last stage does not ask for any other research of the same kind); *iii)* the reason for it is that such a stage is the stage of the actual knowledge, disposability or possibility of the subject. Such a generalization enables us to say that every analysis is, according to Aristotle, a regressive reduction.

Another common aspect of the four previous examples is that they refer to analysis as a form of inferential thinking, rather than merely as a form of a system of sentences or statements. Even though Aristotle directly presents the third argument of chapter I, 22 of the *Posterior Analytics* as "analytic", it seems quite obvious that he means that the conduct of reasoning that follows such an argument is analytic. This is quite evident in the case of deliberation. Thus, we might say that, for Aristotle, analysis is a form of inferential thinking, that is a system, or even a chain, of (intentionally) connected acts which brings us from a certain stage to another, essentially different one. These are acts of representation and assertion of certain contents. Moreover, the representation of these contents may be meant as a certain sentence in an available language. If this is the case, their assertion is a statement in this language. By using—as I have already done above, at a number of occasions—the same term to indicate both the form and the substance of which this form is just the form, we might then say that an analysis is a system of acts of thinking, expressed by a system of statements.

For Aristotle, an analysis, following the two previous remarks, is a system of acts of thinking realizing a regressive reduction. This means that, in order to be an analysis, a system of acts of thinking has to carry one from a certain stage to an essentially different, stage. We could call the first stage, the "initial stage of analysis", and the second the "final stage of analysis". Our previous characterization of the notion of regressive reduction specifies the nature of the final stage. As long as the notion of reduction is taken for granted however, this characterization specifies neither the nature of the initial stage nor the relations between the initial and the final stage.

What seems clear from the previous examples, is that the initial stage has to include the stating of a certain aim, and the final stage has not only to be a conclusive stage, according to the previous conditions (*i*) and (*ii*), but has also to be conclusive with respect to the possibility of realization of such an aim. However the four examples differ on this point. While in deliberation and geometrical reduction a concept is given in the initial stage, in reduction to principles and syllogistic reduction, that which is given in the initial stage is an object. Thus, we have to conclude that, according to Aristotle, there are two kinds of analysis: those which start with an object (we might call them "analysis of objects") and those which start with a concept (and might be called "analysis of concepts").

Moreover, in the reduction to principles the aim is just to prove the given statement (or the classification of the given definition), in syllogistic reduction is the validation of the given inference, in deliberation it is the realization of the end characterized by the given concept, and finally in the geometrical reduction it is the exhibition of one or more objects, which satisfy the given concept. It is then clear that the aim is neither the same for all types of analysis, neither is it the same respectively for all the types of analysis of objects, nor for all the types of analysis of concepts.

Still, while in deliberation, in geometrical reduction, and in reduction to principles, as well—when this does not consist in deducing the negation of one principle starting from the given statement—analysis does not realize the aim, but merely indicates the conditions of its realization, in syllogistic reduction, and in reduction to principles—when this consists in deducing the negation of one principle starting from the given statement—analysis does realize the aim (or at least it provides all the material allowing us to say that the aim has been realized). The latter cases both are examples of analysis of objects. In them the givens are objects that have actually been exhibited to the subject. However these objects are so given that the subject ignores something about them, namely he ignores whether these objects enjoy or do not enjoy certain properties. The aim just specifies which properties they have and further states the will to know whether these objects satisfy these properties or not. Thus, by saying that in these cases an object is given, we are stressing that what is given will be considered as an object in the act of thinking (or, if you prefer, in the statement) that finally states that the aim has been reached. It seems, according to the previous examples, that, when this is the case, analysis can realize the aim alone. Now, in the case of analysis of concepts as well, the givens might be intended, in a sense, as objects, since every concept can be treated as an object and a subject just treats it in this way when taking it as being given. Nevertheless these concepts will not occur as objects in the act of thinking (or, if you prefer, in the statement) that states that the aim has been reached finally; they just occur in it as concepts. This remark should render the previous distinction between analysis of concepts and analysis of objects. Besides, it should also justify the following general conclusion: no analysis of concept can produce as such the realization of the aim occurring in its initial stage. Thus, we can refine our previous distinctions by distinguishing three different genera of analysis: analysis of concepts; analysis of objects which does not realize alone the aim occurring in its initial stage (or "non conclusive analysis of objects"); and analysis of objects which does realize alone the aim occurring in its initial stage (or "conclusive analysis of objects").

Consider first the previous examples of analysis of concepts, that is deliberation and geometrical reduction. It is obvious that the conditions of realizing the aim are not the same in the two cases. The following are two obvious necessary conditions. In the case of geometrical construction, the subject has to operate on the given object which analysis has indicated and realize the construction according to the accepted clauses. If we assume that these clauses are just the Euclidean axioms, such a construction may be intended as a synthesis, in the usual meaning of this term (cf. the next paragraph V.4): it is a construction of a new object starting from given objects. Thus, we could say that in this case, the aim occurring in the initial stage of analysis is not realized as long as no synthesis follows the analysis. In the case of deliberation the subject has to act, he must pass from deliberation to choice. In this case no one of the previous senses of the term "synthesis" seems to entitle us to say that the aim occurring in the initial stage of analysis is not realized as long as no synthesis follows the analysis. Are these two necessary conditions also sufficient? At first glance, we might say that this is not the case, since neither synthesis nor choice produces the realization of the aim, if the analysis has not indicated the correct starting point for them. Such an answer is certainly correct, but it also trivial. And triviality cannot simply be avoided by considering nothing but the case of correct analysis, since we have no general means to distinguish *a priori* between correct analysis and false analysis. The situation is quite different in the two cases. This is clear if we consider examples of geometrical construction taken from Euclid's geometry: while for deliberation we certainly do not dispose of these means, for geometrical reduction we possibly dispose of them. This remark elucidates the essential difference between deliberation and "mathematical analysis" stated by Aristotle. Besides, it makes this distinction independent of the Platonic attitude inherent in the argument of *Nicomachean Ethics*. From an intensional point of view, the correct distinction thus is the one between analysis of concepts regulated by a criterion of correctness (relatively to the aim) which operates *a priori* from the actual application of its indications (or "regulated analysis of concepts") and analysis of concepts which is not regulated by a criterion of this sort (or "non regulated analysis of concepts"). The only example of a regulated analysis of concepts Aristotle presents is an example where the aim is reached if and only if a synthesis follows the analysis.

Consider now the previous example of a non-conclusive analysis of objects. It is a reduction to principles which does not consist in deducing the negation of one principle starting from the given statement. In this example a necessary condition for the aim to be realized is that a deduction of the given statement from first principles is conducted. In this case analysis has two distinct tasks: to indicate which first principles have to be taken as starting points of this deduction, and to suggest the path of this deduction. Clearly, to do it is not to conduct the proof of the given statement. This proof demands that deduction is conducted. If analysis is nothing but a regressive deduction, the indication of the first principles which have to be taken as the starting points of the proof is obvious. In this case, the only criterion for the correctness of the analysis (relatively to the aim) is convertibility of deduction. Now, this criterion is *a priori*, in the previous sense, only if it operates on the analysis itself. Hence, it is *a priori* only if it states that analysis has to contain only inferences by equivalence. This is in general a too restrictive criterion, however, since T might be deductible from certain first principles, even if it is not equivalent to them. Nevertheless, no other *a priori* criterion for the correctness of the analysis seems to be available in this case. As long as it is a non-conclusive analysis (of objects), a reduction to principles is thus either regulated or not regulated; if it is regulated it fails, in general, to exhibit all the sufficient conditions of deduction of the given statement.

In the first as well in the second case, the realization of the aim demands that the analysis is followed by a deduction, which, according to the previous senses of this term, is not a synthesis. There is an aspect of non-conclusive reduction of principles (as it is intended by Aristotle) however, which makes it similar to geometrical construction and even suggests a generalization of the idea of synthesis which includes such a deduction. To understand this point let us come back to the very last remark of the previous paragraph III.2, where I have argued that for Aristotle no proof is possible about something that is P if there is no predicate $P_n = A$, intrinsically indicating a subject. This means that the first principles of any proof are just statements which refer to an object just given as such, rather than to an object which merely satisfies a certain concept of property (ch. 12, par. VI.2). This is to say that no proof is possible if an object is not exhibited as such. As one of the tasks of non-conclusive reduction to principles is to indicate the first principles from which the proof can start, this means that, in this case, one of the tasks of analysis is just to indicate some objects which are given as such, serving as the starting points of the proof (these objects are clearly not first principles, they are rather that about which first principles speak, since no first principle is an object given as such, being rather an object satisfying the concept 'to be known as true'). This is also true of geometrical construction: one of its tasks is to indicate a given object given as such, serving as the starting point of construction. Thus, as long as they follow an analysis, both, proof and geometrical construction, start from objects which have to be given as such, rather than as objects which satisfy certain concepts of property.

Even though there is no evidence to ascribe such a generalization to Aristotle, we might call "synthesis" any conduct of thinking that follows a non-conclusive analysis, realizing the aim occurring in the initial stage of this analysis, and starts from an object that is given as such (rather than as the object which satisfies a certain concept of property). This meaning of the term "synthesis" has become common during the modern age, but it seems to us that there is no room for it in the Greek culture of the classical age. While the notion of analysis, because of Aristotle, grows into gnoseological complexity which enables it to describe a fundamental conduct of knowledge, the notion of synthesis does not seem to suffer a similar evolution and always refers, in the Greek culture of the classical age, to the composition of given objects in order to obtain new objects, or, more in general, to the construction of new objects, starting from given objects. Moreover, when, probably in the first half of the fourth-century of the Christian era, Pappus explicitely contrasts synthesis with analysis, describing them as successive stages of a geometrical method, he does not take into account the notion of analysis in all its Aristotelian complexity. Rather, it seems that the generalization of the notion of synthesis will only occur later, when the Pappusian opposition of it to analysis will be considered in the framework of the general Aristotelian conception of the latter.

V Analysis and Synthesis According to Pappus

V.1 Pappus's Definition

At the beginning of the 7th book of *Mathematical Collection* (VII, 1-2), when Pappus expounds the method of analysis and synthesis, he seems to advance a rational reconstruction of an important fragment of Greek mathematics (here, ch. 6, par. I). He does not say merely that the "domain [or treasury] of analysis [ἀναλυόμενος; literally: being analyzed]"[14] is a certain matter (namely a matter prepared for those who, after having got usual elements, wish to gain "in the (geometrical) figures [ἐν γραμμαῖς]" the power of solving the problems which are proposed to them—and the only matter useful for that). His proposition is more complex: "Ὁ καλούμενος ἀναλυόμενος, my son Hermodorus, κατὰ σύλληψιν ἰδία τίς ἐστιν ὕλη ...". The problem is with the expressions "καλούμενος [being called]" and "κατὰ σύλληψιν [according to the comprehension]". Hintikka and Remes, following Heath, translate the first expression by "so-called" and substitute for the second the adverbial form "in short" ("The so-called Treasury of Analysis, my dear Hermodorus is, in short,..."; Heath's translation is: "The so-called ἀναλυόμενος ('Treasury of Analysis') is, to put it shortly, ..."). Jones also agrees with

Heath about the first expression, but renders the second in the verbal impersonal form "taken as a whole" ("That which is called the Domain of Analysis, my son Hermodorus, is, taken as a whole,..."). The same idea of using a verbal form to translate "κατὰ σύλληψιν" was already used by Hultsch (Pappus CH, 635). Hultsch used however a personal form for that and even the first person singular: "ut paucis comprehendam"[15]. This is also the solution advanced by Ver Eecke who translates the whole expression "ὁ καλούμενος ἀναλυόμενος" by "le champ de l'analyse" and renders "κατὰ σύλληψιν" as an auto-reference: "Le champ de l'analyse, tel que je le conçois, mon fils Hermodore, est...".[16]

From a philological point of view, Ver Eecke's solution is probably too extreme. Nevertheless it at least suggests that Pappus is here interpreting the work of Greek mathematicans of the classical age (here, ch. 6, 170, note 2), rather than expounding a method largely and explicitly employed in Greek geometry. According to such an interpretation (that is also that of Hultsch) we could even guess that, even if they applied conducts of thinking or arguments that could be intended as examples of analysis and synthesis in Pappusian sense, these mathemacians did not conceptualize them as Pappus does.

Pappus' exposition of the method of analysis and synthesis is well known (here ch. 8, par. II), and I may limit myself to some remarks (cf. also here, ch. 12, 320-321). As we have just seen, the domain of analysis is presented first as concerned with non-elementary geometrical problems. According to Pappus, this matter was treated by Euclid, Apollonius and Aristaeus the Elder, by using the method of analysis and synthesis. This method then is applied to the realization of a certain aim; and this is perfectly consistent with the Aristotelian conception of analysis. Pappus' description of the first stage of this method, that is just analysis, is also consistent with Aristotle's views[17]: analysis is presented as a way, or a path (ὁδός; ἔφοδος), which leads from the assumption of what is sought, as if it were admitted, to something that is already admitted, that is a first principle. It is thus an inverted (ἀνάπαλιν) way and, namely, it is an inverted solution or conclusion (ἀνάπαλιν λύσις). The final stage of analysis for Pappus is the initial stage of synthesis. The latter follows after the former and just considers what is given as given. It is also a way, and it is namely the inverted way of analysis. Since Pappus says: "in the synthesis, on the other hand, by inverting the way [ἐξ ὑποστροφῆς], that which has been grasped last in analysis [τὸ ἐν τῇ ἀναλύσει καταληφθὲν ὕστατον] is supposed [to be] already gotten and [its] consequences [ἑπόμενα] and prolegomena [προηγούμενα] [are] ordered according to their nature [κατὰ φύσιν τάξαντες] and [are] linked with one another to arrive, at the end [εἰς τέλος], at the construction of what is sought [τῆς τοῦ ζητουμένου κατασκευῆς]". The Greek term for "construction" is thus a cognate of the verb "κατασκευάζω", which has really a more general sense and also means "to organize", "to set out", or "to prepare", and we could generally intend—coupling it with the term "τέλος"—as referring to the realization of the aim.

Thus Pappus uses the term "synthesis" to refer generally to the argument, which follows a non-conclusive analysis and leads from the final stage of it onto the realization of the aim. Hence, the woolliness of his text has an obvious justification: he is trying to provide a general description of different sorts of processes. However, Pappus's generality goes not as far as Aristotle's. According to him, there are two types of analysis. One of them enables us to research that which is true (ζητητικὸν τἀληθοῦς) and is called "theoretical [θεωρητικόν]", while the other is able to get what was proposed (ποριστικὸν τοῦ προταθέντος) and is called "problematical [προβληματικόν]". In the first one—Pappus says—what is sought is supposed to be true, while in the second what is proposed is supposed to be known. Starting from these suppositions, the theoretical analysis brings us to something which is admitted as being true or false, while the problematical analysis brings us to something that is admitted as being possible (realizable or given) or impossible. Even though Pappus's language is very general (and also quite ambiguous and inaccurate), it seems clear that he is only concerned with geometry and believes that as long as it provides a geometrical argument, analysis is either a regression to principles or a geometrical reduction. Moreover, he seems to restrict his description to convertible analysis, since he argues that both, truth and falsehood, or possibility and impossibility, occurring respectively in the final stage of theoretical or problematical analysis, entail respectively truth and falsehood, or possibility and impossibility of the thing that is sought or proposed. The proof and the construction then are nothing but the reversal of analysis. If this is the case, synthesis only needs to exhibit proof or construction, since analysis is able to conclude, both whether the given sentence is true or false and whether the proposed definition can be satisfied or not, and to indicate the whole conduct of proof and construction. Such a strong (logical) restriction however does not appears to be consistent with Pappus's mathematical practice, nor even with the (historical and mathematical) extension he ascribes to the method of analysis and synthesis[18]. Nevertheless, Pappus's presentation makes his attitude manifest. Even though in a sense Pappus is generalizing the classical notion of synthesis as simple composition, he is, in a different sense, restricting it. Not only does he make of synthesis nothing but the prolongation of analysis, but he also considers both, analysis and synthesis, as quite codified procedures belonging to a technical domain.

V.2 Heron and/or a *Scholium* to Euclid's *Elements*

Pappus's presentation of the method of analysis and synthesis is probably not the very first one, even though it is certainly the most extensive and explicit. We can refer to two pieces of evidence to support this thesis. The first one is al-Nayrīzī's

Arabic account of certain passages from Heron's commentary on book II of the *Elements* (al-Nayrīzī ECC, 89) and the second one is an interpolation introduced at different places in the beginning of the 13th book of Euclid's *Elements* (Euclid OO, IV, 364-381). Because of the similarity between these two expositions, Heiberg (Heiberg 1903, 58) ascribed the second one also to Heron, who lived in Alexandria during the Christian era: during the first century, according to Neugebauer (1938), or during the third-century, a little earlier than Pappus (Heath 1921, II, 298-306). Knorr (1986, 355) guesses that it is successive to Pappus and merely depends on Heron's (and Pappus's) exposition, instead.

According to Gerard's translation from the Arabic[19], Heron describes analysis (*dissolutio*) as a way to answer a question: "first we set that which is in the order of thing sought [*primo ponamus illud in ordinem rei quesite*]" (al-Nayrīzī ECC, 89, 14-15), then we "reduce [it] <to that> of which the proof has already preceded [*reducemus <ad illam>, cuius probatio iam precessit*]" (*ibid.*, 89, 15-16). The synthesis (*compositio*) is then nothing but a composition: "we begin from a thing known, then we compose until the thing sought is come upon [*incipiamus a re nota, deinde componemus, donec res quesita inveniantur*]" (*ibid.*, 89, 18-19).

Heron[20] seems, like Pappus, to include in his general presentation both problematic and theoretical analysis (that is geometrical reduction and reduction to principles). But he presents, differently from Pappus, synthesis as a simple process of composition of objects, which is only consistent with the first sort of analysis. This does not prevent him from exemplifying the method by proving theorems with it, namely by applying it to the demonstration of the first thirteen theorems of book II of the *Elements* (*ibid*, 89-110).

The application of the method of analysis and synthesis to the proof of theorems is however much more clear in the interpolation to book XIII of the *Elements*. Here a proof, different from Euclid's one, is provided for each one of the propositions XIII, 1 - XIII, 5. These proofs consist of two distinct parts, the first of them being called "analysis" and the second "synthesis". Moreover, a general definition is advanced. According to this definition, "analysis, on the one hand, is the assumption of that which is sought as [if it were] admitted up [to arrive], by means of [its] consequences, to something [which is] admitted [as] true [ἀνάλυσις μὲν οὖν ἐστι λῆψις τοῦ ζητουμένον ὡς ὁμολογουμένον διὰ τῶν ἀκολούθων ἐπί τι ἀληθές ὁμολογούμενον]" (Euclid OO, IV, 364, 18-20); while "synthesis [is], on the other hand, the assumption of that which is admitted up [to arrive], by means of [its] consequences, to something [which is] admitted [as] true [σύνθεσις δὲ λῆψις τοῦ ὁμολογουμένον διὰ τῶν ἀκολούθων ἐπί τι ἀληθές ὁμολογούμενον]" (*ibid*. 366, 1-2) or, in the Theonine version, "the assumption of that which is admitted and then, the attainment (or the ending [?]), by means of [its] consequences of what is sought [λῆψις τοῦ ὁμολογουμένον διὰ τῶν ἀκολούθων ἐπί τὴν τοῦ ζητουμένου κατάληξιν ἤτοι κατάληψιν]" (*ibid*. n. 2).

Consider as an example the alternative proof of proposition XIII, 1 (*ibid*, IV, 366-369): if a segment AB (fig. 1) is cut in C (according to the construction exposed in the proposition II, 11), in such a way that AC is the mean proportional between AB and CB, and the segment DA is equal to the half of it, then the square constructed on $AC + DA$ is five times the square constructed on DA:

$$[(AB : AC = AC : CB) \wedge (AB = 2DA)] \Rightarrow \ \mathrm{Sq.}(AC + DA) = 5[\mathrm{Sq.}(DA)]$$

In modern terms, if we put $AB = K$ and $AC = x$, the antecedent provides the equation: $x^2 + Kx - K^2 = 0$, from which we have: $\left(x + \frac{K}{2}\right)^2 = 5\left(\frac{K}{2}\right)^2$, that was to be proved.

The scholiast takes both AB and AC ($< AB$) as given on the same straight line, in such a way that $AB : AC = AC : CB$ and constructs on the same straight line, but on the opposite side than AB, a segment DA, so that $AB = 2DA$. Then he assumes that

$$\mathrm{Sq.}(CD) = 5\mathrm{Sq.}(DA) \qquad (a.1)$$

and proceeds according to the following deduction:

$$\mathrm{Sq.}(CD) = \mathrm{Sq.}(DA + AC) \qquad (a.2)$$

$$\mathrm{Sq.}(CD) = \mathrm{Sq.}(DA) + \mathrm{Sq.}(AC) + 2\mathrm{Rect.}(DA, AC) \qquad (a.3)$$

$$\mathrm{Sq.}(AC) + 2\mathrm{Rect.}(DA, AC) = \mathrm{Sq.}(CD) - \mathrm{Sq.}(DA) \qquad (a.4)$$

$$\mathrm{Sq.}(AC) + 2\mathrm{Rect.}(DA, AC) = 4\mathrm{Sq}(DA) \qquad (a.5)$$

according to (a.1) and (a.4),

$$2\mathrm{Rect.}(DA, AC) = \mathrm{Rect.}(AB, AC) \qquad (a.6)$$

$$\mathrm{Sq.}(AC) = \mathrm{Rect.}(AB, CB) \qquad (a.7)$$

according to the proportion $AB : AC = AC : CB$,

$$\mathrm{Rect.}(AB, AC) + \mathrm{Rect.}(AB, CB) = 4\mathrm{Sq.}(DA) \qquad (a.8)$$

according to (a.5), (a.6) and (a.7),

$$AC + CB = AB \qquad (a.9)$$

$$\mathrm{Rect.}(AB, AC) + \mathrm{Rect.}(AB, CB) = \mathrm{Sq.}(AB) \qquad (a.10)$$

$$\mathrm{Sq.}(AB) = 4\mathrm{Sq.}(DA) \qquad (a.11)$$

according to (a.8) and (a.10).

As $(a.11)$ follows from the hypothesis $AB = 2DA$, without appealing to $(a.1)$, it is true and then $(a.1)$ entails something that is true. Thus, as analysis finishes with it, synthesis has to begin with it:

$$\text{Sq.}(AB) = 4\text{Sq.}(DA) \qquad (s.1)$$

$$\text{Sq.}(AB) = \text{Rect.}(AB, AC) + \text{Rect.}(AB, CB) \qquad (s.2)$$

$$4\text{Sq.}(DA) = 2\text{Rect.}(DA, AC) + \text{Sq.}(AC) \qquad (s.3)$$

according to $(s.1)$, $(s.2)$, $(a.6)$ and $(a.7)$ which do not depend on $(a.1)$,

$$5\text{Sq.}(AD) = \text{Sq.}(CD) \qquad (s.4)$$

according to $(s.3)$ and the figure 1 that is a part of the figure constructed by Euclid in his proof of the same XIII,1.

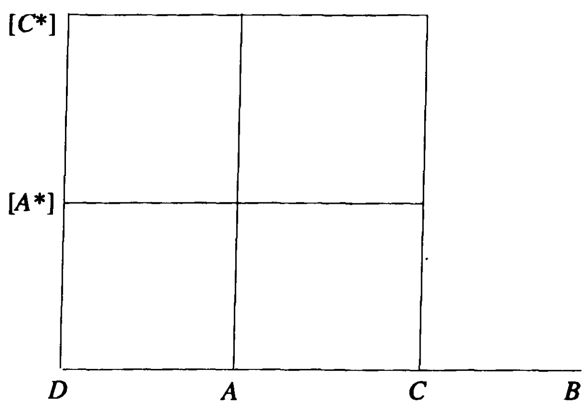


Figure 1

Clearly, the above analysis is, according to our previous terminology, an example of a non-conclusive and non-regulated[21] analysis of objects, namely, it is a non-conclusive and non-regulated reduction to principles. In its final stage, it indicates the starting point of the proof, by expressing an obvious property of an object given as such, namely the segment DB, constructed starting from AB for addition of DA, equal to a half of AB itself. Thus, taken as such, it does not include any logical novelty with respect to Aristotelian conceptions. The same is true for the proof (that is the synthesis), if it is taken as such, since it does not differ, according to its logical aspect, from common Euclidean proofs. The difference

between this proof and that proposed by Euclid for the same proposition XIII, 1 does not concern its logical character. Rather, scholiast's proof (its synthesis) is significantly simpler and wilier than Euclid's. This is clearly possible because of the indication of the analysis that suggests a good (but as such not obvious) starting point for it[22]. What the scholiast does in his interpolation thus is to apply, in a wily way, an Aristotelian indication, in order to obtain a not obvious suggestion to improve Euclid's proof. What is essentially new, with respect to Aristotelian conceptions and Euclid's mathematical practice, is both the explicit presentation of the analysis as a premise of a proof, namely as an argument suggesting the starting point of this proof; and the consequent interpretation of the proof as the second stage of a single and general method to produce (mathematical) arguments, including a heuristic as well a demonstrative aspect. Both the first and the second novelty are underlined by the use of the term "synthesis" to refer to the second stage of this method, which is nothing but what Aristotle and Euclid have called "proof".

