Science in Context **10**, 2 (1997), pp. 253–296

LEO CORRY

The Origins of Eternal Truth in Modern Mathematics: Hilbert to Bourbaki and Beyond[1]

The Argument

The belief in the existence of eternal mathematical truth has been part of this science throughout history. Bourbaki, however, introduced an interesting, and rather innovative twist to it, beginning in the mid-1930s. This group of mathematicians advanced the view that mathematics is a science dealing with structures, and that it attains its results through a systematic application of the modern axiomatic method. Like many other mathematicians, past and contemporary, Bourbaki understood the historical development of mathematics as a series of necessary stages inexorably leading to its current state — meaning by this, the specific perspective that Bourbaki had adopted and were promoting. But unlike anyone else, Bourbaki actively put forward the view that their conception of mathematics was not only illuminating and useful for dealing with the *current* concerns of mathematics, but that this was in fact the ultimate stage in the evolution of mathematics, bound to remain unchanged by any future development of this science. In this way, they were extending in an unprecedented way the domain of validity of the belief in the eternal character of mathematical truths, from the body to the images of mathematical knowledge.

Bourbaki were fond of presenting their insistence on the centrality of the modern axiomatic method as a way to ensure the eternal character of mathematical truth as an offshoot of Hilbert's mathematical heritage. A detailed examination of Hilbert's actual conception of the axiomatic method, however, brings to the fore interesting differences between it and Bourbaki's conception, thus underscoring the historically conditioned character of certain, fundamental mathematical beliefs.

1. Introduction

Throughout history, no science has been more closely linked to the idea of eternal truth than mathematics. This goes without saying. Still, the way this idea has been conceived of has not in itself been eternal and invariable; rather, it has been the subject of a historical process of development and change. Yesterday's conception of the eternal character of mathematical truth is not identical to today's. Surpris-

[1] I wish to thank David Rowe for helpful editorial comments.

ingly, perhaps, this idea has never been subjected to more serious scrutiny and attack than it is nowadays.

In the present article I briefly discuss a short chapter in the long story of the development of the idea of eternal truth in mathematics, focusing on two central figures who have been among the most influential in shaping both the content of twentieth-century mathematics and our conception of it: Hilbert and Bourbaki. My main point of interest will be their respective conceptions of the role of the modern axiomatic method in mathematics and of its significance for the eternal status of mathematical truth. Considering the differences between the two will illustrate the subtle changes that the status of truth in mathematics may undergo as part of its historical process of development. It will also clarify the background against which current debates on these questions are being held.

For the purposes of the present discussion, it is useful to recall the distinction between the "body" and the "images" of scientific knowledge. The body of knowledge includes statements that are answers to questions related to the subject matter of any given discipline. The images of knowledge, on the other hand, include claims that express knowledge about the discipline qua discipline. The body of knowledge includes theories, "facts," methods, open problems. The images of knowledge serve as guiding principles, or selectors. They pose and resolve questions that arise from the body of knowledge but that are in general not part of it and that cannot be settled within it. The images of knowledge determine attitudes concerning such issues as: Which of the open problems of the discipline most urgently demands attention? What is to be considered a relevant experiment, or a relevant argument? What procedures, individuals, or institutions have the authority to adjudicate disagreements within the discipline? What is to be taken as the legitimate methodology of the discipline? What is the most efficient and illuminating technique for solving a certain kind of problem in the discipline? What is the appropriate university curriculum for educating the next generation of scientists in a given discipline? Thus the images of knowledge cover both cognitive and normative views of scientists concerning their own discipline.

The borderline between these two domains is somewhat blurred and it is historically conditioned. Moreover, one should not perceive the difference between the body and the images of knowledge in terms of two layers, one more important, the other less so. Rather than differing in importance, these two domains differ in the range of the questions they address: whereas the former answers questions dealing with the subject matter of the discipline, the latter answers questions about the discipline qua discipline. They appear as organically interconnected domains in the actual history of the discipline. A distinction between them is undertaken for analytical purposes only, usually in hindsight.[2]

Stated in these terms, the issue of the eternal character of mathematical truth is closely connected to the images of mathematical knowledge, since it deals with our

[2] For a more detailed discussion of this scheme see Corry 1989.

conception of the kind of knowledge that mathematics produces. In the sections that follow I will discuss the images of mathematics that underlie the works of Hilbert and of Bourbaki, and how they are connected to the question that occupies us here. Finally, I will discuss very recent debates on the status of mathematical truth, while attempting to place these debates in a proper historical perspective.

2. Hilbert

David Hilbert was among the most influential mathematicians of the beginning of this century, if not *the* most influential. The impact of his ideas may be traced up to the present day in fields as distant from one another as number theory, algebraic invariants, geometry, mathematical logic, linear integral equations, and physics. His name is often associated with the application of the "modern axiomatic approach" to diverse mathematical disciplines and, in the context of the foundations of mathematics, he is usually mentioned as the founder of the formalist school. The term "Hilbert Program," in particular, refers to the attempt to provide a finitistic proof of the consistency of arithmetic, an attempt that Gödel's works in the 1930s proved to be hopeless.

As with any German professor educated in the specific intellectual environment of the end of the nineteenth century, the debates of his philosopher colleagues were not absolutely foreign to Hilbert, and in fact his own conceptual world was heavily loaded with Kantian and neo-Kantian images. A quotation of Kant in the frontispiece of his famous *Grundlagen der Geometrie* (1899) is but one well known instance of this. The lecture notes of his courses in Göttingen contain many more examples. There is also abundant evidence of his interest and involvement in the careers of such philosophers as Edmund Husserl and Leonard Nelson, and of his hope for a fruitful interaction between them and the Göttingen mathematicians (Peckhaus 1990). Likewise, because of his direct involvement in the foundational debates of the 1920s and the influence of his works in this domain on the subsequent development of many metamathematical disciplines, his name has appeared pervasively in the context of twentieth-century discussions about the philosophy of mathematics.

But in spite of all this, one has to exercise great care when referring to Hilbert's philosophy of mathematics. Over his long years of activity Hilbert came to deal with many different aspects of mathematics and of physics, facing the development of ever new theories and empirical discoveries, and amidst changing historical contexts. Hilbert was fond of making sweeping statements about the nature of mathematical knowledge, about the relationship between mathematics and science, and about logic and mathematics. These statements are abundantly recorded in both published and unpublished sources. In some cases the views expressed in them changed from time to time, if current scientific developments demanded this, or if for any other reason Hilbert had changed his mind. And yet the authoritative tone and total conviction with which Hilbert proclaimed his opinions remained forever the same. Concerning the foundations of physics, for instance, he changed his position around 1913 from a total and absolute support of the idea that all physical phenomena can be reduced to mechanical interactions between rigid particles, to an equally total and absolute defense of an electromagnetic reductionism. He produced important works in such fields as the kinetic theory of gases and the general theory of relativity, while holding each of these views. His stress on the importance of each of these positions, respectively, as a starting point for physical research is consistently recorded in his lecture notes. Yet, neither in his publications nor in his lecture notes does one find a clue to the fact that his present view was different from the one held before, nor a word of explanation about the reasons that brought about this change of perspective (Corry forthcoming).

As Hilbert was a "working mathematician," whose main professional interests lay in solving problems, proving theorems, and building mathematical and physical theories, one should not a priori expect to find any kind of systematic philosophical discussions in his writings. When these writings do discuss philosophical issues, they contain claims that are not always supported by solid arguments and that sometimes contradict earlier or later claims. In his interchanges with Gottlob Frege and Luitzen J. E. Brouwer, Hilbert even showed a marked impatience with philosophical discussions. Certainly, it would be misleading to speak about "*the* philosophy of mathematics of Hilbert," without further qualification. Instead, it seems to me much more historically illuminating to speak of Hilbert's images of mathematics, and to attempt to elucidate what was more or less steady and permanent in them, on the one hand, and on the other hand, what changed over time and under what circumstances. In the present section I will discuss some of those images, focusing especially on those aspects that are relevant to the use of the axiomatic method and to the question of the provisory or eternal status of mathematical truth.

