Howard Stein

Logos, *Logic, and* Logistiké: *Some Philosophical Remarks on Nineteenth-Century Transformation of Mathematics*

I

Mathematics underwent, in the nineteenth century, a transformation so profound that it is not too much to call it a second birth of the subject—its first birth having occurred among the ancient Greeks, say from the sixth through the fourth century B.C. In speaking so of the first birth, I am taking the word *mathematics* to refer, not merely to a body of knowledge, or lore, such as existed for example among the Babylonians many centuries earlier than the time I have mentioned, but rather to a systematic discipline with clearly defined concepts and with theorems rigorously demonstrated. It follows that the birth of mathematics can also be regarded as the discovery of a capacity of the human mind, or of human thought—hence its tremendous importance for philosophy: it is surely significant that, in the semilegendary intellectual tradition of the Greeks, Thales is named both as the earliest of the philosophers and the first prover of geometric theorems.

As to the "second birth," I have to emphasize that it is *of the very same subject*. One might maintain with some plausibility that in the time of Aristotle there was no such science as that we call *physics*; that Plato and Aristotle were acquainted with mathematics in our own sense of the term is beyond serious controversy: a mathematician today, reading the works of Archimedes, or Eudoxos's theory of ratios in Book V of Euclid, will feel that he is reading a contemporary. Then in what consists the "second birth"? There was, of course, an enormous *expansion* of the subject, and that is relevant; but that is not quite it: The expansion was itself effected by the very same capacity of thought that the Greeks discovered; but in the process, something new was learned about the nature of that capacity—what it is, and what it is not. I believe that what has been learned, when properly understood, constitutes one of the greatest advances of philosophy—although it, too, like the advance in mathematics itself, has a close relation to ancient ideas (I intend by the word *logos* in the title an allusion to the philosophy of Plato). I also believe that, when properly understood, this philosophical advance should conduce to a certain modesty: one of the things we should have learned in the course of it is how much we do *not* yet understand about the nature of mathematics.

II

A few decades ago, the reigning cliché in the philosophy of mathematics was "the three schools": logicism (Frege-Russell-Carnap), formalism (Hilbert), intuitionism (Brouwer). Many people would now hold that all three schools have failed: Hilbert's program has not been able to overcome the obstacle discovered by Gödel; logicism was lamed by the paradoxes of set theory, impaired more critically by Gödel's demonstration of the dilemma: either paradoxes—i.e., inconsistency—or incompleteness, and (some would add) destroyed by Quine's criticisms of Carnap; intuitionism, finally, has limped along, and simply failed to deliver the lucid and purified new mathematics it had promised. In these negative judgments, there is a large measure of truth (which bears upon the lesson of modesty I have spoken of); although I shall argue later that none of the three defeats has been total, or necessarily final. But my chief interest here in the "three schools" will be to relate their positions to the actual developments in mathematics in the preceding century. I hope to show that in some degree the usual view of them has suffered from an excessive preoccupation with quasi-technical "philosophical"—or, perhaps better, ideological—issues and oppositions, in which perspective was lost of the mathematical interests these arose from. To this end, I shall be more concerned with the progenitors or foreshadowers of these schools than with their later typical exponents; more particularly, with Kronecker rather than Brouwer, and with Dedekind more than Frege. Hilbert is another matter: his program was very much his own creation. Yet in a sense, Hilbert himself, in his mathematical work before the turn of the century, stands as the precursor of his own later foundational program; and in the same sense, as we shall see, Dedekind is a very important precursor of Hilbert as well as of logicism. Since Kronecker and Frege, too, both contributed essential ingredients to Hilbert's program—Kronecker on the philosophical, Frege on the technical side—the web is quite intricate.

III

It is far beyond both the scope of this paper and the competence of its author to do justice, even in outline, to the complex of interrelated investigations and mathematical discoveries (or "inventions") by which mathematics itself was deeply transformed in the nineteenth century; I am going to consider only a few strands in the vast fabric. A good part of the interest will center in the theory of numbers—and in more or less related matters of algebra and analysis—but with a little attention to geometry too.

Let us begin with a very brief glance at the situation early in the century, when the transformation had just commenced. A first faint prefiguration of it can be seen in work of Lagrange in the 1770s on the problems of the solution of algebraic equations by radicals and of the arithmetical theory of binary quadratic forms. On both of these problems, Lagrange brought to bear new methods, involving attention to transformations or mappings and their invariants, and to classifications induced by equivalence-relations. (That Lagrange was aware of the deep importance of his methods is apparent from his remark that the behavior of functions of the roots of an equation under permutations of those roots constitutes "the true principles, and, so to speak, the *metaphysics* of the resolution of equations of the third and fourth degree." Bourbaki[1] suggests that one can see here a first vague intuition of the modern concept of *structure*.) This work of Lagrange was continued, on the number-theoretic side, by Gauss; on the algebraic side, by Gauss, Abel, and Galois; and, despite the failure of Galois's investigations to attract attention and gain recognition until nearly fifteen years after his death, the results attained by the early 1830s were enough to ensure that—borrowing Lagrange's word—the new "metaphysics" would go on to play a dominant role in algebra and number-theory. (I should not omit to remark here that a procedure closely related to that of classification was the introduction of new "objects" for consideration in general, and for calculation in particular. In the *Disquisitiones Arithmeticae* of Gauss, explicit introduction of new ideal "objects" is avoided in favor of the device of introducing new "quasi-equalities" [congruences]; Galois, on the other hand, went so far as to introduce "ideal roots" of polynomial congruences *modulo* a prime number, thus initiating the algebraic theory of finite fields.[2])

By the same period, the early 1830s, decisive developments had also taken place in analysis and in geometry: in the former, the creation—

privately by Gauss, publicly by Cauchy—of the theory of complex-analytic functions; in the latter, besides the emergence of projective geometry, above all the discovery of the existence of at least one geometry alternative to that of Euclid. For although Bolyai-Lobachevskyan geometry made no great immediate impression on the community of mathematicians, Gauss—who, again privately, had discovered it for himself—well understood its importance; and Gauss's influence upon several of the main agents in this story was profound and direct.

