

THE EMERGENCE OF ANALYSIS IN THE RENAISSANCE AND AFTER

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I. THE KEY CONCEPTS OF ANALYSIS

Analysis as an independent division of mathematics—and it is the largest division by far—is a “modern” creation. Greek antiquity did not have it, nor did the Middle Ages, except for occasional gropings, mostly faint ones. But in the sixteenth and seventeenth centuries, analysis began to sprout and rise in many contexts, and its influence began to spread into the farthest precincts of mathematics. All this started very suddenly, and it was as great a revolution as any then proliferating.

The emergence of analysis in the Renaissance created a great divide in all of mathematics; there is a pre-analysis geometry and a post-analysis geometry; a pre-analysis algebra and a post-analysis algebra; a pre-analysis number theory and a post-analysis number theory, very much so; and, very importantly, a pre-analysis astronomy and a post-analysis astronomy. The first epochal manifestation of analysis—as analysis is conceived by us—was Johannes Kepler’s proclamation of his Planetary Laws, especially the Law of Areas. There had been nothing like it before in an astronomy that had

been a mathematically controlled science for two millennia, not even in Copernicus, whatever his astronomical innovations may have been.

Terminologically the Greeks did have analysis, in logic, in epistemology, and also in (late) mathematics, but for the most part in a contrasting parallelism with synthesis, and only rarely by itself. The verb *analysein* occurs already in Homer in the meaning of 'untie, unravel,' and a contrast between analysis (*Resolutio*) and synthesis (*Compositio*) plays a role in philosophy even today. Yet "analysis" and "analysis-synthesis" play only a secondary role in Greek mathematics, as can be gathered from the article of Mahoney [55], although this article is designed to present and elaborate the positive aspects of the Greek analysis. In any event, in mathematics this analysis was something procedural rather than substantive, as described in a famous passage in the work of the mathematician Pappus (third century A.D.), which is the principal ancient description known; see [55, p. 322]. Mahoney himself states:

if the Greeks admitted analysis as a member of the family, they nevertheless had tried to keep it in the background, barring it from any formal appearance. Only Pappus and a few scholiasts talked about it at any length. [56, p. 35]

In Euclid's *Elements* there is no mention of analysis nor even any allusion to it, except for a statement on analysis-synthesis which appears after theorem XIII.5, and which, as everybody agrees, is a scholiast's interpolation. After Euclid, there are occurrences in Archimedes, Apollonius, and perhaps others, but always quite secondary ones, and from the Middle Ages there is nothing remarkable known about "analysis." But in 1591 matters came to life, terminologically and substantively, in work of the algebraist François Viète (1540-1603), who may have been stimulated by Petrus Ramus (1505-1572) (see Mahoney [56, pp. 11-32], *passim*). Viète put "analysis" into the title of a book (*In artem analyticam isagoge*, Introduction into the analytic art), thus creating a fashion in terminology. "Analytic" and "analysis" began to appear frequently in titles of publications, and we will adduce noteworthy instances in due course. Substantively, the aim of Viète's *isagoge* was to develop techniques for operating with algebraic symbols (which was Viète's forte), and it clearly associated "analysis" with this endeavor. This also created a fashion. Many mathematicians began to be known as "analysts"; Fermat for instance so designated himself, as a matter of course. But there were no "synthesists," probably because they would not have been very different from "classical" geometers. Besides, "analysis" was quickly exceeding the bounds within which Viète had conceived it, and it soon began to designate most of the genuinely new mathematics bursting forth all around.

Thus Isaac Newton's first leading essay on mathematics proper, which was conceived and composed in various parts between 1665 and 1669—but

not published until 1711—was avowedly in analysis. Its title is “De analysi per aequationes infinitas” (*Analysis by infinite equations*), and at one point he refers to Descartes as an analyst (see [61, p. 222]). Yet on the title page of his great *Principia* he speaks of “mathematics,” which in the sixteenth and seventeenth centuries also subsumed mathematically controlled science, especially astronomy. The full title of the work is *Philosophiae naturalis principia mathematica* (Mathematical principles of natural philosophy), and on its title page Newton identifies himself as Lucasian Professor of “Mathesis.” Still, in the Scholium to the Laws of Motion, he refers to “Sir Christopher Wren, Dr. Wallis, and Mr. Huygens” as “the greatest geometers of our time” (see [61, p. 22]), just as, in the French language, the encomiastic designation “illustre géomètre” may refer to a distinguished mathematician of any research proclivity.

But what was this new analysis, and how did it relate to the algebra of symbols and to the infinitesimal calculus, which were such “obvious” manifestations of it? Our answer is that the operation with symbols was a kind of new technology, and that the infinitesimal calculus was a kind of automation based on it, but that analysis in a broad sense was a novel intellectual setting in which all this came to pass, and in which operational and foundational aspects were inseparable. The new setting was foreshadowed in Nicholas of Cusa (1401-1464) in the first half of the fifteenth century, but it actually formed itself in the sixteenth century, when rather suddenly, as if on a signal, mathematics began to absorb into its very texture conceptions which are the all and everything of mathematics of today, but which had been quite alien to Greek *mathematics* even at its height, as in the work of Archimedes, say. The leading such conceptions were: *Space, Infinity, Function, Continuity, Real Numbers*. These conceptions do occur, overtly or covertly, in certain areas of Greek thought, as in natural philosophy, cosmology, ontology, logic, and, very importantly, theology. But it is very difficult to locate them in Greek professional mathematics.

It is a fact, however hard to accept, that Greek geometry—Greece’s pride—was a geometry without space. The geometry of Euclid did not have the “Euclidean” spaces E^2 or E^3 , as objects by themselves, independent of or prior to configuration contained in them. There is, however, a separate essay on space (or rather on place, *topos*) in Aristotle’s *Physics*, book 4, chapters 1-5 (compare, for instance, [3]), and a separate one on void, chapters 6-9. There is also, in close proximity, an essay on infinity, book 3, chapters 4-8, and on time, book 4, chapters 10-13. But there is nothing syllogistic about infinity in Greek mathematics, or about continuity, and these have been the intellectual’s hallmarks of Time, from Homer to Newton and beyond (see below, section VII).

Our key conceptions, although distinct from each other, cannot be kept apart in meaningful contexts. Aristotle knew this too. He knew that they

come close together in the conception of motion, and he made great efforts to comprehend their interaction in the second half of the *Physics*, books 5-8. He was especially intrigued by the puzzles of Zeno of Elea (dichotomy, Achilles and the tortoise, the flying arrow, the race-course) and made great efforts to resolve them [25]. But there is absolutely no mention of the puzzles in Greek mathematics, which apparently did not profit at all from philosophical developments towards elucidating them. For instance, there is nothing evidential in the extant corpus of Greek writings to suggest that Archimedes was interested in the puzzles, or even that he knew about them or knew who Zeno of Elea had been. A reader may recoil in disbelief from the supposition that Archimedes could have been totally unaware of such developments in philosophy and of their potential import for mathematics, but the possibility must be faced. The great reluctance to entertain such a "negative" supposition about an Archimedes stems from a presumption that Greek professional mathematics was still close to philosophy, closer than it is today. But this presumption is not justified. It is true that Eudoxus of Cnidus, a great mathematician and member of Plato's Academy, could apparently lecture on Plato's Theory of Forms, and (according to Aristotle, *Nichomachean Ethics*, book X) was even an expert on problems of ethics. But, in Plato's dialogue "Theaetetus," Theodorus of Cyrene—a distinguished mathematician, teacher to Plato and Theaetetus—is reluctant to participate in the discussion of the nature of Knowledge, pleading a lack of competence in the field. Also, it is true that the system of Definitions, Postulates, and Common Notions, on which Euclid's *Elements* is built, was originally debated by philosophers and mathematicians alike (see [42, pp. 114-136]). But there is little evidence for such a continued "cooperation" after Euclid; and, very importantly, Archimedes, for instance, showed almost no regard for principles and niceties of the system, in sharp contrast to Poincaré, Hilbert, L.E.J. Brouwer, or Hermann Weyl of our times (see the thorough-going investigation [38] in its entirety, and especially a critique of Archimedes on p. 57). It is misleading to judge the relation between philosophy and professional mathematics in antiquity by Plato's aphorism that God is a Geometer and his conceit of intervening in matters of professional mathematics (*Republic*, vii).

The key concepts of analysis are in their roots common to mathematics and other knowledge, general or particular, and, to a considerable extent, analysis had to wrench them by a very slow process of adaptation out of their extra-mathematical contexts and fit them to its peculiar molds. It is this genetic universality of the key conceptions of analysis that made modern mathematics so very efficacious in interpreting nature and knowledge, much more so than Greek mathematics ever was or could be. The "tools" of Greek mathematics were by origin narrowly mathematical. Those of modern mathematics, however, are genetically universal; and the more uni-

versal by origin, the more effective they are in mathematics after becoming attuned to its needs and purposes.

II. THE FORCE OF ANALYSIS

It is our thesis that the era of the emergence of analysis is virtually coextensive with the Era of the Scientific Revolution in its full duration; that the emergence of analysis was an all-dominant outcome of the Revolution; and that the one-ness of analysis instills a feature of one-ness into the Scientific Revolution, whatever its diversity.

The Scientific Revolution spans the three centuries 1500-1800 (see [39]), even the four centuries 1400-1800 (compare [24]). There is nothing uncertain about the terminal date 1800; in particular, in our outlook, we would not replace it by an earlier date, although the eighteenth century is a renowned age by itself, the Age of Enlightenment (see [28]). But the beginning date of the Scientific Revolution is flexible. The century 1400-1500 was not as "revolutionary" as the following one, although it brought about two separate achievements both seriously altering our key conception of space. It systematically initiated linear perspective in painting (see [37]), and, in forwards-directed pronouncements of Nicholas of Cusa, it initiated "modern" insights into the spatial fabric of the universe [49 and 17].

It is an essential part of my thesis that the three centuries 1500-1800 in the rise of modern mathematics "genetically" correspond to the three centuries 500-200 B.C. in the rise of Greek mathematics, and the four-century span 1400-1800 similarly corresponds to 600-200 B.C., but that this genetic correspondence breaks down after the terminal dates, totally and irretrievably.

In the century 300-200 B.C., just before the terminal date, Greek mathematics was in its fullest vigor. The century began with Euclid's *Elements*, ended with the *Conics* of Apollonius, and culminated with the works of Archimedes, who was one of the very greatest mathematicians ever. The century could also boast of the great astronomer Aristarchus of Samos, who was even a heliocentrist (according to a statement of Archimedes), and of the polymath Eratosthenes of Cyrene, eminent mathematician (Sieve of Eratosthenes) and even more eminent geographer. But, as I stated elsewhere [12], after 200 B.C., "unexpectedly and inexplicably, as if on a signal," the upward trend of mathematics proper, that is, of mathematics as "research" mathematics, came almost to a halt. The development of mathematics "began to level off, to lose its impetus, and then to falter." An inexorable downward trend set in, which even the imposing figure of a Pappus (third century A.D.) could not stem; and around A.D. 500, that is, around the time of the fall of Rome, came the final extinction of Greek mathematics in its own phase.

To an extent, the decline of Greek mathematics was part of the so-called decline of ancient civilization, many features of which began to manifest themselves in the second century B.C. But (see [12]) the decline of mathematics from the heights of the third century B.C. was too large, too sudden, and too incongruous to be thus fully explained, and an additional cause peculiar to mathematics must have been operative. I maintained (in [12]) that, from first to last, the intellectual base of Greek mathematics was altogether too narrow and, above all, too rigidly fixed in its intellectual narrowness to support an ever heavier mathematical edifice; so that at a certain critical stage the edifice ceased to be firmly grounded. It gradually became unstable, fractured, decadent.

A glance at the fortunes of astronomy will corroborate this outlook. The mathematically controlled astronomy of the Greeks did continue to thrive after 200 B.C. Around 150 B.C. Hipparchus made the breathtaking discovery of the precession of the equinoxes, and three centuries later, around A.D. 150, Claudius Ptolemy composed his *Almagest*, which, by mathematical formalism, held sway over astronomy not only through the length of the medieval Islamic Era, but still in the structural framework of Copernicus's *De Revolutionibus*, however "revolutionary" its astronomical content may have been. This perdurance of the *Almagest* is due not so much to its greatness as to the general stagnation of the Greek mathematics employed. As soon as, after Copernicus, the breath of the new analysis blew into astronomy, it produced the unprecedented Planetary Laws of Kepler, and his new Theory of Vision to boot (see below, section V). Also, not long afterwards, whatever is worth remembering of the work of the Late Hellenistic mathematician Pappus was enhanced by dosages of analysis—directly in the n -line locus problem of Descartes, and indirectly in the Perspective of Kepler and Desargues.