V.3 Evidences for the application of Pappus' method in the classical age: Apollonius, Archimedes and Aristotle once again

In the 7th book of the *Collection*, Pappus argues that the method of analysis and synthesis, as he describes it, was actually working in Greek mathematics of the classical age, and namely in a large *corpus* of texts that, as a whole forms the "καλούμενος ἀναλυόμενος": Euclid's *Data*, *Porisms* and *Surface-Loci*, Apollonius's *Conics*, *Plane Loci*, *Cutting-off of a Ratio*, *Cutting-off of an Area*, *Determinate Section*, *Contacts* and *Vergings*, Aristaeus's *Solid Loci* and finally Eratosthenes' *On Means*. The aim of the 7th book of the *Collection* is to exhibit some results or lemmas (λήμματα) which should be useful to get the main results contained in them.

Unfortunately, among the treatises that Pappus mentions as part of the domain of analysis, only Euclid's *Data* has reached us in an integral Greek version. Besides, we dispose of the Greek text of the first four books of Apollonius's *Conics*, and of Arabic versions both of the books V-VII of the same treatise (the book VIII being lost) and of Apollonius' *Cutting-off of a Ratio*. All the other treatises are lost (except for few fragments).

Euclid's *Data* is concerned with the problem of determining that which can be given (constructed) if certain geometrical objects are taken as given (in magnitude, species, or position) and, according to Pappus's terminology, all its arguments seem to be typically synthetic[23].

Even though it exposes the theory of the conics "in a synthetic mode" (Knorr, 1986, 293), Apollonius's *Conics* in contrast presents many examples of conclusive reduction to principles and we can even find in this treatise some arguments like the following, which aims to prove that if from a point D (fig. 2), external to a conic section we draw both a tangent DB and a chord DEC of this conic section, and from the point B we draw an other straight line BZ that cut DEC in a point Z in such a way that $ZC : EZ = DC : DE$, then this straight line cuts the conic section in a point A such that the straight line DA is the second tangent to it, passing from D (prop. IV, 1). In order to proof this proposition—that clearly teaches as to draw the second tangent to a conic section when a tangent has been already drawn—Apollonius assumes that the tangent DA is already drawn and the straight line BA cuts the chord DEC in a point H, different from the point Z, which satisfies the previous proportion. Then, appealing to proposition III, 37—which is just the reciprocal of the proposition that he is proving—he concludes that this is absurd. Hence, he derives that the straight line BA cuts DEC in a point Z which satisfies the previous proportion and here terminates his proof.

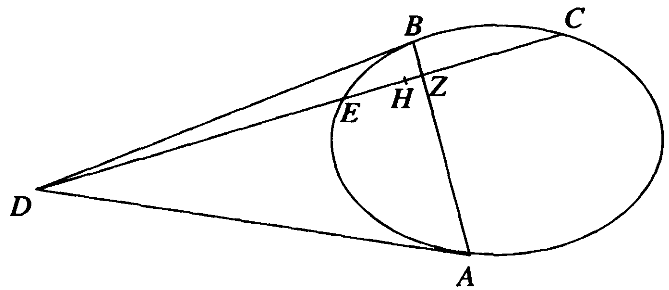


Figure 2

The logical schema of the argument is the following:

$$[\mathrm{Tg}(DB) \wedge \mathrm{Tg}(DA)] \Rightarrow (BA \text{ cuts } DEC \text{ in } Z) \qquad (1)$$

according to III, 37,

$$\mathrm{Tg}(DB) \qquad (2)$$

$$\mathrm{Tg}\ (DA) \wedge \neg(BA \text{ cuts } DEC \text{ in } Z) \qquad (3)$$

by assumption,

$$\neg(1) \qquad (4)$$

by *modus tollens*,

$$\neg(3) \qquad (5)$$

by *reductio ad absurdum*.

It is clear that (5) is not equivalent to Tg(DA) and it thus does not accomplish the proof. To prove the proposition, we still have to appeal, both to the existence and uniqueness of a second tangent and of the fourth proportional. Thus the argument (1)-(4) is not a conclusive analysis. But, according to the Aristotelian conceptions, this is no more a non-conclusive reduction to principles, except if we take it as a suggestion of starting the proof from the contemporary (hypothetical) negation of both the conjuncts of (3). In such a case we face to a non-conclusive reduction to principles, preparing a conclusive reduction to principles. This example could be taken as a symptom of a liberal use of regressive reduction as a heuristic tool in Greek geometry, but not yet as a symptom of the general application of Pappus's method to the proof of theorems.

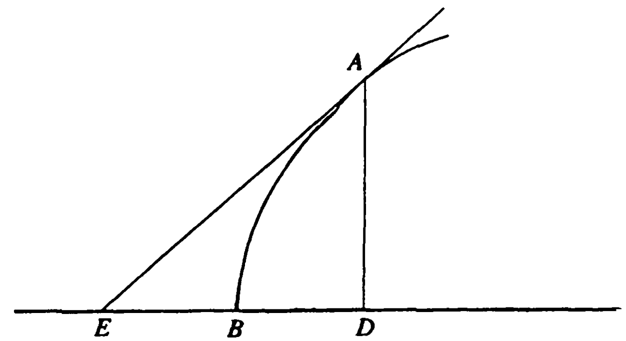


Figure 3

A number of cases of analysis and synthesis occur, in contrast, in book II (propositions II, 49-51: Apollonius GE, I, 274-305), applied to the solution of problems[24], *i. e.* to the construction of geometrical objects satisfying certain conditions. Let us consider, as a simple example, the first part of proposition II, 49, where the problem is to draw a tangent to a parabola in a certain point. Apollonius's argument runs as follows. Let AB (fig. 3) be the parabola and let the point A be given on it. Let us also assume that the tangent is traced, and let it be AE, the point E lying on the straight line prolonging the diameter of the parabola. From the point A let us draw the perpendicular AD to the diameter. As both the point A and the (diameter of the) parabola are given, the segment AD is also given in position. Beside, according to proposition I, 35 of the *Conics* themselves, EB is equal to BD. Thus as BD is given, EB is also given and, as B is given, the point E is given too. Thus the tangent is given in position. This is the first part of the argument. The second one is introduced by the phrases: "it will be synthesized in this way [Συντεθήσεται δὴ οὕτως]" (*ibid*, 274, 21)[25], and it consists of course in the presention of the obvious construction of the tangent. Let the perpendicular AD to the diameter DB be drawn and the point E taken on the straight line prolonging such a diameter in a way such that $EB = BD$. The straight line EA passing through the given points E and A will be the tangent sought.

Even though arguments of this sort are rather exceptional in the *Conics*, they are common in Apollonius' *Cutting-off of a Ratio*, as it is presented to us in the Latin translation from Arabic by E. Halley (Apollonius SRH).

Consider as an example the first problem of such a treatise (*ibid.*, 1-3). Two parallel straight lines AB and CD (fig. 4) are given in position and three points E, Z et T are given as well, the first on AB, the second on CD and the third not on these straight lines, being rather inside the angle DZH (where H is any point on the straigt line EZ after Z itself). Apollonius is searching for the position of a straight line passing from T and cutting AB and CD respectively in two points determining together with points E and Z two segments which are between them in a given ratio. He imagines first that this straight line cuts AB between E and B and CD between Z and D and calls K and L the points where it does it. He assumes these points as given and draws the right TLK. Then he draws the straight line ET which is obviously given, as both the points E and T are given. The point M of intersection of this straight line with CD is given too. Thus also the ratio Rat.(ET, MT) is given. But (for the VI, 2 of the *Elements*) this ratio is clearly equal to the ratio Rat.(EK, ML) and then this latter ratio is given. Thus, as the ratio Rat.(EK, ZL) is given, the ratio Rat.(ZL, ML) is given for composition and therefore the ratio Rat.(ZM, ML) is also given for substraction. Now, as ZM is given, this means that ML is given and thus the point L and the searched straight line TLK are given too.

After this argument is been presented, Apollonius's treatise continues with a new paragraph which is opened by the phrase: "Componetur autem Problema hoc modo" (*ibid.*, 2), and presents an actually construction of the straight line TLK, starting from two segments N and XO that are between them in the same ratio than the two segments that are determined respectivelly on AB and CD by the line sought.

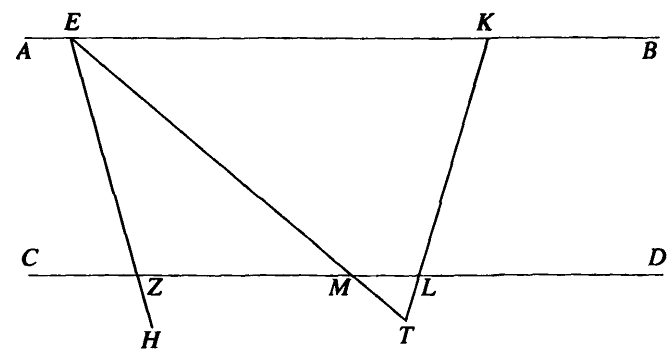


Figure 4

Even though Apollonius does not say so explicitly, the two previous constructions are then preceded by an analysis, and they are thus synthesis in Pappus's sense. Both in the first and in the second case, analysis is clearly problematical, or, if you prefer, it is just a geometrical reduction. If we consider Halley's translation from the Arabic as faithful to Apollonius's treatise, we thus have to conclude that Apollonius not only proceeded as in the Pappus's method in a short fragment of his *Conics*, but he also composed a genuine analytical treatise (in Pappus's sense). This justifies the belief that other treatises of the same Apollonius actually proceed in the same style.

Still, we can find other, similar evidences apart from Pappus' analytical corpus, in the book II of Archimedes's treatise *On the Sphere and Cylinder* (Archimedes OO, I, 168-229; cf. Knorr 1986, 170-174), for example. This is composed of nine propositions: three theorems (2, 8 and 9) and six problems (1 and 3-7). The solution of all the problems runs in two stages: the first is a classical geometrical reduction (or, in Pappus's terms, problematic analysis), while the second is a geometrical construction, explicitly presented by Archimedes himself as a synthesis[26].

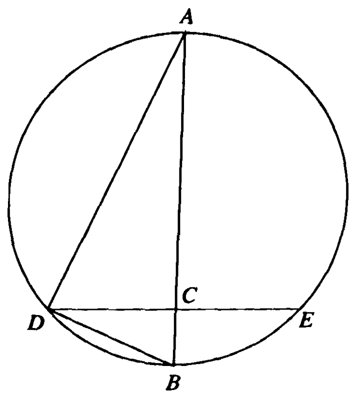


Figure 5

Let us consider as a very simple example problem 3 (*ibid*, 184-187): to cut a sphere by a plane into two segments, in such a way that the ratio between these segments is equal to a given ratio. Archimedes assumes that the plane sought cuts the great circle *ADBE* (fig. 5) of the given sphere in the points *D* and *E*, being perpendicular in *C* to the diameter *AB* of this circle, and he draws the chords *AD* and *DB*. Then he remarks that the surfaces of the segments *ADE* and *ABE* are respectively equal to the surfaces of the circles of radius *AD* and *DB* (for the propositions I, 42 and I, 43 of the same treatise), which are between them as Sq.(*AD*) and Sq.(*DB*), *i. e.*—because of the Pythagorean theorem—as *AC* and *CB*. Hence he concludes that, as the ratio between the surfaces of the segments *ADE* and *ABE* is given, the ratio between *AC* and *CB* is also given and thus the plane sought is given too. The synthesis is then obvious: it is a question of dividing the diameter *AB* by a point *C* such that the ratio between *AC* and *CB* is equal to the given ratio—which is made by a simple application of the proposition VI, 10 of the *Elements*—and of proving that this point satisfies the conditions of the original problem.

The evidence for Archimedes' application of the method of analysis and synthesis becomes even stronger, if we observe that when, in the course of the solution of problem 4, he assumes that a certain problem is solved—namely the problem of dividing a given segment so that one of its parts is to another given segment as a given surface is to the square constructed on the other part of it—, he announces that it will both be analyzed and synthesized at the and of the treatise: "ἐπὶ τέλει ἀναλυθήσεταί τε καὶ συντεθήσεται" (*ibid*, I, 192, 5-6; cf. Dijksterhuis [1956, 195]). Neither the analysis nor the synthesis are actually given by Archimedes in

his treatise, but they are reconstructed by Eutocius in his commentary, and attributed by him to Archimedes himself (Archimedes OO, III, 132-149). Besides, Eutocius also attributes in his commentary three other explicit applications of Pappus's method to mathematicians of the classical age: two to Menaechmus (*ibid.* III, 78-85) and one to Diocles (*ibid.*, III, 160-177)[27]. In all these cases Eutocius introduces the second stage of the solution by the same formula, that we can also find in Apollonius's and Archimedes' treatises: "Συντεθήσεται δὴ οὕτως" (*ibid.*, III, 136, 14; 80, 4; 82, 18; and 168, 26 respectively).

An extrinsic, but relevant argument to accept previous examples as evidence for an explicit application of the method of analysis and synthesis by Greek mathematicians of the classical era could finally come from a short passage taken from chapter 16 of Aristotle's *Sophistici Elenchi.* Here Aristotle insists on the difference between our capacity of seeing and solving the faults of an argument when we consider it and our ability in meeting it quickly in discussion. He argues, both that we often do not know at certain occasions things we know in other circumstances, and that speed and slowness in argumentation depend on training. Thus, he concludes, "sometimes it happens as with (geometrical) figures [καθάπερ ἐν τοῖς διαγράμμασιν], for there sometimes [after] having analyzed, on the other hand, we are not able to synthesize [ἀναλύσαντες ἐνίοτε συνθεῖναι πάλιν ἀδυνατοῦμεν]" (175*a*, 26-28)[28]. The verb "συντίθημι" seems to refer here to the actual construction of the figure after analysis has shown the starting point of it. Thus we could imagine that it occurs here in its common sense in Greek common language and merely indicates a composition of objects in order to produce an object. But it is also possible that Aristotle actually refers to a common procedure in geometry, namely the procedure of analysis and synthesis (Hintikka and Remes, 1974, 87).

V.4 Coming Back to Pappus

The previous examples should be sufficient evidence to support a historical hypothesis: Greek mathematicians of the classical age actually applied a two-stage method to solve problems[29], coupling the construction of mathematical objects which satisfy certain conditions, with a previous geometrical reduction, which indicated to them both a starting point and a plan for construction. This thesis is perfectly consistent with our previous interpretation of Aristotle's comparison of analysis and deliberation in chapter III, 5 of the *Nicomachean Ethics.* However, this comparison disagrees with previous examples of the use of the terms "analysis" and "synthesis". While Aristotle uses only the first, the second occurs very prominently both in Apollonius's and in Archimedes' arguments. Such a prominent occurrence of the second term might perhaps not be very significant, since this term here has a meaning that is very close to the common meaning. Even though here synthesis is not strictly a composition of given objects which form—because of this composition itself—a new object, it is nothing but a construction of a new object, which starts from given objects and follows accepted constructive clauses. The almost complete absence of the term "analysis" in Apollonius's and Archimedes' arguments might in contrast indicate a deep difference between Aristotelian conception of analysis as form of thinking and the conceptualization of a geometrical procedure, consisting in the investigation of that which is given when the objects sought are taken as given[30] and aiming at the individuation, both of a starting point and of a plan for construction. It could be the case that the term "analysis" was used in the classical age to refer to the Aristotelian notion but not, or not frequently, to this geometrical procedure.

If this were the case, the two previously mentioned passages from Aristotle's *Nicomachean Ethics* and *Sophistici Elenchi* would contain, as a philosophical judgment, the acknowledgment of the analytical nature of this geometrical procedure. From such a point of view, Pappus's general description of the method of analysis and synthesis seems to occupy a middle position between Aristotle's conceptions and mathematical practice[31]. Even though Pappus uses the term 'analysis' to refer to this geometrical procedure (that is just a geometrical reduction), he assignes to it a very specific and technical meaning. However this meaning is wide enough such that the term "analysis" also refers to Aristotelian reduction to principles. Moreover Pappus's description associates—as Aristotle did—under the same term of "theoretical analysis", both reduction *ad absurdum* (or conclusive reduction to principles) and non-conclusive reduction to principles. Thus it actually unifies three procedures. The mathematical relevance of such an unification is understandable when we observe that the third of these procedures (namely non-conclusive reduction to principles) is almost absent from the geometrical practice of the classical age. Besides, the previous example, taken from the *scholium* to book XIII of the *Elememts* makes manifest the technical gain of applying non-conclusive reduction to principles to the proof of geometrical theorems. Even though al-Nayrīzī's commentary seems to show that this is not an original idea of Pappus, the available evidence seems to confirm that it is nevertheless an acquisition of Pappus' time, at least if we assume that his *scholium* goes back to that time (like it should be the case if Heiberg and Heath are respectively right in ascribing it to Heron and in guessing that Heron lived in the third century). The passage from the idea of synthesis, as simple composition or construction, to the idea of synthesis, as an inferential procedure following an analysis, seems to be joined by such a later acquisition.

Nevertheless, the interest of Pappus's description is not exausted by that. It is also concerned with the idea of synthesis as reconstruction of the natural order. As we have just seen, Pappus uses the properly Aristotelian term "φύσις", saying that synthesis orders the consequences and the prolegomena of the givens "κατὰ φύσιν". He not merely refers to a logically correct order here, but appeals to the reality of nature. A comparison with the very beginning of Aristotle's *Physics* is thus unavoidable. There (184*a*, 16 - 184*b*, 14) Aristotle argues (184*a*, 16-18) that the way (ὁδός) of knowledge goes from that whch is more knowable and clear to us, up to that which is more knowable and clearer by nature (τῇ φύσει) and specifies (184*a* 21-26) that what is manifest and clearer to us is what is more confused (τὰ συγκεχυμένα μᾶλλον), or the whole (ὅλον). It is only afterwards, he adds, that, starting from it, the elements and principles (τὰ στοιχεῖα αἱ καὶ ἀρχαὶ) become known, by division. Finally he concludes that (in knowledge) we have to proceed "from the general to the particular [ἐκ τῶν καθόλου ἐπὶ τὰ καθ' ἕκαστα]", since the general is a sort of whole, because it contains a plurality of things (πολλὰ) as (its) parts. Aristotle's term for "division" is not "ἀνάλυσις", but "διαίρεσις", and there is a reason for that. In fact, as long as it is the way of knowledge in the Aristotelian sense, division goes from what is given for us to what we seek, from the object that is given as such, to the conditions of its realization. This proceeding is exactly the opposite of a regressive reduction. Nevertheless, Aristotle's description has been understood during the Latin and modern ages as a typical characterization of analysis (and the term "διαίρεσις" has often been translated by "analysis" or "resolutio").

Pappus's reference to the notion of nature provides a key to understand such a shifting. It seems just to result from an inversion of the Aristotelian point of view, according to which what is given as such is not that which is given to us as such. Rather, it is that which is given as such in itself (or in the eternity of truth). Thus the problem becomes one of understanding what is given to us as such, according to the eternal truth of what is given as such in itself, that means to represent it to ourselves as a system or even a collection of parts or properties; these parts or properties being intended as first elements, which are given as such in themselves. I am not arguing that Pappus actually realizes such an inversion (that is quite natural from a Platonic point of view, and particularly with respect to mathematical matters). I am merely observing that Pappus's argument seems to suggest such a possibility or may even be suggested by it[32]. In this non-Aristotelian sense, analysis and synthesis of course come together, since the "resolution" of an object into its elements or parts asks for its reconstitution, according to the nature (or even its nature). However, such a reconstitution (that is just a synthesis in the original sense of this term) is not necessary for the realization of the aim, because the problem was that of understanding the object, not that of reconstructing it as such. As long as we realize the synthesis, it is nothing but a repetition of a process that has to have occurred already in nature. Thus, a new sort of conclusive analysis of objects arises. And, even though its notion is definitely not Aristotelian, it may be characterized in terms that refer to Aristotle's *Physics*. We might call it "reduction to elements"[33].

VI Thomas

Aristotle's exposition of the way of knowledge in the beginning of the *Physics* was one of the major references for medieval conceptions of "*resolutio*" (that is "*re-solutio*": "ἀνα-λύσις") and "*compositio*" (that is "*cum-positio*":"συν-θέσις"). According to B. Gerceau[34] (1968, 217) it is, for example, just on the background of this text that Albertus Magnus, the master of Thomas, read Chalcidius's commentary to Plato's *Timaeus*, where these notions are discussed. This means both that he understands them as referring to the process of knowledge—rather than to the order of cosmological reality—and that he considers that *resolutio* brings us from what is first in our knowledge to what is first as such. Hence, the latter is (from the point of view of the cognitive subject) an upward conduct bringing us from the complex in itself, but first for us, to the simple in itself[35], but last for us. In different terms, it is just a reduction to elements. However, what is complex in itself (and first for us), is the individual as such, while what is simple in itself (and the last for us) is that which makes the individual belong to a certain species; thus *resolutio* brings us from the individual to the species. Still, the individual is a whole, while its elements are parts of it, hence *resolutio* goes also from the whole to its parts. Finally, if the reference is not to a single act of knowledge, but to human knowledge as such, the individual is part of multiple and the species is unity, thus, *resolutio* goes from multiple to unity, as Thomas says in *De Trinitate* (qu. 6, a.1, c.). The *compositio* is then (still for the point of view of the cognitive subject) a downward conduct, bringing from the simple in itself to the complex in itself, from the universal as a principle, to the individual, from the parts to the whole, from the one to the multiple.

Even though this conception inverts the extensional order of Aristotelian analysis, it does not invert its logic (or one of the intensional orders that characterizes Aristotelian analysis): analysis always proceeds regressively from the last to the first, from the not given to the given, from the problem to its solution, or to the conditions of the solution. Moreover, it is a conduct of thinking, a way of knowledge.

This is however not the only sense ascribed to the pair "*resolutio-compositio*" in the 13th-century philosophy. An further sense comes up with Peter of Spain, from the eclectic views exposed by Boethius in his *Commentary* on Porphyry's *Isagoge*, where Platonic and Aristotelian conceptions are applied together to provide a complex representation of logic (Garceau 1968, 210-213). According to Boethius, there are two different but complementary ways of distinguishing the different parts of logic: either these parts are *definitio*, *partitio* and *collectio*, or they are *inventio* and *judicium*. While the second distinction comes from Aristotle, passing through Cicero's *Topics*, the first refers to *Phaedrus's* distinction between διαίρεσις and συναγωγή. The complementarity of these distinctions appears when Boethius argues that *inventio* provides material for *definitio*, *partitio* and *collectio*—which includes, in turn, *demonstratio*, *dialectica* and *sophistica*, dealing respectively with necessary, probable or false arguments—while *judicium* determines whether we are well defined and divided, whether our arguments are necessary, probable or false and whether they are linked by inferential relations, or not. In this way, Plato's distinction between διαίρεσις and συναγωγή is grafted onto an Aristotelian schema. It is hence not surprising that Peter of Spain, more than seven centuries later, in his commentary on *De anima* (*Quæst. Præemb.*) interpreted the ideas of *resolutio* and *compositio* as referring to Plato's dialectic—by effacing the essential distinction between συναγωγή and σύνθεσις. *Resolutio* becomes, in this frame, a downward path bringing us from the genus to the species, from the one to the multiple, while *compositio* becomes an upward path bringing us from the species to the genus, from the multiple to the one.