IV

As pivotal figures for the history now to be discussed, I would name Dirichlet, Riemann, and Dedekind—all closely linked personally to one another, and to Gauss. Among these, Dirichlet is perhaps the poet's poet—better appreciated by mathematicians, more especially number-theorists, with a taste for original sources, than by any wide public. It is not too much to characterize Dirichlet's influence, not only upon those who had direct contact with him—among those in our story: Riemann and Dedekind, Kummer and Kronecker—but upon a later generation of mathematicians, as a spiritual one (the German *geistig* would do better). Let me cite, in this connection, Hilbert and Minkowski. In his Göttingen address of 1905, on the occasion of Dirichlet's centenary, Minkowski, naming a list of mathematicians who had received from Dirichlet "the strongest impulse of their scientific aspiration," refers to Riemann: "What mathematician could fail to understand that the luminous path of Riemann, this gigantic meteor in the mathematical heaven, had its starting-point in the constellation of Dirichlet"; and he then remarks that although the assumption to which Riemann gave the name "Dirichlet's Principles"—we may recall that this had just a few years previously been put on a sound basis by Hilbert—was in fact introduced not by Dirichlet but by the young William Thomson, still "the modern period in the history of mathematics" dates from what he calls "the other Dirichlet Principle: to conquer the problems with a minimum of blind calculation, a maximum of clear-seeing thoughts."[3] Just four years later, in his deeply moving eulogy of Minkowski, Hilbert says of his friend: "He strove first of all for simplicity and clarity of thought—in this Dirichlet and Hermite were his models."[4] And for perhaps the strongest formulation of Minkowski's "other Dirichlet Principle," consider this passage quoted by Otto Blumenthal, in his biographical sketch of Hilbert, from a letter of Hilbert to Minkowski:

"In our science it is always and only the reflecting mind *der überlegende Geist*], not the applied force of the formula, that is the condition of a successful result."[5]

V

I am going to make a sudden jump here: Why did Dedekind write his little monograph on continuity and irrational numbers? To be sure, that work is in no need of an excuse; but I have long been struck by these circumstances: (1) Dedekind himself tells us in his foreword that when, some dozen years earlier, he first found himself obliged to teach the elements of the differential calculus, he "felt more keenly than ever before the lack of a really scientific foundation of arithmetic." He goes on to express his dissatisfaction with "recourse to the geometrically evident" for the principles of the theory of limits.[6] (Note that Dedekind speaks of a "foundation of *arithmetic*," rather than of analysis.) (2) In the foreword to the first edition of his monograph on the natural number, Dedekind invokes, as something "self-evident," the principle that every theorem of algebra and of the higher analysis can be expressed as a theorem about the natural numbers: "an assertion," he says, "that I have also heard repeatedly from the mouth of Dirichlet."[7] (3) From antiquity (e.g., Aristotle, Euclid) through the late eighteenth and early nineteenth centuries (e.g., Kant, Gauss), a prevalent view was that there are two distinct sorts of "quantity": the discrete and the continuous, represented mathematically by the theories of *number* and of *continuous magnitude*. Gauss, for instance, excludes from consideration in the *Disquisitiones Arithmeticae* "fractions for the most part, surds always."[8] But Dirichlet, in 1837, succeeded in proving that there are infinitely many prime numbers in any arithmetic progression containing two relatively prime terms, by an argument that makes essential use of continuous variables and the theory of limits. This famous investigation was the beginning of analytic number theory; and Dirichlet himself signalizes the importance of the new methods he has introduced into arithmetic: "The method I employ seems to me above all to merit attention by the connection it establishes between the infinitesimal Analysis and the higher Arithmetic [*l'Arithmétique transcendante*];[9]

> I have been led to investigate a large number of questions concerning numbers [among them, those related to the number of classes of binary quadratic forms and to the distribution of primes] from an entirely new point of view, which attaches itself to the principles of infinitesimal

analysis and to the remarkable properties of a class of infinite series and infinite products.[10]

From all this it seems reasonable—I would even say, inevitable—to conclude that a part at least of Dedekind's motive was that of clearing "arithmetic" in the strict sense, i.e. the theory of the rational integers, from any taint of reliance upon principles drawn from a questionable source. In any case, this motive is explicitly stressed by Dedekind's great rival, Kronecker, in the first lecture of his course of lectures on number theory (published by Hensel in 1901). He there quotes from Gauss's preface to the *Disquisitiones Arithmeticae*: "The investigations contained in this work belong to that part of mathematics which is concerned with the whole numbers—fractions excluded for the most part, and surds always." But, says Kronecker, "the Gaussian dictum . . . is only then justified, if the quantities he wishes to exclude are borrowed from geometry or mechanics. . . ." Then—after referring to Gauss's own treatment, in the *Disquisitiones*, of cyclometry (thus, irrational numbers) and of forms (thus, "algebra" or *Buchstabenrechnung*)—he cites elementary examples (Leibniz's series for $\pi/4$; partial fraction expansion of $z \tan(z)$) to support the claim that analysis has its roots in the theory of the whole numbers (with, he says, the sole exception of the concept of the limit—as to how this is to be dealt with, he unfortunately leaves us in the dark); and goes on to conclude:

> Thus arithmetic cannot be demarcated from that analysis which has freed itself from its original source of geometry, and has been developed independently on its own ground; all the less so, as Dirichlet has succeeded in attaining precisely the most beautiful and deep-lying arithmetical results through the combination of methods of both disciplines.[11]

Finally, the epistemological connection is made explicit by Kronecker in another place—his essay "Über den Zahlbegriff"—again with a reference to Gauss:

> The difference in principles between geometry and mechanics on the one hand and the remaining mathematical disciplines, here comprised under the designation "arithmetic," consists according to Gauss in this, that the object of the latter, Number, is *solely* the product of our mind, whereas Space as well as Time have also a *reality, outside* our mind, whose laws we are unable to prescribe completely a priori.[12]