After that, until 1800, analysis was in a state of prolonged adolescent growth, as it were, whence comes an impression of freshness, robustness, and inimitable prowess; and the progress of this growing-up can be gauged by a quick assessment of several works which carry "analysis" in the title.

First, we have already noted Newton's work of 1665-1669 on "analysis by infinite equations." It deals with an array of infinite processes, mainly with infinite series for various definite and indefinite integrals, and successive applications of his method of approximation to solutions of polynomial equations.

Second, in 1748, came Euler's most popular treatise among his many popular ones, under the title "Introduction into analysis of infinitude" (*Introductio in analysin infinitorum*), and it is already a veritable grab-bag of analysis of today. It contains, among other things, an exposition of infinite series, including those for e^x , $\cos x$, $\sin x$, and the relation $e^{ix} = \cos x + i \sin x$; curves and surfaces investigated with the aid of their equations; an al-

gebraic theory of elimination; and, mark! a chapter on the Zeta function and its relation to the prime number theorem, as well as a chapter on *partitio numerorum* (see [71, p. 169]).

Next, four decades after Euler's "Analysis," in 1786, came Lagrange's *Mécanique Analytique* (Analytical Mechanics), which was a new kind of title. Whatever Lagrange himself and his immediate contemporaries may have associated with this "analytique," in retrospect it does *not* connote a contrast to something else, like synthetical, practical, or pragmatic, but simply the presentation of mechanics in the spirit, ambience, and style of Lagrange's "today." It almost means "modern" mechanics, mechanics of "today." And it was a long "today," because Lagrange's treatise was a paradigm for textbooks in colleges and universities until World War I. Also only ten years after the *Mécanique Analytique*, in 1795, Monge began to publish his *Feuilles d'analyse* (that is, "Notes" in analysis), which later became the famed *Application d'analyse à la géométrie*, which promulgated a new kind of geometry, an avant garde geometry based on analysis, not at all replacing the "classical" geometry, but becoming a novel field alongside the latter.

Lagrange's *Mechanics* came precisely a hundred years after Newton's *Principia*, and by contents it is, in a sense, a translation of the *Principia* from the ostensibly Archimedean *mise-en-scène* into an up-to-date setting of analysis. But the *Principia* was its basis, foundation, and fount of inspiration, so that in innermost substance the *Principia* must have anticipated the "analytical" character of the *Mechanics*. Such was indeed the case. We will verify that, from our outlook, the *Principia* is simply soaked in analysis, which is of the same substance as in Lagrange's *Mechanics*, and which had been in the making from the first. Nevertheless, syllogistically, at the time of the *Principia*, analysis was still in a state of juvenile un-readiness, which the Archimedean setting of the *Principia* masks and counteracts in one. I argued this already (in [17]), and the emphasis there was on the fact that Kepler's analysis was in an even earlier state of infancy, but that this state of infancy in no wise impaired its power and efficacy.

Even Lagrange's work on "Analytic Functions" (*Fonctions analytiques*) is syllogistically still very wobbly. In fact, the maturing of analysis (and of modern mathematics as a whole) into a syllogistically acceptable doctrine began only after 1800, and the process extended till 1900 and beyond.

The era of the Scientific Revolution contains diverse and disparate developments, but the extramathematical universality of the key concepts of analysis suggests that there ought to be internal features of unity within the diversity, and there are indeed. For instance, Kepler calls his optical work "astronomiae pars optica," thus insisting on a link with his astronomy. Kepler himself does not suggest what the link is, and Alexandre Koyré in his *Révolution Astronomique* [50], a splendid book about Kepler by-and-large,

does not enter into optics at all. But there is indeed such a link, namely through the key conception of Space, as we will see in section V. I will even (*pace* Descartesians) suggest such a link in Descartes, namely a link between his geometry and his body-and-soul problem. Here again I found nothing about it in Koyré, although he wrote two important books about Descartes [47 and 48], in addition to expounding his work in mechanics. But I did find encouraging corroboration in the basic essay of Ernst Cassirer [28], especially in its last chapter.

In the eighteenth century analysis and mechanics were almost inseparable in their growth, but after 1800 they began to separate, although in a very amicable way; and, what is more remarkable, a concern for a syllogistically rigorous foundation of analysis began to spread, however gradually, into ever wider circles of creative mathematicians. Analogous concerns sprang up in virtually all academic fields. In historical sciences, for instance, a concern for rigorous foundation resulted in the emergence of a so-called "higher criticism" (see [9]). There seems to be no satisfactory explanation for this "universal" development.

Similarly, in the unfolding of the key concepts of analysis since 1800 there has been an overall difference between the nineteenth and twentieth centuries, which again appears to have analogs in most other academic departments. The difference is that gradually a certain "secularization" of the key concepts, followed by a certain "flattening-out," has come about. Space, for instance, in the hands of Riemann became something intricately lofty in two separate grandiose constructions, namely construction of a Riemann surface in complex analysis—or, rather, to Riemann, in algebraic geometry—and of a general Hausdorff manifold with or without a Riemannian metric. These are structures as lofty as cathedrals. But a secularized class of spaces nowadays, indispensable, yet very "flat," and, to me "vulgar," are the various linear vector spaces, which are mushrooming all over mathematics with no end in sight. Also, space in cosmology and relativity is highly "structured," and is becoming ever more so. On the other hand, almost any aggregate of mathematical objects, or alternately any "point-set" can be viewed as a space, even if no kind of structure is postulated to begin with.

Or consider continuity. After Cantor and Dedekind had finally settled the nature of real numbers and of the linear *continuum* in one, the nature of continuous *functions* followed next. They were explained as continuous mappings from one topological space into another, and until around 1935 topology was dominated by the quest to elucidate the implications. It was all very exciting. But the sobering "secularization" set in, when "topological structure" of a space was made something very general, subject to some minimal requirements only. Any point-set whatsoever could be given a topological structure without having one, namely the so-called discrete structure. Finally, infinity, which had been highly theological throughout, and

still was an awe-inspiring conception in the nineteenth century, became quite vulgarized in this sense. For instance, any linearly ordered set whatsoever can be enlarged by a fictitious last element, which is irreverently called infinite, and denoted by $+\infty$, say; or it can be deprived of it, which is equally irreverent.

III. FUNCTIONS

The emphasis on operation with symbols in Viète and Fermat undoubtedly had an impact on the rise of functions and of real numbers, because the simplest kinds of functions are those given by algebraic expressions in terms of symbols, and real numbers are the simplest "constant" coefficients that are elements of an algebraic field or ring. But functions and real numbers also had "analytical" roots of their own, which were not intergrown with symbols, reaching back into the sixteenth century and earlier.

Outwardly, functions unfolded rather slowly. According to Moritz Cantor's *History of Mathematics*, the systematic conception of functions began only near 1700 with Leibniz, who also coined the name "function." But, syllogistically, it was a rather crude "approximation" to our $y = f(x)$ of today; in the eighteenth century there were several attempts to do better, but progress was slow. Even Lagrange's book, *Fonctions analytiques*, which was composed toward the very end of the eighteenth century, is not, or not yet, what an unwary reader today would expect. It does have power series and their usual properties. But to Lagrange an analytic function was one appropriate for *his* analysis, and this was the analysis involved in his Analytical Mechanics; thus he was not "programmed" to distinguish between a function which is analytic in our present-day sense and one that is, say, continuous and piecewise analytic only. As a matter of fact, Lagrange knew that in the mechanics of continuous media (vibrating strings, etc.), a "general" solution can be obtained approximately by replacing continuously distributed masses by finite systems of mass points, and he may have therefore taken it for granted that a (uniform) limit of such piecewise analytic functions (in our sense) is analytic (in his sense) too. Cauchy was probably the first successor to Lagrange who could clearly perceive the distinction. Lagrange and Cauchy were in the same French tradition, and chronologically not too far apart. But Lagrange was active before 1800 and Cauchy after 1800, and the difference between them is a telling manifestation of the change-over around 1800. To Lagrange a mathematical function, however "general" and "abstract," is somehow always an orbit from mechanics; but to Cauchy any path or orbit from mechanics, any mechanics, is a mathematical function, as a matter of course.

Unlike power series expansions, trigonometric expansions are suitable for very "general" functions. This was emphatically proclaimed by Fourier,

who was active after Lagrange and before Cauchy, and who asserted that "any" function admits a trigonometric expansion, even if it is "absolument arbitraire." This provoked Dirichlet into defining, in 1837, a general function $y = f(x)$ essentially as an "arbitrary" association of a suitable value y with any value x ; and the priority of Dirichlet in this matter has been "institutionalized" by A. Pringsheim [64] in the German Encyclopedia of Mathematics. But in another volume of the Encyclopedia, H. Burkhardt [23], author of an article which is a fundamental work in the "early" history of Fourier Analysis, states that the twentieth-century conception of a general function originated in the remarkable book (1838) of Antoine Augustin Cournot, *Recherches sur les principes mathématiques des richesses* (Investigation of the mathematical principles of the theory of wealths). The "Calculus" work [62] by A. Ostrowski, which has competent historical notes, agrees with Burkhardt. It should be stated though, that, in a serious vein, a "function" cannot be really defined at all, because any description of it as a "correspondence" or "association," or even "ordered binary relation," is logically a vicious circle.

After this very brief survey of the *overt* career of functions since their formal introduction by Leibniz in 1700, we shall turn to their *covert* presence before 1700.

I find that, covertly, functions are in the very center of Newton's *Principia*, which appeared in 1686 but had been long in the making. Newton's primary *concept* is "quantity of motion." If he had had formulas, then for the motion of a particle on the line, $x = \varphi(t)$, the quantity of motion would be

$$(1) \quad p = mv \quad , \quad v = \frac{dx}{dt} = \frac{d\varphi}{dt} \quad .$$

And Newton's primary *fact* is that the rate of change of the quantity of motion is (equal to, or is determined by, or, perhaps even determines) the total force acting on the mass point, that is, in terms of (1),

$$(2) \quad \frac{dp}{dt} \equiv \frac{d(mv)}{dt} = F \quad .$$

In the case of a brusquely acting force, one might also have

$$\Delta p \equiv p(t + 0) - p(t - 0) = F(t) \quad .$$

There is nothing like all this in Greek mathematics, in any kind of verbal circumlocution of these formulas. We might concede that a point function $\varphi(t)$ is reminiscent of a Greek curve, and that a derivative $\frac{d\varphi}{dt}$ at a point is reminiscent of the tangent to a curve at a point. But gathering up the derivatives at various points into a new function $v(t)$ is not reminiscent of anything occurring in Greek mathematics anywhere, and the formation of the second

derivate of $\varphi(t)$, which occurs in (2) when writing it in Euler's form

$$(3) \quad m \frac{d^2x}{dt^2} = F,$$

is far outside anything Greek thought has ever conceived. (We note that Euler's form (3) was very important in the creation of the theory of elliptic operators. But Newton's form (2) occurs in most theories of physics after Euler, in the Hamilton-Jacobi theory of Mechanical Systems with Restraints; in the theories of Gas Dynamics of Maxwell and Boltzman; in the theory of Relativity; and in Quantum Mechanics.)

But even the concession that the derivative $\frac{d\varphi}{dt}$ is reminiscent of a Greek tangent to a curve goes too far. From what is known, the Greeks envisaged tangents to curves that are convex, at least locally, and only to these, so that their tangents were supporting lines, at least locally (see [12, p. 184]). They apparently never introduced a tangent which crosses the curve at the point of tangency, although some of the curves known to them had such tangents. In fact it was one of Fermat's major achievements to have become aware of such a possibility. In 1640 he envisaged, as a part of his pioneering work on maxima and minima of functions, the problem "of investigating by the (analytic) art the points of inflection at which the curvature changes from convex to concave, and conversely" (see [56, p. 201]). Fermat thus created the concepts of a "point of inflection" of a curve, and of the "(point-by-point) curvature" of the curve, which Frenet and others took up systematically only in the nineteenth century.

This achievement of Fermat belongs to the "theory of functions" irrespective of the geometric setting, and earlier achievements affecting functions also appeared in settings that were not overtly "analytic." Functions in our sense are discernible already in the fourteenth century—as a species of graph and other—in the work of Nicole Oresme (1325-1382); and the renowned mathematician-turned-historian Oswald Spengler was so impressed with them that he made them into a litmus-like indicator by which to distinguish present-day civilization from ancient ones.

In the fifteenth century Regiomontanus created a field in function theory by freeing trigonometry from its subordination to geometry and astronomy and by beginning (but only beginning) to view $\sin x$ and $\cos x$ as functions on the x -axis subject to the familiar addition theorems as functional equations. Later on the logarithmic and exponential functions were also drawn into this context.