The first publication in which Hilbert thoroughly applied the modern axiomatic method was his book *Grundlagen der Geometrie* (1899). Among Hilbert's sources of inspiration when dealing with the issues covered in this book, the most important included the German tradition of work on projective geometry (in particular Moritz Pasch's text of 1882) and, perhaps to a somewhat lesser extent, the recent work of Heinrich Hertz on the foundations of mechanics (Corry 1997a). The *Grundlagen* is often read in retrospect as an early manifestation of the so-called "formalistic" position that Hilbert elaborated and defended regarding the foundations of arithmetic since the 1920s. Under this reading, Hilbert conceived of geometry as a deductive system in which theorems are derived from axioms according to inference rules prescribed in advance; the basic concepts of geometry, the axioms and the theorems, are — under this putative conception — purely formal constructs, having no direct, intuitive meaning whatsoever. This reading of the *Grundlagen,* however, does not faithfully reflect Hilbert's own conception. His approach to geometry at the turn of the century had a meaningful, empiricist hard core, in which the empirical issues of geometry were never lost to sight. In fact, the famous five groups of axioms are so conceived as to express specific, separate ways in which our intuition of space manifests itself. Hilbert's essentially empiricist conception of geometry is one of the aspects of his images of mathematics that remained unchanged over the years. The following quotation, taken from the lecture notes of a course taught in Königsberg in 1891, gives an idea of how he expressed his early conceptions:

> Geometry is the science that deals with the properties of space. It differs essentially from pure mathematical domains such as the theory of numbers, algebra, or the theory of functions. The results of the latter are obtained through pure thinking. . . .The situation is completely different in the case of geometry. I can never penetrate the properties of space by pure reflection, much as I can never recognize the basic laws of mechanics, the law of gravitation or any other physical law in this way. Space is not a product of my reflections. Rather, it is given to me through the senses. (Quoted in Toepell 1986, 21)[3]

The borderline between those disciplines whose truths can be obtained through pure thinking and those that arise from the senses sometimes shifted in Hilbert's thinking: arithmetic, for instance, is found in his writings on different sides of this borderline at different times. But geometry invariably appears as an *empirical* science (Hilbert sometimes even says "experimental"), similar in essence to mechanics, optics, etc. The kind of differences that Hilbert used to stress between the latter and geometry concerned their historical stage of development rather than their essence. As he wrote in 1894:

> Among the appearances or facts of experience manifest to us in the observation of nature, there is a peculiar type — namely, those facts concerning the outer shape of things. Geometry deals with these facts. . . . Geometry is a science whose essentials are developed to such a degree that all its facts can already be logically deduced from earlier ones. Much different is the case with the theory of electricity or with optics, in which many new facts are still being discovered. Nevertheless, with regard to its origins, geometry is a natural science. (Quoted from lecture notes of 1894, reproduced in Toepell 1986, 58)[4]

In other words, eventually, when the other physical sciences have attained the same degree of historical development as geometry has already attained, then there will be no appreciable differences between them, and the kind of axiomatic analysis that one applies now to geometry will be equally useful for studying the other physical sciences.

[3] Similar testimonies can be found in many other of Hilbert's lectures. Cf. e.g. Toepell 1986, 58.
[4] Unless otherwise noted, all translations into English are mine (L.C.).

But what is then the meaning of applying a process of axiomatization to geometry, one may ask, if this science is in essence an empirical one? The aim of the axiomatic analysis that Hilbert presented in the *Grundlagen* — unlike that of the formalistic conception of axiomatization — was to elucidate the logical structure of a given discipline so that it will become clear what theorems follow from what assumptions, which assumptions are independent of which, and what assumptions are needed in order to derive the *whole* body of knowledge in that discipline as we know it at a given stage of its development. In fact, Hilbert's excitement about axiomatization was sparked by his discovery that a classical technical problem in geometry could now be overcome — namely, that in order to reconstruct plane geometry one does not need infinitesimals, whereas in space geometry one does (see Rowe forthcoming).

This way of conceiving of the role of axiomatic analysis helps reading Hilbert's early works on axiomatization from a perspective that is basically different from the traditionally accepted one. The question of the consistency of the various kinds of geometries, for instance — which from the point of view of Hilbert's later metamathematical research and the developments that followed it might be considered the most important one undertaken in the *Grundlagen* — was not even explicitly mentioned in the introduction to that book. Hilbert discussed the consistency of the axioms in barely two pages, and from the content of those pages it is not immediately obvious why he addressed this question at all. In 1899 Hilbert did not seem to have envisaged the possibility that the body of theorems traditionally associated with Euclidean geometry might contain contradictions, since this was a *natural* science whose subject matter is the properties of physical space. Hilbert seems rather to have been echoing here an idea originally formulated in Hertz's book, according to which the axiomatic analysis of physical theories will help to clear away possible contradictions brought about over time by the gradual addition of new hypotheses to a specific scientific theory (Hertz [1894] 1956, 10). Although this was not likely to be the case for the well-established discipline of geometry, it might still happen that the particular way in which the *axioms* had been formulated in order to account for the theorems of this science led to statements that contradict each other. The recent development of non-Euclidean geometries made this possibility only more patent. Thus Hilbert believed that in the framework of his system of axioms for geometry he could also easily show that no such contradictory statements would appear.

Hilbert established through the *Grundlagen* the relative consistency of geometry vis-à-vis arithmetic, i.e., he proved that any contradiction existing in Euclidean geometry must manifest itself in the arithmetical system of real numbers. He did this by defining a hierarchy of fields of algebraic numbers. But in the first edition of the *Grundlagen* Hilbert contented himself with constructing a model that satisfied all the axioms, using only a proper subfield rather than the whole field of real numbers (Hilbert 1899, 21). It was only in the second edition of the *Grundlagen,* published in 1903, that he added an additional axiom, the so-called "axiom of completeness" (*Vollständigkeitsaxiom*); the latter was meant to ensure that although infinitely many incomplete models satisfy all the other axioms, there is only one complete model that satisfies this last axiom as well — namely, the usual Cartesian geometry, obtained when the whole field of real numbers is used in the model (Hilbert 1903, 22–24).[5]

The question of the consistency of geometry was thus reduced to that of the consistency of arithmetic, but the further necessary step of proving the latter was not even mentioned in the *Grundlagen*. It is likely that at this early stage, Hilbert did not yet consider that such a proof could involve a difficulty of principle. Soon, however, he would assign an increasingly high priority to it as an open problem of mathematics.[6] Thus among the famous list of twenty-three problems proposed by Hilbert in Paris in 1900, the second one concerns the proof of the "compatibility of arithmetical axioms." In formulating this problem, Hilbert articulated his views on the relations between axiomatic systems and mathematical truth, and he thus wrote:

> When we are engaged in investigating the foundations of a science, we must set up a system of axioms which contains an exact and complete description of the relations subsisting between the elementary ideas of the science. The axioms so set up are at the same time the definitions of those elementary ideas, and no statement within the realm of the science whose foundation we are testing is held to be correct unless it can be derived from those axioms by means of a finite number of logical steps. (Hilbert 1902, 447)

Views such as this were at the basis of the famous debate that arose between Hilbert and Frege immediately after the publication of the *Grundlagen* (Gabriel et al. 1980). Frege strongly disputed Hilbert's novel idea, according to which logical consistency implied mathematical existence and truth; for Frege, the axioms were necessarily consistent *because* they were true (see Boos 1985). For Hilbert, on the other hand, the freedom implied by the possibility of creating new mathematical worlds based on consistent axiomatic systems was enormously appealing — if not for anything else, for the potential support it seemed to lend to a wholehearted adoption of Georg Cantor's conceptions of the infinite. Moreover, this view endorsed the legitimacy of proofs of existence by contradiction and thus, a posteriori, one of Hilbert's early mathematical breakthroughs — namely, his proof of the finite-basis theorem in the theory of algebraic invariants, which had

[5] The axiom is formulated in Hilbert 1903, 16. Toepell 1986, 254–56, briefly describes the relationship between Hilbert's *Vollständigkeit* axiom and related works of other mathematicians. The axiom underwent several changes in the various later editions of the *Grundlagen*, but it remained central to this part of the argument. Cf. Peckhaus 1990, 29–35. The role of this particular axiom within Hilbert's axiomatics and its importance for later developments in mathematical logic is discussed in Moore 1987, 109–22. In 1904 Oswald Veblen introduced the term "categorical" (Veblen 1904, 346) to denote a system to which no irredundant axioms may be added. He believed that Hilbert had checked this property in his own system of axioms. See Scanlan 1991, 994.