VI

It would be tempting to spend much time on Dedekind's theory of continuity—on its relation to Eudoxos and to geometry, and on the difficulty his contemporaries had in understanding its point. Let me just refer to the extracts from Dedekind's correspondence with Rudolf Lipschitz, given by Emmy Noether in vol. III of Dedekind's *Gesammelte Mathematische Werke*, and particularly to the fact that Lipschitz objected to Dedekind that the property the latter calls "completeness" or "continuity"—in the terminology now standard, *connectedness*—is self-evident and doesn't need to be stated—that no man can conceive of a line without that property. Dedekind replies that this is incorrect, since *he himself* can conceive of all of space and each line in it as entirely discontinuous (adding that Herr Professor Cantor in Halle is evidently another man of the same sort). He refers to §3 of his monograph, where he had said, "If space has a real existence at all, it does *not* have necessarily to be continuous."[13] But it is in the foreword to the first edition of the monograph on the natural numbers that Dedekind returns to this point and indicates the particular grounds for his claim: namely, the existence (as we would say) of *models* of Euclid's geometry in which all ratios of lengths of straight segments are *algebraic numbers*. For the latter concept, he refers the reader to Dirichlet's *Vorlesungen über Zahlentheorie*, §159 of the second, §160 of the third edition. The second edition of Dirichlet's lectures—of which, of course, Dedekind was the editor—was published the year before *Stetigkeit und irrationale Zahlen*; and the section indicated[14] is part of the famous Supplement written by Dedekind in which the theory of algebraic number fields and algebraic integers was developed for the first time.

I have the impression that the central importance of that very great work of Dedekind for the *entire* subsequent development of mathematics has not been generally appreciated. It is certainly well known—to those who know such things—that this Supplement was of the first importance for algebraic number theory and for what is now called "commutative algebra." It is perhaps less well known that this is also the place in which Galois's theory was developed for the first time in its modern form—as a theory of field extensions and their automorphisms, rather than of substitutions in formulas and of functions invariant under substitutions. But that new perspective upon Galois's achievement is itself only one manifestation of a general principle that permeates the work—one that could be summed up in Minkowski's phrase expressing the "other Dirichlet

Principle": "a minimum of blind calculation, a maximum of clear-seeing thoughts." Here is how Dedekind puts it, in the version of his theory that he published in French in 1877:

> A theory based upon calculation would, as it seems to me, not offer the highest degree of perfection; it is preferable, as in the modern theory of functions, to seek to draw the demonstrations, no longer from calculations, but directly from the characteristic fundamental concepts, and to construct the theory in such a way that it will, on the contrary, be in a position to predict the results of the calculation (for example, the composition of decomposable forms of all degrees). Such is the aim that I shall pursue in the following Sections of this Memoir.[15]

The reference to "the modern theory of functions" is, unmistakably, to Riemann; but the methods by which Dedekind pursues his stated aim are distinctly his own. They may be summed up, in a word, as *structural*: Emmy Noether, in her notes to the *Mathematische Werke*, recognizes plainly and with evident enthusiasm the strong kinship of Dedekind's point of view with her own (and I hope it may be presumed that the influence of Noether's point of view upon the mathematics of our century is itself well known).

VII

Perhaps, however, a distinction should be made: One theme associated with the name of Emmy Noether is that of the "abstract axiomatic approach" to algebra; a second is that of attention to *entire algebraic structures and their mappings*, rather than to, say, just numerical attributes of those structures (as a notable example, in topology, attention to homology groups and their induced mappings, not just to Betti numbers and torsion coefficients). One might consider that, of these themes, the second links Noether to Dedekind, the first rather to Hilbert. I want to consider both themes; but I begin with the second, which certainly is present in full measure in Dedekind. And I shall here take up this theme in connection with the monograph *Was sind und was sollen die Zahlen*?

In the foreword to the first edition of that work, Dedekind speaks of "the simplest science, namely that part of logic which treats of numbers."[16] Arithmetic, then, is a "part of logic"; but what is logic?

The same question can be asked of other philosophers who claim that mathematics is, or belongs to, logic; the inquiry tends to be frustrating. For example, Frege says[17] that his claim that arithmetical theorems are analytic can be conclusively established "only by a gap-free chain of inferences, so that no step occurs that does not conform to one of a small number of modes of inference recognized as logical"; but he quite fails to tell us how such recognition occurs—or what its content is. The trouble lies in the reigning presumption that to recognize a proposition as a "logical truth" is to identify its *epistemological basis*; and this is a trouble because—if I may be forgiven for pontificating on the point—no cogent theory of the "epistemological basis" of *any* kind of knowledge has ever been formulated.

On the other hand, some things can be said about Dedekind's claim—and even, I think, some epistemological things. In a general way, we should remember, "logic" in the period in question was taken to be concerned with the "laws of thought." I have said in my opening remarks that the discovery of mathematics was also the discovery of a capacity of the human mind. Dedekind tells us what, in his opinion, this capacity is. His earliest published statement dates from 1879 (by coincidence, the year of Frege's *Begriffsschrift*) and occurs in §161 of the Eleventh Supplement to Dirichlet's *Zahlentheorie*, third edition:[18] in the text he introduces the notion of a *mapping*; in a footnote he remarks, and repeats the statement in the foreword to the first edition of *Was sind und was sollen die Zahlen?*[19] that the whole science of numbers rests upon *this capacity of this mind*—the capacity to envisage mappings—*without which no thinking at all is possible*. So the claim that arithmetic belongs to logic is the claim that the principles of arithmetic are essentially involved in all thought—without anything said about an epistemological basis. Moreover, the principles involved are indeed those employed explicitly in Dedekind's algebraic-number-theoretic investigations: the formation of what he calls "systems"—fields, rings (or "orders"), modules, ideals—and mappings.

But there is another aspect of Dedekind's view that should not be overlooked. What is his answer to the question posed by the title of his monograph? But first, what is the question? The version given by the English translator of the work, *The Nature and Meaning of Numbers*, is (setting aside its abandonment of the interrogative form) quite misleading: the German idiom "Was soll [etwas]?" is much broader than the English "What does [something] mean?" What it connotes is always, in some sense, "intention"; but with the full ambiguity of the latter term. We can, however, see the precise sense of Dedekind's question from the quite explicit answer he gives:

> My general answer [or "principal answer": *Hauptantwort*] to the ques-

> tion posed in the title of this tract is: numbers are free creations of the human mind; they serve as a means for more easily and more sharply conceiving the diversity of things.[20]

The title, therefore, asks: What are numbers, and what are they for? (What is their use, their function?)