Simon Stevin shortly before 1600, and Luca Valerio in 1604, began to consider rather *general* functions when standardizing and streamlining the formation of "Riemann-Darboux Sums" for the computation of volumes, which for *special* functions already occur in the masterful work of Archime-

des on Conoids and Spheroids. Thus Carl Boyer comments on Luca Vale-rio: "this geometrical reasoning is strikingly similar to that presented in many present-day elementary textbooks on the calculus" [21, p. 105]. I fully concur, except that I would have said "analytical" rather than "geometrical" reasoning. And all this was done without Viète symbolism, which, in similar contexts, does not occur before the work of Fermat.

In denying the creation of functions to the Greeks, I do not mean to say that they were not familiar with categories of cognition such as "correspondence," "dependence," "mapping," even "binary relation," without which "functions" cannot be formally conceived, but only that they did not perform mathematically controlled operations with them, in symbols or in words. Loosely conceived functions become mathematical objects in earnest when they are subjected to manipulations which are recognizably mathematical, in symbols or verbal circumlocutions. It is this that the Greeks did not achieve.

IV. REAL NUMBERS

The Greeks could not or would not form the product of two general real numbers. Archimedes did not form it. The Renaissance quickly did it, and the consequences are all around us.

As is done even today, the Greeks represented real numbers by the (lengths) of rectilinear segments in the plane, and when Greek mathematics was at its height it had a (vague) notion that there are as many real numbers as there are (incongruent) segments. The addition of real numbers was obvious: segment was followed by segment. But for a product of two segments, the factor segments invoked the image of a rectangle whose sides they were, and the Greeks could not or would not "convert" the rectangle quantitatively into a new segment. This blockage created a stand-off for two thousand years.

On an early page of his *Geometry* (1637), Descartes performed the conversion as follows: by similar triangles, the equation

$$(4) \quad a : b = c : d$$

determines any of the four magnitudes in terms of the other three. But now *standardize* one of them as a unit, say $d = 1$. Then

$$a : b = c : 1$$

performs the multiplication of b, c , thus $a = bc$. Zeuthen, in his important work [75], celebrates this calculation as a crucial achievement, and Mahoney [56, p. 44], sees in this "bold" move a triumph of the analytic art.

The fact of the matter is that all this had been achieved—standardized unit and all—long before Viète, let alone Fermat and Descartes, in the

famed *Algebra* of Raffael Bombelli (1530- after 1572), in a part of it that was published in 1929 by Ettore Bortolotti [19], but had been unedited till then. In one of the theorems, by putting $b = 1$ in (4) Bombelli performs the division $a = c/d$, and in another theorem he expressly forms for any real number a , its successive powers a^2, a^3, a^4, \dots , for any exponent wanted.

Any history of mathematics quotes Bombelli, but only Bourbaki [20] reports this capital achievement of his. All the others praise him for his anticipation of the fact that for any real numbers a, b the expression $(a + b\sqrt{-1})^{1/3} + (a - b\sqrt{-1})^{1/3}$ is real-valued. This is a striking enough achievement, but in doing this Bombelli was working in the area of many other contemporary mathematicians. In shaping real numbers into a semi-ring he was ahead of them.

But why was this so important? And what were the consequences? The importance and consequences were that with multiplication of real numbers at his command Isaac Newton could resume where Archimedes had left off nineteen centuries before.

Of course in everyday life the Greeks could and did multiply numbers; carpenters, masons, builders, and engineers could not have done without it. What they could not or would not do was to multiply the numerical values of two physical quantities of heterogeneous physical provenance and thereby obtain the numerical value of the quantity of a third physical species. As I have maintained from the first [7, p. 182], the crucial evidence for this is Archimedes' own formulation of his law of the lever in equilibrium. He expressed it by the proportion $L_1 : L_2 = P_2 : P_1$ where L_1, L_2 are the lengths of the two arms of the lever, and P_1 and P_2 are the magnitudes of the weights suspended from them. But he was apparently unable to express it, by introducing the product $L \cdot P$, in the form

$$(5) \quad L_1 \cdot P_1 = L_2 \cdot P_2,$$

let alone to create the conception of the "rotational momentum" $L \cdot P$ in the process, and to interpret (5) as the sameness of the momentum for the two sides of the lever.

But this is precisely the manner in which Isaac Newton did proceed. He did form a product of heterogeneous factors, namely his quantity of motion

$$(6) \quad p = mv$$

and he did state that in the case of equilibrium it is constant in time. He fully knew that he broke new ground, and he described the product (6), entirely without symbols:

The quantity of motion is the measure of the same, arising from the velocity and quantity of matter conjointly.

The motion of the whole is the sum of the motions of all the parts; and therefore in a

body double in quantity, with equal velocity, the motion is double; with twice the velocity, it is quadruple. [60, p. 1]

Note that Newton is on the verge of describing the product (6) as a bilinear functional in (m, v) , which is further evidence for my contention that the notion of function was fermenting inside the *Principia*.

Many developments initiated by Newton were quite slow in unfolding (see [7, pp. 346-348]). Thus, only the nineteenth century extended the process of multiplication involved in (6) to physical magnitudes in general, creating such objects as

$$[\text{energy}] = [M \cdot L^2 \cdot T^{-2}]$$

and, say, $[\text{"action"}] = [M \cdot L^2 \cdot T^{-1}]$,

which is the dimension of Planck's universal constant h . A first essay on this subject, short but systematic, was included in the *Théorie de la chaleur* of J. Fourier, and a much more systematic account was given by Clerk Maxwell and Fleeming Jenkins in 1863 (see [7, pp. 211-212]).

In the case of energy, Leibniz introduced a *vis viva* by mv^2 , which is twice our kinetic energy, and historians of science are unappeasably puzzled by the fact that Newton himself did not introduce any kind of energy at all. Even one grotesque solution of the would-be puzzle has its audience (see [7, pp. 79-80]). The explanation of the "puzzle" is simply that Newton did not need the concept of energy operationally, and not needing it, he did not form it. In traditional physics the concept of energy arises in two ways: It arises in mechanics when variational principles are invoked, but Newton did not resort to such principles. And it is indispensable in physics proper, that is in theories of heat and electrodynamics, but Newton never dealt systematically with physics proper. Besides, quantity of motion, that is momentum, which was Newton's central concept of mechanics, is mathematically more subtle than energy, because it is a vector whereas energy is a scalar. As I have stated previously—and found corroborated in M.J. Crowe [33, pp. 127-128]—Newton was the actual creator of the conception of vector for exact science. "Several significant physical entities of the *Principia*, namely velocities, momenta, and forces are, by mathematical structure, vectors, that is elements of vector fields, and vectorial composition and decomposition of these entities constitute our universal scheme of the entire theory" [7, p. 192].

But Newton did not introduce tensors, and so, in keeping with the general remark made above, only the nineteenth century was ready to create them.

We have to return to the Greeks for two observations regarding multiplication: First, the Greeks did not arrive at the theorem—and they would not have been able to formulate it—that any natural number is a (unique) product of primes, although they had all the "lemmas" at hand for doing so.

Even the Scientific Revolution itself was not yet ready for it, and the theorem surfaced only in 1801 in the "*Disquisitiones Arithmeticae*" of C.F. Gauss (see [7, p. 216] and [15, p. 828]). Second, there is nowhere in Greek mathematics an anticipation of the group operation $a \cdot b = c$; and, for my part, I would say that groups appear only in the work of Euler as groups of motion in 3-space, in his mechanics of rigid bodies. (But see also the book of Wussing [74].)

Finally, the difficulty of multiplication was not the only obstacle that kept the Greeks from arriving at real numbers. There was also the fact that an escalation of abstractions would have been involved. Starting from natural numbers, one must introduce the "cone" of positive fractions, extend it to the field of rational numbers by introducing zero and negative numbers, and last extend this to the field of real numbers by completion in the order topology. Now, as I have frequently stated before (see for instance [7, pp. 51 ff]) the Greeks hardly ever went beyond mere ideations—that is, one-step abstractions, in and out of mathematics, and a veritable scale of abstractions, as involved in the formation of real numbers, was beyond their reach. And may I add that when in modern mathematics Cardano and Bombelli in the sixteenth century ventured the first sallies into the realm of complex numbers, they did so long before Viète symbolism became operative. Descartes, however, in the seventeenth century, who mastered the symbolism as expertly as any of his contemporaries, was one of those who still rejected complex numbers, coining the pejorative appellation "imaginary" for them.

V. SPACE

In our outlook, nothing separates ancient and modern mathematics more decisively and emphatically than the concept of space. Greek mathematics was a mathematics without space, all of it, but post-medieval mathematics was a mathematics with and by space from the very first. Mathematical space is a hallmark of analysis, and analysis was launched by it. Space was not an appurtenance of Greek mathematics, geometry or other, but all geometry since 1600 has been molded in space and thus imbued with the spirit of analysis, the geometry of Desargues (1593-1662) just as much as the geometry of Descartes (1596-1650), the so-called "synthetic" geometry of the nineteenth century just as much as the avowedly "analytical" one.

The advent of mathematical space was heralded in the fifteenth century by the emergence of linear perspective in painting in the work of Alberti (1404-1472) (which was followed by beginnings of projective and descriptive geometry in the sixteenth century), and by the emergence of a mathematically conceived spatial structure of the universe in the theology of Nicholas of Cusa (1401-1464).

In the middle of the sixteenth century there was a sudden change of course in Italian Renaissance Philosophy. It is hard to account for (see Kristeller [51]). The best known representative of the "New Directions" was Giordano Bruno (1540-1600), whose spectacular cosmological speculations became widely known. For our purposes it is more significant that in the work of Patritius (1529-1579) there appears the expression "mathematical space" in a chapter heading *De spatio mathematico*, and also the parallel expression "physical space." I cannot imagine Archimedes announcing a lecture or writing an essay "On mathematical space" or "On space in mathematics."

For us, the paramount achievement involving the rise of space in Renaissance thought is the scientific effort of Johannes Kepler (1571-1630): his momentous Planetary Laws, his New Theory of Vision, and also his purely mathematical analysis of volumes (compare [13, 17, and 16]).

And now for some elaborations of the above.

The Greeks did have space in physics, cosmology, and perhaps also theology, but not in logical, ontological, or psychological perception, and above all not in mathematics, where a modern reader would expect it first of all. There is no *pre-existent* mathematical background space for the configurations and constructs in the mathematical works of Euclid, Archimedes, or Apollonius of Perga, nor in the astronomical work of Ptolemy. In these works, as soon as a mathematical configuration or construction is envisaged, an ambient space is evoked, but there is no Euclidean space E^2 or E^3 as a mathematical object in its own right, independent of configurations in it. When Ptolemy's *Almagest* designs a path of a celestial body, then the path lies in the astronomical universe of Ptolemy, but as a geometrical object of mathematical design and purpose it does not lie anywhere. In his work on the lever and on hydrostatic equilibrium, Archimedes does not have anything remotely resembling Newton's Space in the *Principia*, "absolute" or other.

We will offer an explanation why there is no theorizing on perspective in the arts in Greek works, extant or known by title. Aristotle, for instance, who wrote something about almost everything, apparently never had a treatise on this subject matter. The explanation is simple. Perspective, whatever it be experientially and esthetically, is mathematically a mapping of E^3 into E^2 , and the Greeks, not having created these spaces, could not create such a mapping either. But the fifteenth century obviously did already have such spaces, and linear perspective could and did get under way. I think that my opinion is in accord with, or at least not at variance with, insights of the art historian Erwin Panofsky, who made searching studies about the rise of perspective in the West (see [37, chapter XI] and [63]). But most art histor-

ians (and other scholars) cannot believe and will not concede that the Greeks had not had linear perspective at all. For instance, S.Y. Edgerton's very knowledgeable book [37], from which I have quoted, is entitled "The Renaissance Rediscovery of Linear Perspective," as if it were reasonable to assume that a linear perspective had been known before, that is, in antiquity. But there is no evidence to support such an assumption.

In the early part of the fifteenth century, Cusanus, or Nicholas of Cusa, the theologian-philosopher and gifted mathematician mentioned above, has many intriguing philosophemes on Space and Universe in his famous work "On Learned Ignorance" (*De docta ignorantia*). In his cosmological meditations, Cusanus has something that Ptolemy definitely did not have, namely the first outlines of an abstract mathematical background space in addition to and interpenetrating the concrete physical and cosmological space (see [17], and [47, pp. 5-27]). In Cusanus, and also in others, the universe itself is sometimes called *machina mundi* (the world machine), an expression which occurs already in the poem "De rerum natura" of the great Roman poet Lucretius (first century B.C.). Cusanus makes the following two statements:

(A) The world is neither finite nor infinite, or, what is virtually the same, it is both finite and infinite.