Even though, in this way, Peter of Spain agrees with Aristotle on the regressive nature of analysis, he seems to change the point of view from which analysis is considered. Analysis is not regressive because it brings us from the last to the first, from the not given to the given. It is regressive because it goes from the higher to the lower. It is not a way of knowledge, but a sense in the disposition of being.

In *Quæstio* 14 of *Summa, prima secundæ* (a. 5) Thomas treats the following question: "does deliberation [*consilum*] proceed by *resolutorio* order?" In the first objection, he argues that this cannot always be the case, since deliberation "is concerned with that which is done by us [*est de his quæ a nobis aguntur*]" and our operations (*operationes*) proceed more *modo compositivo*, than *modo resolutorio*; that is, according to Albertus's views: they go *de simplicibus ad composita*. Still, in the second objection, he adds that deliberation is an *inquisitio rationis*, and, according to the most convenient order, reason "begins with that which is prior and goes to that which is posterior [*a prioribus incipit, et ad posteriora devenit*]", such that deliberation has to go from the present (that is prior), to the future (that is posterior), and not *viceversa.* As Thomas refers just to chapter III.5 of the *Nicomachean Ethics*, his answer is obviously positive: deliberation does proceed according to the *resolutorio* order. The argument implicitly refers to the beginning of the *Physics*. We can consider prior and posterior—he argues—either with respect the order of knowledge (*cognitione*) or to the order of being (*esse*). If what is anterior in the first order were also anterior in the second, deliberation would be *compositiva.* But it is not always so, and it is particularly not so in the case of deliberation, where the end (*finis*) is prior in intention (*intentio*), but posterior in being. Thus, deliberation is *resolutiva*. The solutions of the previous objections are not essentially different: deliberation deals with operations and "order of reasoning about operations is contrary to the order of operating [*ordo ratiocinandi de operationibus, est contrarius ordini operandi*]"; reason starts from what is prior for reason (*secundum rationis*), but not always from what is prior in time.

Six orders are mentioned in this argument: the order of knowledge, the order of reason, the order of being, the order of time, the order of intention and the order of (human) operations (or acts). Deliberation—says Thomas—proceeds analytically, since it goes from what is the last in the order of acts to that which is the first in the same order. As deliberation is an *inquisitio* of reason, it has to go from what is first for reason to that which is the last for reason. But when reason applies to action, what is first for reason, is the last in the order of acts. This is just our end. Certainly, it is also the last in the order of time, while it is the first in the order of intention. Moreover, it seems to be, according to Thomas's argument, the first in the order of knowledge, and, if it is so, it is then the last in the order of being too. Therefore for Thomas, deliberation is an example of analysis, since it brings us from the last in the order of being (acts and time) to the first in the order of knowledge (intention) and reason, whereas, for Aristotle, it was an example of analysis, since it brought us from what is given to us as the object of a certain concept (that is just the end), to what is given to us as such (the act we can perform here and now). Thus if Thomas' conclusion is the same as Aristotle's, it is because of the fact that in deliberation knowledge is nothing but a means for action, and it is not intended as such (*ibid.*, I-II, qu. 14, a. 3). Such a remark enables Thomas to accept Aristotle's thesis of chapter III.5 of *Nicomachean Ethics*, by appealing to an argument that is similar to the one Aristotle advances at the beginning of the *Physics*. However such a double agreement stands on many differences. Nevertheless, on two essential points Aristotle and Thomas agree: for both of them, analysis is a regressive conduct of thinking (or reason, according to Thomas' terminology); this conduct can be applied in order to reduce either concepts to the conditions of their satisfaction or aims to the conditions of their realization.

The same tension between the point of view of knowledge and the point of view of being appears when we consider Thomas's conception of relations between the pairs *resolutio-compositio* and *inventio-judicium* (Garceau, 1968, 218-220). As a matter of fact, Thomas sometimes identifies *resolutio* with *judicium* and *compositio* with *inventio*, and at other occasions identifies *resolutio* with *inventio* and *compositio* with *judicium*.

He states the first double identification, when he speaks from the point of view of knowledge and considers *inventio* as a research for conclusions, starting from principles, and *judicium* as an evaluation of conclusions in the light of principles[36]. This seems to be the case of the *Proemio* of the commentary to the *Posterior Analytics* I have quoted before. Now, Thomas is here properly concerned with the conduct of reason that brings us to the act of judging, rather than with this act as such. According to Garceau (*ibid.*) this is also the case of the other occurrences of the first double identification in Thomas's writings. If this is correct, Thomas asserts that the act of judging is prepared by an analysis. *Quaestiones* 13 and 14 of *Summa, prima secundæ* are even more explicit. In the latter (a. 1), Thomas argues that in doubtful and uncertain matters, reason does not pronounce a judgment (*profert iudicium*) without previous *inquistione* "concerned with [his] choice [*de eligendis*]", which is just said "deliberation". Thus, he says that the act of pronouncing a judgment is a sort of choice (*electio*), that is formally an act of reason, but being substantially an act of will, instead (qu. 13, a. 1). If we accept that synthesis is just what comes after analysis and is made possible by it, we can conclude that the act of judging is a synthesis, it is made possible by an analysis and can even express an act of will, as choice is. In this sense, synthesis is no more, strictly speaking, a conduct of thinking or a way bringing us from a certain stage to a different one. It is a singular act of reason which closes an analysis and eventually expresses a will. Whether a judgment, in turn, then is either analytic or synthetic, cannot depend on the nature of this act, but on the characters of the analysis which leads to it. This exactly seems to be the idea of Kant (here, ch. 12).

Thomas states the second double identification instead, when he speaks from the point of view of the intrinsic nature of being, which the results of *inventio* and *judicium* express or identify. From such a point of view, *inventio* assumes the character of an analysis, since it reduces what is the first for us, but the last and the most complex in itself, to the intrinsic simplicity of its principles. *Judicium*, in contrast, is a synthesis, since by it the intrinsic complexity of reality is understood, starting from the intrinsic simplicity of principles. In this case, the term "*judicium*" clearly refers to the act of judgment as such.

Thus, from the point of view of judgment, the two previous double identifications do not contradict one another: in both the cases, the act of judgment seems to be intended as an act of synthesis, preceded and prepared by an analytic conduct.

VII Viète and Descartes

Pappus' characterization of the mathematical method of analysis and synthesis and medieval doctrines of *resolutio et compositio*, in their relations with Thomas's theory of judgment, seem to be the two gateways through which the Aristotelian idea of analysis enters the modern age. By passing through both these gateways it comes to be associated to a non-Aristotelian idea of synthesis, which generalizes Plato's and even a pre-Platonic conception of synthesis as mere composition of objects (by integrating Plato's conception of συναγωγή, for example), but also restricts its range, just because of this association. Still, by passing through the first of these gateways, it is both clarified—or even codified—and restricted to the specific domain of geometry; namely it is identified with nothing but geometrical reduction and reduction to principles. Finally, by passing through the second gateway, it loses its Aristotelian purity, both being confounded with διαίρεσις (and integrating, in such a way, Plato's dialectic) and being projected on a plurality of distinct orders, often opposite each other (here, ch. 13, par. II).

In coming out from the first gateway, Aristotelian idea of analysis is met by Viète, who goes on to formulate a very ambitious program: to apply to geometry both methods and results of Diophantus' arithmetic (ch. 3, par II.1). This program is clearly expounded in the *Isagoge* (1591) and partially realized in a number of works published later.

Application to geometry of arithmetical technics was impeded in pre-modern mathematics for different reasons, the most important of which was probably the absence of a general definition of internal multiplication between geometrical magnitudes. Though for Greek mathematicians (integral positive) numbers could be multiplied with each other and into any sort of magnitude, the same was not true with respect to magnitudes in general. Construction of squares, rectangles, cubes or parallelepipeds was of course intended as particular analogues to the multiplication of numbers, when two or three segments were involved. This was not, however, a general definition of multiplication for geometrical quantities. Moreover, such a geometrical "*proto*-multiplication" was not conservative with respect to homogeneity, by producing a result that could be added neither to its factors, nor to any other magnitude of the same kind. Viète's basic idea, to pass beyond such a difficulty was to provide a *quasi*-axiomatic definition of multiplication as a general operation on quantities (both numbers and magnitudes). In this way, he enabled himself to pass from proportions between geometrical magnitudes like $a : b = A : B$, to equations like $aB = bA$, and to express different sorts of problems involving geometrical magnitudes in terms of equations. In order to accomplish that, Viète proposed to use a genuinely analytic procedure.

In the very beginning of the *Isagoge* he defines analysis as a "certain way to search for the truth in mathematics [*veritatis inquendæ via quædam in Mathematicis*]" (Viète 1591*a*, 4r). He mentions the opinion according to which Plato was the first to come upon (*invenire*) it[37]; he ascribes to Theon (who lived in Alexandria in the 4th-century A. D.) the merit of being the first to call this way "analysis", and asserts that he is just quoting Theon's definition. It is possible that Viète refers here to the *scholium* of book XIII of Euclid's *Elements* in the form it takes in the Theonine version[38]. Analysis, he says, is "the assumption of what is questioned as if it were admitted, [in order to arrive], by means of [its] consequences, to what is admitted to be true [*adsumptio quæsiti tanquam concessi per consequentia ad verum concessum*]"; in contrast (*ut contrà*), synthesis is "the

assumption of what is admitted, [in order to arrive], by means of [its] consequences, to the end and comprehension of that which is questioned [*adsumptio concessi per consequentia ad quæsiti finem & comprehensionem*]" (*ibid.*). The use of the terms "*finem*" and "*comprehensionem*" is perfectly consistent with the Aristotelian conception of analysis, as I have presented it, and serves Viète's program too. In fact, though he mentions the two kinds of analysis distinguished by Pappus (by calling them "ζητητική" and "ποριστική")—saying that the previous definition is perfectly pertinent for them—and even asserts to have added a third kind to them, which he calls "ῥητιξή" (from "ῥέω": to flow; but also: to explain) or "ἰεξηγητική" (from "ἐξηγέομαι": to conduct up to the end, to explain, or to expose), he profoundly changes the intended sense of Pappus's distinction. Far from being three distinct species of the same genus, Viète's zetetics, poristics and exegetics (or rhetics) are three successive stages of the same conduct. According to Viète's general definition, in the first stage "an equation or a proportion is obtained between the magnitude which are sought and that which is given [*invenitur æqualitas proportiove magnitudinis, de quâ quæritur, cum iis quæ data sunt*]"; in the second one "the truth of the theorem concerning with the equation or proportion [*de æqualitate vel proportione ordinati Theorematis veritas examinatur*]"; and finally in the third one "the magnitude is exhibited [starting] from the equation or proportion about what is questioned [*ex ordinata æqualitate vel proportione ipsa de qua quæritur exhibetur magnitudo*]" (*ibid*). However, zetetics more properly consists in transforming the given problem in an equation, eventually by passing from one or more proportions, and in solving it; clearly it is an analytic procedure. Poristics consists in verification of the conclusions of zetetics; it can be as such—as we shall see later—either an analytic or a synthetic procedure. Finally, exegetics consists in the exhibition of the searched magnitude; it is certainly a synthetic proceduire .

In order to understand the relations among the three stages of Viète's methods, we have to investigate the nature of zetetics. As long as it is expounded in general terms, Viète's idea is quite simple. If a problem is advanced according to which certain magnitudes are sought, he proposes to assume that these magnitudes are given and to indicate them with certain letters (Viète actually uses capital vowels (here, ch. 6, notr 13), but we can use the last letters of the Latin alphabet, as we normally do). Then he proposes to work on these magnitudes as if they were actually given, in order to translate, according to the new definition of geometrical multiplication, the conditions of the problem in a certain equation that could be solved according to the usual arithmetical technics, or transformed into a new proportion. Imagine that this problem asks for the construction of two segments which should form a rectangle equal to a certain square B and should have the same ratio as two other segments S and R, which is a particular case of the zetitic II,1 (Viète 1591*b*, lib. II, z. 1). If these segments are called x and y, we have the proportion $x : y = S : R$ and thus, according to Viète (but in modern notation), $x = \frac{Sy}{R}$ and $y = \frac{Rx}{S}$ and then $B = \frac{Sy^2}{R} = \frac{Rx^2}{S}$ or $Sy^2 = RB$ and $Rx^2 = SB$. Even if these equations were solved, as if they were usual arithmetic equations, the exhibition of their roots would not yet be the construction of the segments sought. Therefore, it is not the exhibition of the solution of the problem. The situation does not change if we transform these equations into two corresponding proportions, as Viète actually does. We have then, respectively, the proportions $S : R = B : y^2$ and $R : S = B : x^2$ which do not exhibit as such the segments sought. When we face the roots of the previous equations or the proportions that correspond to the latter, in both cases two problems remain still open: first to verify, starting from the magnitudes that are actually given, whether the relations expressed by these roots or proportions are correct, and second to interpret either of these roots or these proportions as suitable suggestions to actually realize the construction we were seeking. Poristics should solve the first problem, exegetics should solve the second one.

We have just asserted that the first stage of Viète method is an example of analysis. The reason is clear: it is a conduct of thinking, responding to a certain aim, which starts from the hypothesis that a certain object, which is only presented as the object of a certain concept, is given a such, and runs by assuming that we can actually operate on and with such an object. This is also the case of Aristotelian geometrical construction. However zetetics does not bring us from this hypothesis to the exhibition of an object that is given as such, rather it terminates as soon as the object which is sought is presented as the object of a new concept: the concept of root of a certain equation, or, to be more precise, the concept of being the (geometrical) magnitude that is expressed by a root of a certain equation. Thus it is not strictly a regressive conduct, since it does not regress from that which is not given as such to that which is just given as such. Rather, it exploits the admission occurring in its first stage to exhibit a certain operational relation, that was unknown before, between the object sought and the objects given as such. Therefore, as long as it is not a regressive conduct, Viète's zetetics is a way to come upon a certain relational configuration that was unknown before. Even though it is not conclusive with respect to the aim occurring in its first stage, it is conclusive with respect to a different aim, which is just that of exhibiting such a configuration. It is then an example of a new, non-Aristotelian sort of analysis, that we might call a "configurational analysis".

Because of the particular nature of this analysis—and inspite of Viète's declaration in the chapter VI of *Isagoge* (Viète 1591*a*, 8r), where it is described as a sort of synthesis—the stage which follows zetetics can be either an analytic or a synthetic procedure. The reason is clear: its specific aim is proving a theorem—that is just the conclusion of zetetic—and it can do this either by a conclusive reduction to principles or by a synthetic proof that can eventually be preceded by a non-conclusive reduction to principles. However, both in the first and in the second case, poristics does not realize the aim occurring in the first stage of zetetics. Rather, it is conclusive only with respect to an intermediary aim, which is just the specific aim the conclusions of zetetics leave to it. The task of realizing the principal aim is thus left to the third stage, that is exegetics.

Even though, exegetics thus is a geometrical construction and is then, because of its logical form, a quite normal synthesis, its connection with the previous analysis is not the same that in Pappusian and Medieval examples. In fact it does not start from the object that analysis has indicated. As long as it starts from the final stage of analysis (that is the final stage of zetetics), it has to interpret the final stage of analysis; namely, it has to transform the expression of a certain relational configuration into a suggestion for a geometrical construction. Thus, at its very beginning, it has to proceed as analysis does, starting from the presentation of a certain concept and seeking for the first elements of construction. This is specifically difficult, because of the non-geometrical character of the configuration exhibited by zetetics. In fact, in Viète's method, zetetics realizes its specific aim and exhibit such a configuration, thanks to a *quasi*-axiomatic definition of internal multiplication. But, even though such a definition enables mathematicians to write equations where products (and ratios) of magnitudes occur and to manipulate them, it does not specify what a product (or a ratio) of magnitudes is. This is the source of one of the main difficulties of Viète's program, since, in order to assign a geometrical sense to his equations and their roots, Viète proposes to interpret them according to a generalization of the classical definition of product of segments as constructions of rectangles or parallelepipeds. The problem with this suggestion is twofold. First, such a definition does not work for any sort of geometrical magnitude. Second it forces us to distinguish magnitudes according to their order with respect to a certain base, since, according to the previous definition, internal multiplication between segments is not conservative with respect to homogeneity.

This difficulty is one of the starting points of Descartes' program in geometry. Many scholars have underlined that Cartesian geometry is nothing but a collection of methods to solve geometrical problems. I do not believe this is the case. Rather, I think that the aim motivating Descartes' *Geometry* was a new foundation of geometry as a whole. And such a foundation makes an essential appeal to the analytic way of thinking. This is the last stage in the history of the notion of analysis I shall consider here, since it is the first stage of a new era, where the original Aristotelian notion gets its modern character.

As is well known, Descartes, in his *Discours de la méthode*, contrasts the "*Analyse des anciens*" and the "*Algèbre des modernes*" (1637, 19). He refers to them as two "arts" and considers them together with a third "art", that is logic. His famous four precepts (*ibid.*, 20) are expounded by him as the only "laws" of a method that "comprenant les avantages de ces trois [arts], fust exempte de leurs defaux" (*ibid.*, 19). The first and the third precept seem to recommend a quite non-analytic conduct of thinking: never accept as true anything of which we do not have evidence; always start with the simplest and the most easily knowable objects in thinking and proceed step by step upwards to the knowledge of the most composed ones. Though such an apparent refusal of analysis seems to be balanced by the second precept, this precept does not really recommend an analytic conduct, limiting itself to suggest to always divide any difficulty in as many "particles" as possible. Such an attitude seems to be inconsistent with the equally famous precept of the *Géométrie*, which in contrast recommends a very analytic conduct:

> "Ansi, voulant resoudre quelque problesme, on doit d'abord le considerer comme desia fait, & donner des nommes a toutes les lignes qui semblent necessarires pour les construire, aussi bien à celles qui sont inconnuës qu'aux autres" (*ibid.*, 300; cf. here, ch. 1, 25-26 and ch. 8, 208).

The contrast appears to be even more evident when we observe that, just after having set forth his four precepts, Descartes presents a very short abstract of his geometry, as an example of his method.

How can these precepts for a good conduct of thinking be rendered consistent? The answer depends on Descartes' conception of his method, as a combination of the advantages of (Aristotelian) logic, classical geometry (which, by referring to Pappus' interpretation of it, he calls "analysis of the ancients") and of what he calls "the algebra of moderns". From the first of these "arts"—which he here understands as the art of conducting logical proofs—Descartes takes the progressivity of thinking and the certainty of the starting points. From the second, he takes both the modalities of giveness of objects and the conditions of their possible comparison. Finally, from the third, he takes the modalities of expressing both objects and operations and the agility of deduction that these modalities permit; in fact, when he speaks of "algebra", he seems to refer to the modern (for him) technics of transforming and solving equations. The key to understanding Descartes' point of view seems just to lie in the previous distinction between modalities of giveness and comparison and modalities of expression. This distinction is already visible in Viète's program, the aim of which is just to find a way to work with the "algebraic technics" on certain expressions of geometrical objects, in order to obtain suitable suggestions to perform classical constructions. However, in Descartes's new geometry it seems to become much more explicit.

In following Israel's suggestion (here, ch. 1), we might come back to the *Regulæ* in order to understand Descartes' views. In the *Regula* XIV (Descartes AT, X, 450-52), Descartes states that there are only two sorts of things which compare themselves to each other (the Latin verb is "*confero*": literally "to bring together", and it is used here in the passive form): multitudes and magnitudes. And he adds that we dispose of two sorts (*genara*) of figures "to conceive them [*ad illas conceptui nostro proponendas*]". The first type of these figures are diagrams (as systems of points or genealogical trees), the second are geometrical figures. By using Descartes' terms, they respectively "exhibit [or are to exhibit: *exhibenda*]" multitudes and "explicate [*explicant*]" magnitudes. Among all the possible classes of figures of these sorts, Descartes wants to choose only one and use its elements as general representations of multitudes and magnitudes. In order to justify his choice, he remarks that all the conditions (*habitudines*) which can subsist (*esse*) between entities of the same genus (that is all the relations between such entities) refer (*esse referenda*) either to order or to measure. Then he states that measure essentially differs from order because of the necessity of the consideration of a third term, when two entities are compared according to it (which is not the case of order). Finally, he argues that "as far as a unity is assumed [*beneficio unitatis assumptiæ*]" magnitudes can be reduced to multitudes, and the multitudes of unities can be disposed in such an order, that every difficulty "concerning the knowledge of measure [*quæ ad mensuræ cognitionem pertinebat*]" only depends on order. Starting from these premises, Descartes concludes that, as long as it is question of proportions between magnitudes, only segments can be considered and that the same figures can be used to exhibit both, multitudes and magnitudes.

Descartes' argument may appear rather obscure, but it becomes very clear as soon as it is considered in connection with his geometry. What Descartes says, is that if a certain magnitude is assumed as a parameter to measure all the other magnitudes of the same genus (a unity of measure), then the essential difference between comparison by order and comparison by measure—that is just the necessity of a third term—fails, since the third term is given already once for all. Thus, it is possible to intend any proportion as a relation with respect to the order and pass from it to a usual identity. Namely, as he will teach in the very beginning of *Géométrie* (Descartes 1637, 297-298) and as he anticipates in the *Reguala* XVIII (Descartes AT, X, 463), a proportion like $u : a = b : c$ (where u is just the unit) means that c is the product of a and b. Such a definition is completely independent of the nature of the measured quantities: they can be multitudes or magnitudes, and, if they are magnitudes, they can be any sort of magnitudes. Therefore, to give a sense to the product of two magnitudes a and b, it is merely necessary that the unity is chosen as homogenous either to a or to b. But, if this is the case, the comparison of distinct quantities can be expressed by a consistent formalism, which does not depend on the particular nature of these quantities, and thus, as long as we are comparing them, all quantities can be intended as being segments.

Regula XIV stops here. This is not however the end of the story, since these considerations do imply neither that each quantity can be compared to every other (since Descartes' argument refers to the modalities of comparison, but not to the possibility of it), nor that the product of two quantities can be exhibited, if these quantities are given, together with a unit homogeneous to one of them. According to the previous definition, this is only possible, if the fourth proportional between these quantities and the unit can be exhibited. If both these quantities are segments, theorem VI, 12 of Euclid's *Elements* teaches us that this is always possible. But if this is not the case, no *a priori* guarantee can be given for that. Thus, Descartes' definition of internal product for any sort of magnitudes (that can be easily applied to multitudes too) does not go together with the possibility of exhibiting this product under any circumstances. If we want this possibility to exist always, we have not only to treat or represent all quantities as segments—as long as we are measuring them—, we have also to assume that they are segments. The same argument may be applied to internal division, integral power and any sort of root (the only difference being, for the last case, that the possibility of exhibition of every root of a given segment does not depend on any theorem of Euclid's geometry, but on Descartes' enlargement of Euclid's constructive clauses).

If we want to do geometry in general, we of course, cannot restrict ourselves to the consideration of segments. However, we may assume that only segments are given as such and try to construct any geometrical entity (that is a magnitude or a form), step by step, starting by segments. This is the progressive way of (Aristotelian) logic. Nevertheless, if we want to reach non-rectilinear figures by this construction, we cannot limit ourselves to Euclid's constructive clauses. According to Descartes, there is no question of adding further postulates to Euclid's. It is even preferable to eliminate these postulates as such. We have only to be confident of our capacity to distinguish and trace segments and to perform elementary operations with them (like to construct a circle by rotating a segment) or with ideal machines composed by segments or other objects, which have already been constructed (like in the case of the construction of the ellipse by means of the gardener's method). Hence, the construction of geometrical objects, starting from segments, is not submitted to any general rule, but has simply to satisfy a condition of exactness, which Descartes actually formulates in his *Géométrie* in different and not always consistent ways. This general precept both expresses the condition of certainty of the starting points—which Descartes inherited from (Aristotelian) logic—and the modalities of giveness of geometrical objects. I have just said that Descartes inherited these modalities from classical geometry (read through the glasses of Pappus' interpretation). In fact, these modalities are formally the same which work in classical geometry: only objects that are explicitly constructed starting from elementary objects are given as such. However, the substance of this condition has changed, since such a condition is no longer expressed

in terms of deductive constraints (like in the Euclidean deductive system), but it is merely satisfied by the application of a constructive capacity which looks after its own exactness. Thus the progressive order of Descartes's method is not the order of Aristotelian proof, it is rather an order of construction, or, in the original Latin sense of the term, an order of *inventio* (that is literally the act of coming in or upon)[39].