The answer is not very satisfying. I think Dedekind makes a mistake by assuming that numbers are "for" some one thing. But it is a venial mistake that lies really only on the surface of his formulation. It is, indeed, part of the great discovery of the nineteenth century that mathematical constructs may have manifold "uses," and uses that lie far from those envisaged at their "creation." On the other hand, this formulation of Dedekind's is so general and vague that perhaps it could be stretched to cover absolutely any application of the concept of number.

What I think is more interesting in all this is that it is not what numbers "are" *intrinsically* that concerns Dedekind. He is not concerned, like Frege, to identify numbers as particular "objects" or "entities"; he is quite free of the preoccupation with "ontology" that so dominated Frege, and has so fascinated later philosophers. Dedekind's general answer to his first question, "Numbers are free creations of the human mind," later takes the following specific form: He defines—or, equivalently, axiomatizes—the notion of a "simply infinite system"; and then says (in effect: this is my own free rendering) *it does not matter what numbers are; what matters is that they constitute a simply infinite system*. He adds—and here I translate literally—"In respect of this freeing of the elements from any further content (abstraction), one can justly call the numbers a free creation of the human mind."[21] Of course it follows from this characterization that numbers are "for" *any use to which a simply infinite system can be put*; it is because this answer does follow, and because it is the right one, that I described Dedekind's mistake about this as venial.

It should be noted that a very similar point applies to Dedekind's analysis of real numbers. Here again the contrast with Frege is instructive. In the second volume of his *Grundgesetze der Arithmetik*, Frege moves slowly toward a definition of the real numbers.[22] He does not quite reach it—that was reserved for the third volume, which never appeared; but what it would have been is pretty clear. Frege's idea was that real numbers are "for" representing ratios of measurable magnitudes (as, for him, whole numbers—*Anzahlen*—are "for" representing sizes of sets—in his terminology, sizes of "extensions of concepts"); and he wants the real numbers to be "objects" *specifically adapted to that function.* That is why he rejects Dedekind's, Cantor's, and Weierstrass's constructions. But Dedekind says, again, that *it does not matter what real numbers "are."* In particular, he does *not* define them as cuts in the rational line. He says, rather, "Whenever a cut (A_1,A_2) is present that is induced by no rational number, we *create* a new, an *irrational* number α, which we regard as completely defined by the cut (A_1,A_2)."[23] In a letter to his friend and collaborator Heinrich Weber,[24] he defends this point, together with the related one concerning the integers, in a rather remarkable passage. Weber has evidently suggested that the natural numbers be regarded primarily as cardinal rather than ordinal numbers, and that they be defined as Russell later did define them. Dedekind replies:

> If one wishes to pursue your way—and I would strongly recommend that this be carried out in detail—I should still advise that by number . . . there be understood not the *class* (the system of all mutually similar finite systems), but rather something *new* (corresponding to this class), which the mind *creates*. We are of divine species [*wir sind göttlichen Geschlechtes*] and without doubt possess creative power not merely in material things (railroads, telegraphs), but quite specially in intellectual things. This is the same question of which you speak at the end of your letter concerning my theory of irrationals, where you say that the irrational number is nothing else than the cut itself, whereas I prefer to create something *new* (different from the cut), which corresponds to the cut. . . . We have the right to claim such a creative power, and besides it is much more suitable, for the sake of the homogeneity of all numbers, to proceed in this manner.

Dedekind continues with essentially the points made in a well-known paper of Paul Benacerraf: there are many attributes of cuts that would sound very odd if one applied them to the corresponding numbers; one will say many things about the class of similar systems that one would be most loath to hang—as a burden—upon the number itself. And he concludes with a reference to algebraic number theory: "On the same grounds I have always held Kummer's *creation* of the ideal numbers to be entirely justified, if only it is carried out rigorously"—a condition that Kummer, in Dedekind's opinion, had not fully satisfied.

In contrast with this last remark of Dedekind's, no less cultivated a mathematician than Felix Klein, as late as the 1910s (when his invaluable lectures on the development of mathematics in the nineteenth century were delivered), felt it necessary to demystify Kummer's "creation" by insisting on the fact that one can—although not in a uniquely distinguished (as one would now say, "canonical") fashion—identify Kummer's "ideal divisors" with actual complex algebraic numbers in the ordinary sense.[25] There can be no doubt in the mind of anyone acquainted with the later development of the subject that—at least in point of actual practice (but I would argue, also in principle)—it was Dedekind in 1888, not Klein thirty years later, who had this right.

VIII

Dedekind's term "*free* creation" also deserves some attention. (The theme has, again, some Dirichletian resonance, since Dirichlet in his work on trigonometric series played a significant role in legitimating the notion of an "absolutely arbitrary function," unrestricted by any necessary reference to a formula or "rule.") It is very characteristic of Dedekind to wish to open up the possibilities for developing concepts, and to wish also that alternative, and new, paths be explored. We have just seen him urging Weber to develop his own views on the natural numbers; he repeatedly urged Kronecker to make known his way of developing algebraic number theory; in the foreword to his fourth (and last) edition of Dirichlet's *Vorlesgunen über Zahlentheorie*, containing his final revision of the Eleventh Supplement, he expresses the hope that one of Kronecker's students may prepare a complete and systematic presentation of Kronecker's theory—and also recommends the attempt to simplify the foundations of his own theory to younger mathematicians, who enter the field without preconceived notions, and to whom therefore such simplification may be easier than to himself.[26] (This was in September 1893. Kronecker had recently died; Hilbert had just begun to work on algebraic number theory.)

If this has come to sound too much like a panegyric on Dedekind, I can only say that that is because he does seem to be a great and true prophet of the subject—a genuine philosopher, of and in mathematics.