(B) The *machina mundi* has its center everywhere, and thus nowhere, and its circumference is nowhere.

Statement (A) will be discussed in the next section; statement (B) suggests to me a homogeneous manifold whose center, due to the homogeneity, is everywhere and thus, in a sense, nowhere. And statements (A) and (B) taken together suggest the Bolyai-Lobachevsky geometry in the unit ball $y_1^2 + y_2^2 + y_3^2 < 1$ of the E^3 . In the non-Euclidean metric, this unit ball is homogeneous relative to a transitive group of motions, so that there can be no "absolute" center, and it has no circumference because it is open. It is infinite in its own metric, but finite in the metric of the ambient E^3 .

These forward directed insights of Cusanus were adumbrations only, but unique ones; and until Kepler almost nothing like them occurred for over a century. It is true that before Kepler there was Giordano Bruno. But Bruno was unmathematical and his speculations have no nuance of mathematical persuasion, whereas Cusanus and Kepler were real mathematicians, while Copernicus was primarily an astronomer.

Copernicus decomposed the motion of the earth into a yearly translation around the sun and a daily spinning around the axis; and in Kepler's subsequent outlook the two motions are in effect represented by two vectorially independent paths in an underlying E^3 , so that, in suitable coordinates $\{x, y, z\}$ the location of a point of the body of the Earth at time t is representable by the formulas

$$\begin{aligned}
 (7) \quad x(t) &= r \sin \theta \cos (\varphi + nt) + \xi(t) , \\
 y(t) &= r \sin \theta \sin (\varphi + nt) + \eta(t) , \\
 z(t) &= r \cos \theta + \zeta(t) ,
 \end{aligned}$$

where r is the distance of the point of the body of the earth from the center of the earth, (θ, φ) are its latitude and longitude, n is a constant depending on the normalization of time, and $\{\xi(t), \eta(t), \zeta(t)\}$ are the coordinates of the center point of the earth at time t .

In Copernicus there is no such underlying mathematical paradigm for the compound motion of the earth. As I have described it in [16 and 17], the Copernican *World Machine* was, vaguely, an arrangement of cycles or epicycles in the shape of wheels, rods, and gears, in which the movements of the parts of the machine had to be centrally controlled and hierarchically ordered. Nothing could be "loose" and "free-floating," and no two sub-movements could be "independent" and "commutative." Also, the celestial body itself was firmly affixed to a spot on the hierarchically last *orbis*, and it is this *orbis* which moved (as did other wheels), not the body itself. Even Copernicus never decided whether an *orbis* was a circular hoop or a spherical shell. Kepler very knowingly swept away the hoops and shells, and let the planet traverse a path in E^3 , as envisioned today.

The outlook of Copernicus led him to a peculiar construction. Since, in his machine, the daily and yearly motions of the earth were hierarchically ordered, therefore, *prima facie*, under whichever ordering, the axis of daily rotation "ought" to be firmly affixed to the plane of the ecliptic in which the yearly motion takes place. But it is not so affixed, since in E^3 the axis of the earth remains parallel to itself. Copernicus accounted for this by introducing a third motion, a Motion of Declination, in which "the axis of the Earth describes the surface of a cone in a year, moving in an opposite direction to that of the Earth's center, that is from east to west" [35, pp. 140-141]. This Third Motion had no appeal. Even before Kepler it was rejected by Christoph Rothmann and Giordano Bruno [58, p. 199], although Copernicus was able to make magnificent use of it. He made the period of the Third Motion slightly less than a year, and explained the precession of the equinoxes thereby.

Furthermore, unlike Kepler after him, Copernicus does not, because he cannot, show an awareness of the fact that, due to his heliocentric hypothesis, the orbit of each planet is in a (mathematical) plane of its own, and that these planes are various planes in a common three-dimensional (mathematical) space. It seems that Copernicus expressly introduces only the plane of the ecliptic, and if he also speaks of the *line* of intersection of the planetary orbit with the plane of the ecliptic then he apparently refers only to the line joining the two nodes of the planetary orbit without envisaging a plane of the planetary orbit too.

Kepler's "post-Copernican" work began in earnest with his *New Astronomy*. In it he immediately takes it for granted and expressly posits that any planetary orbit—which he frequently calls an "excenter"—lies in a (mathematical) plane. If he had not taken the plane for granted, how could he have debated for so long what kind of "oval" the orbit of Mars actually is? Or, even more pertinently, how could Kepler have arrived at his tremendous *Law* that the radius vector from the focal center of the orbit to the planet sweeps out equal areas in equal times, unless he took it for granted that the various areas are "flat" areas in one and the same plane. Copernicus did not even begin to conceive such a law because he was still far removed from the thought patterns of space and analysis that the inception of such a law presupposes. The great originality of the Law of Areas is best attested to by the fact that Isaac Newton made his version of it the very first theorem of the *Principia*, although for reasons best known to himself Newton did not choose to quote Kepler in the context.

Kepler's planetary laws are not the only evidence for his awareness of the importance of space in science. Other evidence is a new spatial setting, a revolutionary one, in his optical Theory of Vision, which came into being virtually simultaneously with his *New Astronomy* and which he even called "the optical part of astronomy (*astronomiae pars optica*).” The change of spatial setting was as follows. Before Kepler—with one exception to which we will soon turn—the light rays between the eye and the object seen by the eye were thought to move in a cone which had its vertex in the pupil and the base on the object. Kepler, however, changed and reversed the setting. In the process of seeing there are many cones, narrow ones. Each point of the object seen is the vertex of a cone, and all cones thus arising have their bases on the eyeball. Thus Kepler views the object as an aggregate of points, à la Cantor, and it is obviously a pointset in a pre-existent mathematical space. Before Kepler, the cone had the same "space-less" nature and setting as in Apollonius. But Kepler's cones are "modern" cones, in a "modern" pre-existent space. The great originality of Kepler's innovation is emphasized in Vasco Ronchi [67 and 68], and a valuable account is also in Alistair Crombie [32].

The one exception to the pre-Kepler conception was the mathematician Francesco Maurolico (1494-1575) whose work showed that he already recognized the reversal of cones [57]. But Ronchi feels certain that Maurolico's work was unknown to Kepler when he worked out his optics. As stated at the beginning of this section, in the second half of the sixteenth century there was among philosophers a sudden awakening of interest in the role of mathematics in science whose suddenness is hard to explain. Thus the present-day historian P.O. Kristeller says:

The thinkers whom we shall discuss in the three remaining chapters were all active during the second half of the sixteenth century, and what separates them from those we

have considered so far is not merely the passing of a few decades, but the emergence of a completely different intellectual atmosphere. [51, p. 91]

Apart from observing that the first half of the sixteenth century was the era of the great religious revolution that goes under the name of Reformation, Kristeller makes no real attempt to trace back this intellectual change-over to tangible roots.

Kepler also demonstrated penetrating insights into the "analytical" role of space in purely mathematical contexts; I will adduce two instances. First, with skill and imagination, he computed the volumes of a large number of solid bodies, many of which were of his own devising, by viewing them as infinite series of infinitely thin laminas, or of infinitesimal wedge-shaped segments radiating from the axis, or of other types of vertical or oblique sections (see Boyer [21, p. 108, and also pp. 110-118] for similar exploits).

Second, in the context of his optical work, Kepler introduced into the theory of conics a certain "space-related" way of seeing things which may appear commonplace today but was unprecedented in its time, just as many a present cliché was unexpectedly novel when Shakespeare uttered it first; and Shakespeare and Kepler were contemporaries. Kepler introduced foci of conics (which Apollonius did not have) and infinitely distant points in space, and he interconnected various conics in the following way: A circle, in which the foci coincide, can be stretched into an ellipse, and then into a parabola, in which one focus, the "blind" one, is at infinity; and this process can be reversed. Similarly, from a pair of intersecting lines, which is a "degenerate" hyperbola in which the foci and vertices all coincide, one passes through an infinity of hyperbolas to the parabola.

Kepler summarized these findings by saying that the conics are related by "analogy." Seemingly independently, over two centuries later Jean-Victor Poncelet stated a "Law of Continuity" in projective geometry which subsumes Kepler's finding, and Charles Taylor [72], a strong advocate of Kepler's originality in the field, demands that the Law of Continuity be credited to him by priority. He maintains even more emphatically that Newton's "essay" on conics, which is incorporated in the *Principia* (see [60, pp. 76-108]), and in which Kepler is not mentioned, is a legacy of Kepler's. In this assertion Taylor may be right because the style and diction of the "essay," especially the manner in which it speaks of points at infinity, are all entirely Keplerian.

Taylor [72] also assigns to Kepler statements in Desargues that are frequently assumed to be original with Desargues. This, I think, has to be qualified. Desargues's theorem on triangles in perspective (published in Abraham Bosse) has a "message" about space in geometry for which I find no precedent in Kepler. The theorem states that if two triangles in E^3 are in perspective—that is, if the three lines joining corresponding vertices meet at a point, finite or infinite—then the three finite or infinite points at which cor-

responding sides of the triangle intersect lie in a straight line, finite or infinite. The theorem also holds for two triangles in a plane, but as Hilbert has shown [44], the plane must be imbeddable in a three-dimensional space in the following sense: There is a system of Euclid-like axioms for a 3-space for which the theorem holds throughout, but which is such that if one omits those axioms which refer to three-dimensionality then the theorem may fail (Non-Desarguean Geometry). I cannot imagine the Greeks or the medieval schoolmen conceiving a theorem in two dimensions whose proof requires that the 2-space be imbeddable in a 3-space in which a three-dimensional version of the theorem is demonstrable; and I do not think that Kepler had such anticipations either.

The same book of Hilbert has another feature that quite directly highlights the supremacy of analysis over geometry in mathematics today. Hilbert introduces a system of axioms that *implicitly* defines objects like points, lines, and planes, and in order to verify the consistency of the axioms he *explicitly* defines the objects using various kinds of numbers whose existence and consistency is taken as known. But these numbers are in the last instance grounded in analysis, whose consistency is thus taken for granted, so that altogether geometry is built on analysis. Archimedes would have been perplexed. By his entire mathematical upbringing, it is geometry that would have a claim for being assumed consistent, with analysis, whatever that be, to be founded on it.

We now turn to Descartes. His Geometry (*La Géométrie*, 1637) is etched on the E^3 of today and is thus entirely "modern," its only flaws resulting from Descartes's uncertainty about negative numbers. But in his physical science Descartes has a species of space, called *extensio* or *étendue*, in which the structure of E^3 manifests itself so little that commentators are provoked into remarking that it is "still medieval." And yet the sharp separation of the two spaces in Descartes was in keeping with an irreversible development then in progress. Beginning with Kepler, with anticipations in Cusanus and Copernicus, every physical (or cosmological) system had two spaces in its fabric, whether the author of the system realized it or not: an "objective" space of the *Physis* of the system, and a "subjective" mathematical background space; the latter space being "subjective" because all objects of mathematics—however much suggested by the external world—are nevertheless *internally* conceived, *internally* created, and *inwardly* structured (see [7, p. 47]). Usually these two spaces are interpenetrating and intergrown to some extent. But Descartes, not being a "mathematical" physicist at all, and leaning towards neat, crisp separations, kept the (would-be) mathematical (background) space virtually out of direct physical settings, and displayed it mainly as the E^3 of his *Geometry*.

We note in passing that the emergence of a "subjective" mathematical background space in contrast to the "objective" space of physical manifes-

tations is only a special aspect of the emergence of an attitude of "subjectivity" in contrast to one of "objectivity" in a vast realm of intellectuality during the Renaissance which has become a leading permanent distinction between "ancient" and "modern." Descartes's revolution in philosophy was a signal event in this context; and, to my way of thinking, even the body-soul dualism in Descartes is an instance of the permanent opposition between objective and subjective, which has thus come into being. A key reference to this entire subject-matter is *The Individual and the Cosmos in Renaissance Philosophy*, by Ernst Cassirer [29], especially chapter IV, "The subject-object problem in the philosophy of the Renaissance." I found further enlightenment in the even more difficult book of Georg Lukács, "The Theory of the Novel," but it would exceed the plan of the present article to unravel its pertinence to our context.