[6] As is well known, Kurt Gödel proved in 1931 that such a proof is impossible in the framework of arithmetic itself.

initially encountered serious dissent by mathematicians of older generations (see Corry 1996, 138–47).

Still, it would be misleading to believe that the mathematical freedom pursued by Hilbert implied a conception of mathematics as a discipline dealing with arbitrarily formulated axiomatic systems devoid of any intuitive, direct meaning. The analysis that Hilbert applied to the axioms of geometry in the *Grundlagen* was based on demanding four properties that need to be met by that system of axioms: completeness,[7] consistency, independence, and simplicity. It is true that in principle, there is no reason why a similar analysis could not be applied to any other axiomatic system, and in particular to an arbitrarily given system of postulates that establishes mutual abstract relations among undefined elements arbitrarily chosen in advance and having no concrete mathematical meaning. But *in actuality*, Hilbert's own conception of axiomatics did not convey or encourage the formulation of abstract axiomatic systems as such: instead, his work was directly motivated by the need for better understanding of existing mathematical and scientific theories. In Hilbert's view, the definition of systems of abstract axioms and the kind of axiomatic analysis described above was meant to be carried out, retrospectively, for "concrete," *well-established and elaborated* mathematical entities. In this context, one should note that in the years immediately following the publication of the *Grundlagen* several mathematicians, especially in the United States, undertook an analysis of the systems of abstract postulates for algebraic concepts such as groups, fields, Boolean algebras, etc., based on the application of techniques and conceptions similar to those developed by Hilbert in his study of the foundations of geometry (for instance Moore 1902, Huntington 1902). These kinds of systems provided an archetype on which the Bourbaki group eventually modeled the basic definitions of the mathematical structures that in their view constitute the heart of the various mathematical disciplines. Thus this is one of the points at which Bourbaki saw their work as a direct continuation of Hilbert's intellectual legacy. However, we have no direct evidence that Hilbert showed any interest in the work of the American postulationalists, or any similar undertakings, and in fact there are many reasons to believe that such works implied a direction of research that Hilbert did not contemplate when putting forward his axiomatic program. It seems safe to assert that Hilbert even thought of this direction of research as mathematically ill conceived.[8]

Hilbert's actual conception of the essence of the axiomatic method is lucidly condensed in the following passage, taken from a 1905 course devoted to exposing the principles of the method and its actual application to diverse mathematical and

[7] It is necessary to remark that the term "completeness" used by Hilbert has a different meaning from the one later assigned to this term in the context of model theory. Hilbert's idea of completeness of an axiomatic system was derived from Hertz's "correctness" of scientific images, and it meant simply that all the known facts of the theory in question might be derived from the given system of axioms. See Corry 1997a.

[8] On the American postulationalists and Hilbert's response (or lack of it) to their works, see Corry 1996, § 3.5.

scientific domains:

> The edifice of science is not raised like a dwelling, in which the foundations are first firmly laid and only then one proceeds to construct and to enlarge the rooms. Science prefers to secure as soon as possible comfortable spaces to wander around and only subsequently, when signs appear here and there that the loose foundations are not able to sustain the expansion of the rooms, it sets to support and fortify them. This is not a weakness, but rather the right and healthy path of development. (Quoted in Corry 1996, 162)

After the publication of the *Grundlagen,* Hilbert continued to work on the foundation of geometry for the next two or three years, but he soon switched to his next focus of inquiry: the theory of integral equations. Some of his collaborators in Göttingen, however, continued to explore the application of the axiomatic method to many domains. For instance, Ernst Zermelo studied in detail the axiomatic foundation of set theory, while Hermann Minkowski discussed the application of the axiomatic analysis to the latest developments in the electrodynamics of moving bodies. Hilbert followed all these developments closely, and to a certain extent actively participated in them (Corry forthcoming).

Over time, the issue of consistency became increasingly central to the axiomatic analysis as conceived by Hilbert, especially given the increasing centrality of this question to the foundations of arithmetic. Eventually, the requirements of completeness and simplicity of axiomatic systems were paid no more attention, and only independence and consistency of the axioms mattered. Simultaneously, the connection between the axiomatic analysis and the foundational aspects of mathematics attained more prominence in Hilbert's thought. Hilbert continued to relate to the axioms of a given theory as historically determined and subject to change, but at the same time he also developed a differentiation between at least two kinds of axioms. This idea was exposed in a now famous lecture held in Zurich in 1917, in which Hilbert explained the essentials of the axiomatic method as he then conceived of it.

Hilbert opened his Zurich lecture by presenting again the idea that every elaborated scientific and mathematical theory can be reorganized in such a way that its whole body of propositions is derived from a very limited number of them — the axioms of that theory. Hilbert mentioned many different kinds of examples of this situation, among them: the parallelogram law as a basic axiom of statics; the law of entropy as a basis for thermodynamics; Kirchhoff's laws of emission and absorption for the theory of radiation; Gauss's error law as the basic axiom of the calculus of probabilities; the theorem establishing the existence of roots as a basis for the theory of polynomial equations; and — especially interesting for the present discussion — the Riemann conjecture concerning the purely real character and the frequency of the roots of the function $\varsigma(s)$ as the "foundational law" of the theory of prime numbers (I will return to this example below). All these examples, by the way, had already been mentioned by Hilbert on many earlier occasions, and he had shown in a more or less detailed fashion how the derivation of the whole discipline can be realized. The axiomatic derivation of the theory of radiation from Kirchhoff's law, for instance, constituted an original and important contribution of Hilbert, whose publication attracted much attention — though not always of a favorable kind (Corry forthcoming).

All these examples, Hilbert explained, illustrate *provisory* solutions to foundational questions concerning each of the mentioned theories. Very often in science, however, the need arises to clarify whether these axioms can themselves be expressed in terms of more basic propositions belonging to a deeper layer. It has been the case that "proofs" have been advanced of the validity of some of the axioms of the first kind mentioned above: the linearity of the equations of the plane, the laws of arithmetic, the parallelogram law for force addition, the law of entropy and the theorem of the existence of roots of an algebraic equation. Hilbert discussed this situation as follows:

> [The] critical test for these "proofs" is manifest in the fact that although they are not themselves proofs, at bottom they enable the reduction to deeper-lying propositions which from then on have to be considered as new axioms instead of as the original axioms we intended to prove. Thus emerge what are today properly called *axioms* of geometry, of arithmetic, of statics, of mechanics, of the theory of radiation, or of thermodynamics. . . . The operation of the axiomatic method, as it has been described here, is thus tantamount to a deepening of the foundations of the individual scientific disciplines, very similar to that which eventually becomes necessary when an edifice is enlarged and built higher, and we then want to ensure its safety. (Hilbert 1918, 148; italics in the original)

Hilbert thus stuck to the edifice metaphor as an explanation of the role of the axiomatic method in science; but at the same time he laid some stress on the more basic role that certain axioms play from the point of view of foundations. The differentiation suggested here by Hilbert did not explicitly appear in many other places among his writings. It seems to me, however, that the ambiguous attitude inherent in this passage implicitly comes to the fore in many such instances, giving rise to diverging interpretations of Hilbert's views according to which of the two aspects, the empirical or the formal, is more strongly stressed.

How are these issues related to the question of eternal truths in mathematics? In the first place, it has already been made clear that Hilbert's interest in the axiomatic method was closely connected with his awareness of the constant changes that scientific theories undergo in the course of their historical development. This applies to physical as well as to mathematical disciplines. For Hilbert, one of the aims of the axiomatic analysis of theories was the possibility of analyzing whether the adoption of a new hypothesis into existing theories would lead to contradiction with the existing body of knowledge, a situation that in his view had been very frequent in the history of science. Hilbert thought that the axiomatic analysis of theories could help to minimize the appearance of difficulties in the logical structure of theories, but certainly could not avoid them completely. Still, the question arises how Hilbert thought that open questions at the level of the images of knowledge should be settled, and whether the axiomatic method would play any role in this, as Bourbaki was later to believe. The answer is that Hilbert was somewhat ambiguous about it.