As long as geometrical objects are given as such, the modalities according to which we can operate on them and compare them, are the same as in classical geometry: two segments are added, for example, by juxtaposition (the term is explicit) and compared by referring to the conditions of their mutual inclusion. This is the second aspect of classical geometry inherited by Descartes. Nevertheless the objects, which are given as such are not the only ones we are able to consider. We can also consider objects, which are simply characterized by the conditions they have to satisfy. These objects are not given as such, but, as long it is question of their comparison with other objects (which are given, instead), we can express them by means of suitable terms and apply to them the usual rules of proportion. Moreover, if a unit is given, proportions can be expressed by equations (or, if you prefer, translated into them). Such a possibility enable us to determine the relational configuration of any domain of known or unknown quantities and to characterize them as the objects which satisfy (or, better, would satisfy) certain conditions. This is the modality for representing both quantities as well as operations on quantities. It is the consequent agility of formalism that Descartes inherits from the "algebra of moderns". This is also the analytic procedure on which Descartes' geometry is founded. However, this is not a regressive conduct, being rather, as in the case of Viète, a configurational analysis (here, ch. 8).

However, two novelties make Descartes' analysis essentially different from Viète's. First, the introduction of a unit (that is, in modern terms, the neutral element of a multiplicative group) eliminates any necessity of distinguishing quantities with respect to their (multilicative) order, as long as it is only question of expressing their mutual relations; and, if these quantities are supposed being segments, it enable us to perform a finite and regulated construction, which exhibits the object (obviously a segment) expressed by every finite algebraic composition of given quantities. This means that if analysis terminates in the exhibition of an identity like $x = f(a, b, ..., q)$—where $f(a, b, ..., q)$ is a finite algebraic composition of the given quantities $a, b, ..., q$—then the successive construction is certainly possible and is completely determined by analysis itself. Second, the introduction of the idea of coordinates, makes it possible to express geometrical loci by means of equations, independently from our capacity of solving the latter. Here to express is not the same than to give; but it is no more the same than to denominate. In fact, thanks to the expression of these loci by means of equations, we can establish a number of geometrical properties of them and even classify them. Moreover, if these equations are solvable, we can even actually construct any finite number of points belonging to these loci. Once again, this is not giveness of these objects as such, but it is a very strong and geometrically informative characterization of them as objects satisfying certain concepts.

These differences between Viète's and Descartes' analysis are responsible for the results of a new "art", namely modern analysis as a mathematical theory, the new theory of functions. I do not think this to be the effect of a simple oblivion of geometrical construction or even of the transformation of the previous conditions of characterization into conditions of giveness. Rather, it seems that it is the effect of Descartes' introduction of a new sort of constructive objects, which are not particular quantities, but are the relational expressions of quantities or—as they will become in the 18th-century—abstract quantities or functions (here, ch. 3 and 5 and Panza 1992). From here stem a number of new and more modern meanings of the terms "analysis" and "synthesis". The different chapters of the present book should make the greater part of these meanings clear and elucidate their mutual relations. My aim here was only to suggest the intrinsic dependence of these meanings on a single source: the Aristotelian notion of analysis as a regressive conduct of thinking performed in order to make the realization of a given aim possible.

Centre F. Viète of the History and Philosophy of Sciences,
University of Nantes

Notes

* I thank Clotilde Calabi, Jean Dhombres, Agnese Grieco, Michael Hoffmann, Francois Loget, Michael Otte, Jackie Pigeaud, Bernard Vitrac for their suggestions and linguistic and philosophical advices.

1 We owe a number of our examples of occurrences of the term "analysis", both in the Greek *corpus* and somewhere, to Timmermans (1995).

2 True to say, Aristotle's definition is not so clear. The passage I have mentioned belongs to a larger argument, where Aristotle states four different meanings for the expression "(to be) in itself". According to the third of these meanings (*Posterior Analytics*, 73*b* 5-10) Q is in itself if it is not said to be of a certain subject, let us say P, while, according to the fourth (*ibid.*, 73*b* 10-16) P is Q in itself, if it is Q because of it is just P (and for no other external reason). The first two meanings are those we have just exposed in the text. However, Aristotle seems to insist on the circumstance that predications "P is Q" and "Q is P" occur respectively in the definition of P and Q. Because of that, Barnes (1975, 114 and 112) argues both that the third and fourth meanings are ontological, while the first and second are logical, and that all of them are meanings of "Q holds of P in itself". Moreover, he maintains that the arguments of chapters 19-22 which will be discussed below only refer to the two first meanings. However, it seems to me that the third of these meanings is quite different from the other and specifically concerns the fact that the predicate 'Q' indicates a certain subject, while the fourth integrates the first two by making clear that they refer to essence, rather than merely to definition (or even to essential definition, rather than to purely linguistic definition).

3 Even though I come far from that in certain points, my reconstruction is largely indebted to Barnes' translation and commentaries as they appear in Barnes (1975).

4 Remark that, as such, this neither entails the main thesis of chapters I, 19 - I, 22, nor it is entailed by it, since it is possible that P is not able to be defined and known, and all the series of predications as the previous ones are finite, these series being infinitely many.

5 According to Barnes (1975, p. 180) the argument for the downward series of P_j is not correct. This is quite right if we consider, as Barnes does, this series as a series of predications where the predicate "inheres in the definition" (*ibid*, p. 112) of the subject and we directly refer Aristotle's argument to the possibility of a (finite) proof and definition. If it is so, the fact that for every (natural) number j there is a predicate P_j such that "P_j is P" is an essential predication (in the previous sense) only means that P inheres in the definition of infinitely many subjects. In order to make Aristotle's argument correct, we have to assume that P_j is a subject and namely a species of P—(essentially) defined by the genus to which it belongs—and that no subject can contain an infinite number of species (what makes clear the role of the second premise advanced by Aristotle at the beginning of chapter I, 22). In any case, if we do not refer, as Barnes does, the Aristotle's argument directly to the possibility of a (finite) proof and definition, it is the argument for the upward series of Q_j which fails, except if we accept that the predicate of an essential definition is a genus of the subject and no subject can belong to infinitely many embedded genus (cf. the previous endnotes 2 and 4).

6 The interpretation of Waitz (Aristotle AOG, II, 353-354), according to which an "analytical" proof is rigorous, while a "logical" one is not, seem to be unacceptable.

7 This is one of the roots of the wrong idea of many amateur philosophers, who think that synthesis is nothing but invention (or even "intuition" as a creative act).

8 Cf. Proclus (PEEL, ed Friedlein, 17-19), which ascribes this sort of analysis to Eratosthenes.

9 Cf. the previous endnote (5).

10 Literally: "pre-delibered", since the verb "βουλεύω" means "to deliberate", as an act of a council, the "βουλή" being just the administrative council of a political community.

11 Cf. the previous note (10).

12 Aristotle's identification of eternity and necessity (his non-modal conception of necessity) has been discussed by a number of scholars. Cf. for example Hintikka (1975).

13 Of course regressive reduction is part of what we do when we "work backwards". Thus the Aristotelian notion of analysis is completely compatible with the general meaning that Szabó (1974) has ascribed to the term "ἀνάλυσις" as referring to a "working backwards". It appears to me, however, that the Aristotelian notion of analysis is more profound. It is not at all restricted to the level of methodology, but is related to foundamental questions of epistemology and metaphyics. We can even regard it as the source of modern epistemological conceptions which are not merely concerned with the examples that Szabó discusses, that are, Pólya's heuristic and Lakatos' "proof-analysis" or "method of proof(s) and refutations" (cf. Pólya 1945, particularly 141, and Lakatos 1976 and PP, II, ch. 5: "The Method of Analysis-Synthesis", 70-103).

14 "Treasury of Analysis" is Heath's and Hintikka-Remes's translation (Euclid EH, I, 138; and Hintikka and Remes 1974, 8). Jones and Ver Eecke translate the same Greek expression respectively with "Domain of analysis" (Pappus CJ, 82; cf. here, ch. 8, par. II) and "champ de l'analyse" (Pappus CVE, 477).

15 Hultsch was here following Halley's translation in the preface to Apollonius *Cutting-off of a Ratio* (SRH, XXVIII), which translated "κατὰ σύλληψιν" with "ut paucis dicam".

16 For the references, cf. note (14).

17 On the correspondence between Pappus's definition and Aristotle's argument of the chapters III, 3-5 of *Nicomachean Ethics* cf. Hintikka and Remes (1974, 86-87) and Knorr (1986, 356-357) that even guesses that Pappus "may present not a distillation of [..] ancient tradition , but rather a rephrasing of standard philosophical views" (*ibid.*, 357).

18 On all the question cf. Hintikka and Remes (1974, 11-19).

19 For a tentative literally translation of the Arabic text cf. Knorr (1986, 376).

20 Other examples of analysis in Heron's works are listed by *Hintikka and Remes (1974, 19-20, n. 2)* and Knorr (1986, 376-377, n. 87). You can also consider the paragraph 136,7 of the [pseudo-] Heron's *Definitiones*, which (as it mention Porphyry) can not be antecedent to the 3rd century A.D.

21 It is clear that, even though it is possible to intend all the identities (1)-(10) as logical equivalencies, the inferential chain (1)-(10) is not convertible as such, because of the essential occurrence of (1) in the passage from (4) to (5).

22 The particular aim of analysis, here, seems just to provide such a suggestion. Thus it does not seem to us be "completely artificial" as Knorr says (1986, 358).

23 A reason justifying Pappus' inclusion of Euclid's *Data* in the corpus of analysis is advanced in the note (30) above.

24 On the classical distinction between theorems and problems in Greek mathematics cf., for example, Caveing (*1990, 133-37*).

25 The same formula appears, sometimes without the particle "δή" also in: 276, 3 and 18; 278, 13 and 24; 280, 15; 282, 8; 284, 8; 286, 5; 298, 20; and 300, 22; while in: 288, 15; 290, 24; and 297, 7 we found the more explicit formula "the problem will be synthesized in this way [Συντεθήσεται δὴ τὸ πρόβλημα οὕτως]". Beside, after having presented the last analysis in prop. II. 49, Apollonius shortly concludes by observing that "the synthesis [is] like [that] of the previous [problem] [ἡ δὲ σύνθεσις ἡ αὐτη τῇ πρὸ αὐτοῦ]"

26 The second stage in the solution of the problems 1, 4, 5, and 7 is introduced by the formula. "Συντετήσεται δὴ τὸ πρόβλημα οὕτως" (Archimedes OO, 172, 7; 192, 7; 198, 13; and 205, 15 respectively), while the second stage in the solution of the problems 3 and 6 is introduced by the formula. "Συντετήσεται δὴ οὕτως" (*ibid.*, 184, 21 and 204, 11).

27 The arguments of Menaechmus aim to solve the same problem, namely, that of finding two segments which are medium proportional, according to a continuous proportion, between two given segments. Consider as an example the first of these arguments. A and E being the given segments, let us call B and C the searched ones. Imagine that these latter are taken on two straight lines perpendicular each other, in such a way that they have a common extreme. As Rect.(A, C) = Sq.(B) it is clear that the other extreme of B belongs to a given parabola passing for the other extreme of C. But as Rect.(C, B) is given—being equal to Rect.(A, E)—this point also belongs to an hyperbola that is given too. Thus it is given as well, by intersection of two conics. This makes clear that this point can be easily constructed by constructing two suitable conics.

28 Remark that Aristotle is clearly not concerned here with a possible convertibility of analysis.

29 The problematic character of geometrical analyses of the classical age is stressed by Knorr (1986). Cf. also Hintikka and Remes (1974, 84).

30 As a matter of fact this is the structure of all the previous problematic analyses, which are, because of that, very similar to many arguments found in the 7th book of Pappus *Collection*. Cf. as an example the

proposition 155 (Pappus CH, 905-907), quoted and discussed as paradigmatic by Hintikka and Remes (1974, 52-53). A similar argument is also in Aristotle's *Meteorologics*, 375*b*, 30-376*a*, 9. The fact that analysis is concerned here with what is given when the problem is assumed to be solved might explain Pappus' inclusion of Euclid's *Data* in the corpus of treatise belonging to the domain of analysis (Heath 1921, 422 and Knorr, 1986, 109-110).

31 Cf. the previous note (17).

32 Hintikka and Remes (1974, 91) observe that "analysis as a philosophical method was in vogue in the centuries before Pappus", when "widely different methods were called 'analysis'" and (*ibid.* 89-91) evoke the compound influences of Platonic and Stoic traditions on the these conceptions. Knorr (1986, 357) even argues that "Pappus could pick up [...][his] general views through te medium of commentators like Geminus and others, conversant with a syncretizing form of Platonism".

33 We have not to confound reduction to elements in the previous sense, with a natural process of decomposition as that which Aristotle evokes in the chapter 4 of book H of *Metaphysics* (1044*a*, 15-25). Here Aristotle is opposing two (natural) processes according to which a thing comes from an other. The first one goes from the matter to the substance and is exemplified by the passage from the sweet to the fat and from the fat to the phlegm. At the opposite, the second one goes from the substance to the matter and is exemplified by the passage from the bile to the phlegm. Aristotle describes this process in general, by saying that a thing comes from another "as being analyzed in (its) principles [ὅτι ἀναλυθέντος εἰς τὴν ἀρχήν]" (1044a, 24-25) and says that the phlegm comes from the bile "by analyzing [τῷ ἀναλύεσθαι]" the latter "in [his] first matter [εἰς τὴν πρώτην ὕλην]" (1044a, 23). Clearly, analysis is not here a conduct of thinking, it is rather a natural process of decomposition of objects, the verb "ἀναλύω" being used with a meaning close to the one we have evoked in the previous paragraph I. The fact that this meaning occurs sometimes in Aristotle's writings does not entail that Aristotle does not refer in general to analysis as to a (regressive) conduct of thinking. It is just in this sense that Aristotelian notion of analysis interests us here.

34 The following remarks on Thomas's conception of analysis and synthesis and its sources rest largely on Garceau's book (1968, specially 209-220).

35 The idea that (geometrical) analysis brings us "from a complex to the simple" was advanced in the 6th-century by John Philoponus in his commentary to Aristotle's *Prior Analytics* (*Comm. Ar. Gr.*, XIII-2, 2, 16-17: cf. Hintikka and Remes 1974, 94). It is not clear however wheter the starting point of analysis is the complex in itself or the complex for us; as far as geometrical analysis is concerning, it is probably both.

36 We could maintain that this is due to Albertus's views, since research necessarily goes from the simple to the complex, while the evaluation of the results of a certain research goes from the complex to the simple. However, it seems that here we are not speaking of simple and complex in themselves, but of simple and complex for us, which is not necessarily the same.

37 This is what Proclus says (PEEL, ed Friedlein 211). It is remarkable that Proclus opposes here the method of analysis both to the method for separation (ἡ διαιρητική)—that he equally ascribes to Plato and considers as proper to every science—and to the reduction to the absurd—that, he says, does not show what is sought and only refutes its contrary (cf. also *ibid.*, 225, 8-12).

38 The terms "*finem*" and "*comprehensionem*" (cf. below) could in fact translate the terms "κατάληξιν" and "κατάληψιν", which appears there.

39 Of course, geometrical construction is not blind, it does not work without aims and it does not provide objects merely by chance. Rather, it is guided by the aim of constructing objects which satisfy certain conditions which are given *a priori* with respect to it. Thus, either it is preceded (both for Euclid and Descartes) by a geometrical reduction (that is just an analysis) or it consists in this reduction itself (this

is obviously possible as far as all its steps are trivially convertible). As a matter of fact, Descartes*Géométrie* is especially rich in examples where construction is exposed as a progressive conduct. However, the essential difference between Euclid's progressivity (that is and was intended as a synthesis by Pappus) and Descartes' progressivity could have suggested the latter is less far from analysis that the former is, or is even easily convertible into it. This could explain the famous remarks on analysis and synthesis advanced by Descartes in the "second answers" following his *Meditationes*, quoted by Israel (here, ch. 1, 5-6), where analysis is both considered as a conduct of proof and *inventio*. The essential difference between Descartes views—as expounded here—and Aristotelian ones does not lies, as many scholars have observed (for example, Timmermans, who construct his book (1995) on this opposition), in Descartes' identification of analysis with a conduct of invention. Foremost, the modern meaning of the term "invention" (both in English or French) is strictly different from the meaning of the Latin *"inventio"* (which in 17th-century is simply transferred to the French *"invention"*), being closer to the original idea expressed by the verb *"invenire"* (literally "to come in, or upon"), which is more like "to found"; to obtain, or even "to reach" than "to invent". And, if we speak of *inventio* in this sense, it is very easy to observe that for Aristotle too, analysis was a conduct of *inventio*. The problem rather is that for Aristotle analysis (as long as it is not conclusive) does not reach a theorem, or generally the realization of the aim, but reaches the first principles of the proof, or generally the conditions of realization of the aim. Thus, it is just "inventive" as long as it is not, as such, demonstrative (or at least conclusively demonstrative). For Descartes, in contrast, it seems to be "inventive" and "demonstrative" at the same time. As we have just said, we can eliminate such a difficulty in the interpretation of Descartes' text by referring to the difference between Descartes' proofs and usual deductions. But we can also remark that the difficulty is a very local one, since a few lines after, when he speaks of the application of analysis and synthesis to metaphysics, Descartes comes back to a very classical point of view, speaking about the "first notions *[primæ notiones]*" of geometry (here, ch. 1, 6) and remarking (AT, VII, 157) that analytic conduct is the most suitable one in metaphysics, since here that which is really important is "to perceive the first notions clearly and distinctly *[de primis notionibus clare & distincte percipiendis]*". Thus the difference with Aristotle reduces to one we have extensively discussed above: Descartes is simply referring to ontological (rather than epistemological) notions of clearness, evidence and firstness.

BIBLIOGRAPHY

Alembert, (d') J. B. le R. (1746) "Recherches sur le calcul intégral", *Hist. Acad. Roy. Sci. et Bell. Lettr. [Berlin]*, **2**, 1746 (publ. 1748), 182-224.

Apollonius (CH) *Treatise on conic sections* (ed. by T. L. Heath), Cambridge Univ. Press, Cambridge, 1896.

Apollonius (GE) *Apollonii Pergaei quae Graece exstant cum comentariis antiquis, edidit et latine interpretatus est I. L. Heiberg*, Teubner, Leizig 1891-1893 (2 vols.).

Apollonius (SRH) *De sections rationis libri duo. Ex Arabico MS^{to}. Latine Versi*, Opera & studio E. Halley, E Theatro Sheldoniano, Oxonii, 1706.

Arbuthnot, J. (1692) *Of the Laws of Chance, or, a Method of Calculation of the Hazards of Game*, Benj. Motte, London, 1692.

Archimedes (BL) *[Œuvres d'] Archimède*, texte établi et traduit par C. Mugler, Les Belles Lettres, Paris, 1970-1971 (4 vols.).

Archimedes (OO) *Archmedis Opera Omnia, iterum editit I. L. Heiberg*, Teubner, Leipzig, 1910-1915. (3, vols.).

Aristotle (AOG) *Aristotelis Organon Graece, Novis codicum auxilii aditus recognovit scholiis ineditis et commentario instruxit T. Waitz*, Neudruck der Ausgabe, Leipzig 1844-1846 (2 vols.).

Aristotle (WMC) *The Basic Works of Aristotle*, ed. with an introduction by R. McKeon, Random House, New York, 1941.

Armstrong, D.M. (1983) *What is a Law of Nature?*, Cambridge Univ. Press, Cambridge, 1983.

Arnauld, A. (1667) *Nouveaux elemens de geometrie [...]*, C. Savreux, Paris, 1667.

Artin, E. (1925-1926) "Theorie der Zöpfe", *Abh. Math. Sem. Univ. Hamburg*, **4**, 1925-1926, 47-72.

Artin, E. (1947) "Theory of braids", *Ann. Math.*, **48**, 1947, 101-126.

Artin, E. (1950) "The theory of braids", *American Scientist*, **38**, 1950, 112-119.

Bachelard, G. (1928) *Etude sur l'évolution d'un problème de physique. La propagation thermique dans les solides*, Vrin, Paris, 1928; new ed., 1973.

Barnes, J. (1975) *Aristote's Posterior Analytics*, translated with notes by J. Barnes, Clarendon Press, Oxford, 1975 (2nd ed., 1993).

Barrow, I. (1670) *Lectiones Geometricae: In quibus (praesertim) Generalia Curvarum Linearum Symptomata declarantur*, G. Godbid, Londini, 1670; 2nd ed. in I. Barrow, *Lectiones Opticae et Geometricae: In quibus Phaenomenon Opticorum Genuinae Rationes investigantur, ac exponuntur: et Generalia Curvarum Linearum Symptomata declarantur*, G. Godbid, Londini, 1674

Benacerraf, P. and Putnam, H. (1983) *Philosophy of mathematics. Selected Readings*, 2nd. ed., Cambridge Univ. Press, Cambridge, 1983.

Bernoulli, Jacob (1686) *Theses Logicae de Conversione et Oppositione Enunciationum. [...] cum Adnexis Miscellaneis*, Typis Bertschianis, Basileæ, 1686; ed. quot.: in Jacob Bernoulli (O), I, 225-238.

Bernoulli, Jacob (1691) "Specimen alterum calculi differentialis in dimetienda spirali logarithmica, loxodromiis nautarum et areis triangulorum sphaericorum; una cum additamento quodam ad problema funicularium, aliisque", *Acta Eruditorum*, Junii 1691, 282-290; in Jacob Bernoulli (O), 442-453.

Bernoulli, Jacob (1713) *Ars Conjectandi. Opus posthumum*, impensis Thurnisiorum fratum, Basileæ, 1713.

Bernoulli, Jacob (O) *Opera*, Cramer & Philibert, Genevæ, 1744 (2 voll.).

Bernoulli, Jacob (W) *Die Werke von Jacob Bernoulli*, Birkhäuser, Basel, 1969-1993 (4 voll.).

Biot, J. B. (1803) *Essai de géométrie analytique*, Duprat, Paris, 1803.

Blauberg, I.V., Sadovsky, V.N. and Yudin, E.G. (1977) *Systems Theory. Philosophical and Methodological Problems*, Progress Publ., Moscow, 1977.

Blay, M. (1992) *La naissance de la mécanique analytique*, P. U. F., Paris, 1992.

Bloch, M. (1964) *Apologie pour l'histoire ou métier d'historien*, A. Colin, Paris, 1964.

Blondel, M. (1893) *L'Action. Essai d'une critique de la vie et d'une science de la pratique*, Alcan, Paris, 1893; new ed. quoted: Paris, P.U.F., 1990.

Bloor, D. (1983) *Wittgenstein: A Social Theory of Knowledge*, Macmillan Press, London, Basingstoke, 1983.

Bochner, S. (1974) "Mathematical Reflections, Part II: Charles Sanders Peirce", *American Mathematical Monthly*, **81**, 1974, 838-852.

Bolzano, B. (1816) *Der Binomische Lehrsatz und als Folgerung aus ihm der polynomische, und die Reihen, die zur Berechnung der Logarithmen und Exponentialgrössen dienen, genauer als bisher erwiesen*, C. W. Enders, Praga, 1816; reprint with English transaltion in S. B. Russ, *The mathematical work of Bernard Bolzano published between 1804 and 1817*, Ph. D. dissertation. Open University, Great Britain, 1980, vol. III.

Bolzano B. (1817) *Rein analytischer Beweis des Lehrsatzes, dass zwischen je zwey Werthen, die entgegengesetztes Resultat gewaren, wenigstens eine reelle Wurzel der Gleichung liege*, Kummer, Prag, 1817; English translation quoted, by S. B. Russ: in *Historia Mathematica*, 7, 1980, 156-185.

Bolzano, B. (W) *Wissenschaftslehre*, herausg von A. Höfler und W. Schultz, Meiner, Leipzig, 1914-1931 (4 vols.).

Bos, H. J. M. (1981) "On the Representation of Curves in Descartes' Géométrie", *Archive for History of Exact Sciences*, **24**, 1981, 295-338.

Bos, H. S. M. (1984) "Arguments on Motivation in the Rise and Decline of a Mathematical Theory: the 'Construction of Equations', 1637-ca.1750", *Archives for History of Exact Sciences*, **30**, 1984, 331-380.

Bottazzini, U. (1986) *The higher calculus: A history of real and complex analysis from Euler to Weierstrass*, Springer Verlag, Heidelberg, New York, 1986.