IX

In his brief account of Dirichlet, Felix Klein mentions[27] as "a particular characteristic" of Dirichlet's number-theoretic investigations the type of proof, which he was the first to employ, that establishes the existence of something without furnishing any method for finding or constructing it. (One recalls that when, some forty-five years later, Hilbert published proofs of a similar type in the algebraic theory of invariants, they were regarded as unprecedented; and that Paul Gordan is said to have pronounced, "This is not mathematics, it is theology!"[28]) The nonconstructive character of Dirichlet's theorem on the arithmetic progression is noted by Kronecker in his lectures, where he is able to tell his students, with legitimate pride, that he had himself succeeded in the year 1885 in repairing this defect and that the more complete result is to be presented for the first time in the course of those lecture.[29]

What is perhaps most notable about this is the absence, in these published lectures of Kronecker's, of any polemical tone in his comments on constructive vs. nonconstructive methods, and—in the passage I quoted earlier—his positive emphasis upon the role of irrational numbers and limiting processes in number theory. It is of course possible that the restrained tone of these comments, which contrast so markedly with what is generally reported about Kronecker (and also with the more explosive reaction of Gordan to Hilbert), is partly conditioned by his reverence for Dirichlet (it is a rather pleasing fact that the two great number-theoretic rivals and philosophical opponents, Kronecker and Dedekind, each edited publications of work of Dirichlet), and partly by the exigencies of the subject itself (it is clear enough that Kronecker hoped to reduce everything to a constructive and finite basis, but clear also that he was far from having any definite idea of how to do this for the theory of limits).[30] It is possible also, since the work as published was assembled from a variety of manuscript sources,[31] that Hensel in editing this material exercised some moderating influence. But the foreword by Hensel does provide us with a more substantive clue to Kronecker's philosophical stand on the nonconstructive in mathematics:[32]

> He believed that one can and must in this domain formulate each definition in such a way that its applicability to a given quantity can be assessed by means of a finite number of tests. Likewise that an existence proof for a quantity is to be regarded as entirely rigorous only if it contains a method by which that quantity can really be found. Kronecker was far from the position of rejecting entirely a definition or proof that did not meet those highest demands, but he believed that in that case something remained lacking, and held a completion in this direction to be an important task, through which our knowledge would be advanced in an essential point.

No mathematician could quarrel with the statement that a constructive definition or proof adds something to our knowledge beyond what is contained in a nonconstructive one. However, Kronecker's public stand on the work of others was certainly a more repressive one than Hensel's characterization suggests. I think the issue concerns definitions rather more crucially than proofs; but let me say, borrowing a usage from Plato, that it concerns the mathematical *logos*, in the sense both of "discourse" generally, and of definition—i.e., the *formation of concepts*—in particular.

Kronecker's vehement polemic against the ideas and methods of Cantor is more or less notorious. So far as I am aware, that polemic is not represented in the published writings. The first place I know of in which Kronecker published strictures against nonconstructive definition is a paper of 1886, "Über einige Anwendungen der Modulsysteme auf elementare algebraische Fragen"; and here it is in the first instance the apparatus of Dedekind's algebraic number-theory—"jene Dedekind'sche Begriffsbildungen wie 'Modul', 'Ideal', u.s.w"—that comes under attack.[33] Unfortunately, this attack of Kronecker's contains *no* hint that nonconstructive definitions can be accepted at least provisionally (or as a Platonic "second best")—even where, as in the case of analysis, Kronecker has in fact nothing to offer that meets his "higher" demands. It is possible, therefore, that Hensel's reading is off on this point: it is possible, and seems to accord with the fact, that whereas Kronecker was willing to acknowledge the provisional value of a nonconstructive *argument* like Dirichlet's, he meant to exclude more rigidly any introduction of *concepts* by nonconstructive *logoi*. (It must be confessed, again, that this seems to leave analysis, for Kronecker, in limbo.)[34]

X

On the other hand, Kronecker appears uniformly to exempt "geometry and mechanics from his stringent requirements.[35] Are these still to be considered parts of mathematics? Kronecker does not seem to want to exclude them; but perhaps he regards them (e.g., on the basis of Gauss's remarks) as in some measure empirical sciences.

From this a Dedekindian question arises: "Was ist und was soll die Mathematik?" As in the case of number, I immediately repudiate the idea of seeking a definite formula to state "what mathematics is for"—it is "for" whatever it proves to be useful for. Still, what may that be?

Riemann's wonderful habilitation-lecture begins with a characterization of an "n-tuply extended magnitude" in terms that it would not be unreasonable to describe as belonging to "logic":[36] the points of such a manifold are "modes of determination [or "specification"] of a general concept"; examples of concepts whose modes of specification constitute such a manifold are found both in "ordinary life" places in sensible objects; colors) and within mathematics (e.g., in the theory of analytic functions). In the concluding section of that paper, Riemann considers the question of the bearings of his great generalization of geometry upon our understanding of ordinary physical space; *this*, he says, is an empirical question, and must remain open, subject to what developments may occur in physics itself: "Investigations which, like that conducted here, proceed from general concepts, can serve only to ensure that this work shall not be hindered by a narrowness of conceptions, and that progress in the knowledge of the connections of things shall not be hampered by traditional prejudices."[37]

If I may paraphrase: Geometry is not an empirical science, or a part of physics; it is a part of mathematics. *The role of a mathematical theory is to explore conceptual possibilities*—to open up the scientific *logos* in general, in the interest of science in general. One might say, in the language of C. S. Peirce, that mathematics is to serve, according to Riemann, among other interests (e.g., that of facilitating calculation), the interest of "abduction"—of providing the means of *formulating* hypotheses or theories for the empirical sciences.

The requirement of constructiveness is the requirement that all mathematical notions be effectively computable; that mathematics be fundamentally reduced to what the Greeks called *logistikê*: to processes of calculation. Kronecker's concession to geometry and mechanics of freedom from this requirement is tacit recognition that there is no reason to assume a priori that structural relationships in nature are necessarily all of an effectively computable kind. But then, when one sees, with Riemann, the usefulness of elaborating *in advance*, that is, independently of empirical evidence (and, in this sense, a priori), a theory of structures that need not but may prove to have empirical application, Kronecker's limitation to "geometry and mechanics" of the license he offers to those sciences loses much of its plausibility.

Another aspect of this point: Kant made a distinction between *logic*, which, concerned exclusively with *rules of discursive thought* in abstraction from all "content," is a "canon" but not an "organon"—not an instrument for gaining knowledge; and *mathematics*, which *is* an organon, because it is not purely discursive but has a definite content, given in the a priori intuition of space and time.[38] But the geometry of Riemann is quite freed from any such specific content: Kant's view that the creative or productive power of geometry rested upon its concrete "intuitive" spatial content was simply mistaken, and mathematics is seen to be an *abstract* and a priori "organon" of knowledge (whether one chooses to call it a "logic," or a "dialectic," or whatever). And again, in view of this its *general* capacity, there is no more reason to hamper it by restrictions to the effectively computable than to hamper it by restrictions to the "spatially intuitive."