Aware of the power of reflexive mathematical reasoning, Hilbert obviously thought that some metaquestions about mathematics could be solved *within* the body of mathematical knowledge, definitely endorsing the answers by means of standard mathematical proofs. The formalistic program for the foundations of mathematics, in which he was involved in the 1920s, was based precisely on transforming the very idea of a mathematical proof into an entity susceptible to mathematical study in itself. Occasionally, Hilbert also suggested that additional meta-questions could be solved with the help of axiomatic analysis. In his 1917 lecture on axiomatic thinking, Hilbert explained that a solid foundation for the whole of mathematics would be attained if logic could be properly axiomatized in terms of a consistent system of abstract postulates.[9] But he mentioned additional issues, as being closely related to the latter task:

> On closer reflection we soon recognize that the question of consistency is not an isolated one concerning the integer numbers and the theory of sets alone, but rather that it is part of a larger domain of very difficult, epistemological, questions of a specific mathematical hue: If in order to characterize this domain of questions briefly, I mention the problem of the *solvability in principle of every mathematical question,* the problem of the retrospective *controllability* of the results of any mathematical investigation, then I must also consider the question of a *criterion for the simplicity* of a mathematical proof, the question of the relation between *contents and formalism* in mathematics and in logic, and finally, the problem of the *decidability* of a mathematical question in a finite number of steps. (Ibid., 153; italics in the original)

As a matter of historical fact, Hilbert did not himself deal with all these problems in the way formulated here. Only the last of them, the *Entscheidungsproblem,* subsequently became the basis of an actual, fruitful research program. But the specific point I want to make here is that in instances of this kind, Hilbert did discuss the possibility of solving metamathematical issues inside the body of mathematical knowledge and, more specifically, with the help of the axiomatic

[9] It is interesting to note that Hilbert's foundational reductionism was expressed in this article in purely logicistic, rather than formalistic, terms. Hilbert mentioned the efforts of Frege and Russell in this direction and stated that in the eventual completion of Russell's program for axiomatizing logic one could recognize the highest achievement of axiomatization in general ("In der Vollendung dieses grosszügigen Russellschen Unternehmens der *Axiomatisierung der Logik* könnte man die Krönung des Werkes der Axiomatisierung überhaupt erblicken"). See Hilbert 1918, 153.

method. He was however aware of the limitations of this approach to solving such issues, and on many occasions he also stressed the contextual, historical, or sociological factors that affected the actual answers to such questions as the relative importance of mathematical theories or the appropriate way to organize mathematical knowledge. In the opening lecture of a course on the foundations of physics, taught in Göttingen in 1917, Hilbert very clearly expressed this position, while discussing the interrelation of physics and geometry in the aftermath of the development of general relativity. In a passage that brings to the fore once again his empiricist view of geometry at a relatively later stage of his career, he stated:

> In the past, physics adopted the conclusions of geometry without further ado. This was justified insofar as not only the rough, but also the finest physical facts confirmed those conclusions. This was also the case when Gauss measured the sum of angles in a triangle and found that it equals two right ones. That is no longer the case for the new physics. *Modern physics must draw geometry into the realm of its investigations.* This is logical and natural: every science grows like a tree, of which not only the branches continually expand, but also the roots penetrate deeper.
>
> Some decades ago one could observe a similar development in mathematics. According to Weierstrass a theorem was considered to have been proved if it could be reduced to relations among integer numbers, whose laws were assumed to be given. Any further dealings with the latter were laid aside and entrusted to the philosophers. Kronecker said once: "The good Lord created the integer numbers." These were at that time a touch-me-not (*noli me tangere*) of mathematics. That was the case until the logical foundations of this science began to stagger. The integer numbers then turned into one of the most fruitful research domains of mathematics, and especially of set theory (Dedekind). The mathematician was thus compelled to become a philosopher, for otherwise he ceased to be a mathematician.
>
> The same happens now: *the physicist must become a geometer,* for otherwise he runs the risk of ceasing to be a physicist and vice versa. The separation of the sciences into professions and faculties is an anthropological one, and it is thus foreign to reality as such. For a natural phenomenon does not ask about itself whether it is the business of a mathematician or of a physicist. On these grounds we should not be allowed simply to accept the axioms of geometry. The latter may be the expression of certain facts of experience that further experiments would contradict. (Hilbert 1916–17, 2–3; emphasis in the original)[10]

[10] "Früher übernahm die Physik die Lehren der Geometrie ohne weiteres. Dies war berechtigt, solange nicht nur die groben, sondern auch die feinsten physikalischen Tatsachen die Lehren der Geometrie bestätigen. Dies war noch der Fall, als Gauss die Winkelsumme im Dreieck experimentell mass und fand, dass sie zwei Rechte beträgt. Dies gilt aber nicht mehr von der neuesten Physik. *Die heutige Physik muss vielmehr die Geometrie mit in den Bereich ihrer Untersuchungen ziehen.* Das ist

The 1920s are the years of Hilbert's more intense involvement with the questions of foundations. But even in this period there is plenty of evidence that his basic views concerning the place of uncertainty in mathematics did not change in any essential way. In 1919–20, Hilbert gave a series of popular lectures in Göttingen under the general title "Nature and Mathematical Knowledge." In these lectures he sharply criticized accepted views of mathematics and physics. He explicitly discarded the view that mathematics can be reduced to a formal game played with meaningless symbols according to rules established in advance, while stressing the role of intuition and experience as a source of mathematics. He also discussed the place of conjectural thinking and the fallibility of mathematical reasoning. Of particular relevance for the present discussion is the connection that Hilbert established between the axiomatic method and conjectural thinking in mathematics.

It is clear that any axiomatically developed theory has a hypothetical character, in the sense that the conclusions of the theory are valid whenever the validity of the axioms is assumed. For instance, in every mathematical situation in which the conditions of the elementary axioms of the theory of rings are satisfied, the theory provides certain theorems of unique factorization that are valid in that situation. This is the way in which Bourbaki, as we will see, presented the essence of the axiomatic method; and clearly this description applies to a large extent to Hilbert's view as well. But in these lectures of 1919–20 we also find a broader conception of the application of the method as Hilbert conceived of it: the possibility of incorporating into the body of mathematical knowledge theories that are based on unproved, though perhaps plausible, theorems of significant content.

Hilbert had in mind, in this case, the particular example of the already mentioned Riemann conjecture. From the point of view of the calculus of probability, this conjecture certainly appears, a priori, as a rather implausible one, since it demands that the zeros of a certain function will all lie within a very limited area. Still, what we know about mathematics and, in particular, our knowledge of the fruitful results that seem to follow from this conjecture lead us to assign it a high possibility

logisch und naturgemäss: jede Wissenschaft wächst wie ein Baum, nicht nur die Zweige greifen weiter aus, sondern auch die Wurzeln dringen tiefer.

"Vor einigen Jahrzehnten konnte man in der Mathematik eine analoge Entwicklung verfolgen; einen Satz hielt man damals nach Weierstrass dann für bewiesen, wenn er auf Beziehungen zwischen ganzen Zahlen zurückführbar war, deren Gesetz man als gegeben hinnahm. Sich mit diesen zu beschäftigen, wurde abgelehnt und den Philosophen überlassen. Kronecker sagte einmal: 'Die ganzen Zahlen hat der liebe Gott geschaffen.' Diese waren damals noch einen *noli me tangere* der Mathematik. Das ging so fort, bis die logischen Fundamente dieser Wissenschaft selbst zu wanken begannen. Nun wurden die ganzen Zahlen eines der fruchtbarsten Arbeitsfelder der Mathematik uns speziell der Mengenlehre (Dedekind). Der Mathematiker wurde also gezwungen, Philosoph zu werden, weil er sonst aufhörte, Mathematiker zu sein.