Bourbaki, N. (1974) *Éléments d'histoire des mathématiques*, nouvelle édition, revue, corrigée et augmentée, Hermann, Paris, 1974.

Boutroux, P. (1920) *L'ideal scientifique des mathematiciens*, Alcan, Paris, 1920.

Boyer, C. B. (1939) *The Concepts of the Calculus: A Critical and Historical Discussion of the Derivative and the Integral*, Columbia Univ. Press, New York, 1939; quot. ed.: *The History of the Calculus and Its Historical Developpement*, Dover, New York, 1959.

Boyer, C. B. (1956) *History of Analytic Geometry*, Scripta Mathematica (*Scripta Mathematica studies*, 6 and 7), New York, 1956.

Boyer, C. B. (1968) *A History of Mathematics*, John Wiley and Sons, New York, London, Sydney, 1968.

Busard, H. L. L. (1976) "François Viète", *Dictionary of Scientific Biography*, XIV, 1976, 18-25.

Carathéodory, C. (1952) "Einführung in Eulers Arbeiten über Variationsrechnung", in Euler (OO), XXIV, viii-li.

Cardano G. (1545) *Artis Magnæ, sive de Regulis algbraicis liber unus*, J. Petreium, Norimbergæ, 1545.

Carnap, R. (1952) "Meaning Postulates", *Philosophical Studies*, **3**, 1952, 65-73.

Casati, R. (fc) "Rest and motion: A Newtonian Framework", forthcoming.

Cassirer, E. (1907) "Kant und die moderne Mathematik", *Kantstudien*, **12**, 1907, 1-49.

Cassirer, E. (1910) *Substanzbegriff und Funktionsbegriff. Untersuchungen über die Grundfragen der Erkenntniskritik*, B. Cassirer, Berlin, 1910

Cassirer, E. (1923-1929) *Philosophie der symbolischen Formen*, B. Cassirer, Berlin 1923-1929 (3 vols).

Cauchy, A. (1817) "Sur une loi de réciprocité qui existe entre certaines fonctions", *Bulletin des sciences par la societé Philomatique de Paris*, **2**, 1817, 121-124.

Cauchy, A. L. (1821) *Cours d'Analyse de l'École polythechnique. Première partie, Analyse Algébrique*, Debure frères, Paris, 1821; in Cauchy (OC), 2, III.

Cauchy, A. L. (1823) *Résumé des leçons données à l'École polytechnique sur le calcul infinitésimal*, tome I, Impr. Roy. Debure, Paris, 1823; in Cauchy (OC), 2, IV.

Cauchy, A. L. (OC) *Œuvres Complètes*, Gauthier-Villars, Paris, 1882-1970 (27 vols. in two series).

Caveing, M. (1990) "Introduction générale", in Euclide (EV), I, 13-148.

Chandler, B. and Magnus, W. (1982) *The History of Combinatoal Group Theory: a case study in the history of ideas*, Springer, New York, Berlin, 1982.

Chasles, M. (1875) *Aperçu historique sur l'origine et le développement des méthodes en Géométrie*, Gauthier-Villars, Paris, 1875.

Chomsky, N. (1976) *Reflections on Language*, Temple Smith, London, 1976.

Chuquet, N. (G) *La Géométrie. Première géométrie algébrique en langue française (1484)*, Introduction, texte et notes par Hervé l'Huillier, Vrin, Paris, 1979.

Commandino (1588), *Pappi Alexandrini Mathematicæ collectiones a Federico Commandino* [...] *in latinum conversæ*, H. Concordiam, Pisauri, 1588.

Comte, A. (1830-1842) *Cours de philosophie positive*, Bachelier, Paris, 1830-1842 (6 vols.).

Comte, A. (SDS) *Philosophie première. Cours de philosophie positive, Leçons 1-45*, ed. by M. Serres, F. Dagognet, H. Sinaceur, Hermann, Paris, 1975.

Coolidge, J. L. (1940) *A History of Geometrical Methods*, Clarendon Press, Oxford, 1940.

Coolidge, J. L. (1945) *A History of the Conic Sections and Quadric Surfaces*, Clarendon Press, Oxford, 1945; quot. ed.: Dover, New York, 1968.

Couturat, L. (1893) "L'année philosophique" de F. Pillon", *Revue de Métaphysique et de Morale*, **1**, 1893, 63-85.

Dahan, A. (1992) "Lagrange: la méthode critique du mathématicien philosophe", in Dhombres (1992*a*), 171-192.

Dahan, A. (1993) *Mathématisations. Augustin-Louis Cauchy et l'Ecole Française*, Editions du Choix and Blanchard, Paris, 1993.

Dascal, M. (1988) "On Knowing Truths of Reason", *Studia Leibnitiana*, Sonder. **15**, 1988, pp. 27-37.

Daston, L. (1988) *Classical Probability in the Enlightenment*, Princeton Univ. Press, Princeton (N. J.), 1988.

Daston, L. (1991) "History of science in an elegiac mode. E. A. Burtt's *Metaphysical foundations of modern physical science* revisited", *Isis*, **82**, 1991, 522-531.

Daston, L. (1992) "The Doctrine of Chances without Chance: Determinism, Mathematical Probability, and Quantification in the Seventeenth Century", in M. J. Nye, J. Richards, and R. Stuewer (ed. by), *The Invention of Physical Science*, Kluwer, Dordrecht, Boston, London, 1992, 27-50.

De Gand, F. (1986) "Le style mathématique des *Principia* de Newton" *Revue d'histoire des sciences*, **39**, 1986, 195-222.

De Moivre, A. (1711) "De mensura sortis seu de probabilitate eventuum in ludis a casu fortuito pendentibus", *Philosophical Transactions*, **27**, 1711, 213-264.

De Moivre, A. (1718) *The Doctrine of Chances: or A Method of Calculating the Probability of Events in Play*, W. Pearson, London, 1718; 2nd ed. W. Woodfall, London, 1738; 3rd ed. quot., A. Millar, London, 1756.

De Moivre, A. (1730) *Miscellanea Analytica de Seriebus et Quadraturis*, excudebant J. Tonson & J. Watts, Londini, 1730.

Delambre, J.-B. (1810) *Rapport historique sur les progrès des sciences mathématiques depuis 1789, et sur leur état actuel*, Impr. Imper. Paris, 1810; critical edition with presentation and notes by J. Dhombres: *Rapport à l'Empereur sur le progrès des sciences, des lettres et des arts depuis 1789. I. Sciences Mathématiques*, par Jean-Baptiste Delambre, Belin, Paris, 1989.

Demidov, S. S. (1982) "Création et développement de la théorie des équations différentielles aux dérivées partielles dans les travaux de J. d'Alembert", *Revue d'histoire des sciences*, **35**, 1982, 3-42.

Dennett, D. (1991) *Consciousness Explained*, Little, Brown and Company, Boston, 1991.

Descartes, R. (1637) *La Géométrie*, *in* R. Descartes, *Discours de la Méthode [...]. Plus la Dioptrique. Les Météores. Et la Géométrie qui sont des essaies de cette Méthode* (without indication of the author), I. Maire, Leyde, 1637, 295-413; A.T., VI, 368-485.

Descartes, R. (1641) *Meditationes de Prima Philosophia*, 1st. ed.: apud M. Soly, Paris, 1641; 2nd ed.: apud L. Elzeverium, Amstelodami, 1642; A.T., VII.

Descartes, R. (1647) *Les Méditations Métaphysiques de René Descartes*, I. Camusat et P. Le Petit, Paris, 1647; A.T. VII; quot. ed.: in Descartes (OLB), 147-409.

Descartes, R. (1659-1661) *Geometria a Renato Des Cartes [...]*, L. & D. Elzevirii, Amstelædami, 1659-1661 (2 vols.).

Descartes, R. (AT) *Oeuvres de Descartes* (publiées par Ch. Adam e P. Tannery), Vrin, Paris, 1897-1910 (12 voll.).

Descartes, R. (GSL) *Geometry*, English translation by D. Smith and M. Latham with a facsimile of the first edition, Dover, New York, 1954.

Descartes, R. (LR) *Regulæ ad directionem ingenii. Regles par la direction de l'esprit*, texte revu et trad. par G. Le Roy, Boivin, Paris, 1933.

Descartes, R. (OLB) *Œuvres et lettres*, textes présentés par A. Bridoux, Gallimard, Bibliothéque de la Pléiade, Paris, 1949.

Descartes, R. (PW) *The Philosophical Writings of Descartes*, translated by J. Cottingham, R. Stoothoff, and D. Murdoch, Cambridge Univ. Press, Cambridge, 1985.

Descartes, R. (ROP) "Regulæ ad directionem ingenii", in R. Descartes, *Opuscula Posthuma, physica et mathematica*, ex typ. P. et J. Blaeu, Amstelodami, 1701, separated pagination: 1-66; AT, X, 349-348.

Dhombres, J. (1978) *Nombre, mesure et continu, épistémologie et histoire*, Cedic/F. Nathan, Paris, 1978.

Dhombres, J. (1982-1983) "La langue des Calculs de Condillac (ou comment propager les Lumières?)" *Sciences et Techniques en Perspective*, **2**, 1982-1983, 197-230.

Dhombres, J. (1986) "Mathématisation et communauté scientifique française (1755-1825)", *Archives internationales d'histoire des sciences*, **36**, 1986, 249-293.

Dhombres, J. (1987) "Les présuposés d'Euler dans l'emploi de la méthóde fonctionelle", *Revue d'histoire des sciences*, **40**, 1987, 179-202.

Dhombres, J. (1992*a*) (sous la direction de), *L'école normale de l'an III. Leçons de Mathématiques. Laplace-Lagrange-Monge*, Dunod, Paris, 1992.

Dhombres, J. (1992*b*) "L'Affirmation du Primat de la Démarche Analytique" in Dhombres (1992*a*), 11-43.

Dhombres, J. (1995) "Adégaliser en Occitanie", in J. Cassinet (èd.), *Chemins mathématiques occitans de Gerbert à Fermat*, cherat de Troie, Toulouse, 1995,. 161-200.

Dhombres, J. (fc*a*) "Euclide revisité par Port-Royal, la révolution naturelle", Colloque Abraham Bosse, Tours, forthcoming.

Dhombres, J. (fc*b*) "L'histoire selon Bourbaki", in *Revolutions mathématiques, le sens de l'histoire*, forthcoming.

Dhombres, J. (fc*c*) "Une mathématique baroque en Europe, réseaux, ambitions et acteurs", in C. Goldstein, J. Gray, J. Ritter (éd.), *Une Europe mathématique, mythes et réalités historiques*, forthcoming.

Dhombres, J. and Robert, J.-B. (1996) Et ignem regunt numeri, *Fourier ou la chaleur mathématisèe*, Belin, Paris, 1995.

Dieudonné, J. (1939) "Les méthodes axiomatiques modernes et les fondements des mathématiques", *Revue scientifique*, **77**, 1939, 224-232.

Dieudonné, J. (1963-1982) *Élements d'analyse*, Gauthier-Villars, Paris, 1963-1982 (9 vols.).

Dieudonné, J. (1968) *Algèbre linéaire et géométrie élémentaire*, Hermann, Paris, 1968.

Dieudonné, J. (1969) *Calcul infinitésimal*, Hermann, Paris, 1991.

Dieudonné, J. (1970) "The work of Nicolas Bourbaki", *American Mathematical Monthly*, **77**, 1970, pp. 134-145.

Dieudonné, J. (1978) (dirigé par) *Abrégé d'histoire des mathématiques 1700-1900*, Hermann, Paris, 1978 (2 vols.).

Dijksterhuis, E. J. (1956) *Archimedes*, Ejnar Munksgaard, Copenhagen, 1956.

Dijksterhuis, E. J. (1961) *The Mechanization of the World Picture*, Oxford University Press, Oxford, 1961.

Dockès, P. (1969) *L'espace dans la pensée économique du XVIe au XVIIIe siècle*, Flammarion, Paris, 1969.

Duchesneau, F. (1993) *Leibniz et la méthode de la science*, Paris, P.U.F., 1993.

Duhamel, J. M. C. (1865) *Des Méthodes dans les Sciences de Raisonnement*, Gauthier-Villars, Paris, 1865; 2ed. quot.: 1875.

Echeverria, J. (1992) "Observations, Problems and Conjectures in Number Theory", *in* J. Echeverria, A. Ibarra and D. Mormann, (ed. by), *The Space of Mathematics*, Walter de Gruyter, Berlin, New York, 1992, 230-252.

Eisler, R. (1930) *Kant Lexikon*, E. S. Mittler, Berlin, 1930.

Engel-Tiercelin, C. (1993) "Peirce's Realistic Approach to Mathematics: Or, Can One Be a Realist without Being a Platonist?", in E. C. Moore, (ed. by), *Charles S. Peirce and the Philosophy of Science*, Papers from the Harvard Sesquicentennial Congress, The University of Alabama Press, Tuscaloosa and London, 1993, 30-48.

Engelsman, S. B. (1984) *Families of Curves and the Origins of Partial Differentiation*, North Holland P. C., Amsterdam, 1984.

Epple, M. (1994) "Das bunte Geflecht der mathematischen Spiele", *Math. Semesterberichte*, **41**, 1994, 113-133.

Euclid (EH) *Euclid's Elements* (trans. with introduction and commentary by Sir Thomas Heath), Cambridge Univ. Press, Cambridge, 2th edition, 1926.

Euclid (EV) *Les Éléments*, traduction et commentaires par B. Vitrac, P.U.F., Paris, 1990 (2 vols. appeared)

Euclid (OO) *Euclidis Opera Omnia*, ediderunt I. L. Heibrg et H. Menge, Teubner, Leipzig, 1983-1899 (8 vols. + 1 vol. supl.)

Euler, L. (1734-1735) "De infinitis curvis eiusdem generis seu methodus inveniendi aequationes pro infinitis curvis eiusdem generis" *Commentarii academiae scientiarum Petropolitanae*, **7**, 1734-1735 (publ. 1740), 174-189 (pp. 180-189 are incorrectly numbered as 190-199); in Euler (OO), ser. 1, XXII, 36-56.

Euler, L. (1736) *Mechanica, sive motus scientia analytice exposita*, ex typ. Acad. sci. imp., Petropoli, 1736 (2 vols.).

Euler, L. (1744) *Methodus inveniendi lineas curvas maximi minimive proprietate gaudentes sive solutio problematis isoperimetrici lattisimo sensu accepti,*, M.-M. Bousquet & soc. Lausannæ et Genevæ, 1744; in Euler (OO), ser. 1, XXIV.

Euler, L. (1748) *Introductio in Analysin Infinitorum*, M.-M. Bousquet & soc. Lausannæ, 1748 (2 vols.).

Euler, L. (1749) "Recherches sur les racines imaginaires des équations", *Hist. Acad. Roy. Sci. et Bell. Lettr. [Berlin]*, **5**, 1749 (publ. 1751), 222-288.

Euler, L. (1756) "Exposition de quelques paradoxes dans le calcul intégral", *Hist. Acad. Roy. Sci. et Bell. Lettr. [Berlin]*, **12**, 1756, (publ. 1758), 300-321; in Euler (OO), ser 1, XXII, 214-236.

Euler, L. (OO) *Opera Omnia*, ed. by the Soc. Sci. Nat. Helveticæ, Teubner, Leipzig, Berlin, Zürich, Basel, 1911 ff.

Fermat, P. de (TH) *Œuvres de Fermat*, éditées par P. Tannerry et C. Henry, Gauthier-Villars, Paris, 1891-1923 (5 vols.).

Ferrarin, A. (1995) "Construction and Mathematical Schematism. Kant on the Exhibition of a Concept in Intuition", *Kantstudien*, **86**, 1995, 131-174.

Fleck, L. (1980) *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*, Suhrkamp, Frankfurt, 1980.

Flegg, G., Hay, C. and Moss, B. (1985) *Nicolas Chuquet, Renaissance mathematician, A study with extensive translation of Chuquet's mathematical manuscript completed in 1484*, D. Reidel P. C., Dordrecht, Boston, Lancaster, 1985.

Forster, G. (W) *Works in 2 volumes*, Aufbau-Verlag, Berlin and Weimar, 1983.

Fourier, J. (1807) "Théorie de la propagation de la chaleur dans les solides", Ms. of 1807, 1-st ed. in I. Grattan-Guinness (in collaboration with J. R. Ravetz), *Fourier 1768-1830*, M.I.T. press, Cambridge (Mass.), 1972, 31-440.

Fourier, J. (1819-1820) "Théorie du mouvement de la chaleur dans les corps solides", *Mém. Acad. Roy. Sc. de l'Institut de France*, **4**, 1819-1820 (publ. 1824), 185-556.

Fourier, J. (1822) *Théorie analytique de la chaleur*, F. Didot, Paris, 1822.

Fourier, J. (OD) *Œuvres de Fourier*, publiées par les soins de M., G. Darboux, Gauthier-Villars, 1888-1890 (2 vols.).

Fraser, C. G. (1985) "J.L. Lagrange's changing approach to the foundations of the calculus of variations", *Archive for History of Exact Sciences,*, **32**, 1985, 151-191.

Fraser, C. G. (1987) "Joseph Louis Lagrange's algebraic vision of the calculus", *Historia Mathematica*, **14**, 1987, 38-53.

Fraser, C. G. (1989) "The Calculus as Algebraic Analysis: Some Observations on Mathematical Analysis in the 18th Century", *Archives for the History of Exact Sciences*, **39**, 1989, 317-335.

Fraser, C. G. (1990) "Lagrange's analytical mathematics, Its Cartesian origins and reception in Comte's positive philosophy", *Studies in the History and Philosophy of Science*, **21**, 1990, 243-256.

Fraser, C. G. (1992) "Isoperimetric problems in the variational calculus of Euler and Lagrange", *Historia Mathematica*, **19**, 1992, 4-23.

Fraser, C. G. (1994) "The origins of Euler's variational calculus", *Archive for History of Exact Sciences*, **47**, 1994, 103-141.

Frege, G. (1884) *Die Grundlagen der Arithmetik*, W. Köbner, Breslau, 1884; English translation quot., by J. L. Austin: *The foundations of Arithmetic*, Basil Blackwell, Oxford, 1951^1, 1953^2.

Frege, G. (1893-1903) *Grundgesetze der Arithmetik*, H. Pohle, Jena, 1893-1903 (2 vols.).

Fricke, R.and Klein, F. (1897-1912) *Vorlesungen über die Theorie der automorphen Funktionen*, B. G. Teubner, Leipzig, Berlin, 1897-1912 (2 vols.).

Friedman, M. (1992) *Kant and the Exact Sciences*, Harvard Univ. Press, Cambridge (Mass.), London, 1992.

Galilei, G. (1638), *Discorsi e dimostrazioni matematiche intorno a due nuove scienze [...]*, appresso gli Elsevirii, Leida, 1638.

Garber, D. and Zabell, S. (1979) "On the Emergence of Probability", *Archive for History of Exact Sciences*, **21**, 1979, 33-53.

Garceau, B. (1968) Judicium. *Vocabulaire, sources, doctrine de Saitn Thomas d'Aquin*, Inst. d'Études Médiévales, Montréal and Vrin, Paris, 1968.

Gardies, J.-L. (1984) *Pascal entre Eudoxe et Dedekind*, Vrin, Paris, 1984

Gilain, C. (1991) "Sur l'histoire du théorèm fondamental de l'algèbre: théorie des équations et calcul intégral", *Archives for the History of Exact Sciences*, **42**, 1991, 91-136.

Girard, A. (1629) *Invention nouvelle en algèbre*, G. Jansson Blaeuw, Amsterdam, 1629.

Giusti, E. (1984) "Gli 'errori' de Cauchy e i fondamenti dell'analisi", *Bolletino di storia delle scienze matematiche*, **4**, 2, 1984, 24-54.

Glas, E. (1985) "On the dynamics of mathematical change in the case of Monge and the French Revolution", *Stud. Hist. Phil. Sci.*, **17**, 1985, 249-268.

Gödel, K. (1944) "Russell's Mathematical Logic", in P. A. Schilpp (ed. by), *The philosophy of Bertrand Russell*, Northwestern Univ. Press, Evanston (Illinois) and Chicago, 1944, 125-153; in Benacerraff and Putnam (1983), 447-469 and in Gödel (CW), II, 119-141.

Gödel, K. (1961) "The Modern Development of the Foundations of Mathematics in Light of Philosophy", first publication, in Gödel (CW), III, 375-387.

Gödel, K. (CW) *Collected Works* (ed. by S. Feferfan, J. W. Dawson, S. C. Kleene, G. H. Moore, R. M. Solovay and J. van Heijenort), Oxford Univ. Press, Oxford, 1986-1995 (3 vols.).

Goldstine, H. H. (1980) *A history of the calculus of variations from the 17th through the 19th century*, Springer-Verlag, Heidelberg, 1980.

Grabiner, J. V. (1978) "The Origins of Cauchy's Theory of the Derivative", *Historia Mathematica*, **5**, 1978, 379-409.

Grabiner, J. V. (1981) *The Origins of Cauchy's Rigorous Calculus*, M.I.T. press, Cambridge (Mass.), 1981.

Granger, G.-G. (1968) *Essai d'une philosophie du style*, A. Colin, Paris, 1968.

Granger, G.-G. (1982) "On the Notion of Formal Content", *Social Research*, **49**, 1982, 359-382.

Grattan-Guinness, I. (1972) (in collaboration with J. R. Ravetz) *Fourier 1768-1830*, M.I.T. press, Cambridge (Mass.), 1972.

Grice, H. P. and Strawson P. F. (1971) "In Defense of a Dogma", in S. Munsat (ed. by), *The Analytic-Synthetic Distinction*, Wadsworth Publishing Comp., Inc., Belmont (CA), 111-127.

Grimsley, R. G. (1963) *Jean d'Alembert (1717-1783)*, Clarendon Press, Oxford, 1963.

Grisard, P. (w.d.), *François Viète, mathématicien de la fin du seizième siècle*, thèse de Doctorat, EHESS, Paris, without date (2 vols.).

Gueroult, M. (1946) "Substance and the Primitive Simple Notion in the Philosophy of Leibniz", *Philosophical and Phenomenological Research*, **7**, 1946, 293-315, in M. Gueroult, *Etudes sur Descartes, Spinoza, Malebranche et Leibniz*, G. Olms, Hildesheim, 1970, 229-251.

Haack, S. (1993) "Peirce and Logicism: Notes Towards an Exposition", *Transactions of the C. S. Peirce Society*, **29**, 1993, 33-56.

Hacking, I. (1975) *The Emergence of Probability*, Cambridge Univ. Press, Cambridge, 1975.

Hacking, I. (1984) "Leibniz and Descartes: Proof and Eternal Truths", in T. Honderich (ed. by), *Philosophy Through its Past*, Penguin Books, Harmondsworth (Middlesex), 1984, 207-224.

Hald, A. (1990) *A History of Probability and Statistics and Their Applications before 1750*, John Wiley and Sons, New York, 1990.

Hankel, H. (1874) *Zur Geschichte der Mathematik in Alterthum und Mittelalter*, B. G. Teubner, Leipzig, 1874.

Hausdorff, F. (1914) "Bemerkung über den Inhalt von Punktmengen", *Math. Ann.*, **75**, 1914, 428-433.

Heath, T. L. (1921) *A history of Greek Mathematics*, Clarendon Press, Oxford, 1921 (2 vols.).

Hegel, G. W. (W) *Werke*, Duncker und Humblot, Berlin and Liepzig, 1832-1887 (19 vols.).

Heiberg, J. L., "Paralipomena zu Euklid", *Hermes*, **38**, 1903, 46-74, 161-201, 321-356.

Heidegger, M. (1927) *Sein und Zeit*, Max Niemeyer, Halle, 1927.

Herivel, J. (1975) *Joseph Fourier, the Man and the Physicist*, Clarendon Press, Oxford, 1975.

Hermann, J. (1729) "Consideratio curvarum in punctum positione datum projectarum, et de affectionibus earum inde pendentibus", *Comm. Acad. Sci. Imp. Petropolitanae*, **4**, 1729 (pub. 1735), 37-46.

Hesse, O. (1861) *Vorlesungen über analytische Geometrie der Raumes*, Teubner, Leipzig, 1861.

Hilbert, D. (1899) *Grundlagen der Geometrie*, in *Festschrift zur Feier der Enthüllung des Gauss- Weber-Denkmals in Göttingen*, herausg. von dem Fest-Comitee, Teubner, Leipzig, 1899.