Isaac Newton undoubtedly had our E^3 in his thinking when he started the very first theorem of the *Principia* with his version of the Law of Areas (wording it thus: "the areas which revolving bodies describe by radii drawn to an immovable center of force do lie in the same immovable planes, and are proportional to the time in which they are described"), and when in the converse to the Law, which is the next theorem, the center is even "moving forwards with an uniform rectilinear motion" [60, pp. 40-43]. On the other hand, in book III, p. 419, Newton states the hypothesis "that the center of the system of the world is immovable," which suggests a gravitational space of the kind that eventually the Theory of General Relativity would explicate. Yet, as if fusing the two spaces into one, Newton solemnly introduces a single space, which he terms *absolute*, obviously endowing it even with physico-metaphysical features beyond the call of operational necessity. This space failed, although many physicists tried to make sense of it. But the failure did not affect the course of physics. As Jammer put it:

It is interesting to note how little the actual progress of the science of mechanics was affected by general considerations concerning the nature of absolute space. Among the great French writers on mechanics, Lagrange, Laplace and Poisson, none of them was much interested in the problem of absolute space. They all accepted the idea as a working hypothesis without worrying about its theoretical justification. In reading the introductions to their works, one discovers that they felt that science could very well dispense with general considerations about absolute space. [45, pp. 137-138]

Also, in the eighteenth century, Diderot and D'Alembert, in the article "Espace" of their *Encyclopédie*, say with regard to the existence of absolute space: "cette question obscure est inutile à la Géométrie et à la Physique."

But all such negative statements about Newton's absolute space must be tempered by the observation that Newton refers to this space only in the so-called "Scholia" of the *Principia*, and not in the operational body of the

Principia itself, and that the total omission of the Scholia from the work would in no wise affect its operational integrity or its scientific standing.

After Newton, in the eighteenth century there was no major overt development involving space as a key concept of analysis. In the nineteenth century, however, space made world-wide "headlines" through non-Euclidean and Riemannian geometry, and in the twentieth century through the General Theory of Relativity. None of the other key concepts received this kind of publicity even in scientific circles, except that Georg Cantor's Theory of Aggregates affected each of them to an extent.

The *Encyclopedic Almanac 1970* of *The New York Times* (on p. 458, under *Landmarks of Science*, which are listed chronologically) assigns to the years 1825-1826 the "foundation of basic concepts of non-Euclidean geometry" by Lobachevsky and Bolyai, and (on p. 471, under *Men of Science and Mathematics*) it states that Bernhard Riemann "developed non-Euclidean system of geometry representing elliptic space," obviously referring to his renowned paper [65], which was composed and orally presented in 1856 and published posthumously in 1867. A comparison of these two listings cannot but suggest to the reader, general or mathematical—or rather confirm him in a belief already vaguely held—that the achievement of Bolyai-Lobachevsky was *the* landmark event—undoubtedly instantly hailed as such—and that the achievement of Riemann, however important, was only a kind of follow-up to it.

But this is not how developments unfolded, in the realm of mathematics at any rate. The work of Bolyai and Lobachevsky lay half-dormant for many years. Mathematicians like Jacobi, Hamilton, Poncelet, Möbius, Plücker, and others in the first half of the nineteenth century showed almost no awareness of it [18]. There is absolutely no allusion to it in [65], or anywhere else in Riemann. It was first incorporated into a textbook by R. Baltzer in 1867, the same year in which Riemann's memoir was published, and only then did things begin to happen very briskly. In 1868, E. Beltrami linked up the hyperbolic geometry of Bolyai-Lobachevsky with the elliptic geometry of Riemann, by assimilating the style of the hyperbolic geometry to Riemann's. In 1871, Felix Klein made the distinction, which was subtle for that period, between Riemann's elliptic line-element on the sphere, as Riemann himself had introduced it, and the same line element on projective space, which arises from the sphere by identifying pairs of antipodal points. And in 1877 the American astronomer Simon Newcomb, referring back to Riemann, in an article (written in English) in the then very prestigious *Crelle Journal* [59], was obviously groping for this projective space in three dimensions as a possible model for our universe, thus anticipating by forty years Albert Einstein's proposal that the universe might be a relativistic compact space of the type of the sphere. It is worth quoting the last paragraph of Newcomb's article:

It may be also remarked that there is nothing within our experience which will justify a denial of the possibility that the space in which we find ourselves may be curved in the manner here supposed. It might be claimed that the distance of the farthest visible star is but a small fraction of the greatest distance D , but nothing more. The subjective impossibility of conceiving of the relation of the most distant points in such a space does not render its existence incredible. In fact our difficulty is not unlike that which must have been felt by the first man to whom the idea of the sphericity of the earth was suggested in conceiving how, by travelling in a constant direction, he could return to the point from which he started without, during his journey, finding any sensible change in the direction of gravity.

But why did Riemann gain the attention of other mathematicians so much more readily than Bolyai and Lobachevsky? Because the latter, although playing a new game in geometry, were nevertheless playing it on the traditional turf of Euclid and Archimedes. They may have revolted against Euclid, but they were fighting the revolt with the traditional Greek weaponry. And in a certain sense they were not even the first to play on Euclid's own field a game different from his. The geometry of perspectivity, of Kepler and Desargues, of Monge, Lazar Carnot, and Poncelet had already been "new" in this sense. Furthermore, a kind of non-Euclidean geometry was involved in Lagrange's mechanics of finite systems of mass points with constraints. As I have put it: The emergence of general coordinates and free parameters in the eighteenth century was intellectually a prelude to the rise of multi-dimensional geometries, Euclidean and non-Euclidean, in the nineteenth century [7, p. 201].

But Riemann's turf was a radically new one. It was the most general Hausdorff space as a topological substratum, the like of which Bolyai and Lobachevsky did not have in their vision at all. And on this substratum Riemann built a new kind of geometric structure. He took the theorem of Pythagoras, which is the pinnacle of attainment of book I of Euclid's *Elements*, and turned it into a presumptive (infinitesimal) building stone of the structure. This turning of a conclusion into an assumption set the pattern for many similar "inversions" in the twentieth century, but none was as profoundly original as Riemann's own.

The aim and achievement of Bolyai and Lobachevsky, who were interested in Euclid's axiom on parallels and hardly anything else, was the erection of axiomatics, and methodologically their work culminated in Hilbert's book [44], which, for the working mathematician, is a textbook in applied axiomatics, as it were. But Riemann, during his relatively short span of mathematical activity (he did not start publishing very early and died before reaching forty), showed no interest in axiomatics, mathematical logic, or anything else "foundational," nor in any kind of "abstract" algebra, but only in the promotion of analysis, in the very broad sense of the term in which it is used in the present paper. In this aim he succeeded as had nobody since Pythagoras, even Euler not excepted.

Half a century after its publication, Riemann's memoir led, via C.G. Ricci, to Einstein's General Theory of Relativity, as it came about (with Einstein aficionados maintaining that he would have found the theory anyway, Ricci or no Ricci); and we note that the theory is providing a vindication of the thesis that there is no physical "foreground" space without a mathematical "background" space. Thus the treatise of Hawking and Ellis [40] firmly posits a four-dimensional Hausdorff manifold globally, with physical data imposed on it. And not only a final dissolution of the physical universe but also the initial creation of it are nothing but "singularities" in the physical data, with the mathematical substratum itself being "intact."

Albert Einstein himself would probably have hesitated to agree to a "dualistic" distinction between physical and mathematical space in a universe as he envisaged it. He probably would have tried to argue that in any creation of the universe—be it big-bang or peaceful—the instantly evolving physical space ought to co-evolve a mathematical background space too, by a kind of "analytic continuation," as it were. But it would be very difficult to find a specific "model" for the would-be universe in which to rationalize such an "evolution." Even "mythological" cosmogonies knew this. In Plato's dialogue *Timaeus*, Space (*chora*) is a pre-existent datum, a feature of Necessity (*Ananke*), whereas Time (*chronos*), a "moving likeness of Eternity," came into existence along with the Heaven (*Ouranos*) (see [31, p. 102]).

H.P. Robertson (1903-1961), a talented cosmologist, tried to popularize a slogan of his, that "Geometry is a part of Physics." The slogan is all right, as long as one realizes that the physics to which geometry is to be subordinated presupposes a mathematical background space on which to be erected.

A clear-cut, though not intended corroboration of our thesis can be found in Alfvén [2, p. 68], and I have already adduced it in [17, p. 137]. In *Worlds – Antiworlds*, Alfvén is concerned with the creation of a megagalaxy out of previously given ambiplasma, and, guided by a hydrodynamical paradigm that had been set up by James Jeans decades before, he makes the following assumption: "The starting point in our model is an ambiplasma, which fills the huge sphere. Its density is uniform throughout." This huge primordial blob of ambiplasma is obviously inert, but something somehow sets it in motion, and a universe emerges with a cosmological space-setting peculiar to itself. But before such an "individual" cosmological space can be called into being there must at first be, in a mathematical "somewhere," a "neutral" space in which the "sphere"-like blob of ambiplasma is located. This neutral space is apparently a Euclidean background space, because in such a space it is easy to verify mathematically the demand that the ambiplasma shall have a density, and uniformly the same; or it would have to be another kind of "neutral" space in which spheres and point densities can be readily depicted.

This section began with the introduction of linear perspective in painting and will be terminated with one or two observations on the abandonment of perspective in painting about a century or so ago. I have already commented on this situation very briefly in [7, p. 250 and 13, p. 302], and having since then found my outlook corroborated by R.L. Delevoy [34] (see also [37, chapter XI], I now venture to be a little more explicit. The grand name in the abandonment of perspective—the Riemann of the movement, as it were—is Paul Cézanne. He did not “destroy” linear perspective, as is sometimes asserted, but replaced it by a richer and more variegated one, just as Riemann in creating a general manifold did not “destroy” Euclidean space but replaced it by a rich variety of “space forms,” as Felix Klein termed them. As I have gathered from reading Loran [53]—whether this was precisely Loran’s judgment or not—what Cézanne did, perhaps more by inspired drive than by calculated design, was, in effect, to retain the principle of perspectivity, but to replace the strict linear perspective with only one vanishing point by a much more richly structured perspective in which there are several “regional” vanishing points towards which lines of vision can and do severally converge. Analogously, by Riemann’s express prescription, a manifold is a union of several overlapping neighborhoods, each Euclidean. Just as even before Riemann there were non-Euclidean, or less-than-Euclidean structures on a Euclidean substratum, so also before Cézanne there were painters who, without violating linear perspective, were straining to become dissociated from it. A prominent such predecessor of Cézanne, who is singled out as such somewhere in Delevoy [34], was Dominique Ingres.

After Cézanne, an actual break with perspective began to manifest itself in Cubism, and in Picasso’s *Guernica* not only is there no perspective left, but there is only the flattest lamina of space left at all; this development is in accord with the statement made towards the end of section II, that in the twentieth century, space, like other key concepts of analysis, has been “secularized” and “flattened-out” in various ways. Finally, however superficial the observation may be, the pointillism of Seurat and others, which appeared between Cézanne and Picasso, reminds me of the lesson taught by Georg Cantor (a contemporary of Seurat), that any space, however tightly organized, is an aggregate of points first of all.

In art, as opposed to mathematics, there is still a presumption that linear perspective, however painters may have rebelled against it, is the one and only way for the human eye actually to “see” things in space. The late art historian Erwin Panofsky, in a very erudite early essay, raised doubts whether the optical situation is really so, unchangeably; and even some of his most admiring students and friends have never since forgiven him this “heresy.” A precedent from mathematics ought to warn against being too dogmatic in such contexts. G. Helmholtz, also a great student of human vi-

sion, once aimed at proving that man's ambient space must be Euclidean if it is sufficiently homogeneous to human vision. Whereupon Friedrich Schur [69] pointed out (see also [36]) that in *any* space of constant curvature, positive, zero, or negative, at any point, and perpendicular to any direction, there is a totally geodesic hyperplane E^{n-1} ; and conversely, that any two such hyperplanes can be transformed into each other by a motion. Since for negative curvature, geodesics emanating from the same point never meet again, it is certainly impossible by *vision* to separate Euclidean space from a hyperbolic one.

VI. INFINITY

In Greek natural philosophy the meaning of infinity was a major problem from first to last, from the earliest pre-Socratics in the sixth century B.C. to the last "Commentators" in the fifth century A.D. Anaximander of Miletus, a younger contemporary of Thales, had an intellectual brush with infinity, not much more, and this was enough to immortalize him in the annals of philosophy, any philosophy of the West.

But Greek professional mathematics, although vaguely familiar with the concept of infinity, shied from any direct face-to-face encounter with it, even in contexts which, from our retrospect, would cry out for at least a mention of it, if not for an outright involvement with it. For instance, Euclid has the theorem (9,20) that "*prime numbers are more than any assigned multitude of prime numbers.*" Wording and proof of the theorem are flawless; the proof is ingenious and the same as today's. But what is missing, in retrospect at least, is the paraphrase of this assertion: that *the number of prime numbers is infinite*. No such straight paraphrase appears to be found anywhere in extant Greek writings, although the content of the paraphrase must have been in the forefront of the mind of Eratosthenes when he constructed his famous "sieve." Thus Greek mathematics shied away from the infinitely *large*; and in the case of the "infinitely *small*," the mighty Archimedes apparently never "saw" that the position of the tangent to a curve at a given point is the "limit" of positions of chords through the given point and a movable second point, as the movable point converges to the given one. Yet Archimedes was not at all far from such an insight; while Euclid defines a tangent as a supporting line to the curve in its entire extent, Archimedes in his book *On Spirals* demands the property only locally, as a matter of course.