"So ist es auch jetzt wieder: *der Physiker muss Geometer werden*, weil sonst Gefahr läuft, aufzuhören, Physiker zu sein und umgekehrt. Die Trennung der Wissenschaften in Fächer und Fakultäten ist eben etwas Antropologisches, und der Wirklichkeit Fremdes; denn eine Naturerscheinung frägt nicht danach, ob sie es mit einem Physiker oder mit einem Mathematiker zu tun hat. Aus diesem Grunde dürfen wir die Axiome der Geometrie nicht übernehmen. Darin könnten ja Erfahrungen zum Ausdruck kommen, die den ferneren Experimenten widersprächen."

of being true. Moreover, Hilbert saw it as legitimate to build a full mathematical theory based on the assumption of the validity of this conjecture, but only insofar as a correct application of the axiomatic method would help us keep track of the limitations of such a theory. In these lectures Hilbert repeated many of the ideas expressed in Zurich in 1917, in particular the idea of two different layers of axioms bearing a different foundational character. Echoing this distinction, Hilbert explained the possible use that can be made of conjectures such as Riemann's:

> In discussing the method of mathematics, I have already stressed that when building a particular theory, it is a fully justified procedure to assume still unproved, but plausible, theorems (as axioms), provided one is clear about the incomplete character of this way of laying the foundations of the theory. (Hilbert [1919–20] 1992, 78)

In 1922–23 Hilbert gave another series of popular lectures in Göttingen. Among other topics he discussed the place of error in the history of mathematics, declaring plainly that errors had played a significant role in the development of this science. The passage where he explains his view presents an image of how progress is attained in mathematics — which, again, is totally opposed to the one Bourbaki tried so hard to put forward more than two decades later:

> Every time that a new, fruitful method is invented in order to solve a problem, in order to expand our knowledge, or in order to conquer new provinces of science, there are, on the one hand, critical researchers who distrust the novelty and, on the other hand, the courageous ones, who before all others deplete the inexhaustible and productive source, swiftly achieve innovation, and soon even gain overweight of it, so that they can silence the objections of the critics. This is the period of the swift advancement of science. Often the best pioneers are those who dare to advance deeper and are the first to arrive at unsafe territory. Signs of the latter are unclearness and uncertainty in the results obtained, to the point that even visible contradictions and countersenses — the so-called paradoxes — arise. At this moment the critical tendencies, which until now have stood aside, reappear on the stage. They take possession of the paradoxes, uncover real mistakes, and thus attempt to incriminate the whole method and to reject it. The danger exists that all the progress achieved will be lost. The main task in such a situation is to hold back (*einzudämmen*) this criticism and seek a reformulation of the foundations of the method so that it remains safe from all its false applications and, at the same time, that the ordinary results of the established portions of mathematical knowledge can be incorporated into it. (Hilbert [1922–23] 1988, 38–39)

Earlier, Hilbert had himself played the role of a courageous pioneer in the framework of his early work on algebraic invariants. In 1888 he had proved the existence of a finite basis for every system of invariants, using new methods that were at first harshly criticized by more conservative mathematicians. In 1922 he was clearly referring to his current concern with the foundations of mathematics. From very early on Hilbert had defended the new conception of the infinite implied by Cantor's work on sets, and his formalist program was conceived as a way of countering the criticism of those who thought that the acceptance of the actual infinite in mathematics was damaging, as the appearance of paradoxes suggested. But in any case, Hilbert's open-minded attitude toward alternative and innovative views in mathematics highly contrasts with that of Bourbaki, as will be described below. And again, Hilbert did not believe that the axiomatic conception of mathematical theories would totally safeguard against error.

Hilbert's conception of the axiomatic method and of its role in science, then, contained various and somewhat diverging elements. Small wonder that different mathematicians at different times derived different ideas from it. Clearly, Bourbaki's conceptions elaborate on some of these elements but leave aside others. Yet even among Hilbert's students and colleagues, and still during his lifetime, one can observe how these various elements are differently stressed.

On the occasion of Hilbert's sixtieth birthday, the journal *Die Naturwissenschaften* dedicated one of its issues to celebrating the achievements of the master. Several of his students were commissioned to write articles summarizing Hilbert's contributions in different fields. Max Born, who as a young student in Göttingen had attended many of Hilbert's courses and later on as a colleague continued to participate in his seminars, wrote about Hilbert's physics. Born was perhaps the physicist who expressed the most sustained enthusiasm for Hilbert's physics. He also seems truly to have appreciated the exact nature of Hilbert's program for axiomatizing physical theories and the potential contribution that realization of that program could yield. His description of the essence of this program stressed its empiricist underpinnings and at the same time attempted to explain why, in general, physicists tended not to appreciate it. Curiously, he directly addressed the issue of the relationship between the modern axiomatic method and eternal mathematical truth. In Born's words:

> The physicist sets out to explore how things are in nature; experiment and theory are thus for him only a means to attain an aim. Conscious of the infinite complexities of the phenomena with which he is confronted in every experiment, he resists the idea of considering a theory as something definitive. He therefore abhors the word "axiom," which in its usual usage evokes the idea of definitive truth. The physicist is thus acting in accordance with his healthy instinct, that dogmatism is the worst enemy of natural science. The mathematician, in contrast, deals not with factual phenomena but with logic interrelations. In Hilbert's language, the axiomatic treatment of a discipline in no sense implies a definitive formulation of specific axioms as eternal truths but rather the following methodological demand: specify the assumptions at the beginning of your deliberation, stop for a moment and investigate

> whether or not these assumptions are partly superfluous or contradict each other. (Born 1922, 591)

In Born's view, then, Hilbert's axiomatic approach is not applied in order to attain eternal truth, as Bourbaki later implied, but rather to enable a clearer understanding of the nature of our provisory conceptions, and to provide the means to correct errors that might arise in them. But in the same issue of that journal dedicated to Hilbert a somewhat different (although not contradictory) assessment of his work appears, in an article by Paul Bernays on "Hilbert's Significance for the Philosophy of Mathematics." Bernays was at that time Hilbert's closest assistant, and together they were dedicating most of their current efforts to foundational questions — in particular, to lay the basis for the realization of the so-called "Hilbert Program." Clearly, when presenting Hilbert's ideas Bernays stressed mainly those connected with his foundational concerns. Bernays explained the essence of Hilbert's axiomatic conception and claimed that the main task of his analysis was to prove the consistency of the theories involved. This was certainly true of Hilbert's current concerns, but it was much less true of his early investigations of the foundations of geometry and of physics. From the vantage point of later developments, Bernays saw this as a constant point of major interest for Hilbert. After explaining the motivation for Hilbert's current interest in investigating the nature of mathematical proofs, Bernays explained the philosophical meaning of the master's entire endeavor in the following terms:

> While clarifying the workings of mathematical logic, Hilbert transformed the meaning of this method [of the logical calculus] in a way very similar to that earlier applied to the axiomatic method. Very much as he had once stripped the visualizable (*Anschaulich*) content out of the basic relations and out of the axioms of geometry, so he now detached the mental content of deductions from the proofs of arithmetic and analysis, which he had made the subject matter of his investigations. He did so by taking as his immediate object of consideration the systems of formulae through which such proofs are represented in the logical calculus, cut off from any logical-contextual interpretation. In this way he could substitute the methods of proof used in the analysis of purely formal transactions, which are performed on determinate signs according to fixed rules.
>
> By means of this approach, in which the separation of what is specifically mathematical from anything that has to do with content reaches its peak, the Hilbertian conception of the essence of mathematics and of the axiomatic method attains its true realization for the first time. For we recognize from now on that the sphere of the mathematical-abstract, into which the mathematical way of thinking translates all that is theoretically conceivable, is not the sphere of that which has logical content; rather it is the domain of pure formalism. Mathematics thus appears as the general theory of formal systems (*Formalismen*), and since we are able to conceive of it in that way, the

> universal meaning of this science also becomes at once clear. (Bernays 1922, 98)

As will be seen below, Bernays's characterization of Hilbert's axiomatic method is very close to that accorded to Hilbert by Bourbaki and also to that put forward in Bourbaki's mathematics. This is also the characterization that has come to be most closely associated with Hilbert's name. But this is only one aspect of Hilbert's much more complex conception, and excessive stress on it runs the risk of leading to misinterpretation; this is particularly the case when it comes to the link between the axiomatic method and eternal truth in mathematics.

3. Bourbaki

I will now proceed to discuss the work of Nicolas Bourbaki and the conception of the status of mathematical truth put forward in this work. Nicolas Bourbaki is the pseudonym adopted in the mid-1930s by a group of young French mathematicians who undertook the task of collectively writing an up-to-date treatise of mathematical analysis, suitable both as a textbook for students and as a source of reference for researchers. The founding members of the group were initially motivated by an increasing dissatisfaction with the texts of mathematical analysis currently used in their country, and by a feeling that French mathematical research was lagging far behind that of other countries, especially Germany. The project materialized in a way that the members of the group had perhaps not quite anticipated, and the published treatise — which covers many fields of modern mathematics, not just analysis — became one of the most influential texts of twentieth-century mathematics: the *Eléments de Mathématique*. Among the many ways that Bourbaki's influence was felt, most relevant for the present discussion is the entrenchment and broadening of the idea that mathematics deals with eternal truths.