Hilbert, D. (1926) "Über das Unendliche", *Mathematische Annalen*, **95**, 1926, 161-190; English translation in, Benacerraf and Putnam, (1983), 183-201.

Hintikka, J. (1973) *Logic, Language-Games and Information*. Kantian Themes in the Philosophy of Logic, Clarendon Press, Oxford, 1973.

Hintikka, J. (1975) *Time and Necessity. Studies in Aristotle's theory of modality*, Clarendon Press, Oxford, 1975.

Hintikka, J. and Remes, U. (1974) *The Method of Analysis: Its Geometrical Origin and Its General Significance*, Reidel, Dordrecht, 1974.

Hintikka, J. and Remes, U. (1976) "Ancient Geometrical Analysis and Modern Logic", in R. S. Cohen, et al., (ed. by), *Essays in Memory of Imre Lakatos*, Reidel, Dordrecht, 1976, 253-276.

Hofmann, J. E. (1960) "Über zahlentheoretische Methoden Fermats und Eulers, ihre Zusammenhänge und ihre Bedeutung", *Archive for History of Exact Sciences* **1**, 1960-1962, 122-159.

Hölder, O. (1924) *Die Mathematische Methode*, Springer, Berlin, 1924.

Holton, G. (1978) *The scientific imagination: case studies*, Cambridge Univ. Press, Cambridge, London, 1978.

Houser, N. (1993) "On 'Peirce and Logicism': A Response to Haack", *Transactions of the C. S. Peirce Society*, **29**, 1993, 57-67.

Hull, K. (1994) "Why Hanker After Logic? Mathematical Imagination, Creativity and Perception in Peirce's Systematic Philosophy", *Transactions of the C. S. Peirce Society*, **30**, 1994, 285-295.

Husserl, E. (1929) *Formale und Transzendentake Logik*, Niemeyer, Halle, 1929.

Husserl, E. (1935-1936) *Die Krisis der europäischen Wissenschaften und die transzendentale Phänomenologie*, M. Ninjhoff, The Hague, (1935-1936).

Huygens. C. (1657) "De Ratiociniis in Ludo Aleae", in F. Van Schooten, *Exercitationum mathematicorum libri quinque*, lib. V, J. Elsevirii, Lugduni Batavorum, 1657, 519-534.

Israel, G. (1990) "Federigo Enriques: A Psychologistic Approach for the Working Mathematician", in M. A. Notturno (ed. by) *Perspectives on Psychologism*, Brill, Leiden, New York, København, Köln, 1989, 426-457.

Itard, J. (1956) *La géométrie des Descartes*, Les conférences du palais de la découverte, Paris, série D, 39, 1956; reprinted in J. Itard, *Essais d'Histoire des Mathématiques*, réunis et introduits par R. Rashed, Blanchard, Paris, 1984, 269-279.

Jahnke, H. N. (1992) "Beweisbare Widersprüche-Komplementarität in der Mathematik", in E. P. Fischer, H. S. Herzka and K. H. Reich (ed. by), *Widersprüchliche Wirklichkeit*, Piper, München, 98-130.

Kant, I. (A) *Kritik der reinen Vernunft*, Hartknoch, Riga, 1781; the pages 1-405 are in Kant (SA), IV, 1911, 1-252.

Kant, I. (B) *Kritik der reinen Vernunft*, Hartknoch, Riga, 1787; *in* Kant (SA), III, 1911, 1-552.

Kant, I. (1790) *Kritik der Urteilskraft*, Lagarde und Friederich, Berlin und Libau, 1790; in Kant (SA), V, 165-485.

Kant, I. (CJM) *Kant's Critique of aestetic judgment*, transl. with seven introductory essays, notes and analytical index, by J. C. Meredith, Clarendon Press, Oxford, 1911.

Kant, I. (CS) *Critique of Pure Reason*, English translation by N. K. Smith, Mac Millan, London, Basingstoke, 1929[1], 1933[2].

Kant, I. (JL) "Logik, ein Handbuch zu Vorlesungen", edit by G. B. Jäsche, F. Nicolovius, Könisberg, 1800; in Kant (SA), IX, 1-150.

Kant, I. (LY) "Jäsche Logic", in I. Kant, *Lectures on Logic*, translated and edited by J. M. Young, Cambridge Univ. Press, Cambridge, 1992, 519-640.

Kant, I. (SA) *Kant's gesammelte Schriften* (herausg. von der Königlich-Preußischen Akademie der Wissenschaften, afterwards Deutsche Akademie der Wissenschaften zu Berlin, afterwards Akademie der Wissenschaften der D.D.R.), G. Reimer and (from 1922) W. de. Gruyter, Berlin, 1900 ff.

Kitcher, P. (1975) "Bolzano's Ideal of Algebraic Analysis", *Studies in History and Philosophy of Science*, **6**, 1975, 229-269.

Klein, F. (1872) *Vergleichende Betrachtungen über neuere geometrische Forschungen*, Deichert, Erlangen, 1872; new ed. quot.: *Das Erlangen Programm*, Akademische Verlagsgesellschaft Geest & Portig K.-G. (Ostwalds Klassiker der exakten Wissenschaften, 253), Leipzig, 1974.

Klein, F. (1893) *Einleitung in die höhere Geometrie*, handwritten notes by F. Schilling, Göttingen, 1893; 3-rd ed. quot.: *Vorlesungen über höhere Geometrie*, bearb. und herausg. von W. Blaschke, Springer, Berlin, 1926.

Klein, F. (1908-1909) *Elementarmathematik vom hoeheren Standpunkte*, B. G. Teubner, Leipzig, 1908-1909 (2 vols.); English transl. quot., from the 3-rd ed. (Springer, Berlin, 1924-1928, (3 vols.)), by E. R. Hedrick and C. A. Noble: Dover, New York, 1939.

Klein, F. (1926-1927) *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, Springer, Berlin, 1926-1927 (2 vols.)

Klein, J. (1934-1936) "Die griechische Logistik und die Entstehung der Algebra," *Quellen und Studien zur Geschichte der Mathematik, Astronomie und Physik*, Abteilung B: *Studien*, **3**, 1934, 18-105 and 122-235; English transl. quot. by Eva Brann: *Greek mathematical thought and the origin of algebra*, M.I.T. Press, Cambridge (Mass.), 1968 (the appendix contains J.W. Smith's English translation of Viète (1591*a*)).

Kline, M. (1972) *Mathematical thought from ancient to modern times*, Oxford Univ. Press, Oxford, New York, 1972.

Knorr, W. (1983) "Construction as Existence Proof in Ancient Geometry", *Ancient Philosophy*, **3**, 1983, 125-148.

Knorr, W. R. (1986) *The Ancient Tradition of Geometric Problems*, Birkhäuser, Boston, Basel, Stuttgart 1986.

Kolmogorov, A. (1932) "Zur Deutung der intuitionistischen Logik", *Mathematische Zeitschrift*, **35**, 1932, 58-65.

Kuhn, T. S. (1962) *The Structure of Scientific Revolutions*, Univ. of Chicago Press, Chicago, 1962.

L'Hôpital, G. F. Marquis de (1696) *Analyse des Infiniment petits, pour l'intelligence des lignes courbes*, de l'Imprimerie Royale, Paris, 1696.

Lachterman, D. (1989) *The Ethics of Geometry*, Routledge, London, New York, 1989.

Lacroix, S.-F. (1797-1798) *Traité du calcul différentiel et du calcul intégral*, Duprat, Paris, 1797-1798 (2 vols.).

Lagrange, J. L. (1788) *Mécanique analytique*, la veuve Desaint, Paris, 1788; in Lagrange (OS), XI-XII.

Lagrange, J. L. (1797) *Théorie des fonctions analytiques*, impr. de la République, Paris, an. V (*Journal de l'École Polytechnique*, cah. 9); in Lagrange (OS), IX.

Lagrange, J. L. (1798) *Traité de la résolution des équations numériques de tous les degrés*, Duprat, Paris, an VI; in Lagrange (OS), VIII.

Lagrange, J. L. (1799) "Discours sur l'objet de la Théorie des Fonctions analytiques", *Journal de L'Ecole Polytechnique*, cah. 6, t. II, 232-235.

Lagrange, J. L. (1806) *Leçons sur le calcul des fonctions*, nouv. éd. revue, corrigée et augmentée par l'auteur, Courcier, Paris, 1806; in Lagrange (OS), X.

Lagrange, J. L. (LEN) "Leçons de Lagrange à l'École Normale de l'an III", in Dhombres (1992*a*), 193-260 (with introdutions and notes of A. Dahan: Dahan (1992) and 261-265).

Lagrange, J. L. (OS) *Œuvres de Lagrange*, publiées par les soins de J.-A. Serret [et G. Darboux], Gauthiers-Villars, Paris 1867-1892 (14 vols.).

Lakatos, I. (1976) *Proofs and Refutations*, ed. by J. Worral and E. Zahar, Cambridge Univ. Press, Cambridge, London, 1976.

Lakatos, I. (PP) *Philosophical Papers*, Cambridge Univ. Press, 1978 (vol. I: *The Methodology of Scientific Research Programmes*; vol. II: *Mathematics, Science and Epistemology*).

Laplace, P. S. (1799-1825) *Traité de mécanique celeste*, Duprat, after Mme Ve Courcier, after Bachelier, Paris, 1799-1825 (5 vols.)

Laplace, P. S. (LEN) "Leçons de Lagrange à l'École Normale de l'an III", in Dhombres (1992*a*), 45-140 (with introdutions and notes of J. Dhombres: Dhombres (1992*a*) and 141-167).

Leibniz, G. W. (1684) "Nova methodus pro maximis et minimis, itamque tangentibus, qua nec fractas, nec irrationales quantitates moratur, et singulare pro illis calculi genus", *Acta Eruditorum*, **3**, 1684, 467-473.

Leibniz, G. W. (A) *Sämtliche Schriften und Briefe*, Leipzig-Berlin, Akademie der Wissenschaften zu Berlin, 1923-...

Leibniz, G. W. (GM) *Leibnizens mathematische Schriften* (herausg. von C. I. Gerhardt), Asher & Comp., Berlin, 1849-1850: voll. I-II, H. W. Schmidt, Halle, 1855-1863: voll. III-VII; repr., G. Olms, Hildesheim, 1961-62.

Leibniz, G. W. (GP) *Die philosophischen Schriften von G. W. Leibniz*, (herausg. von C. I. Gerhardt), Weidmann, Berlin, 1875-1890 (7 vols.); rist. anast. G. Olms, Hildesheim, 1965.

Leibniz, G. W. (VE) *Vorausedition zur Reihe VI — Philosophische Schriften — in der Ausgabe der Akademie der Wissenschaften*, Manuskriptdruck ad usum collegialem, Münster, Leibniz-Forschungsstelle.

Lektorsky, V. (1980) *Subject, Object, Cognition*, Progress Publ., Moscow, 1980.

Lenoir, T. (1979) "Descartes and the Geometrization of Thought: The Methodological Background of Descartes's Géométrie", *Historia Mathematica*, **4**, 1979, 355-379.

Lewis, D. (1973) *Counterfactuals*, Harvard University Press, Cambridge (Mass.), 1973.

Loria, G. (1923) "Qu'est-ce que la géométrie analytique?" *L'Enseignement Mathématique*, **23**, 1923, 142-147.

Loria, G. (1924) "Da Descartes a Fermat a Monge a Lagrange. Contributo alla storia della geometria analitica", *Memorie dell'Accademia dei Lincei*, Cl. Sci. Fis. Mat. e Nat., Serie 5a, **14**, 1924, 777-845.

Loria, G. (1932) "A. L. Cauchy in the history of analytic geometry", *Scripta Mathematica*, **1**, 1932, 123-128.

Lovejoy, A. O. (1936) *The great chain of being; a study of the history of an idea. The William James lectures delivered at Harvard university*, Harvard Univ. Press, Cambridge, Mass., 1936.

Lützen, Jesper (1983) "Euler's vision of a general partial differential calculus for a generalized kind of function", *Mathematics Magazine*, **56**, 1983, 299-306.

Maddy, P. (1990) *Realism in Mathematics*, Oxford. Univ. Press, Oxford, 1990.

Mäenpää, P. (1993) *The Art of Analysis. Logic and History of Problem Solving*, PhD thesis, University of Helsinki, 1993.

Magnus, W. (1974) *Braid groups. A Survey*, in M. F. Newman (ed. by), *International Conference on the Theory of Groups. Proceedings*, Springer, Berlin, New York, 1974 (SLNM 372), 463-487.

Mahoney, M. (1968) "Another Look at Greek Geometrical Analysis," *Archive for History of Exact Sciences*, **5**, 1968, 318-348.

Mahoney, M. S. (1973) *The mathematical career of Pierre de Fermat (1601-1665)*, Princeton Univ. Press, Princeton (N. J.), 1973.

Martin-Löf, P. (1982) "Constructive Mathematics and Computer Programming", in L. J. Cohen et al., (ed by.), *Proceedings of the Sixth International Congress of Logic, Methodology and Philosophy of Science* (Hannover 1979), North-Holland, Amsterdam, 1982, 153-175.

Martin-Löf, P. (1984) *Intuitionistic Type Theory*, Notes by G. Sambin of a series of lectures given in Padua (June 1980), Bibliopolis, Napoli, 1984.

Mehrtens, H. (1990), *Moderne-Sprache-Mathematik [...]*, Suhrkamp, Frankfurt a. Main, 1990.

Milhaud, G. (1921) *Descartes Savant*, Alcan, Paris, 1921.

Molland, A. G. (1976) "Shifting the Foundations: Descartes's Transformation of Ancient Geometry", *Historia Mathematica*, **3**, 1976, 21-49.

Monge, G. (1799) *Géométrie Descriptive. Leçons données aux écoles normales de l'an 3 de la République*, Baudovin, Paris, an VII.

Monge, G. (LEN) "Leçons de Monge à l'École Normale de l'an III", in Dhombres (1992*a*), 305-453 (with introdutions and notes of B. Belhoste and R. Taton, 267-303 and 455-459).

Monge, G. and Hachette, M. (1802) "Application d'Algèbre à la Géométrie", *Journal de l'Ecole Polytéchnique*, chaier 11 (Messidor, an X: 1802), 143-172.

Montmort, R. (1713) *Essai d'analyse sur les jeux de hazard*, 2nd ed., Quillau, Paris, 1713.

Moore, G. E. (1922) *Philosophical Studies*, Harcourt, Brace, New York, 1922; quot. ed.: Routledge & Kegan Paul, London, 1974.

Morse, J. (1981) *The Reception of Diophantus' Arithmetic in the Renaissance*, Ph.D dissertation, Princeton University, 1981.

Mueller, I. (1981) *Philosophy of Mathematics and Deductive Structure in Euclid's Elements*, MIT Press, Cambridge (Mass.), 1981.

Mugnai, M. (1992) *Leibniz's Theory of Relations*, Steiner, Stuttgart, 1992.

Murphey, M. G. (1961) *The Development of Peirce's Philosophy*, Harvard Univ. Press, Cambridge, 1961.

Nayrīzī, al- (EEC) *Anaritii in decem libros priores Elementorum Euclidis commentarii, ex interpretazione Gherardi Cremonensis [...]*, edidit M. Curtze, supplementum of Euclid (OO), Teubner, Leipzig, 1899.

Neugebauer, O. (1938) "Über eine Methode zur Distanzbestimmung Alexandria-Rom bei Heron", *Kongelige Danske Videnskabernes Selskabs Skriften*, **26**, 2, 1938, 21-24.

Newton, I. (1687) *Philosophiae Naturalis Principia Mathematica*, J. Streater, Londini, 1687; 2nd ed. W. P. Catabrigiæ, 1713; 3rd ed. apud G. & J. Innys, Londini, 1726; English transl of the 3-rd ed. by Andrew Motte (1729), revides. by F. Cajori, Univ. of California Press, Berkeley, 1934.

Newton, I. (1704) *Opticks [...] Also two treatises of the species and Magnitudine of Curvilinear Figures*, S. Smith & B. Walford, London, 1704; ed. quot. (based on the 4rt ed., W. Innys, London 1730), with a foreword by A. Einstein and an introduction by E. Whittaker: Dover Publications, New York, 1952.

Newton, I. (MF) *The method of fluxions and infinite series with its application to the geometry of curve lines*, translated by J. Colson, printed by H. Woodfall, sold by J. Nourse, London 1736.

Newton, I. (MP) *The mathematical papers of Isaac Newton*, ed. by D. T. Whiteside, Cambridge Univ. Press, 1967-1981 (8 vols.).

Nordström, B., Petersson, K. and Smith, J. (1990) *Programming in Martin-Löf's Type Theory—An Introduction*, Oxford Univ. Press, Oxford, 1990.

O'Neill, J. (1991) *Worlds without Content: Against Formalism*, Routledge, London, 1991.

Ore, O. (1948) *Number theory and its history*, McGraw-Hill, New York, 1948.

Otte, M. (1989) "The Ideas of Hermann Grassmann in the Context of the Mathematical and Philosophical Tradition since Leibniz", *Historia Mathematica*, **16**, 1989, 1-35.

Otte, M. (1990) "Arithmetic and Geometry: Some Remarks on the the Concept of Complementary", *Studies in Philosophy and Education*, **10**, 1990, 37-62.

Otte, M. (1992) "Das Prinzip der Kontinuität", *Math. Semesterberichte*, **39**, 1992, 105-125.

Otte, M. (1993) "Kontinuitätsprinzip und Prinzip der Identität des Ununterscheidbaren", *Studia Leibnitiana*, **36**, 1993, 70-89.

Otte, M. (1994*a*) *Das Formale, das Soziale und das Subjektive. Eine Einführung in die Philosophie und Didaktik der Mathematik*, Suhrkamp, Frankfurt a. M., 1994.

Otte, M. (1994*b*) "Intuition and Logic in Mathematics", in D. F. Robitaille, D. H., Wheeler and C. Kieran (ed. by), *Selected Lectures from the 7th International Congress on Mathematical Education*, Les Press de l'Univ. de Laval, Sainte-Foy (Québec), 1994, 271-284.

Otte, M. (1995) "La philosophie des mathématiques de Charles S. Peirce (1839-1914)", in Panza and Pont (1995), 89-108.

Palummbo, M. (1984) *Immaginazione e matematica in Kant*, Laterza, Bari, Roma, 1984.

Panza, M. (1992) "La forma della quantità. Analisi algebrica e analisi superiore: il problema dell'unità della matematica nel secolo dell'illuminismo", *Cahiers d'histoire et philosophie des sciences*, **38-39**, 1992.

Panza, M. (1995*a*) "L'intuition et l'évidence. La philosophie kantienne et les géométries non euclidiennes: relecture d'une discussion", in Panza and Pont (1995), 39-87.

Panza, M. (1995*b*) "Platonisme et intentionnalité", in Panza and Salanskis (1995), 85-132.

Panza, M. (fc) "Quelques distinctions à l'usage de l'historiographie des mathématiques", Proceedings of the Conference, *Herméneutique, textes et sciences*, Cerisy-la-Salle, September 1994 (ed. by F. Rastier and J.-M. Salanskis), forthcoming.

Panza, M. and Pont, J.-C. (1995) (sous la direction de) *Les savants et l'épistémologie vers la fin du XVIIIème siècle*, Blanchard, Paris, 1995.

Panza, M. and Salanskis, J.-M. (1995) (sous la direction de) *L'objectivité mathématique. Platonismes et structures formelles*, Masson, Paris, 1995.

Pappus (CH) *Pappi Alexandrini Collectionis quae supersunt, e lehri manu seriptis edidit, latina interpretatione et commendariis instruxit Fridericus Hultsch [...]*, Weidmann, Berolini, 1876-1878 (3 vols.).

Pappus (CJ) *Book 7 of the Collection [by] Pappus of Alexandria*, edited with [English] transl. and commentary by A. Jones, Springer-Verlag, New York, Heidelberg, 1986.

Pappus (CVE) *La Collection mathématique* (Trad. de P. Ver Eecke), Desclée de Brouwer et C., Paris, Bruges, 1933.

Parrini, P. (1990) "Sulla teoria kantiana della conoscenza: verità, forma, materia", in F. Alessio, E. Garroni, M. Mamiani, V. Mathieu, M. Miglio, M. Mori, and P. Parrini, *Kant. Lezioni di aggiornamento*, Zanichelli, Bologna, 1990, 35-67.

Parsons, C. (1983) *Mathematics in Philosophy*, Cornell Univ. Press, Ithaca (N.Y.), 1983.

Parsons, C. (1995) "Quine and Gödel on Analyticity", in P. Leonardi and M. Santambrogio (ed. by), *On Quine*, Cambridge Univ. Press, Cambridge, 1995, 297-313.

Pasch, M. (1882) *Vorlesungen über neuere Geometrie*, Teubner, Leipzig, 1882.

Pasini, E. (1966) *Corpo e funzioni cognitive in Leibniz*, F. Angeli, Milano, 1996.

Peano, G. (1901) "Les définitions mathématiques", in *Compte rendu du Deuxième Congres International des Mathématiques, Paris 1900*, Gauthier-Villars, Paris 1901 (3 vols.), III, 279-288.

Pécot, J.-B. (1992) *Histoire des relations d'orthogonalité*, thése de Doctorat, Université de Nantes, 1992 (4 vols.).

Peirce, C. S. (1885) "On the Algebra of Logic. A Contribution to the Philosophy of Notation", *American Journal of Mathematics*, **7**, 1885, 180-202; in Peirce (W), V, 162-190.

Peirce, C. S. (CCL) *Reasoning and the Logic of Things. The Cambridge Conferences Lectures of 1898* (ed. by K. L. Ketner, with an introduction by K.L. Ketner and H. Putnam, Harvard Univ. Press, Cambridge, London, 1992.

Peirce, C. S. (CP) *Collected Papers of Charles Sanders Peirce* (ed. by C. Hartshorne and P. Weiß, (vols. I-VI) and A. W. Burks, (vols. VII-VIII), Harvard Univ. Press, Cambridge (Mass.), 1931-1958 (8 vols.).

Peirce, C. S. (Ms) *Manuscript, after Richard S. Robin, Annotated Catalogue of the Papers of Charles S. Peirce*, Univ. of Massachusetts Press, Amherst, 1967.

Peirce, C. S. (NEM) *The New Elements of Mathematics by Charles S. Peirce* (ed. by C. Eisele), Mouton, The Hague, Paris and Humanities Press, Atlantic Highlands (N. J.), 1976 (4 vols.).

Peirce, C. S. (W) *Writings of Charles S. Peirce. A Chronological Edition* (ed. by C. J. W. Kloesel) Indiana Univ. Press, Bloomington, Indianapolis, 1982-1992 (5 vols. appeared containing writings from 1857 to 1886).

Plato (OC) *Œuvres complètes*, Gallimard, Etitions de la Pléiade, Paris, 1950.

Poincaré, H. (1891) "Les géométries non-euclidiennes", *Revue générale des sciences pures et appliquées*, **2**, 1891, 769a-774b; republished in Poincaré (1902), 35-50.

Poincaré, H. (1894) "Sur la nature du raisonnement mathématique", *Revue de Métaphysique et de Morale*, **2**, 1894, 371-384; republished in Poincaré (1902), 1-16.

Poincaré, H. (1902) *La science et l'hypothèse*, Flammarion, Paris, 1902.

Poincaré, H. (1905) *La science et l'hypothèse*, Flammarion, Paris, 1905.

Poincaré, H. (1905) *La valeur de la science*, Flammarion, Paris, 1905.

Pólya (1945) *How to Solve It*, Princeton Univ. Press, Princeton (N. J.), 1945.

Poncelet, J. V. (AAG) *Applications d'analyse et de géométrie qui ont servi, en 1822, de principal fondement au* Traité des propriétés projectives des figures*par J.-V. Poncelet*, éd. par V. M. A. Mannheim et T. F. Moutard, Mallet-Bachelier, Paris, 1862-1864 (2 vols.).

Proclus (PEEL) *Procli diadochi in primum Euclids Elementorum librum, ex recognitione G. Friedlein*, B. G. Teubner, Leipzig, 1873; English translation by G. Morrow, quot.: Princeton Univ. Press, Princeton (N. J.), 1970.

Putnam, H. (1975) *Mathematics, Matter and Method*, Cambridge Univ. Press, Cambridge, London, New York, 1975.