But this limitation changed visibly with the earliest advent of the Renaissance; infinity in mathematics and in mathematically oriented science began to manifest itself directly and openly, and in various aspects.

The clearest and most unmistakably "modern" manifestation of infinity occurred when John Wallis in 1656 introduced the perduring symbol " ∞ " for an infinitely large number and started operating with it, faultily and per-

haps even recklessly, but determinedly, as with any other symbol or number. This was far from a front-ranking achievement of the century. But it was modernism vibrating. Archimedes knew—in his own thought patterns as clearly and sharply as we in ours—that $1 + \frac{1}{n}$ tends to 1 as n tends to infinity; but he did not have the elation of being able to express this by

$$\lim_{n \rightarrow \infty} \left(1 + \frac{1}{n}\right) = 1$$

or

$$\lim_{n \rightarrow \infty} \left(1 + \frac{1}{n}\right) = 1,$$

and he could articulate the statement and its proof only by a mass of words which would weary even his most ardent admirer of today.

Not all the limitations of Greek mathematics in comprehending the infinitely small were limitations of Greek intellectuality in its entirety. Thus, Aristotle made a strenuous effort in the second half of his *Physics* to shed light on the nature of the linear continuum of real numbers, especially by examining the precise meaning of concepts like together (*háma*), apart (*chóris*), in contact (*haptómenon*), between (*metaxý*), in succession (*ephexés*), contiguous (*echómenon*), continuous (*synechés*); but Greek professional mathematics became in no wise involved in the effort, as if it were totally unaware of it. Mathematics could not or would not recognize that the problem involved, although “masquerading” as physics or natural philosophy, was in fact a profoundly mathematical one. There is no evidence that the Middle Ages recognized this either; for instance, there certainly is nothing in this direction in the extensive commentary on the *Physics* by Thomas Aquinas (1225-1274). The first to recognize the mathematical nature of the problem was Gregory St. Vincent. In a large mathematical work which appeared in 1647 (but was apparently done twenty years before), he put Zeno’s puzzle, Achilles and the Tortoise, into a mathematical setting, and thus resolved the puzzle mathematically (see [55, pp. 79-80]).

The puzzle maintains, against all evidence, that in a race between the quick-footed Achilles and the slow-moving tortoise, if the tortoise has any head start at all, then Achilles cannot overtake him, ever. By the time Achilles has reached the starting point of the tortoise, the latter has moved on by a certain distance. When Achilles covers that distance, the tortoise has gained a further distance, etc. Thus the tortoise always remains ahead. Aristotle maintains that the puzzle is wrong. He knows that the refutation of the puzzle turns on using the structural properties of the linear continuum and of uniform motion, and he talks endlessly about it all. But it is almost impossible to say whether and how the actual refutation of the puzzle by Aristotle comes about. Gregory St. Vincent, however, sets up the obvious geometric series involved in bringing Achilles and the Tortoise abreast of

each other and verifies that the series has a finite sum, which he calls the limit of the series (see [46, p. 437]). Having done this he takes it for granted that the puzzle is refuted thereby. His contemporary readers applauded his solution, totally unconcerned about the “foundational” question of whether there really is a point on the line of pursuit which the sum of the geometric series represents. The seventeenth and even eighteenth centuries were not worried, not yet, by such questions of rigor. It is worth observing that only in 1760 did Father Gerdil “simplify” Gregory’s argument by noticing that if the starting distance is one league and Achilles runs ten times faster than the Tortoise, and x is the distance covered by the Tortoise before they meet, then x is a solution of the equation $10x = 1 + x$.

The slowness with which leading analytic conceptions crystallized even in the fast-moving seventeenth century can be gauged by the following comparison. Gregory, in his *Opus geometricum* of 1647, says that the

terminus of a progression is the end of the series to which the progression does not attain, even if continued to infinity, but to which it can approach more closely than by any given interval [46, p. 437],

which is a rather clear definition of the sum of an infinite series. But the concepts of the limit of a function at a point, and of the derivative of a function at a point, are deeply embedded in the fabric of Newton’s *Principia* of 1686 and are defined there by the following statements, which are hardly an advance on Gregory’s formulation of forty years before.

Quantities, and the ratios of quantities, which in any finite time converge continually to equality, and before the end of that time approach nearer to each other than by any given difference, become ultimately equal.

For those ultimate ratios with which quantities vanish are not truly the ratios of ultimate quantities, but limits towards which the ratios of quantities, decreasing without a limit, do always converge; and to which they approach nearer than by any given difference, but never go beyond, nor in effect attain to, until the quantities have diminished *in infinitum*. [60, pp. 29 and 39]

Returning from the infinitely small to the infinitely large, with which this section actually began, we note that there is one area of the infinitely large to which the age of the Scientific Revolution did not contribute anything constructive. Galileo observed something that had been sporadically noticed since antiquity: that an infinite set can be in one-to-one correspondence with a proper part of itself—or, rather, he adhered to a vaguely shared consensus that because of such a possibility infinite sets do not “in actuality” exist. Galileo’s observation was that two line segments of *unequal* length can be put into a point-by-point correspondence by a perspective; and, what is a little subtler, that the set of integers $\{n\}$, $n \geq 1$, can be put into a one-to-one correspondence with its subset $\{n^2\}$ (see [46, p. 993]). It is remarkable that until the middle of the nineteenth century such obser-

variations were discouraging rather than provocative. A possible explanation for this is that transfinite numbers somehow belong to, or at least are contiguous to "foundations" of mathematics and that the mathematicians of the seventeenth and eighteenth centuries were fully occupied with first amassing "substantive" subject matter, for which to lay "foundations" afterwards.

Also, there were the misgivings of philosophers to overcome. Aristotle's *Physics* has a series of arguments against the existence of the infinitely large, one of them being that if there were an infinitely large body (*soma*), then any finite body when added to it would be "annihilated" by it. Georg Cantor himself interpreted this argument to be a reasoned opposition against the relation $n + a = a + n = a$, which holds if a is any infinite cardinal number and n is a finite one. And he adds that even for an infinite ordinal number α , one has $n + \alpha = \alpha$, but that $\alpha + n > \alpha$. Cantor apparently considers that to be a sufficient refutation of Aristotle because he adds, as if in support of the refutation, that if a finite ordinal number has the temerity to range itself in front of an infinite one, then it is being absorbed by the latter, but if it has the modesty of placing itself in the rear of the infinite one, it is saved (see [26, p. 176], also [11, p. 612]). Even gods nod at times.

Aristotle's argument as stated above has some application even today. The "classical" principle from thermodynamics that the total energy of a closed physical system is constant becomes meaningless if the total energy is infinite, and a physicist who strongly believes in the principle might therefore hesitate to advocate a cosmological theory of the universe in which the total energy need not be finite. Aristotle already argues, in thought patterns of his, that an infinite universe would mean an infinite total mass and an infinite rotational momentum (his universe performed a diurnal rotation), both of which he considered too absurd to contemplate (*De caelo*, book I, chapters 5 and 6). The statement that the total energy of the universe, or of any closed physical system, is finite, or that it is infinite, is a mathematical statement of the kind that can be articulated only if the system is presented by a mathematical paradigm in a mathematical setting; because energy, as usually conceived, is a non-negative real-valued additive set function, so that a ring of sets, preferably of Borel subsets of a manifold M , must be given before any statement can be made. And the total energy is finite or infinite depending on whether the least upper bound of the set function is finite or infinite. Thus, barring an unusual structure of the ring of sets on which the energy is defined and finitely-valued, the total energy will also be finite if the manifold M is compact; unless the mass-distribution has singularities S , in which case, however, $M - S$ will usually cease to be compact.

It was this kind of intricacy that caused Willem de Sitter, the eminent early student of general relativity, to declare that "*Infinity is not a physical, but a mathematical concept.*" This is a very important "key phrase" [17, p.

152] about the role of infinitude in physics and cosmology especially with regard to the problem of the size of the universe, to which we will turn next. But before taking up the problem, we ought to quote the context from which the key phrase is taken, because we are not adopting the particulars of de Sitter's rationale:

How the $g_{\alpha\beta}$ outside our neighborhood are, we do not know, and how they are at infinity, of either space and time, we shall never know, otherwise it would not be infinity. That is what Archimedes meant when he said that the universe could not be infinite. The universe that we know cannot be infinite, because we ourselves are finite. Infinity is not a physical, but a mathematical concept, introduced to make our equations more symmetrical and elegant. [70, p. 113]

I have dealt with the problem of the size of the universe extensively in [8, 9, chapter 14, 11, and 17], and I will restrict myself now to the basic interactions between this problem and the emergence of analysis in the Renaissance and after. The fact of the matter is that the only persons loudly and flatly to proclaim the infinitude of the universe were *non*-scientists like Giordano Bruno (1548-1600) and, long before him, Hasdai Crescas (1340-1411) (see [45, p. 79]), and that scientists proper were very circumspect in asserting the infinity of the universe even if they came close to it.

Thus, as we have already observed in section V, Cusanus held that the universe is both finite and infinite. Copernicus, a century later, was even more restrained. In "On the Revolutions of the Heavenly Spheres" [30, book I, chapter 6], he states that the heaven (*Caelum*) is "immense," without ever amplifying what "immense" means. Further on, in chapter 8 of book I, he touches upon the question whether the world (*mundus*) is finite or infinite, but evades making a decision by declaring that this is the kind of question which should be left to *physiologi* (obviously: philosophers of nature) to argue about. Johannes Kepler, in a stirring polemic against Bruno, maintains with astrophysical arguments that the universe is finite [49, pp. 58-87]. Alexander Koyré, a "secret" admirer of Kepler, presents Kepler's case with admirable objectivity and sympathy, and makes him out to be a better philosopher than Bruno. But not being able to control his own hankering after "infinetization," Koyré terminates the eulogy of Kepler with the following "rebuke":

All that is not new, nor specific to Kepler; it is the traditional teaching of Aristotelian scholasticism. Thus we have to admit that Johannes Kepler, the great and truly revolutionary thinker, was, nevertheless, bound by tradition. In his conception of being, of motion, though not of science, Kepler, in the last Analysis remains an Aristotelian. [49, p. 87]

Galileo could not make up his mind whether the universe is finite or infinite [49, pp. 95-99]. Descartes, however, with his customary incisiveness, came to a wonderful compromise decision. He called God infinite and only Him, and everything else was either finite or indefinite: thus his "extension,"

which was his space of physics and cosmology, and which he identified with matter, was to him indefinite, but not infinite; and he would not budge from this position even under threat of intellectual "excommunication" by the British Platonist Henry More, a partisan of Descartes turned adversary [49, pp. 101-124]. More maintained in correspondence with Descartes that the world, being God's creation, is as infinite as God himself, and that to declare it to be only indefinitely large is an unpardonable blasphemy. But Descartes would not yield; and sudden death relieved him of further acrimony.

Finally we come to Isaac Newton, who is supposed to have brought the infinitization of the universe to its completion, primarily in the *Principia*, I presume. But I do not find it there. In book III of the *Principia*, entitled "The system of the world," the main characterization of the size of the universe, which occurs a number of times, is the same as in Copernicus, as if it were taken from there, namely that it is "immense." Now, in the General Scholium of the book, on p. 544, the term "infinite" does occur, but unmistakably as an attribute of God, in the sentence: "the Supreme God is a Being eternal, infinite, absolutely perfect." Similarly, when infinity occurs in the Scholium of book I, it is in a *temporal* sense (see [49, p. 166]); and, when it occurs in the "essay" on conics which is incorporated in the *Principia* (see [60, pp. 76-108])—which we have described in the preceding section—then it refers only to infinite points of mathematical space, and not at all to points in physical or cosmological space. I do not mean to say, and it would be incorrect to assert, that these are the only occurrences of infinitude in the works and correspondence of Newton. In correspondence with Richard Bentley, Newton makes some kind of admission, reluctantly, that the gravitational universe may be infinite [49, pp. 178-179]. But I do wish to emphasize that in the *Principia* themselves, which went through three editions in forty years, the matter of the infinity of the universe is handled extremely gingerly.