But then—so *this* dialectic goes—why restrict the license at all (e.g., to mathematical disciplines that one thinks *might* serve the ends of some empirical science)? Why not complete freedom of conceptual elaboration in mathematics? Then, *within* any *logos* so freely developed, one can pay appropriate attention to the distinction between what is and what is not constructive.

XI

This point of view—which is really not very far from the moderate position attributed to Kronecker by Hensel—is the one that Hilbert so vigorously championed. Let me call attention, in briefest outline, to a few salient points.

Notice of Hilbert's interest in the foundations of geometry dates back to the earliest days of his work in algebraic number-theory, or even somewhat before: it was in 1891, according to Otto Blumenthal,[39] that Hilbert, in a mathematical discussion in a Berlin railway waiting room, made his famous statement that in a proper axiomatization of geometry "one must always be able to say, instead of 'points, straight lines, planes', 'tables, chairs, beer mugs'." This view—that the basic terms of an axiomatized system must be "meaningless"—is often misconstrued as "formalism." But the very same requirement was stated, fifteen years earlier, by the "logicist" Dedekind, in his letter to Lipschitz already cited: "All technical expressions [are to be] replaced by arbitrary newly invented (heretofore nonsensical) words; the edifice must, if it is rightly constructed, not collapse.'[40]

Hilbert uses as epigraph to his *Grundlagen der Geometrie* a well-known Kantian aphorism:[41] "Thus all human knowledge begins with intuitions, proceeds to concepts, and ends with ideas." For Hilbert, axiomatization, with all basic terms replaced by "meaningless" symbols, is *the elimination of the Kantian "intuitive"*—the "proceeding to concepts"; hence the axioms taken together constitute a *definition*. Of what? Of a *species of structure*. Now, by this, we need not mean "a set." It is a point of interest, and a very useful one, that the notion of set proves so serviceable both as a tool for concept-formulation *within* a mathematical discipline (ideals, etc.) and as affording a *general framework* for many or all mathematical disciplines; but set theory is a tool, not a foundation. For Hilbert, it is the axiomatized discourse itself that constitutes the mathematical *logos*; and the only restriction upon it that he recognizes is that of formal consistency.

As to "ideas": Kant associates ideas with *completeness* or *totality*. For Hilbert, I believe, this connoted the survey of the general characteristics of the structures of a species and of related species—more explicitly, in the geometrical case, the study of the problems of consistency and categoricity of the axioms, their independence, and the sorts of alternatives one gets by changing certain axioms; in short, something very much like model-theory. (Of course, when one now gets down to brass tacks, sets are pretty much indispensable; and because of the essential incompletability of set theory, the "absolute completeness or totality" fails: Kantian "dialectical illusion" has not after all been avoided.)

In the correspondence of Frege and Hilbert,[42] it is amusing or exasperating, depending upon one's mood, to see Frege, wondering what Hilbert can mean by calling his axioms "definitions," come in his ponderous but thorough way to the conclusion that *if* they are definitions, they must define what he calls a "concept of the second level"—and then more or less drop this notion as implausible or uninteresting to pursue; and to see Hilbert, not very interested in Frege's terminology or his point of view, fail to understand what Frege's undervalued insight really was: for a Fregean "second-level concept" simply *is* the concept of a species of structure. So: a tragically or comically missed chance for a meeting of minds.

But now, what of Hilbert's "program"? I think it is unfortunate that Hilbert, in his later foundational period, insisted on the formulation that ordinary mathematics is "meaningless" and that only finitary mathematics has "meaning." Hilbert certainly never abandoned the view that mathematics is an organon for the sciences: he states this view very strongly in the last paper reprinted in his *Gesammelte Abhandlungen*, called

"Naturerkennen und Logik" (1930);[43] and he surely did not think that physics is meaningless, or its discourse a play with "blind" symbols. His point is, I think, this rather: that the mathematical *logos* has no responsibility to any imposed *standard* of meaning: not to Kantian or Brouwerian "intuition," not to finite or effective decidability, not to anyone's metaphysical standards for "ontology"; its *sole* "formal" or "legal" responsibility is to be consistent (of course, it has also what one might call a "moral" or "aesthetic" responsibility: to be useful, or interesting, or beautiful; but to this it cannot be constrained—poetry is not produced through censorship).

In proceeding to his "program," however, Hilbert set as his goal the *mathematical investigation of the mathematical* logos *itself*, with the principal aim of establishing its consistency (or "their" consistency—for he did not envisage a single canonical axiom-system for all of mathematics). And here he made essential use both of Frege and of Kronecker. For Frege's extremely careful and minute regimentation of logical language or "concept-writing" did not, as Frege himself thought it would, serve as a guarantee of consistency; but the techniques he used for that regimentation did render the formal languages of mathematical theories, and their formal rules of derivation, subject to mathematical study in their own right, regarded as purely "blind" symbolic systems. Moreover, the regimentation was itself of such a kind that the play with symbols was a species of calculation—of *logistiké*. Without Frege, proof-theory in Hilbert's sense would have been impossible.

Of course, the final irony of this story, and the collapse of Hilbert's dream of establishing the consistency of the logic of the *logos* by means restricted to *logistiké*, lies in the discovery by Gödel, Post, Church, and Turing that there is a *general theory* of *logistiké*, and that this theory is nonconstructive; in particular, that neither the notion of consistency nor that of provability is (in general) effective; and further that all sufficiently rich consistent systems fall short of the Kantian "ideal"—are incomplete.