Quine, W. v. O. (1953) *From a Logical Point of View*, Haward Univ. Press, Cambridge (Mass.) 1953.

Quine, W. v. O. (1968) "Onthological Relativity", *The Journal of Philosophy*, **65**, 1968, 185-212.

Rescher, N. (1967) *The Philosophy of Leibniz*, Prentice-Hall, Englewood Cliffs (N. J.), 1967.

Reye, T. (1866-1867) *Die Geometrie der Lage*, C. Rümpler, Hannover, 1866-1867 (2 vols.); 4th ed. verb. und verm., Baumgartner's Buchhanlung, Leipzig, 1899 (3 vols.).

Ross, W. D. (1949) *Aristotle's Prior and Posterior Analytics* (A revised text with introduction and commentary by W. D. Ross), Clarendon Press, Oxford, 1949.

Rossi, P. (1962) *I filosofi e le macchine*, Feltrinelli, Milano, 1962.

Rowe, D. E. (1989) "Klein, Hilbert, and the Göttingen mathematical tradition", *Osiris*, 2nd ser., **5**, 1989, 186-213.

Russell, B. (1903A) *The Principles of Mathematics*, Cambridge Univ. Press, Cambridge, 1903.

Russell, B. (1903*b*) "Recent Works on the Philosophy of Leibniz", *Mind* **13**, 1903, 177-201.

Salanskis, J. - M. (1991) *L'herméneutique formelle*, éd. du CNRS, Paris, 1991.

Salanskis, J.-M. (1995) "Platonisme et philosophie des mathématiques", in Panza and Salanskis (1995), 179-212.

Santambrogio, M. (1992) *Forma e oggetto*, Il Saggiatore, Milano 1992.

Scharlau, W. (1979) "Zur Entstehung der Reinen Mathematik", in *Epistemologische und soziale Probleme der Wissenschaftsentwicklung im frühen 19. Jahrhundert* (ed. by the Institut für Didaktik der Mathematik der Universität Bielefeld), Arbeitstagung im Zentrum für interdisziplinäre Forschung in Bielefeld vom 27-30. Nov. 1979, 267-284.

Schuster, J. (1980) "Descartes' mathesis universalis; 1618-1628", in Gaukroger, S. W., (ed. by) *Descartes; philosophy, mathematics and physics*, Harvester Press, Sussex, 41-96.

Scott, J. F. (1938) *The Mathematical Work of John Wallis, D.D., F.R.S. (1616-1703)*, Taylor and Francis, Ltd. London, 1938.

Scriba, C. J. (1960-1962) "Zur Lösung des 2. Debeaunischen Problems durch Descartes. Ein Abschnitt aus der Frühgeschichte der inversen Tangentenaufgaben", *Archive for History of Exact Sciences*, **1**, 1960-1962, 406-419.

Searle, J. (1992) *The Rediscovery of the Mind*, MIT Press, Cambridge (Mass.), 1992.

Sebestik, J. (1992) *Logique et mathématique chez Bolzano*, Vrin, Paris, 1992.

Sinaceur, H. (1991) *Corps et modèles*, Vrin, Paris, 1991.

Smith, R. (1983) *Aristotle's Prior Analytics* (translation with introduction, notes and commentary by R. Smith), Haeken P. C., Indianapolis, Cambridge, 1983.

Stäckel, P. (1894) "Abhandlungen über Variations-rechnung, erster Theil: Abhandlungen von Joh. Bernoulli (1696), Jac. Bernoulli (1697) und Leonhard Euler, (1744)", *Ostwalds klassiker der exakten Wissenschaft*, 4b, Engelmann, Leipzig, 1894 (contains a partial German translation of Euler (1744): chapters 1, 2, 5 and 6).

Stigler, S. (1986) *The History of Statistics*, Belknap Press of Harvard University Press, Cambridge (Mass.), 1986.

Struik, D. J. (1969) *A Source Book in Mathematics, 1200-1800*, Harvard Univ. Press, Cambridge (Mass.), 1969.

Szabó, A. (1974) "Working backwards and proving by synthesis", in Hintikka and Remes (1974), 118-130.

Taton, R. (1951) *L'Œuvre scientifique de Monge*, P.U.F., Paris, 1951.

Tharp, L. (1989-1991) "Myth and Mathematics: A Conceptualistic Philosophy of Mathematics", *Synthese*, **81**, 1989, 167-201 and **88**, 1991, 179-199.

Thiel, C. (1988) "Begriff und Geschichte der Abstraktion", in K. Prätor (ed. by), *Aspekte der Abstraktionstheorie, Aachener Schriften zur Wissenschaften-Theorie, Logic und Sprachphilosophie*, **2**, 1988, 36-48.

Tieszen, R. (1989) *Mathematical Intuition: Phenomenology and Mathematical Knowledge*, Kluwer, Dordrecht, 1989.

Tieszen, R. (1994) "Mathematical Realism and Gödel's Incompleteness Theorems", *Philosophia Mathematica*, 3-rd ser., **2**, 177-201.

Tieszen, R. (1995) "Mathematics", in B. Smith and D. Smith (ed. by), *Cambridge Companions to Philosophy: Husserl*, Cambridge Univ. Press, Cambridge, 438-462.

Timmermans, B. (1995) *La résolution des problèmes de Descartes à Kant*, P.U.F., Paris, 1995.

Todhunter, I. (1949) *A History of the Mathematical Theory of Probability from the Time of Pascal to that of Laplace*, Chelsea Publishing Company, New York, 1949.

Toepell, M. M. (1986) *Über die Entstehung von David Hilberts 'Grundlagen der Geometrie'*, Vandenhoeck und Ruprecht, Göttingen, 1986.

Trabal, P. (1995) *Le sens commun, les mathématiques et les sciences : une approche de la sociologie des sciences par une étude des représentations sociales des mathématiques et des sciences*, thèse de Doctorat, EHESS, Paris, 1995.

Turing, A. (1936) "On Computable Numbers, with an Application to the Entscheidungsproblem", *Proceedings of the London Mathematical Society*, **42**, 230-265.

Van der Waerden, B. L. (1930-1931) *Moderne Algebra*, unter Benutzung von Vorlesungen von E. Artin und E. Nöther, Springer, Berlin, 1930-1931 (2 vols.); 2nd ed., 1937-1940 (2 vols.).

Varignon, P. (1701) "Des forces centrales, ou des pesanteurs necessaires aux planetes pour leur faire décrire les orbes qu'on leur a supposés jusqu'icy", *Hist. Acad. Roy. Sci. [Paris], Mém. Math. et Phy.*, 1701 (publ. 1703), 218-237.

Varignon, P. (1704) "Nouvelle formation de spirales beaucoup plus différentes entr'elles que tout ce qu'on peut imaginer d'autres courbes quelconques à l'infini; avec les touchantes, les quadratures, les déroulemens, & les longueurs de quelques-unes de ces spirales qu'on donne seulement ici pour éxemples de cette formation générale", *Hist. Acad. Roy. Sci. [Paris], Mém. Math. et Phy.*, 1704 (publ. 1706), 69-131.

Viète, F. (1591*a*) *In artem analyticem Isagoge*, Mettayer, Turonis, 1591; English transl. by J. W. Smith in Klein (1968), 312-353.

Viète, F. (1591*b*) *Zeteticorum libri quinque*, Mettayer, Turonis, 1591.

Viète, F. (1593) *Francisci Vietæ Variorum de rebus mathematicis responsorum, liber VIII. Cuius præcipua capita sunt, De duplicatione Cubi, et Quadratione Circuli. Quæ claudit* próxeiron, *seu Ad Usum Mathematici Canonis Methodica*, Mettayer, Tvronis, 1593; a translation into French, with a commentary is due to appear soon.

Viète, F. (1615) *De æquqtiones recognitione et emendatione tractatus duo*, J. Laquehay, Paris, 1615.

Viète, F. (IV) *Introduction en l'art analytic, ou, Nouvelle algebre de Francois Viete [....]. Traduit en notre langue & commenté & illustré par I. L. sieur de Vaulezard*, I. Jacquin, Paris, 1630; new ed. quot., in the *Corpus des Œuvres de Philosophie en langue française*: [J. L.] Vaulézard, *La nouvelle algèbre de H. Viète*, Fayard, Paris, 1986.

Vuillemin, J. (1960) *Mathématiques et métaphysique chez Descartes*, P.U.F., Paris, 1960.

Wallis, J.(1656) *Arithmetica Infinitorum, sive Nova Methodus Inquirendi in Curvilineorum Quadraturam, aliaque difficiliora Matheseos Problemata*, L. Lichfield, Oxonii, 1656 (in J. Wallis, *Operum Mathematicorum Pars Altera [...]*, L. Lichfield, Oxonii, 1956, separated pagination).

Wang, H. (1987) *Reflections on Kurt Gödel*, MIT Press, Cambridge (Mass.), 1987.

Westfall, R. S. (1980) *Never at Rest A biography of Isaac Newton*, Cambridge Univ. Press, Cambridge, London, 1980.

Weyl, H. (1910) "Ueber die Definitionen der mathematischen Begriffe", *Mathematisch-naturwissenschaftliche Blaetter*, **7**, 1910, 93-95 and 109-113.

Weyl, H. (1913) "Die Idee der Riemannschen Flaeche", Teubner, Leipzig und Berlin, 1913.

Whitehead, A. N. and Russell, B. (1910-1913) *Principia Mathematica*, Cambridge Univ. Press, Cambridge, 1910-1913 (3 vols.).

Whiteside, D. T. (1960-1962) "Patterns of Mathematical Thought in the Later Seventeenth Century", *Archive for History of Exact Sciences*, **1**, 1960-1962, 179-388.

Wiener, N. (1923) "On the Nature of Mathematical Thinking", *Australian Journal of Psychology and Philosophy*, **1**, 1923, pp. 268-272; in *Collected Works*, MIT Press, Cambridge (Mass.), 1976-1985 (4 vols.), vol. I, 234-238.

Wiener, N. (1930) "Generalized Harmonic Analysis", *Acta Mathematica*, **55**, 1930, 117-258.

Wiener, N. (1938) "The historical background of harmony analysis", *Amer. Math. Soc. semicentenial publ.*, **2**, 1938, 56-58.

Woodhouse, R. (1810) *A treatise on isoperimetrical prǫblems and the calculus of variations*, Cambridge Univ. Press, Cambridge, 1810.

Wright, C. (1983) *Frege's conceptions of Numbers as Objects*, Aberdeen Univ. Press, Aberdeen, 1983.

Zeuthen, H. G (1886) *Die Lehre von die Kegelschnitten im Altertum*, A. F. Host & Sohn, Kopenhagen, 1896.

Zeuthen, H. G. (1893) *Forelæsning over Mathematikens Historie, Oldtid og Middelalder*, Høst & Søns, Kjøbenhavn; 2-nd ed. revised by O. Neugebauer, 1949.

Zeuthen, H. G. (1896) "Die geometrische Construction als 'Existenzbeweis' in der antiken Geometrie, *Mathematische Annalen*, **47**, 1896, 222-228.

INDEX OF NAMES

K

L

M

N

O

P

Q

R

S

Boston Studies in the Philosophy of Science

Editor: Robert S. Cohen, *Boston University*

1. M.W. Wartofsky (ed.): *Proceedings of the Boston Colloquium for the Philosophy of Science, 1961/1962.* [Synthese Library 6] 1963 ISBN 90-277-0021-4
2. R.S. Cohen and M.W. Wartofsky (eds.): *Proceedings of the Boston Colloquium for the Philosophy of Science, 1962/1964.* In Honor of P. Frank. [Synthese Library 10] 1965 ISBN 90-277-9004-0
3. R.S. Cohen and M.W. Wartofsky (eds.): *Proceedings of the Boston Colloquium for the Philosophy of Science, 1964/1966.* In Memory of Norwood Russell Hanson. [Synthese Library 14] 1967 ISBN 90-277-0013-3
4. R.S. Cohen and M.W. Wartofsky (eds.): *Proceedings of the Boston Colloquium for the Philosophy of Science, 1966/1968.* [Synthese Library 18] 1969 ISBN 90-277-0014-1
5. R.S. Cohen and M.W. Wartofsky (eds.): *Proceedings of the Boston Colloquium for the Philosophy of Science, 1966/1968.* [Synthese Library 19] 1969 ISBN 90-277-0015-X
6. R.S. Cohen and R.J. Seeger (eds.): *Ernst Mach, Physicist and Philosopher.* [Synthese Library 27] 1970 ISBN 90-277-0016-8
7. M. Čapek: *Bergson and Modern Physics.* A Reinterpretation and Re-evaluation. [Synthese Library 37] 1971 ISBN 90-277-0186-5
8. R.C. Buck and R.S. Cohen (eds.): *PSA 1970.* Proceedings of the 2nd Biennial Meeting of the Philosophy and Science Association (Boston, Fall 1970). In Memory of Rudolf Carnap. [Synthese Library 39] 1971 ISBN 90-277-0187-3; Pb 90-277-0309-4
9. A.A. Zinov'ev: *Foundations of the Logical Theory of Scientific Knowledge (Complex Logic).* Translated from Russian. Revised and enlarged English Edition, with an Appendix by G.A. Smirnov, E.A. Sidorenko, A.M. Fedina and L.A. Bobrova. [Synthese Library 46] 1973 ISBN 90-277-0193-8; Pb 90-277-0324-8
10. L. Tondl: *Scientific Procedures.* A Contribution Concerning the Methodological Problems of Scientific Concepts and Scientific Explanation.Translated from Czech. [Synthese Library 47] 1973 ISBN 90-277-0147-4; Pb 90-277-0323-X
11. R.J. Seeger and R.S. Cohen (eds.): *Philosophical Foundations of Science.* Proceedings of Section L, 1969, American Association for the Advancement of Science. [Synthese Library 58] 1974 ISBN 90-277-0390-6; Pb 90-277-0376-0
12. A. Grünbaum: *Philosophical Problems of Space and Times.* 2nd enlarged ed. [Synthese Library 55] 1973 ISBN 90-277-0357-4; Pb 90-277-0358-2
13. R.S. Cohen and M.W. Wartofsky (eds.): *Logical and Epistemological Studies in Contemporary Physics.* Proceedings of the Boston Colloquium for the Philosophy of Science, 1969/72, Part I. [Synthese Library 59] 1974 ISBN 90-277-0391-4; Pb 90-277-0377-9
14. R.S. Cohen and M.W. Wartofsky (eds.): *Methodological and Historical Essays in the Natural and Social Sciences.* Proceedings of the Boston Colloquium for the Philosophy of Science, 1969/72, Part II. [Synthese Library 60] 1974 ISBN 90-277-0392-2; Pb 90-277-0378-7
15. R.S. Cohen, J.J. Stachel and M.W. Wartofsky (eds.): *For Dirk Struik.* Scientific, Historical and Political Essays in Honor of Dirk J. Struik. [Synthese Library 61] 1974 ISBN 90-277-0393-0; Pb 90-277-0379-5
16. N. Geschwind: *Selected Papers on Language and the Brains.* [Synthese Library 68] 1974 ISBN 90-277-0262-4; Pb 90-277-0263-2
17. B.G. Kuznetsov: *Reason and Being.* Translated from Russian. Edited by C.R. Fawcett and R.S. Cohen. 1987 ISBN 90-277-2181-5

18. P. Mittelstaedt: *Philosophical Problems of Modern Physics*. Translated from the revised 4th German edition by W. Riemer and edited by R.S. Cohen. [Synthese Library 95] 1976 ISBN 90-277-0285-3; Pb 90-277-0506-2
19. H. Mehlberg: *Time, Causality, and the Quantum Theory*. Studies in the Philosophy of Science. Vol. I: *Essay on the Causal Theory of Time*. Vol. II: *Time in a Quantized Universe*. Translated from French. Edited by R.S. Cohen. 1980 Vol. I: ISBN 90-277-0721-9; Pb 90-277-1074-0 Vol. II: ISBN 90-277-1075-9; Pb 90-277-1076-7
20. K.F. Schaffner and R.S. Cohen (eds.): *PSA 1972*. Proceedings of the 3rd Biennial Meeting of the Philosophy of Science Association (Lansing, Michigan, Fall 1972). [Synthese Library 64] 1974 ISBN 90-277-0408-2; Pb 90-277-0409-0
21. R.S. Cohen and J.J. Stachel (eds.): *Selected Papers of Léon Rosenfeld*. [Synthese Library 100] 1979 ISBN 90-277-0651-4; Pb 90-277-0652-2
22. M. Čapek (ed.): *The Concepts of Space and Time*. Their Structure and Their Development. [Synthese Library 74] 1976 ISBN 90-277-0355-8; Pb 90-277-0375-2
23. M. Grene: *The Understanding of Nature*. Essays in the Philosophy of Biology. [Synthese Library 66] 1974 ISBN 90-277-0462-7; Pb 90-277-0463-5
24. D. Ihde: *Technics and Praxis*. A Philosophy of Technology. [Synthese Library 130] 1979 ISBN 90-277-0953-X; Pb 90-277-0954-8
25. J. Hintikka and U. Remes: *The Method of Analysis*. Its Geometrical Origin and Its General Significance. [Synthese Library 75] 1974 ISBN 90-277-0532-1; Pb 90-277-0543-7
26. J.E. Murdoch and E.D. Sylla (eds.): *The Cultural Context of Medieval Learning*. Proceedings of the First International Colloquium on Philosophy, Science, and Theology in the Middle Ages, 1973. [Synthese Library 76] 1975 ISBN 90-277-0560-7; Pb 90-277-0587-9
27. M. Grene and E. Mendelsohn (eds.): *Topics in the Philosophy of Biology*. [Synthese Library 84] 1976 ISBN 90-277-0595-X; Pb 90-277-0596-8
28. J. Agassi: *Science in Flux*. [Synthese Library 80] 1975 ISBN 90-277-0584-4; Pb 90-277-0612-3
29. J.J. Wiatr (ed.): *Polish Essays in the Methodology of the Social Sciences*. [Synthese Library 131] 1979 ISBN 90-277-0723-5; Pb 90-277-0956-4
30. P. Janich: *Protophysics of Time*. Constructive Foundation and History of Time Measurement. Translated from German. 1985 ISBN 90-277-0724-3
31. R.S. Cohen and M.W. Wartofsky (eds.): *Language, Logic, and Method*. 1983 ISBN 90-277-0725-1
32. R.S. Cohen, C.A. Hooker, A.C. Michalos and J.W. van Evra (eds.): *PSA 1974*. Proceedings of the 4th Biennial Meeting of the Philosophy of Science Association. [Synthese Library 101] 1976 ISBN 90-277-0647-6; Pb 90-277-0648-4
33. G. Holton and W.A. Blanpied (eds.): *Science and Its Public*. The Changing Relationship. [Synthese Library 96] 1976 ISBN 90-277-0657-3; Pb 90-277-0658-1
34. M.D. Grmek, R.S. Cohen and G. Cimino (eds.): *On Scientific Discovery*. The 1977 Erice Lectures. 1981 ISBN 90-277-1122-4; Pb 90-277-1123-2
35. S. Amsterdamski: *Between Experience and Metaphysics*. Philosophical Problems of the Evolution of Science. Translated from Polish. [Synthese Library 77] 1975 ISBN 90-277-0568-2; Pb 90-277-0580-1
36. M. Marković and G. Petrović (eds.): *Praxis*. Yugoslav Essays in the Philosophy and Methodology of the Social Sciences. [Synthese Library 134] 1979 ISBN 90-277-0727-8; Pb 90-277-0968-8

Boston Studies in the Philosophy of Science

37. H. von Helmholtz: *Epistemological Writings*. The Paul Hertz / Moritz Schlick Centenary Edition of 1921. Translated from German by M.F. Lowe. Edited with an Introduction and Bibliography by R.S. Cohen and Y. Elkana. [Synthese Library 79] 1977 ISBN 90-277-0290-X; Pb 90-277-0582-8
38. R.M. Martin: *Pragmatics, Truth and Language*. 1979 ISBN 90-277-0992-0; Pb 90-277-0993-9
39. R.S. Cohen, P.K. Feyerabend and M.W. Wartofsky (eds.): *Essays in Memory of Imre Lakatos*. [Synthese Library 99] 1976 ISBN 90-277-0654-9; Pb 90-277-0655-7
40. Not published.
41. Not published.
42. H.R. Maturana and F.J. Varela: *Autopoiesis and Cognition*. The Realization of the Living. With a Preface to 'Autopoiesis' by S. Beer. 1980 ISBN 90-277-1015-5; Pb 90-277-1016-3
43. A. Kasher (ed.): *Language in Focus: Foundations, Methods and Systems*. Essays in Memory of Yehoshua Bar-Hillel. [Synthese Library 89] 1976 ISBN 90-277-0644-1; Pb 90-277-0645-X
44. T.D. Thao: *Investigations into the Origin of Language and Consciousness*. 1984 ISBN 90-277-0827-4
45. Not published.
46. P.L. Kapitza: *Experiment, Theory, Practice*. Articles and Addresses. Edited by R.S. Cohen. 1980 ISBN 90-277-1061-9; Pb 90-277-1062-7
47. M.L. Dalla Chiara (ed.): *Italian Studies in the Philosophy of Science*. 1981 ISBN 90-277-0735-9; Pb 90-277-1073-2
48. M.W. Wartofsky: *Models*. Representation and the Scientific Understanding. [Synthese Library 129] 1979 ISBN 90-277-0736-7; Pb 90-277-0947-5
49. T.D. Thao: *Phenomenology and Dialectical Materialism*. Edited by R.S. Cohen. 1986 ISBN 90-277-0737-5
50. Y. Fried and J. Agassi: *Paranoia*. A Study in Diagnosis. [Synthese Library 102] 1976 ISBN 90-277-0704-9; Pb 90-277-0705-7
51. K.H. Wolff: *Surrender and Cath*. Experience and Inquiry Today. [Synthese Library 105] 1976 ISBN 90-277-0758-8; Pb 90-277-0765-0
52. K. Kosík: *Dialectics of the Concrete*. A Study on Problems of Man and World. 1976 ISBN 90-277-0761-8; Pb 90-277-0764-2
53. N. Goodman: *The Structure of Appearance*. [Synthese Library 107] 1977 ISBN 90-277-0773-1; Pb 90-277-0774-X
54. H.A. Simon: *Models of Discovery* and Other Topics in the Methods of Science. [Synthese Library 114] 1977 ISBN 90-277-0812-6; Pb 90-277-0858-4
55. M. Lazerowitz: *The Language of Philosophy*. Freud and Wittgenstein. [Synthese Library 117] 1977 ISBN 90-277-0826-6; Pb 90-277-0862-2
56. T. Nickles (ed.): *Scientific Discovery, Logic, and Rationality*. 1980 ISBN 90-277-1069-4; Pb 90-277-1070-8
57. J. Margolis: *Persons and Mind*. The Prospects of Nonreductive Materialism. [Synthese Library 121] 1978 ISBN 90-277-0854-1; Pb 90-277-0863-0
58. G. Radnitzky and G. Andersson (eds.): *Progress and Rationality in Science*. [Synthese Library 125] 1978 ISBN 90-277-0921-1; Pb 90-277-0922-X
59. G. Radnitzky and G. Andersson (eds.): *The Structure and Development of Science*. [Synthese Library 136] 1979 ISBN 90-277-0994-7; Pb 90-277-0995-5