Having adduced all this evidence against the presumption that the seventeenth century made the universe infinite, we still have to explain how, in spite of all such evidence, the impression does prevail that an irreversible infinitization of some very tangible kind did take place. From our outlook the explanation is very simple. Something did happen, something very momentous indeed. It was the creation and emergence of Euclidean space, E^3 and E^2 , as an object in its own mathematical right, and as an indispensable background space for many other contexts; first, as background space for geometry itself, which had never had a ready-made background space before, and then, conjointly with this, as background space for any kind of "perspective" in the arts, for astronomy and cosmology, for mechanics, terrestrial and celestial, and for physics, inorganic and also organic (as in Kepler's theory of vision). And this Euclidean space, which thus intruded it-

self as background space into everything, is of course infinite, by whatever mathematical criteria of infinitude there are. The theory of relativity added other space forms as candidates for background space, some of which are compact and thus "finite." But the ordinary Euclidean background space was in no wise displaced thereby. Classical mechanics, and most parts of classical physics in their classical Euclidean setting, continue to be the basis of college and university education for purposes of science and technology.

Now, an infinite mathematical background space is of course not the same as an infinite universe. But, beginning with Giordano Bruno, philosophers and philosophizing scientists have been tending to equate the two, sometimes even to the detriment of their philosophical systems. We will conclude this section by adducing three instances of such "detrimental," or at least "unnecessary," identification.

As pointed out above, Newton in book III of the *Principia* appraised the size of the universe as "immense" and not as "infinite." In a letter to Richard Bentley, when pressed hard to come out for infinitude of the total gravitational mass of the universe, Newton mused that this could come about only by a suitable gravitational clustering of the masses, which, however, he could "not think explicable by mere natural causes," but would be forced to ascribe "to the counsel and contrivance of a voluntary Agent" [49, p. 185]. Yet his absolute space is a metaphysical (and theological) feature of his universe, and in its true role it is the Euclidean background space which is indispensable to his analysis, although it is burdened with extra-mathematical attributes as unnecessary as they are obscure.

Next, in his *Critique of Pure Reason*, Immanuel Kant introduced *a priori* intuitions of space and time which are fascinating by their originality, whether they be accepted or not. But, quite unnecessarily, he identified this space of intuitive perception with the mathematical background space of Euclid's geometry, thus causing embarrassment to his followers after non-Euclidean geometry came to the fore. As we have seen at the end of the preceding section, spaces of any constant curvature are indeed not separable from Euclidean space in many respects.

Finally, let us adduce an instance from philosophy in the twentieth century. The *Tractatus logico-philosophicus* of the linguo-philosopher Ludwig Wittgenstein—which originally appeared in German in 1918—introduces a "logical space" (*logischer Raum*) and a "world" or "universe" (*Welt*). The logical space is a kind of background space to the world, but it is hardly recognizable as such, being a kind of aggregate or congeries of logical entities like "facts," "atomic facts," "states of affairs," "propositions," etc. Also, in a certain sense the logical space seems to be more substantive than the world, inasmuch as the constituents of the world are only some kind of "pictorial" representation of the constituents of the logical space. Yet, quite unnecessarily, Wittgenstein asserts that the logical space is "infinite";

this is simply a standard philosophers' assertion since Giordano Bruno, and nothing else (for further details see [13, pp. 304-305]).

VII. CONTINUITY

As elaborated in previous contexts ([10] and the second half of [15]), continuity was highly non-mathematical by origin, extending, frequently under one synonym or another, into vast areas of cognition, knowledge, and belief. It had an early affiliation with time and eternity, sharing their mystique and perennial appeal. Its non-mathematical roots were older, deeper, and stronger than those of any other key concept of analysis, and although the mathematization was partially begun already in antiquity, it proceeded rather slowly even through the length of the Scientific Revolution. Only in the course of the nineteenth century was continuity finally fitted to the exigencies of working mathematics, and after this process had come to an end, the consequences became immeasurable; most of topology since the late nineteenth century has been one of the consequences, in a sense.

The Greek word for "continuous" is *synechés*. It is an all-purpose word, which can be used on any level of abstraction, and its basic meaning is "to keep, or hold together" (which is also, as it happens, the basic meaning of the root of the Latin form "con-tinuous"). It occurs already in Homer, once in the *Iliad*, and once in the *Odyssey*. Ulysses relates that after one of his adventures was all over, he and his companions slept three days and three nights "continuously" (*synechés*). It is remarkable that according to the Oxford English Dictionary the first verbal form pertaining to "continuous" in the English language was also associated with time. It is the word *continual* (in time), and it occurs, already before Chaucer, in the phrase "great exercise of body and continual travail of the spirit," in one of the so-called English Prose Treatises of the hermit Richard Rolle of Hampole (1290-1349). The word "continuous" itself gained currency only in the seventeenth century, and the antonyms to it have been not only "discontinuous," "discrete," and suchlike, but also "atomic," "particulate," and "monadic."

A philosophico-mathematical conception of continuity pervades the work of G.W. Leibniz (1646-1716), who was a mathematician, philosopher, and logician all in one. He affirmed the presence, in many contexts, of something he called Law of Continuity (*Loi de Continuité; lex continui*). It runs through his entire metaphysics and science, and also seems to involve mathematical continuous functions, but in rather broad settings and applications. It is not easy to give a fully satisfactory presentation of this Law of Continuity because Leibniz himself did not present it in a systematic study of its own, but reverted to it in various contexts, presenting each time some of its aspects pertinent to the context. But if I extract from his various pro-

nouncements what might be applicable and relevant to mathematics, I am led to crediting him with the following two insights:

A. First, Leibniz somehow conceived, broadly and systematically, the continuity of a function $y = f(x)$ in a rather general setting. This was original with him. Whatever continuity had been anticipated by the Greeks referred only to the element of continuity that is involved in the structure of the *linear continuum* (of real numbers). And while Aristotle tried to approach the problem of the linear continuum front-face (see the preceding section), Greek professional mathematics dealt with it only indirectly by creating the Eudoxian theory of proportions (Euclid, book 5), and conjointly with it the Archimedean process of exhaustion for the computation of areas, volumes, and similar objects.

B. Second, Leibniz made a statement or statements that can be interpreted to imply that the solutions of functional equations governing physics and cosmology depend on initial data in a continuous manner. This means that the equations are "stable," in the sense that "when the essential determinations of one being approximate those of another, as a consequence, all the properties of the former should also gradually approximate those of the latter" [10, p. 498]. This of course is part of the vision and outlook of the Age of Enlightenment: that we live in an orderly, sensible, rational world governed by reason, perhaps even the best possible world attainable. The nineteenth century cast mathematical doubts on this expectation, and the twentieth century proved it deathly wrong; but of this there is more later on.

The above features of continuity in Leibniz, although not mathematically operational immediately, became so eventually. There is nothing similar in Newton's *Principia* except for aspects of continuity in the description of limits and derivatives cited in the preceding chapter. There is also a meta-physical circumlocution of continuity in Newton's description of his absolute time which runs as follows:

Absolute, true, and mathematical time, of itself, and from its own nature, flows equably [Latin: *aequaliter*] without relation to anything eternal, and by another name is called duration. [60, p. 6]

This "equably" is a kind of circumlocution for "uniformly" or "continuously," so that we again have a link between time and continuity, as we have had since Homer.

Surprisingly, Immanuel Kant, after linking his *a priori* space to Euclid's geometry, links his *a priori* time to (the obviously Greek) enumerative numbers, which constitute a discrete set, as if he had never outgrown an attachment to antiquarian Greek mathematical conceptions of his early schooling. There have been, though, occasional attempts, sometimes implied ones, to "quantize" time by introducing minimal time lapses in atomic theories of

various ages: in Epicurean philosophy, in medieval Islamic philosophy [10, p. 494]), and even in twentieth-century quantum physics; but they were not serious enough to engage our (or anybody's) attention.

Going back from the era of Newton to the earliest days of the seventeenth century, we recall from section V that Kepler's procedure of merging various types of conics into one comprehensive family by adding to E^2 points at infinity was broadened by Poncelet, in the first half of the nineteenth century, into something which he also called *Law of Continuity*, a law which by intent was quite different from that of Leibniz.

Going even further back, into the last decades of the sixteenth century, I must emphasize that François Viète and Simon Stevin, especially the latter, were vigorously recommending and propagating the use of decimal fractions for daily and theoretical purposes [22, pp. 347 ff]. In the twentieth century nothing brings the "continuity property" of the linear system of real numbers more intuitively to light and life than their representation by (infinite) decimal expansions. But this was not the immediate effect, and only in the second half of the nineteenth century were the real numbers syllogistically secured. That this development was almost unbelievably protracted can be seen from the following: in the very first theorem of his *Elements*, Euclid took it for granted that a circular arc joining a point inside the circle with a point outside the circle meets its circumference somewhere. Virtually the same syllogistic gap was still present in C.F. Gauss, when, in the proofs of the theorem that a polynomial with complex coefficients has a complex root, he took it for granted that a polynomial of odd degree with real coefficients has a real root. But soon afterwards, in 1817, Bolzano published an essay under the self-explanatory title: *Purely analytical proof of the proposition that between any two values which yield an opposite result, there lies at least one root of the equation*. In a sense, Archimedes had already laid bare the syllogistic gap [41, chapter V] when, making a virtue of necessity, he advertised a "principle" which he called *neusis*, and which, somewhat anachronistically, can be stated as the following broad proposition: if in a plane there are given two curves and the half-lines emanating from a fixed point, and if two of the half-lines intercept intervals of different length between the two curves, then any interval of intermediate length can be intercepted by a half-line of intermediate position.

Bernhard Riemann was a great elucidator of the role of continuity, and of continuity versus discontinuity, in the structure of functions, especially functions that actually occur in analysis. And these elucidations were near the center of his achievements. Most intriguing of all is his conception of a removable singularity, which he formed for holomorphic functions in one complex variable; actually Riemann speaks not of a "singularity" but of a "discontinuity" (*unstetigkeit*), and he does not call it "removable" but "liftable" (*hebbar*), both of which when taken together suggest something

rather gentle. And, while most significant in the case of holomorphic functions of one complex variable, a “removable singularity” can of course also occur in other contexts. In the same memoir, which is Riemann’s doctoral dissertation, he also constructs his Riemann surfaces for holomorphic functions, and the heart of the construction is the introduction of certain points of ramification, and then of local parameters to fit the neighborhoods of such points. A holomorphic function on the Riemann surface, when projected down to the base space, may become many-valued in the neighborhood of a point of ramification and thus cease to be “regular” there, so that, in a sense, the Riemann surface also “removes” certain isolated singularities of the function.

Next, Riemann’s grand memoir on trigonometric series has as a “preamble” his theory of the Riemann integral on an interval. This theory was something radically new. Before it, “everybody,” that is, Cauchy, Dirichlet, and others started with defining the definite integral for a function which is continuous to begin with, but which afterwards is permitted to have certain isolated discontinuities or singularities. Riemann, however, starts out only with the demand that the function be bounded, then sets up a precise condition for the function to have an integral, and envisages the class of functions that are integrable in this sense. There was nothing like it before; that is, nobody before him introduced, with syllogistic precision, a class of functions to suit a pre-conceived purpose. In the creation of the integral there is no mention of continuity at all. Only after the class of functions has been secured does Riemann show by an example that an integrable function may have discontinuities everywhere dense. Riemann was not yet “ready” to formulate Lebesgue’s criterion that a bounded function is Riemann integrable if and only if the set of its discontinuities has outer measure zero; but in style and spirit he was not far from it.

Finally, in his seminal work on shock waves [66, pp. 156-178], Riemann pioneered in exhibiting for certain partial differential equations solutions with rather violent discontinuities, and he coined the strong term *compression thrust* (*Verdichtungstoss*) in the context. All Riemann’s work was exceptionally future-oriented, but this essay was perhaps more so than any other. The paper appeared in 1860. In 1877 E.B. Christoffel added some elaborations, and in 1887 a French mathematician, P.H. Hugoniot, rediscovered the results, apparently independently, so that sometimes a Riemann-Hugoniot theory is referred to. In 1903 the distinguished analyst Jacques Hadamard, in a book on propagation of waves, incorporated a detailed account of such discontinuous solutions, which represent shock waves. Whatever the original appeal of Hadamard’s book may have been, during World War II it was suddenly closely studied by some members of the Manhattan Project for the construction of an atomic bomb.

Whether Riemann, in the nineteenth century, realized it or not, his mem-

oir on the possibility of shock waves was a rebuttal of Leibniz's presumption that the mathematical functional equations which govern the universe depend continuously on initial values each time, and thus also on intermediate values, and therefore remain "stable" indefinitely, as the Age of Enlightenment was intellectually conditioned to envisage the situation. And it was left to the twentieth century to turn the speculative possibility into a physical reality. Riemann gives the impression of having been a gentle person himself. But by *Zeitgeist* he was a contemporary of Karl Marx and other propagators of violence, and being one of the most far-sighted mathematicians ever, he could not but foresee that "discontinuities," even violent ones, would be the need, demand, and fate of the future, for good or ill (see [14]).