The name Bourbaki has been associated more than any other with a very influential and pervasive image of twentieth-century mathematics — namely, the idea that mathematics is a science of "structures."[11] Volume 1 of the *Eléments* deals with the theory of sets, and its fourth chapter introduces and discusses the concept of *structure*.[12] Allegedly, this formally defined mathematical concept is meant to provide the solid foundation on which the whole picture of mathematics put forward by the treatise will be built. This is a unified and on the face of it very coherent picture, in which mathematics is seen as a hierarchy of structures of increasing complexity. According to Bourbaki's image, the aim of mathematical research is to elucidate the essence of each of these structures.

[11] On Bourbaki and the place of the idea of mathematical structures in his work, see Corry 1996, chap. 7. Readers interested in additional historical details and specific references to issues discussed in this section will find them in that same chapter.

[12] To avoid ambiguity I use italics to denote Bourbaki's technical term *structures* (as defined in volume 1 of the *Eléments*). The term without italics denotes all other nonformally defined meanings of the term.

However, the actual place of the idea of structure in Bourbaki's mathematics is much more complex than this simple — and very often uncritically accepted — account seems to imply. The term "structure" is used in Bourbaki's texts, rather indiscriminately, with two different meanings. On the one hand there is the above-mentioned formal concept of *structure*; on the other hand there is a more general, undefined, and nonformal idea of what a "mathematical structure" is. Bourbaki's theory of *structures* is hardly used in developing the theories that Bourbaki included in the treatise, and where it does appear it can absolutely be dispensed with. Outside Bourbaki's treatise, moreover, *structures* were scarcely ever mentioned by mathematicians. On the contrary, the structural conception of mathematics, understood as a nonformally conceived image of mathematical knowledge, proved extremely fruitful for Bourbaki's own work, and at the same time exerted a profound influence on generations of mathematicians all around the world.[13]

The roots of both senses of the term "structure," and the details regarding the way in which they were used by Bourbaki and their followers are not relevant to the present article. Neither will we consider the extent of the group's influence and how it contributed to shaping the development of mathematics over several decades of the present century. The focus of attention will be on the interrelation between structures, axiomatic thinking, and eternal truth in Bourbaki's conception of mathematics, and on the alleged influence of Hilbert's ideas on these issues. The main claim I want to stress here is that Bourbaki's introduction of the concept of structure can be explained in terms of the group's images of mathematics: Bourbaki's account of mathematics in terms of "structures" was an attempt to extend the idea that mathematics produces eternal truths from the domain of the body of mathematics (in which it is commonly accepted) to include also the images of mathematics. I will then discuss Bourbaki's images of mathematics and the way the idea of "structure," as well as the theory of *structures*, are connected with them.

Although such terms as "Bourbaki's philosophy of mathematics" or "Bourbaki's structuralist program for mathematics" are very frequently used, one rarely finds a detailed account of what these terms mean. Morris Kline, for instance, described this program as follows:

> Nicolas Bourbaki undertook in 1936 to demonstrate in great detail what most mathematicians believed must be true, namely, that if one accepts the Zermelo-Fraenkel axioms of set-theory, in particular Bernay's and Gödel's modification, and some principles of logic, one can build up all of mathematics on it. (Kline 1980, 256)

Isolated passages such as this one abound, but they are both inaccurate and very partial as a description of what Bourbaki's undertaking amounted to. Some more

[13] This point is discussed in detail in Corry 1996, chap. 7.

articulate attempts to explain it (e.g., Fang 1970; Stegmüller 1979) do not throw much light on the issue either.

As a matter of fact, a consistent and systematic program, clearly formulated and generally accepted by all members of the group, was never at the basis of Bourbaki's work. As I noted earlier, the group's work began as an attempt to write a new treatise on analysis, and it was only in the process of writing that its scope was broadened to include many other fields of mathematics. Also, the austere axiomatic style that characterizes this work was adopted only progressively. Moreover the very idea of attributing such centrality to the notion of "structure" for their presentation of mathematics arose relatively late, it was never fully adopted by all members as a leading principle, and in fact proved quite problematic. Very much like Hilbert before them, the members of the group saw themselves above all as "working mathematicians" focused on such activities as problem solving, research, and the exposition of theorems and theories. Their interest in philosophical or foundational issues was only oblique, and certainly not systematic. Bourbaki never formulated an explicit philosophy of mathematics and, in retrospect, individual members of the group even denied any interest whatever in philosophy or in foundational research of any kind.

Nevertheless, it is possible to reconstruct what in retrospect can be defined as Bourbaki's images of mathematics, as a way to understand the context of their mathematical activity. Obviously this system of images of knowledge is one that was subject to constant criticism (both external and internal), that evolved over the years, and that occasionally involved ideas that were in opposition to the actual work whose setting they were meant to provide. This can be said of the images of knowledge of every individual scientist; but in the case of Bourbaki, a group that comprised a number of various leading mathematicians with strong opinions about every possible issue, all these factors need to be more strongly stressed. One has to take into account that during their many years of activity members of the group very often professed conflicting beliefs at the level of the images of knowledge.

Taking all these necessary qualifications into account, Bourbaki's images of mathematics can be reconstructed by directly examining the mathematical work and the historical accounts of the development of mathematics published by the group, and by examining pronouncements not only of the group's members but of several other sources as well. Jean Dieudonné was no doubt the most outspoken member of the group. More than anyone else, he was responsible for producing and spreading the popular conception of what Bourbaki's mathematics are. The views of the majority of the group's members — in particular those concerning the structural conception of mathematics and the role of the concept of structure in the work of Bourbaki — have been much less documented or not documented at all.

Bourbaki began their work amidst a multitude of newly obtained results, some of them belonging to branches of mathematics that at the time were only incipient. The early years of Bourbaki's activity witnessed a boom of unprecedented scope in mathematical research. In 1948 Dieudonné published (under the name of Bourbaki) a now famous article that was later translated into several languages and that has come to be considered the group's programmatic manifesto: "The Architecture of Mathematics." According to the picture of mathematics presented in that article, the boom in mathematical research at the time of its writing raised the pressing question of whether it could still be legitimate to talk about a single discipline called "mathematics" or "whether the domain of mathematics is not becoming a tower of Babel, in which autonomous disciplines are being more and more widely separated from one another, not only in their aims, but also in their methods and even in their language" (Bourbaki 1950, 221). Dieudonné stressed the role of the axiomatic method as an underlying common basis for a unified view of mathematics, in face of its apparent disunity:

> Today, we believe however that the internal evolution of mathematical science has, in spite of appearance, brought about a closer unity among its different parts, so as to create something like a central nucleus that is more coherent than it has ever been. The essential part of this evolution has been the systematic study of the relations existing between different mathematical theories, and which has led to what is generally known as the "axiomatic method." . . . Where the superficial observer sees only two, or several, quite distinct theories, lending one another "unexpected support" through the intervention of mathematical genius, the axiomatic method teaches us to look for the deep-lying reasons for such a discovery. (Ibid., 222–23)

According to Dieudonné, then, the modern axiomatic method lies at the heart of mathematics, and it is precisely the use of this method that preserves its unity. I will call this idea "the axiomatic image of mathematics" — i.e., that mathematics is a science dealing with axiomatic systems.