60. T. Nickles (ed.): *Scientific Discovery.* Case Studies. 1980
ISBN 90-277-1092-9; Pb 90-277-1093-7
61. M.A. Finocchiaro: *Galileo and the Art of Reasoning.* Rhetorical Foundation of Logic and Scientific Method. 1980 ISBN 90-277-1094-5; Pb 90-277-1095-3
62. W.A. Wallace: *Prelude to Galileo.* Essays on Medieval and 16th-Century Sources of Galileo's Thought. 1981 ISBN 90-277-1215-8; Pb 90-277-1216-6
63. F. Rapp: *Analytical Philosophy of Technology.* Translated from German. 1981
ISBN 90-277-1221-2; Pb 90-277-1222-0
64. R.S. Cohen and M.W. Wartofsky (eds.): *Hegel and the Sciences.* 1984
ISBN 90-277-0726-X
65. J. Agassi: *Science and Society.* Studies in the Sociology of Science. 1981
ISBN 90-277-1244-1; Pb 90-277-1245-X
66. L. Tondl: *Problems of Semantics.* A Contribution to the Analysis of the Language of Science. Translated from Czech. 1981 ISBN 90-277-0148-2; Pb 90-277-0316-7
67. J. Agassi and R.S. Cohen (eds.): *Scientific Philosophy Today.* Essays in Honor of Mario Bunge. 1982 ISBN 90-277-1262-X; Pb 90-277-1263-8
68. W. Krajewski (ed.): *Polish Essays in the Philosophy of the Natural Sciences.* Translated from Polish and edited by R.S. Cohen and C.R. Fawcett. 1982
ISBN 90-277-1286-7; Pb 90-277-1287-5
69. J.H. Fetzer: *Scientific Knowledge.* Causation, Explanation and Corroboration. 1981
ISBN 90-277-1335-9; Pb 90-277-1336-7
70. S. Grossberg: *Studies of Mind and Brain.* Neural Principles of Learning, Perception, Development, Cognition, and Motor Control. 1982
ISBN 90-277-1359-6; Pb 90-277-1360-X
71. R.S. Cohen and M.W. Wartofsky (eds.): *Epistemology, Methodology, and the Social Sciences.* 1983. ISBN 90-277-1454-1
72. K. Berka: *Measurement.* Its Concepts, Theories and Problems. Translated from Czech. 1983
ISBN 90-277-1416-9
73. G.L. Pandit: *The Structure and Growth of Scientific Knowledge.* A Study in the Methodology of Epistemic Appraisal. 1983 ISBN 90-277-1434-7
74. A.A. Zinov'ev: *Logical Physics.* Translated from Russian. Edited by R.S. Cohen. 1983
[*see also* Volume 9] ISBN 90-277-0734-0
75. G-G. Granger: *Formal Thought and the Sciences of Man.* Translated from French. With and Introduction by A. Rosenberg. 1983 ISBN 90-277-1524-6
76. R.S. Cohen and L. Laudan (eds.): *Physics, Philosophy and Psychoanalysis.* Essays in Honor of Adolf Grünbaum. 1983 ISBN 90-277-1533-5
77. G. Böhme, W. van den Daele, R. Hohlfeld, W. Krohn and W. Schäfer: *Finalization in Science.* The Social Orientation of Scientific Progress. Translated from German. Edited by W. Schäfer. 1983 ISBN 90-277-1549-1
78. D. Shapere: *Reason and the Search for Knowledge.* Investigations in the Philosophy of Science. 1984 ISBN 90-277-1551-3; Pb 90-277-1641-2
79. G. Andersson (ed.): *Rationality in Science and Politics.* Translated from German. 1984
ISBN 90-277-1575-0; Pb 90-277-1953-5
80. P.T. Durbin and F. Rapp (eds.): *Philosophy and Technology.* [*Also* Philosophy and Technology Series, Vol. 1] 1983 ISBN 90-277-1576-9
81. M. Marković: *Dialectical Theory of Meaning.* Translated from Serbo-Croat. 1984
ISBN 90-277-1596-3

Boston Studies in the Philosophy of Science

82. R.S. Cohen and M.W. Wartofsky (eds.): *Physical Sciences and History of Physics.* 1984.
ISBN 90-277-1615-3
83. É. Meyerson: *The Relativistic Deduction.* Epistemological Implications of the Theory of Relativity. Translated from French. With a Review by Albert Einstein and an Introduction by Milić Čapek. 1985 ISBN 90-277-1699-4
84. R.S. Cohen and M.W. Wartofsky (eds.): *Methodology, Metaphysics and the History of Science.* In Memory of Benjamin Nelson. 1984 ISBN 90-277-1711-7
85. G. Tamás: *The Logic of Categories.* Translated from Hungarian. Edited by R.S. Cohen. 1986
ISBN 90-277-1742-7
86. S.L. de C. Fernandes: *Foundations of Objective Knowledge.* The Relations of Popper's Theory of Knowledge to That of Kant. 1985 ISBN 90-277-1809-1
87. R.S. Cohen and T. Schnelle (eds.): *Cognition and Fact.* Materials on Ludwik Fleck. 1986
ISBN 90-277-1902-0
88. G. Freudenthal: *Atom and Individual in the Age of Newton.* On the Genesis of the Mechanistic World View. Translated from German. 1986 ISBN 90-277-1905-5
89. A. Donagan, A.N. Perovich Jr and M.V. Wedin (eds.): *Human Nature and Natural Knowledge.* Essays presented to Marjorie Grene on the Occasion of Her 75th Birthday. 1986
ISBN 90-277-1974-8
90. C. Mitcham and A. Hunning (eds.): *Philosophy and Technology II.* Information Technology and Computers in Theory and Practice. [*Also* Philosophy and Technology Series, Vol. 2] 1986 ISBN 90-277-1975-6
91. M. Grene and D. Nails (eds.): *Spinoza and the Sciences.* 1986 ISBN 90-277-1976-4
92. S.P. Turner: *The Search for a Methodology of Social Science.* Durkheim, Weber, and the 19th-Century Problem of Cause, Probability, and Action. 1986. ISBN 90-277-2067-3
93. I.C. Jarvie: *Thinking about Society.* Theory and Practice. 1986 ISBN 90-277-2068-1
94. E. Ullmann-Margalit (ed.): *The Kaleidoscope of Science.* The Israel Colloquium: Studies in History, Philosophy, and Sociology of Science, Vol. 1. 1986
ISBN 90-277-2158-0; Pb 90-277-2159-9
95. E. Ullmann-Margalit (ed.): *The Prism of Science.* The Israel Colloquium: Studies in History, Philosophy, and Sociology of Science, Vol. 2. 1986
ISBN 90-277-2160-2; Pb 90-277-2161-0
96. G. Márkus: *Language and Production.* A Critique of the Paradigms. Translated from French. 1986 ISBN 90-277-2169-6
97. F. Amrine, F.J. Zucker and H. Wheeler (eds.): *Goethe and the Sciences: A Reappraisal.* 1987 ISBN 90-277-2265-X; Pb 90-277-2400-8
98. J.C. Pitt and M. Pera (eds.): *Rational Changes in Science.* Essays on Scientific Reasoning. Translated from Italian. 1987 ISBN 90-277-2417-2
99. O. Costa de Beauregard: *Time, the Physical Magnitude.* 1987 ISBN 90-277-2444-X
100. A. Shimony and D. Nails (eds.): *Naturalistic Epistemology.* A Symposium of Two Decades. 1987 ISBN 90-277-2337-0
101. N. Rotenstreich: *Time and Meaning in History.* 1987 ISBN 90-277-2467-9
102. D.B. Zilberman: *The Birth of Meaning in Hindu Thought.* Edited by R.S. Cohen. 1988
ISBN 90-277-2497-0
103. T.F. Glick (ed.): *The Comparative Reception of Relativity.* 1987 ISBN 90-277-2498-9
104. Z. Harris, M. Gottfried, T. Ryckman, P. Mattick Jr, A. Daladier, T.N. Harris and S. Harris: *The Form of Information in Science.* Analysis of an Immunology Sublanguage. With a Preface by Hilary Putnam. 1989 ISBN 90-277-2516-0

105. F. Burwick (ed.): *Approaches to Organic Form*. Permutations in Science and Culture. 1987 ISBN 90-277-2541-1
106. M. Almási: *The Philosophy of Appearances*. Translated from Hungarian. 1989 ISBN 90-277-2150-5
107. S. Hook, W.L. O'Neill and R. O'Toole (eds.): *Philosophy, History and Social Action*. Essays in Honor of Lewis Feuer. With an Autobiographical Essay by L. Feuer. 1988 ISBN 90-277-2644-2
108. I. Hronszky, M. Fehér and B. Dajka: *Scientific Knowledge Socialized*. Selected Proceedings of the 5th Joint International Conference on the History and Philosophy of Science organized by the IUHPS (Veszprém, Hungary, 1984). 1988 ISBN 90-277-2284-6
109. P. Tillers and E.D. Green (eds.): *Probability and Inference in the Law of Evidence*. The Uses and Limits of Bayesianism. 1988 ISBN 90-277-2689-2
110. E. Ullmann-Margalit (ed.): *Science in Reflection*. The Israel Colloquium: Studies in History, Philosophy, and Sociology of Science, Vol. 3. 1988 ISBN 90-277-2712-0; Pb 90-277-2713-9
111. K. Gavroglu, Y. Goudaroulis and P. Nicolacopoulos (eds.): *Imre Lakatos and Theories of Scientific Change*. 1989 ISBN 90-277-2766-X
112. B. Glassner and J.D. Moreno (eds.): *The Qualitative-Quantitative Distinction in the Social Sciences*. 1989 ISBN 90-277-2829-1
113. K. Arens: *Structures of Knowing*. Psychologies of the 19th Century. 1989 ISBN 0-7923-0009-2
114. A. Janik: *Style, Politics and the Future of Philosophy*. 1989 ISBN 0-7923-0056-4
115. F. Amrine (ed.): *Literature and Science as Modes of Expression*. With an Introduction by S. Weininger. 1989 ISBN 0-7923-0133-1
116. J.R. Brown and J. Mittelstrass (eds.): *An Intimate Relation*. Studies in the History and Philosophy of Science. Presented to Robert E. Butts on His 60th Birthday. 1989 ISBN 0-7923-0169-2
117. F. D'Agostino and I.C. Jarvie (eds.): *Freedom and Rationality*. Essays in Honor of John Watkins. 1989 ISBN 0-7923-0264-8
118. D. Zolo: *Reflexive Epistemology*. The Philosophical Legacy of Otto Neurath. 1989 ISBN 0-7923-0320-2
119. M. Kearn, B.S. Philips and R.S. Cohen (eds.): *Georg Simmel and Contemporary Sociology*. 1989 ISBN 0-7923-0407-1
120. T.H. Levere and W.R. Shea (eds.): *Nature, Experiment and the Science*. Essays on Galileo and the Nature of Science. In Honour of Stillman Drake. 1989 ISBN 0-7923-0420-9
121. P. Nicolacopoulos (ed.): *Greek Studies in the Philosophy and History of Science*. 1990 ISBN 0-7923-0717-8
122. R. Cooke and D. Costantini (eds.): *Statistics in Science*. The Foundations of Statistical Methods in Biology, Physics and Economics. 1990 ISBN 0-7923-0797-6
123. P. Duhem: *The Origins of Statics*. Translated from French by G.F. Leneaux, V.N. Vagliente and G.H. Wagner. With an Introduction by S.L. Jaki. 1991 ISBN 0-7923-0898-0
124. H. Kamerlingh Onnes: *Through Measurement to Knowledge*. The Selected Papers, 1853-1926. Edited and with an Introduction by K. Gavroglu and Y. Goudaroulis. 1991 ISBN 0-7923-0825-5
125. M. Čapek: *The New Aspects of Time: Its Continuity and Novelties*. Selected Papers in the Philosophy of Science. 1991 ISBN 0-7923-0911-1

Boston Studies in the Philosophy of Science

126. S. Unguru (ed.): *Physics, Cosmology and Astronomy, 1300-1700*. Tension and Accommodation. 1991 ISBN 0-7923-1022-5
127. Z. Bechler: *Newton's Physics on the Conceptual Structure of the Scientific Revolution*. 1991 ISBN 0-7923-1054-3
128. É. Meyerson: *Explanation in the Sciences*. Translated from French by M-A. Siple and D.A. Siple. 1991 ISBN 0-7923-1129-9
129. A.I. Tauber (ed.): *Organism and the Origins of Self*. 1991 ISBN 0-7923-1185-X
130. F.J. Varela and J-P. Dupuy (eds.): *Understanding Origins*. Contemporary Views on the Origin of Life, Mind and Society. 1992 ISBN 0-7923-1251-1
131. G.L. Pandit: *Methodological Variance*. Essays in Epistemological Ontology and the Methodology of Science. 1991 ISBN 0-7923-1263-5
132. G. Munévar (ed.): *Beyond Reason*. Essays on the Philosophy of Paul Feyerabend. 1991 ISBN 0-7923-1272-4
133. T.E. Uebel (ed.): *Rediscovering the Forgotten Vienna Circle*. Austrian Studies on Otto Neurath and the Vienna Circle. Partly translated from German. 1991 ISBN 0-7923-1276-7
134. W.R. Woodward and R.S. Cohen (eds.): *World Views and Scientific Discipline Formation*. Science Studies in the [former] German Democratic Republic. Partly translated from German by W.R. Woodward. 1991 ISBN 0-7923-1286-4
135. P. Zambelli: *The Speculum Astronomiae and Its Enigma*. Astrology, Theology and Science in Albertus Magnus and His Contemporaries. 1992 ISBN 0-7923-1380-1
136. P. Petitjean, C. Jami and A.M. Moulin (eds.): *Science and Empires*. Historical Studies about Scientific Development and European Expansion. ISBN 0-7923-1518-9
137. W.A. Wallace: *Galileo's Logic of Discovery and Proof*. The Background, Content, and Use of His Appropriated Treatises on Aristotle's *Posterior Analytics*. 1992 ISBN 0-7923-1577-4
138. W.A. Wallace: *Galileo's Logical Treatises*. A Translation, with Notes and Commentary, of His Appropriated Latin Questions on Aristotle's *Posterior Analytics*. 1992 ISBN 0-7923-1578-2
Set (137 + 138) ISBN 0-7923-1579-0
139. M.J. Nye, J.L. Richards and R.H. Stuewer (eds.): *The Invention of Physical Science*. Intersections of Mathematics, Theology and Natural Philosophy since the Seventeenth Century. Essays in Honor of Erwin N. Hiebert. 1992 ISBN 0-7923-1753-X
140. G. Corsi, M.L. dalla Chiara and G.C. Ghirardi (eds.): *Bridging the Gap: Philosophy, Mathematics and Physics*. Lectures on the Foundations of Science. 1992 ISBN 0-7923-1761-0
141. C.-H. Lin and D. Fu (eds.): *Philosophy and Conceptual History of Science in Taiwan*. 1992 ISBN 0-7923-1766-1
142. S. Sarkar (ed.): *The Founders of Evolutionary Genetics*. A Centenary Reappraisal. 1992 ISBN 0-7923-1777-7
143. J. Blackmore (ed.): *Ernst Mach – A Deeper Look*. Documents and New Perspectives. 1992 ISBN 0-7923-1853-6
144. P. Kroes and M. Bakker (eds.): *Technological Development and Science in the Industrial Age*. New Perspectives on the Science–Technology Relationship. 1992 ISBN 0-7923-1898-6
145. S. Amsterdamski: *Between History and Method*. Disputes about the Rationality of Science. 1992 ISBN 0-7923-1941-9
146. E. Ullmann-Margalit (ed.): *The Scientific Enterprise*. The Bar-Hillel Colloquium: Studies in History, Philosophy, and Sociology of Science, Volume 4. 1992 ISBN 0-7923-1992-3

147. L. Embree (ed.): *Metaarchaeology*. Reflections by Archaeologists and Philosophers. 1992 ISBN 0-7923-2023-9
148. S. French and H. Kamminga (eds.): *Correspondence, Invariance and Heuristics*. Essays in Honour of Heinz Post. 1993 ISBN 0-7923-2085-9
149. M. Bunzl: *The Context of Explanation*. 1993 ISBN 0-7923-2153-7
150. I.B. Cohen (ed.): *The Natural Sciences and the Social Sciences*. Some Critical and Historical Perspectives. 1994 ISBN 0-7923-2223-1
151. K. Gavroglu, Y. Christianidis and E. Nicolaidis (eds.): *Trends in the Historiography of Science*. 1994 ISBN 0-7923-2255-X
152. S. Poggi and M. Bossi (eds.): *Romanticism in Science*. Science in Europe, 1790–1840. 1994 ISBN 0-7923-2336-X
153. J. Faye and H.J. Folse (eds.): *Niels Bohr and Contemporary Philosophy*. 1994 ISBN 0-7923-2378-5
154. C.C. Gould and R.S. Cohen (eds.): *Artifacts, Representations, and Social Practice*. Essays for Marx W. Wartofsky. 1994 ISBN 0-7923-2481-1
155. R.E. Butts: *Historical Pragmatics*. Philosophical Essays. 1993 ISBN 0-7923-2498-6
156. R. Rashed: *The Development of Arabic Mathematics: Between Arithmetic and Algebra*. Translated from French by A.F.W. Armstrong. 1994 ISBN 0-7923-2565-6
157. I. Szumilewicz-Lachman (ed.): *Zygmunt Zawirski: His Life and Work*. With Selected Writings on Time, Logic and the Methodology of Science. Translations by Feliks Lachman. Ed. by R.S. Cohen, with the assistance of B. Bergo. 1994 ISBN 0-7923-2566-4
158. S.N. Haq: *Names, Natures and Things*. The Alchemist Jābir ibn Ḥayyān and His *Kitāb al-Aḥjār* (Book of Stones). 1994 ISBN 0-7923-2587-7
159. P. Plaass: *Kant's Theory of Natural Science*. Translation, Analytic Introduction and Commentary by Alfred E. and Maria G. Miller. 1994 ISBN 0-7923-2750-0
160. J. Misiek (ed.): *The Problem of Rationality in Science and its Philosophy*. On Popper vs. Polanyi. The Polish Conferences 1988–89. 1995 ISBN 0-7923-2925-2
161. I.C. Jarvie and N. Laor (eds.): *Critical Rationalism, Metaphysics and Science*. Essays for Joseph Agassi, Volume I. 1995 ISBN 0-7923-2960-0
162. I.C. Jarvie and N. Laor (eds.): *Critical Rationalism, the Social Sciences and the Humanities*. Essays for Joseph Agassi, Volume II. 1995 ISBN 0-7923-2961-9
    Set (161–162) ISBN 0-7923-2962-7
163. K. Gavroglu, J. Stachel and M.W. Wartofsky (eds.): *Physics, Philosophy, and the Scientific Community*. Essays in the Philosophy and History of the Natural Sciences and Mathematics. In Honor of Robert S. Cohen. 1995 ISBN 0-7923-2988-0
164. K. Gavroglu, J. Stachel and M.W. Wartofsky (eds.): *Science, Politics and Social Practice*. Essays on Marxism and Science, Philosophy of Culture and the Social Sciences. In Honor of Robert S. Cohen. 1995 ISBN 0-7923-2989-9
165. K. Gavroglu, J. Stachel and M.W. Wartofsky (eds.): *Science, Mind and Art*. Essays on Science and the Humanistic Understanding in Art, Epistemology, Religion and Ethics. Essays in Honor of Robert S. Cohen. 1995 ISBN 0-7923-2990-2
    Set (163–165) ISBN 0-7923-2991-0
166. K.H. Wolff: *Transformation in the Writing*. A Case of Surrender-and-Catch. 1995 ISBN 0-7923-3178-8
167. A.J. Kox and D.M. Siegel (eds.): *No Truth Except in the Details*. Essays in Honor of Martin J. Klein. 1995 ISBN 0-7923-3195-8

Boston Studies in the Philosophy of Science

168. J. Blackmore: *Ludwig Boltzmann, His Later Life and Philosophy, 1900–1906*. Book One: A Documentary History. 1995 ISBN 0-7923-3231-8
169. R.S. Cohen, R. Hilpinen and R. Qiu (eds.): *Realism and Anti-Realism in the Philosophy of Science*. Beijing International Conference, 1992. 1996 ISBN 0-7923-3233-4
170. I. Kuçuradi and R.S. Cohen (eds.): *The Concept of Knowledge*. The Ankara Seminar. 1995 ISBN 0-7923-3241-5
171. M.A. Grodin (ed.): *Meta Medical Ethics*: The Philosophical Foundations of Bioethics. 1995 ISBN 0-7923-3344-6
172. S. Ramirez and R.S. Cohen (eds.): *Mexican Studies in the History and Philosophy of Science*. 1995 ISBN 0-7923-3462-0
173. C. Dilworth: *The Metaphysics of Science*. An Account of Modern Science in Terms of Principles, Laws and Theories. 1995 ISBN 0-7923-3693-3
174. J. Blackmore: *Ludwig Boltzmann, His Later Life and Philosophy, 1900–1906* Book Two: The Philosopher. 1995 ISBN 0-7923-3464-7
175. P. Damerow: *Abstraction and Representation*. Essays on the Cultural Evolution of Thinking. 1996 ISBN 0-7923-3816-2
176. G. Tarozzi (ed.): *Karl Popper, Philosopher of Science*. (in prep.)
177. M. Marion and R.S. Cohen (eds.): *Québec Studies in the Philosophy of Science*. Part I: Logic, Mathematics, Physics and History of Science. Essays in Honor of Hugues Leblanc. 1995 ISBN 0-7923-3559-7
178. M. Marion and R.S. Cohen (eds.): *Québec Studies in the Philosophy of Science*. Part II: Biology, Psychology, Cognitive Science and Economics. Essays in Honor of Hugues Leblanc. 1996 ISBN 0-7923-3560-0
    Set (177–178) ISBN 0-7923-3561-9
179. Fan Dainian and R.S. Cohen (eds.): *Chinese Studies in the History and Philosophy of Science and Technology*. 1996 ISBN 0-7923-3463-9
180. P. Forman and J.M. Sánchez-Ron (eds.): *National Military Establishments and the Advancement of Science and Technology*. Studies in 20th Century History. 1996 ISBN 0-7923-3541-4
181. E.J. Post: *Quantum Reprogramming*. Ensembles and Single Systems: A Two-Tier Approach to Quantum Mechanics. 1995 ISBN 0-7923-3565-1
182. A.I. Tauber (ed.): *The Elusive Synthesis: Aesthetics and Science*. 1996 ISBN 0-7923-3904-5
183. S. Sarkar (ed.): *The Philosophy and History of Molecular Biology: New Perspectives*. 1996 ISBN 0-7923-3947-9
184. J.T. Cushing, A. Fine and S. Goldstein (eds.): *Bohmian Mechanics and Quantum Theory: An Appraisal*. 1996 ISBN 0-7923-4028-0
185. K. Michalski: *Logic and Time*. An Essay on Husserl's Theory of Meaning. 1996 ISBN 0-7923-4082-5
186. G. Munévar (ed.): *Spanish Studies in the Philosophy of Science*. 1996 ISBN 0-7923-4147-3
187. G. Schubring (ed.): *Hermann Günther Graßmann (1809–1877): Visionary Mathematician, Scientist and Neohumanist Scholar*. Papers from a Sesquicentennial Conference. 1996 ISBN 0-7923-4261-5
188. M. Bitbol: *Schrödinger's Philosophy of Quantum Mechanics*. 1996 ISBN 0-7923-4266-6
189. J. Faye, U. Scheffler and M. Urchs (eds.): *Perspectives on Time*. 1997 ISBN 0-7923-4330-1
190. K. Lehrer and J.C. Marek (eds.): *Austrian Philosophy Past and Present*. Essays in Honor of Rudolf Haller. 1996 ISBN 0-7923-4347-6

Boston Studies in the Philosophy of Science

191. J.L. Lagrange: *Analytical Mechanics*. Translated and edited by Auguste Boissonade and Victor N. Vagliente. Translated from the *Mécanique Analytique, novelle édition* of 1811. 1997 ISBN 0-7923-4349-2
192. D. Ginev and R.S. Cohen (eds.): *Issues and Images in the Philosophy of Science*. Scientific and Philosophical Essays for Azarya Polikarov. 1997 ISBN 0-7923-4444-8
193. R.S. Cohen, M. Horne and J. Stachel (eds.): *Experimental Metaphysics*. Quantum Mechanical Studies for Abner Shimony, Volume One. 1997 ISBN 0-7923-4452-9
194. R.S. Cohen, M. Horne and J. Stachel (eds.): *Potentiality, Entanglement and Passion-at-a-Distance*. Quantum Mechanical Studies for Abner Shimony, Volume Two. 1997 ISBN 0-7923-4453-7; Set 0-7923-4454-5
195. R.S. Cohen and A.I. Tauber (eds.): *Philosophies of Nature: The Human Dimension*. 1997 ISBN 0-7923-4579-7
196. M. Otte and M. Panza (eds.): *Analysis and Synthesis in Mathematics*. History and Philosophy. 1997 ISBN 0-7923-4570-3

Also of interest:

R.S. Cohen and M.W. Wartofsky (eds.): *A Portrait of Twenty-Five Years Boston Colloquia for the Philosophy of Science, 1960-1985*. 1985 ISBN Pb 90-277-1971-3

Previous volumes are still available.

KLUWER ACADEMIC PUBLISHERS – DORDRECHT / BOSTON / LONDON