This leaves us with a mystery—a subject of "wonder," in which, according to Aristotle, philosophy begins. He says it *ends* in the contrary state; I am inclined to believe, as I think Aristotle's master Plato did, that philosophy does not "end," but that mysteries become better understood—and deeper. The mysteries we now have about mathematics are certainly better understood—and deeper—than those that confronted Kant or even Gauss. To attempt, however, to survey what they are, even in the sketchiest way, although enticing, is a task that would require another paper, and is certainly beyond the scope of this one.[44]

Notes

1. N. Bourbaki, *Éléments d'histoire des mathématiques*, 2d ed. (Paris: Hermann, 1974), p. 100.
2. Ibid., pp 108-9.
3. Hermann Minkowski, *Gesammelte Abhandlungen*, vol. 2, (Leipzig, 1911; reprinted New York: Chelsea 1967), pp. 460-61.
4. Ibid., vol. 1, p. xxix; David Hilbert, *Gesammelte Abhandlungen*, vol. 3, (Berlin: Springer, 1935), p. 362.
5. Ibid., p. 394.
6. Richard Dedekind, *Stetigkeit und irrationale Zahlen*, Vorwort; in his *Gesammelte Mathematische Werke*, vol. 3, ed. R. Fricke, E. Noether, and O. Ore (Braunschweig: F. Vieweg, 1932), pp. 315-16; translation in Dedekind, *Essays on the Theory of Numbers*, trans. W. W. Beman (Open Court, 1901; reprinted New York: Dover, 1963), p. 1.
7. Dedekind, *Was sind und was sollen die Zahlen?*, Vorwort zur ersten Auflage; *Werke*, vol. 3, p. 338; *Essays*, p. 35.
8. Carl Friedrich Gauss, *Disquisitiones arithmeticae*, preface; quoted by Leopold Kronecker, *Vorlesungen über Zahlentheorie*, vol. 1, ed. Kurt Hensel (Leipzig: Teubner, 1901), p. 2.
9. Peter Gustav Lejeune Dirichlet, "Sur l'usage des séries infinies dans la théorie des nombres," in *G. Lejeune Dirichlet's Werke*, vol. 1, ed. L. Kronecker (Berlin, 1889; reprinted New York: Chelsea, 1969), p. 360.
10. Dirichlet, "Recherches sur diverses applications de l'analyse infinitésimale à la théorie des nombres," ibid., p. 411.
11. Kronecker, *Vorlesungen über Zahlentheorie*, vol. 1, pp. 2-5 (italics added).
12. Kronecker, *Werke*, vol. 3, 1st half-volume, ed. K. Hensel (Leipzig: Teubner, 1899), p.253 (emphasis in original; Kronecker quotes, in a footnote, a letter from Gauss to Bessel, 9 April 1830).
13. Dedekind, *Werke*, vol. 3, p. 478.
14. See Dedekind, *Werke*, vol. 3, pp. 223ff. (§159, in Supplement X to the second edition of Dirichlet's lectures), and ibid., pp. 2ff. (§160, in Supplement XI to the third and fourth editions of that work).
15. Dedekind, "Sur la théorie des Nombres entiers algébrique," in his *Werke*, vol. 3, p. 296.
16. Dedekind, *Werke*, vol. 3, p. 335; *Essays*, p. 31.
17. Gottlob Frege, *Die Grundlagen der Arithmetik* (Breslau: Verlag von W. Koebner, 1884; edition with parallel German and English texts, trans. J. L. Austin, reprinted Evanston, Ill.: Northwestern University Press, 1968), p. 102; cf. also his *Grundgesestze der Arithmetik*, vol. 1 (Jena, 1893; reprinted (two volumes in one) Hildesheim: Georg Olms Verlagsbuchhandlung, 1962), p. vii.
18. Dedekind, *Werke*, vol. 3, p. 24.
19. Ibid., p. 336; *Essays*, p. 32.
20. Dedekind, *Werke*, vol. 3, p. 335; *Essays*, p. 31.
21. Dedekind, *Was sind und was sollen die Zahlen?*, §73; *Werke*, vol. 3, p. 360, *Essays*, p. 68.
22. Frege, *Grundgesetze der Arithmetik*, vol. 2 (Jena, 1903; reprinted, together with vol. 1, as cited in n. 17 above), pp. 155-243; for general discussion, see §§156-65, 173, 175, 197, and 245—pp. 154-63, 168-69, 170-72, 189-90, 243.
23. Dedekind, *Werke*, vol. 3, p. 325; *Essays*, p. 15.
24. Dedekind, *Werke*, vol. 3, pp. 489-90 (letter dated 24 January 1888).

25. Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im 19ten Jahrhundert*, vol. 1, ed. R. Courant and O. Neugebauer (reprinted New York: Chelsea 1956), p. 322.

26. Dedekind, *Werke*, vol. 3, p. 427. Another passage is worth quoting as an illustration both of Dedekind's generous attitude toward alternative constructions and of the Dirichletian ideal that governs his own preferences; it comes, again, from the foreword to *Was sind und was sollen die Zahlen?*, but refers back to his procedure in constructing the real numbers. Dedekind recognizes that the theories of Weierstrass and of Cantor are both entirely adequate—possess complete rigor; but of his own theory, he says that "it seems to me somewhat simpler, I might say *quieter*," than the other two. (See *Werke*, vol. 3, p. 339. Italics added—Dedekind's word is *ruhiger*; the published English translation, "easier," loses the point of the expression.)

27. Klein, *Entwicklung der Mathematik*, vol. 1, p. 98.

28. See Hilbert, *Gesammelte Abhandlungen*, vol. 3, pp. 394-95.

29. Kronecker, *Vorlesungen über Zahlentheorie*, vol. 1, p. 11.

30. Kronecker had, of course, a generally workable technique for dealing with algebraic irrationals; but when he speaks in Lecture 1 of the definitions that occur in analysis, he remarks that "from the entire domain of this branch of mathematics, only the concept of limit or bound has thus far remained alien to number theory" (ibid., vol. 1, pp. 4-5).