The mathematical conception of continuity as syllogistically formulated in the course of the nineteenth century has been an indispensable leaven in the growth of mathematics ever since. It is a fact, though, that these syllogistic formulations have not directly affected age-old conceptions of continuity in some descriptive sciences, especially in earth and life sciences, in which presumptions of continuity have been of the very essence from the first. This may help to explain why a new generation of investigators spearheaded by René Thom and C.H. Waddington [73] has been introducing radically new mathematical paradigms into these disciplines.

It is of course obvious that continuity is the backbone of any evolutionary theory. But continuity had been present, in a descriptive non-evolutionary manner, long before Darwin, in the conception of a Great Chain of Being, which had been perceived by Aristotle, and was advocated with special ardor by Leibniz. The standard reference for this subject matter is Lovejoy [54], whom I have cited before [10, pp. 498-500].

Aristotle merely said that "Nature passes little by little from things lifeless to animal life, so that, by continuity, it is impossible to present the exact lines of demarcation, or to determine to which of the two groups intermediate forms belong." But the Great Chain of Being in Leibniz can be briefly described thus: "All the order of natural beings form but a single chain in which the various classes, like so many rings, are so closely linked one to another that it is impossible for the senses or the imagination to determine precisely the point at which one ends and the next begins." Leibniz also equated the adjective "great" in the Great Chain variously with "maximal," "optimal," "perfect," "complete," and "*continuous*," thus subordinating it to his *Law of Continuity*. Leibniz obviously shut his eyes, and perhaps even his mind, to the very simple verity, which was clearly recognized by the redoubtable Dr. Samuel Johnson, that a Great Chain of Being, however "complete," cannot be "continuous" because each individual "link" in the chain is isolated, meaning that it has a neighbor that is hierarchically above it, and another that is hierarchically below it.

This kind of disparity between physical actuality and mathematical possibility began to be much more significant and pronounced after evolutionary outlooks began to demand everybody's attention [10, 59, and 1]. In 1837 the leading historian of science, William Whewell, in his *History of the Inductive Sciences*, coined the name *uniformitarianism* for hypotheses of continuity peculiar to geology and *catastrophism* for its contrary. For geology, and also biology, and even linguistics, uniformitarianism asserts that there has been a certain continuity of evolution since the formation of the Earth. But no satisfactory definition of this continuity has been agreed upon. The indecision is largely a substantive one, in that the demand of continuity may mean different things purely geologically. But it is also a fact that mathematically uniformitarianism, in almost any of its versions, is a compound of continuity proper, strict constancy, and even cyclicity, and that there is no "special" mathematical conception of continuity by which to bring about some kind of "synthesis" or conflation of the various aspects of uniformitarianism by sheer force of mathematical intervention, as it came about in the seventeenth century when Newton's definitions of momentum and force, based on those of velocity and acceleration, made an end to uncertainties and inconsistencies in the work of Galileo and others.

Even in general philosophy a similar phenomenon can be observed. I have described (in [10 and 15]) how the eminent American philosopher Charles Sanders Peirce—whose basic academic training was in mathematics with subsequent durable achievements in algebra of logic—strove to formulate a universal law of continuity, which he termed *Synechism*, for the governance of the universe; but he was not able to arrive at such a universal law, strenuous efforts notwithstanding. Peirce knew quite well that there are ineluctable discontinuities in the world, and he even adumbrated the fundamental discontinuity inherent to the world picture of quantum physics. But he was also a philosopher, very much so, and a philosopher has to have an integrated system of Thought and thoughts, and there can be no such system without a measure of continuity of some kind of another. Peirce was thoroughly familiar with the Dedekind-Cantor continuity of his time, and he probably also knew a good part of the theory of real variables of his time. But apparently none of his labors would assist and guide him in his quest for a principle of continuity on which to erect a comprehensive philosophical structure of his own.

VIII. POST-ANALYSIS NUMBER THEORY AND ALGEBRA

I have stated repeatedly that in the case of astronomy, geometry, optics, and also mechanics the irruption of background space and of other appurtenances of analysis into their texture took place rather suddenly around 1600, and that the effect was instantaneous and unmistakable.

In the case of number theory, I am not sufficiently familiar with the texture of the subject matter to be able to judge what it was specifically that made the number theory of Fermat and John Wallis different from that of Euclid and Diophantus, and how to attribute the differences, such as they were, to an upsurge of analysis. My cursory reading would suggest that, although there palpably was a change of outlook, spirit, and intellectual motivation, still the achievements of Fermat and Wallis themselves might conceivably have come about in the idiom and with the resources of antiquity (although the formulation of the Fermat conjecture regarding $x^n + y^n = z^n$ for "all" and therefore arbitrary large natural numbers n might have strained the perceptions of a Diophantus to the limit).

But in the eighteenth century a number theory of a genuinely *analytical* affiliation and orientation began to take shape. The Goldbach conjecture and the Waring problem may sound deceptively pre-analytical, but their solutions, complete or partial, have thus far been swathed in analysis.

Furthermore, in the eighteenth century an extremely recondite arithmetical problem has been posed, however indirectly, which can be stated only within analysis itself. One of Euler's finest triumphs is the formula, $k \geq 1$,

$$(8) \quad \sum_{n=1}^{\infty} \frac{1}{n^{2k}} = (-1)^{k-1} \frac{B_{2k}}{(2k)!} 2^{2k-1} \pi^{2k},$$

in which the B_{2k} are rational numbers, so-called Bernoulli numbers, which are given by

$$\frac{t}{e^t - 1} = \sum_{k=0}^{\infty} \frac{B_k}{k!} t^k,$$

see, for instance, [46, pp. 449-452]). There is absolutely nothing corresponding to (8) known for the odd-powered sums

$$(9) \quad \sum_{n=1}^{\infty} \frac{1}{n^{2k+1}}, \quad k = 1, 2, \dots,$$

and particularly irritating is the "limiting" case $k = 0$. In this case the series (9) does not converge. But, as already found by Euler, the limit

$$\gamma = \lim_{n \rightarrow \infty} \left(\frac{1}{1} + \frac{1}{2} + \dots + \frac{1}{n} - \log n \right)$$

exists, and is a positive fractional number, approximately $\gamma = 0.577218 \dots$. It is embarrassing to have to say that nothing arithmetical is known about this number, not even whether it is rational or irrational. There is certainly a problem for some future generations to attend to.

Of pivotal consequence to the future of number theory was another achievement of Euler, his creation of the function

$$(10) \quad \Gamma(s) = \int_0^{\infty} e^{-t} t^{s-1} dt$$

as a holomorphic extension of the factorial function $n!$ from the domain of integers to the domain of complex numbers, the extension being

$$(11) \quad \Gamma(n+1) = n! \quad , \quad \Gamma(s+1) = s\Gamma(s) \quad .$$

Now, this function $\Gamma(s)$ appears, without any number-theoretic rhyme or reason, in the Riemann function equation

$$(12) \quad \Gamma\left(\frac{s}{2}\right) \pi^{-\frac{s}{2}} \zeta(s) = \Gamma\left(\frac{1-s}{2}\right) \pi^{-\frac{1-s}{2}} \zeta(1-s) \quad ,$$

that is

$$(13) \quad \Delta(s) \zeta(s) = \Delta(1-s) \zeta(1-s) \quad ,$$

where

$$(14) \quad \Delta(s) = \Gamma\left(\frac{s}{2}\right) \pi^{-\frac{s}{2}} \quad ,$$

and the renowned "Zeta function"

$$(15) \quad \zeta(s) = \sum_1^{\infty} \frac{1}{n^s}$$

admits the Euler product

$$(16) \quad \zeta(s) = \prod_p (1 - \frac{1}{p^s})^{-1} \quad ,$$

the index p running over all primes.

Presumably, the two relations (12) and (16) between them hold the key to the secret of the Riemann hypothesis; and this places the gamma function, as a participant in (12), into the very midst of the intriguing number-theoretic setting that the two relations create and represent. But from looking at the integral (10) directly, and surveying the properties of the gamma function as emanating from it, one would never suspect it of being involved in such a highly charged context. Except for being outwardly involved in (12), and in a plethora of other functional equations of a similar structure—which were devised by Dirichlet, Dedekind, Erich Hecke, Carl Ludwig Siegel, Max Koecher, and others—the gamma function is really a humdrum function with very "predictable" properties. As G.H. Hardy said casually, he was never held up in his research by not being able to decide a specific question relating to the behavior of the gamma function. And I have never heard it said that in developing the standard theory of the gamma function a piece of syllogism can be modified or illuminated by a number-theoretic argument. Thus Euler's gamma function is an opaque piece of "hard-rock" analysis indispensably lodged among sparkling "jewels" of number theory.

Another gift from analysis to number theory, even grander, was the very mechanism by which the Riemann functional equation and then numerous other functional equations were established, at first indirectly and then ever

more directly. The gift was a piece of Fourier analysis, which the twentieth century has named the Poisson summation formula, but which was introduced into number theory in the first half of the nineteenth century by Lejeune Dirichlet, who may have learned it while studying in Paris as a young man from Joseph Fourier himself. Dirichlet's own application of the Poisson summation formula was to the theory of Gaussian Sums (see [4, pp. 146-187; also 6]). An exposition of the theory of Gaussian Sums in Hecke [43, pp. 235-248] features a contribution of Cauchy, who applied his "residue formula" to it. As already stated before [15, pp. 832-833], the Poisson summation formula and Cauchy's integral and residue formulas are two different aspects of a comprehensive broad-gauged duality formula which lies athwart most of analysis.

The Poisson summation formula does not establish the Riemann-type functional equations themselves, but only "intermediate" theta relations which were the creation of Jacobi; and theta relations lead to functional equations by way of the so-called Mellin Transform. The Poisson summation formula did grow with its application to analytic number theory and is being gradually "identified" with it. Hecke's application of the formula to situations in E^n (in his memoirs, articles 7, 8, 12, and 14 in [43]) was n -dimensional Fourier analysis of high order, and the part of it represented in Landau [52] is only a foretaste of it (see also the Appendix in [5]). After that, extensions of the formula to non-Euclidean settings, in work by André Weil and also by Atle Selberg (Selberg trace formula) have even "algebrified" the formula to an extent. The broadest settings of the formula and of correspondingly broad functional equations have been initiated in the doctoral thesis of J.T. Tate (see [27, pp. 305-347]). But when we come down to the original Hecke cases, the gamma function remains the *deus ex machina* as before.

We may conclude with a few remarks on algebra itself. According to Otto Neugebauer, algebra started quite early in the West; what the Babylonians had before the Greeks was mainly algebra rather than geometry. Similarly, after the Middle Ages, algebra resumed with the algebra of polynomials of third or fourth degree long before analysis started properly around 1600. And yet, after this promising beginning, as if deferring its development to that of analysis, the algebra of polynomials stood virtually still for about two-and-a-half centuries, and after preliminaries by Lagrange and Gauss prominently started up again in the nineteenth century with the work of Abel and Galois. Even though to a twentieth-century algebraist the work of Galois is apparently lucid and comprehensible, in the nineteenth century it was reputed to be obscure and recondite. Until after World War I there were very few textbooks in algebra from which the Galois theory could be conveniently learned; but until books like van der Waerden's *Algebra* began

to appear, a very accessible text from which an analyst could learn it was a chapter in Picard's *Traité d'Analyse*.

While the syllogistic formalization of analysis was started in the early nineteenth century, the corresponding formalization of algebra was a hundred years behind (only after World War I did a textbook in algebra dare to introduce concepts like fields, rings, ideals, etc., in relatively early chapters). This observation was made to me around 1930 by the leading analyst C. Carathéodory, who also had intended to put it in print in some publication *à l'occasion*, but was prevented from doing so by a fellow editor of the planned publication, a young algebraist of the Emmy Noether school who was outraged by this kind of judgment. Yet Carathéodory was right. For instance, when E. Steinitz in a memoir in 1910 introduced the concept of a characteristic for a general commutative field and showed that it can be 0 or a positive prime, this was considered something of a milestone. But this construction was hardly more innovative than a Weierstrass continuous function which is nowhere differentiable, let alone a Cantor non-constant monotone continuous function which has a zero derivative almost everywhere, both of which were discovered long before.

I think it can be said that during the three centuries 1615-1915 analysis was blazing the trails, with algebra somehow following along. After 1915 algebra was beginning to catch up, and in the second half of the twentieth century it has even happened that certain partial algebraizations of analysis have taken place, at least ostensibly. But this last phenomenon belongs not to the "emergence" of analysis but to its "maturing," and so we need not concern ourselves with it.

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