It is interesting to note that in his Paris address of 1900 Hilbert had already pointed up the possible danger of internal dismemberment of mathematics, given the current diversity among its subdisciplines. Hilbert expressed himself in terms similar to those later used by Dieudonné in 1948, which may be more than a simple coincidence:

> The question is urged upon us whether mathematics is doomed to the fate of those other sciences that have split up into separate branches, whose representatives scarcely understand one another and whose connections become ever more loose. I do not believe it nor wish it. Mathematical science is in my opinion an indivisible whole, an organism whose vitality is conditioned upon the connection of its parts. For with all the variety of mathematical knowledge, we are still clearly conscious of the similarity of the logical devices, the relationship of the ideas in mathematical theory and the numerous analogies in its different departments. We also notice that, the farther a mathematical theory is developed, the more harmoniously and uniformly

> does its construction proceed, and unsuspected relations are disclosed between hitherto separate branches of the science. (Hilbert 1902, 478–79)

But between Hilbert's 1900 address and Dieudonné's manifesto almost half a century had elapsed, and the problem of the (dis)unity of mathematics was more pressing than ever. At the same time, the modern axiomatic method had become a mainstream language of many mathematical branches, and in a certain sense, it had departed from Hilbert's initial conception. Still, what exactly the application of the modern axiomatic method amounts to, and what its meaning is for mathematics was not a straightforward issue even among Bourbaki members. Henri Cartan, one of the founding members of Bourbaki, defined it as follows:

> A mathematician setting out to construct a proof has in mind well defined mathematical objects which he is investigating at the moment. When he thinks he has found the proof, and begins to test carefully all his conclusions, he realizes that only a very few of the special properties of the objects under consideration played a role in the proof at all. He thus discovers that he can use the same proof for other objects which have only those properties he had employed previously. Here we can see the simple idea underlying the axiomatic method: instead of declaring which objects are to be investigated, one has to list those properties of the objects to be used in the investigation. These properties are then brought to the fore expressed by axioms; whereupon it ceases to be important to explain what these objects *are,* that are to be studied. Instead, the proof can be constructed in such a way as to hold true for every object that satisfies the axioms. It is quite remarkable how the systematic application of such a simple idea has shaken mathematics so completely. (Cartan [1958] 1980, 176–77; italics in the original)

This description of the essence of the axiomatic method is not only simple and clear; it is also very similar to the one intended by Hilbert when he first applied it to the foundations of geometry. However, influenced by later works of Hilbert, particularly his works on the foundations of logic and arithmetic, different aspects of the axiomatic method came to be stressed more strongly, and a somewhat different picture arose. This is the picture that relates Hilbert to the formalist approach of mathematics, of which Bourbaki became a leading promoter. Dieudonné, for instance, described Hilbert's conception by comparing it to a game of chess: one does not speak about truths but rather about following a set of stipulated rules correctly. If we transpose that idea into mathematics, wrote Dieudonné, we arrive at Hilbert's conception: mathematics becomes a game, in which the pieces are graphical signs, distinguished from one another by their form alone and not by their content (Dieudonné 1962, 551).

Dieudonné's rendering of Hilbert's position, though not absolutely faithful as a historical description, is the one that more faithfully describes the kind of mathematics that developed under the influence of Bourbaki's own textbooks. These books present the various domains discussed in them as defined by a list of apparently meaningless axioms; all the results are derived with reference only to these axioms, while explicitly excluding any kind of motivation or intuition. The trademark of these texts is that they exclude any external references (though there are many cross-references) as well as any reliance on figures, even in domains as strongly geometrically motivated as topology. The axiomatic image of mathematics more closely associated with the name of Bourbaki — and the one I will refer to here — is the image according to which mathematics is a series of formal theories, at the basis of which stand axioms without any specific, intuitive meaning, and the results of which are supported by formally constructed proofs without any appeal to external intuitions. As Bourbaki has been declared the "legitimate heir of Hilbert," this image of mathematics has, by implication, also been associated very often with Hilbert himself.

The axiomatic image of mathematics is, then, the first basis on which Bourbaki's conception of mathematics is built. Closely associated with it, but nonetheless different, is "the structural image of mathematics," according to which the object of mathematical research is the elucidation of the various structures that appear in it. The first full-fledged realization of this image was put forward in a classical textbook published in 1930 by the Dutch mathematician B. L. van der Waerden, under the title *Moderne Algebra.* Building on ideas he had learnt as a student of Emmy Noether and Emil Artin, van der Waerden presented the various branches of this mathematical domain under the leading notion that all of them are manifestations of a single, unifying idea — namely, that algebra is the discipline dealing with the study of the various algebraic structures.[14] Deeply impressed by van der Waerden's achievement in algebra, Bourbaki undertook to present much larger portions of mathematics in a similar way. Dieudonné described the unifying role of the structures as follows:

> Each structure carries with it its own language, freighted with special intuitive references derived from the theories from which the axiomatic analysis . . . has derived the structure. And, for the research worker who suddenly discovers this structure in the phenomena which he is studying, it is like a sudden modulation which orients at once the stroke in an unexpected direction in the intuitive course of his thought and which illumines with a new light the mathematical landscape in which he is moving about. . . . Mathematics has less than ever been reduced to a purely mechanical game of isolated formulas; more than ever does intuition dominate in the genesis of discoveries. But henceforth, it possesses the powerful tools furnished by the theory of the great types of structures; in a single view, it sweeps over immense domains, now unified by the axiomatic method, but which were formerly in a completely chaotic state. (Bourbaki 1950, 227–28)

[14] For more details see Corry 1996, chap. 1.

Thus Dieudonné attributed to the structures — and especially to "the theory of the great types of structures" — a central role in the unified picture of mathematics. This was an innovative idea, on the basis of which Bourbaki was able to exert a deep influence on future research.

Van der Waerden defined all the algebraic structures studied in his book in strictly axiomatic terms. Bourbaki did the same for the structures investigated in the various volumes of the *Eléments*. Still, it must be stressed that the axiomatic image and the structural image are different ideas; a significant manifestation of this is that a notion of mathematical structure, similar to that of van der Waerden or of Bourbaki, is absent from Hilbert's work, in spite of the centrality of the axiomatic image of mathematics in the latter. In fact, comparing the role played by the idea of structure in *Moderne Algebra*, on the one hand, and in Bourbaki and in Hilbert respectively, on the other hand, sheds additional light on the different roles that Hilbert and Bourbaki assigned to the place of axiomatically defined theories in their overall conception of mathematics.

Van der Waerden's is the first paradigmatic book in which a mathematical discipline, algebra, was presented as a science of structures. *Moderne Algebra* profoundly influenced the conceptions of the founding members of Bourbaki, and their whole endeavor may be considered, to a large extent, as an attempt to extend this view from the relatively limited scope of algebra alone to mathematics at large. Van der Waerden himself worked in Göttingen in 1927 with Emmy Noether, and her courses provided a main source of inspiration for his book. These circumstances might tend to support the view that there is a clear thread connecting Hilbert's basic conceptions of the nature of mathematics to those of Bourbaki. On closer examination, however, a different picture emerges.

Van der Waerden's book appeared around the time of Hilbert's retirement. We have no direct evidence of his opinion of the book and of the image of algebra presented there. We do know, however, that during his lifetime Hilbert never taught courses on the issues that constitute the hard core of van der Waerden's presentation of algebra. The abstract theory of rings, for instance, and its use of the abstract concept of ideal as a main tool for studying unique factorizations in the most general terms, were recent developments that Hilbert never used in its newer formulation. The abstract fields that van der Waerden used in his work were very different from the concrete fields of algebraic numbers that Hilbert had thoroughly analyzed in his own work. The theory of abstract groups was a relatively well elaborated mathematical domain by the turn of the century, but Hilbert never showed a specific interest in it, independently of its application to other, more classical domains of nineteenth-century mathematics. Moreover, among the sixty-eight dissertations that Hilbert supervised in his lifetime, none deals with these kinds of issues. Likewise, although five among the twenty-three problems that Hilbert included in his 1900 list can be considered in some sense as belonging to algebra in the nineteenth-century sense of the word, none of them deals with problems connected with more modern algebraic concerns, and in particular not with the theory of groups.

Hilbert's mathematical work surely implied important innovations at many levels, in both the body and the images of knowledge, but at the same time it had deep roots in the classical domains of nineteenth-century mathematics and in the views associated with them. One of these views concerned the foundational status of the various systems of numbers. Under the classical, nineteenth-century image of mathematics these systems lie at the heart of all mathematical knowledge and algebra is built on top of them. Hilbert contributed to the elaboration of many new tools for the study of algebra and of the systems of numbers, but he never changed the traditional conceptual hierarchy. In van der Waerden's structural image of algebra the hierarchy is totally reversed, and these tools — defined by means of abstract systems of postulates — transform into the basic mathematical entities. The basic systems of numbers (integers, rationals, reals) become particular cases of more general algebraic structures.