31. Ibid., p. viii.

32. Ibid., p. vi.

33. Dedekind, *Werke*, vol. 3, p. 156 n. What Kronecker criticizes in the *second* instance here is "the various concept-formations with the help of which, in recent times, it has been attempted from several sides (first of all by Heine) to conceive and to ground the 'irrational' in general." That Kronecker mentions Heine—who in fact adopted, with explicit acknowledgment, the method introduced by Cantor—as "first" is striking; was his antipathy to Cantor so great that he refused to mention the latter's name at all—or, indeed, suppressed his recollection of it? The suspicion that some degree of pathological aversion is involved here is increased by an example cited by Fraenkel in his biographical sketch of Cantor appended to Zermelo's edition of Cantor's works (Georg Cantor, *Abhandlungen mathematischen und philosophischen Inhalts*, ed. Ernst Zermelo [Berlin, 1932; reprinted Hildesheim: Georg Olms Verlagsbuchhandlung, 1966], p. 455, n. 3). Cantor published in 1870 his famous theorem on the uniqueness of trigonometric series (see his *Abhandlungen*, pp. 80ff.). In the following year, he published the first stage in his extension of that result (allowing a finite number of possibly exceptional points in the interval of periodicity); this paper also contains a simplification of his earlier proof, which he acknowledges as due to "a gracious oral communication of Herr Professor Kronecker" (ibid., p. 84). What Fraenkel reports—with an exclamation point—is that in his posthumously published lectures on the theory of integrals, Kronecker cites the problem of the uniqueness of trigonometric expansions as still open! It is worth remembering that Cantor achieved his broadest generalization of his uniqueness theorem in 1872, in the same paper (ibid.) pp. 92ff.) in which he first presented his construction of the real numbers and initiated the study of the topology of point sets. As to Dedekind, he responds, in his own quiet style, to Kronecker's footnote attack in a footnote of his own to §1 of *Was sind und was sollen die Zahlen?* (see Dedekind, *Werke*, vol. 3, p. 345).

34. This point deserves a little further emphasis. Kronecker had a *program* for the elimination of the nonconstructive from mathematics. Such a program was of unquestionably great interest. It continues to be so, in the double sense that a radical elimination—showing how to render constructive a sufficient body of mathematics, and at the same time so greatly simplifying that body, as to make it plausible that this constructive part is really *all* that deserves to be studied and that the constructive point of view is the clearly most fruitful way of studying it—would be a stupendous achievement; whereas in the absence of such radical elimination, it remains always important to investigate how far constructive methods may be applied. But Kronecker's own published ideas for constructivization appear to have extended only to the *algebraic*, as indeed the remark cited in n. 28 above explicitly acknowledges (for when Kronecker says that "the concept of the limit . . . has thus far remained *alien to number theory*," that last phrase has to be taken to mean *irreducible to*

the finitary theory of the natural numbers—which reducibility is what Kronecker's constructive program aimed at). Kronecker, therefore, in his *documented* strong statements against "the various concept-formations with the help of which . . . it has been attempted . . . to conceive and ground the 'irrational' in general," is in the position of arguing that, in the absence of a constructive definition, nonconstructive ones are nevertheless to be avoided; that it is better to have *no definition at all*. This is the position that seems to me to follow from Kronecker's statements, although it conflicts with the report of Hensel.

35. See Kronecker, *Werke*, vol. 3, pp. 252-53; *Vorlesungen über Zahlentheorie*, vol. 1, pp. 3, 5.

36. *Bernhard Riemann's Gesammelte Mathematische Werke*, ed. Heinrich Weber with the assistance of Richard Dedekind, 2d ed. (1892; reprinted New York: Dover, 1953), pp. 273-74.

37. Ibid., p. 286.

38. See *Logik nach den Vorlesungen des Herrn Prof. Kant im Sommerhalbenjahre 1792*, in *Die philosophischen Hauptvorlesungen Immanuel Kants*, ed. Arnold Kowalewski (Munich, 1924; reprinted Hildesheim: Georg Olms Verlagsbuchhandlung, 1965), pp. 393-95.

39. Hilbert, *Gesammelte Abhandlungen*, vol. 3, pp. 402-3.

40. Dedekind, *Werke*, vol. 3, p. 479.

41. Kant, *Critique of Pure Reason*, A702/B730.

42. Gottlob Frege, *On the Foundations of Geometry and Formal Theories of Arithmetic*, trans. Eike-Henner W. Kluge (New Haven: Yale University Press, 1971), pp. 6-21. See esp. p. 19 (where Frege says quite distinctly, "It seems to me that you really intend to define second-level concepts"); but cf. also Frege's subsequent remark in the first of his two papers, "On the Foundations of Geometry" (ibid., p. 36): "If any concept is defined by means of [Hilbert's axioms], it can only be a second-level concept. It must of course be doubted whether any concept is defined at all, since not only the word 'point' but also the words 'straight line' and 'plane' occur." In the latter passage, Frege's notion seems to be that Hilbert must have set out to define the (second-level) concept of a *concept of a point*; and similarly for line, etc. The earlier passage, in his letter to Hilbert, was preceded by this: "The characteristics which you state in your axioms . . . do not provide an answer to the question, 'What property must an object have to be a point, a straight line, a plane, etc.?' Instead they contain, for example, second-level relations, such as that of the concept of 'point' to the concept 'straight line'." It is not far from this to the conclusion that what Hilbert's axioms *do* define is a *single* second-level relation, namely, that relation among a whole system of first-level concepts—corresponding to the undefined terms of the axiomatization, 'point', 'line', 'plane', 'between', and 'congruent'—which qualifies the whole as a Euclidean geometry. But however near that conclusion lies, Frege evidently failed to draw it.

43. Hilbert, *Gesammelte Abhandlungen*, vol. 3, pp. 378-87.

44. As I have suggested earlier in this paper, the role that may yet be played in further clarification of the nature of mathematics by the views of the "three schools" cannot be regarded as certain. My own opinion is that all three views (if they can really be called "three": within each "school," there have been quite significant differences) have made useful contributions to our understanding, but also that each will remain as merely partial and not fully satisfactory. Yet, as I have also remarked (above, n. 34), one cannot exclude the possibility that a genuine constructivization of mathematics will yet be achieved (although I consider the arguments of section X above as counting rather strongly against this). In the same way, it remains possible that a recognizably constructive proof of the consistency of analysis, or even of (some version of) set theory, will be found; and such an event would afford great impetus to the revival of (a modified form of) the Hilbert program. As to "logicism," as it seems to me the vaguest of the three doctrines, I find it hardest to envisage prospects for it, and am most strongly inclined to see its positive contribution as exhausted by the insight that mathematics is in some sense "about" conceptual possibilities or conceptual structure—an insight, as I have remarked, already clearly present in Riemann, but much more fully worked out after the stimulus provided by Dedekind, Frege, Russell, and

Whitehead. Nevertheless it is (barely, I think) conceivable that progress in understanding the structure of "knowledge" will succeed in isolating a special kind of knowledge that reasonably deserves to be considered "logical," thus conferring new interesting content upon the question whether mathematics does or does not belong to logic.