How did Hilbert react to this change in the conceptual order, in which the system of real numbers is dependent on the results of algebra rather than being the basis for it? From what we know about Hilbert's images of mathematics, it seems safe to conjecture that his attitude in this respect may have been ambiguous at best. An indication of what his attitude may have been appears in Hermann Weyl's obituary to Hilbert:

> Hilbert is the champion of axiomatics. The axiomatic attitude seemed to him one of universal significance, not only for mathematics, but for all sciences. His investigations in the field of physics are conceived in the axiomatic spirit. In his lectures he liked to illustrate the method by examples taken from biology, economics, and so on. The modern epistemological interpretation of science has been profoundly influenced by him. Sometimes when he praised the axiomatic method he seemed to imply that it was destined to obliterate completely the constructive or genetic method. I am certain that, at least in later life, this was not his true opinion. For whereas he deals with the primary mathematical objects by means of the axioms of his symbolic system, the formulas are constructed in the most explicit and finite manner. In recent times the axiomatic method has spread from the roots to all branches of the mathematical tree. Algebra, for one, is permeated from top to bottom by the axiomatic spirit. One may describe the role of axioms here as the subservient one of fixing the range of variables entering into the explicit constructions. But it would not be too difficult to retouch the picture so as to make the axioms appear as the masters. An impartial attitude will do justice to both sides; not a little of the attractiveness of modern mathematical research is due to the happy blending of axiomatic and genetic procedures. (Weyl 1944, 645)

The kind of impartial attitude promoted here by Weyl, and which probably was very close to Hilbert's original views, is not really the attitude that the images of mathematical knowledge put forward in Bourbaki's treatise make manifest.

Whereas in Bourbaki's structural image the axiomatic systems lie at the basis of mathematics and are the starting point for the development of theories, for Hilbert the axiomatic analysis is a relatively late stage in it. Hilbert's axiomatic analysis is part of an open-ended, flexible, and mainly empirically motivated process of knowledge creation in mathematics, rather than the origin and justification of a rigidly conceived and a priori determined course of evolution, that is realized by means of logical deduction alone.

Bourbaki, then, attempted to extend the structural image from algebra to the whole of mathematics. But beyond the different scopes of these two enterprises — Bourbaki's and van der Waerden's — there is an additional, much more significant difference between Bourbaki's and van der Waerden's structural images of mathematics, and between the notions of structure underlying each of them. Van der Waerden never provided an explicit explanation, either formal or nonformal, of what is to be understood by an "algebraic structure" or by "structural research in algebra"; he showed what this is by simply doing it. Bourbaki on the other hand not only attempted at various times to explain what the structural approach is and why it is so novel and important for mathematics, but formulated what they expected to be an elaborate mathematical theory, the theory of *structures,* meant to sustain and endorse their explanations — indeed, their whole system of images of mathematics — by means of an allegedly unifying, mathematical theory.

This attempt is connected to a third basic element — the others being the axiomatic image and the structural image — of Bourbaki's conception of mathematics: the reflexive image. In contrast with the other exact sciences, mathematical knowledge displays the peculiarity of making possible — within the body of mathematics proper and using similar standards of proof and similar techniques to those used in any other mathematical domain — the study and elucidation of particular aspects of that system of knowledge constituted by mathematics. In other words: mathematics affords the possibility of reflexively studying, within the body of knowledge, certain issues belonging to the images of knowledge. This reflexive capacity has brought about some well-known and important advances in our understanding of the scope and limitations of mathematical knowledge, such as are unknown in any other scientific field. During the first half of the present century important achievements were gained in this direction, in particular in those disciplines usually grouped under the common heading of metamathematics. These achievements (Gödel's theorems are the classical example that immediately comes to mind) were certainly limited to very specific domains and did not necessarily have any significant, direct impact on the work of the overwhelming majority of mathematicians engaged in different areas of research. Still, they helped reinforce a point of view according to which any claims — whether historical, philosophical, or methodological — *about* the discipline of mathematics become meaningful and worthy of attention only insofar as they may be endorsed by formal mathematical arguments. I call this latter point of view "the reflexive image of mathematics."

Echoes of the reflexive image of mathematics can be found in much philosophical work published in the twentieth century, as reflected in the absolute dominance of the foundationalist currents over this period (see below, page 284). Normal mathematical research on the other hand was probably not much affected by conceptions of this kind; but as with other, similar ideas, mathematicians have often resorted to the reflexive image when the need arose to explain — to themselves or to others — the nature of their own business. Bourbaki's attempts to define a formal theory of *structures* can to a considerable extent be understood in these terms: as an attempt to elucidate reflexively the notion of a "mathematical structure" and the significance of conceiving mathematics in terms of it. In fact, developing a reflexive, formal-axiomatic elucidation of the idea of mathematical structure could have proved useful not only as a general frame of reference but also as a tool for addressing some very specific and central open questions that Bourbaki's adoption of the axiomatic and the structural images of mathematics made patent. One such issue was that of selection.

Theory selection and problem choice are central questions in science in general, at the level of the images of knowledge. What individual scientists select as their research discipline, and the particular problems they choose to deal with in that discipline largely determines, or at least conditions, the scope and potentiality of their own personal research. What a community of scientists establishes as major open problems and major active subdisciplines substantially influences the future development of that discipline as a whole. Clearly, the content of the body of knowledge directly delimits the potential selection of scientists. On the other hand, this content alone cannot provide clear-cut answers to the issue of selection. Criteria of selection are open to debate, and obviously there are several possible factors that determine a particular scientist's choice when confronted with a given body of knowledge.

Bourbaki was clearly conscious of the centrality of the issue of selection, and from the very beginning of the group's activities considerable effort was invested in debating it. In the early meetings that eventually led to the creation of the core Bourbaki group, an important criterion for the selection of issues to be treated in the projected treatise on analysis was their external applicability and their usefulness for physicists and engineers. Over the first years of activity, however, given the more abstract inclinations of certain members and the way in which the writing of the chapters evolved, gradual changes affected the criteria of selection guiding the group's work (see Beaulieu 1993, 1994).

As the axiomatic approach gradually became a dominant concern for Bourbaki, the problem of selecting, and especially of justifying, the most interesting theories to be included in the treatise became an increasingly pressing one. As Henri Cartan wrote in retrospect, on the face of it the choice of axioms might seem to be completely arbitrary; in practice, however, a very limited number of such systems constitute active mathematical research disciplines, since theories "built upon different axiomatic systems have varying degrees of interest" (Cartan [1958] 1980, 177). Dieudonné did not hesitate to use the term "axiomatic trash" (1982, 620), to designate theories based on the axiomatic treatment of systems that he considered unimportant or uninteresting. But what actually is the criterion for winnowing the chaff of "axiomatic trash" from the wheat of the mathematically significant axiomatic systems?

Under the spell of the reflexive image of mathematics it would be natural, or at least plausible, to expect an answer to the above question to be given by means of a reflexive mathematical theory. The correct choices could in this case be endorsed by results attained work within a standard mathematical theory. Bourbaki's formulation of the theory of *structures* could be seen as a possible response to that expectation. On the other hand, considering the intellectual inclinations of the mathematicians involved in the Bourbaki project, one is justified in thinking that each member of the group had strongly conceived opinions of what should and should not be considered mathematically interesting, *independently* of the elaboration of a formal theory of *structures*.[15]

At any rate, one can see how the thorough adoption of the axiomatic approach as the main tool for the exposition of mathematical theories, together with the images of knowledge associated with that approach, create a direct connection between the issue of selection and Bourbaki's formulation of the theory of *structures*. As it happened, however, this theory did not effectively provide answers to this or to any other reflexive issue. Nevertheless, Bourbaki's images of mathematics, and in particular the group's actual choices, proved to be enormously fruitful in certain quarters of mathematics. Still more interesting, Bourbaki's criteria of selection have very often been accepted *as if* they were actually backed by such a reflexive theory; and the writings of some of the members of the group, particularly of Dieudonné, have strongly enhanced this belief. This has contributed to Bourbaki's images of mathematics being presented as fully backed by knowledge drawn from the body of mathematics, and it is in this sense that the eternal character usually attributed to the body of knowledge has come to be extended, in the picture of mathematics promoted by Bourbaki, to the images of knowledge as well.

Perhaps the most interesting example of how this extension has worked concerns the issue of the "mother structures," allegedly one of the central pillars associated with Bourbaki's mathematics. The role of the mother structures appears in Bourbaki's architecture manifesto as follows:

> At the center of our universe are found the great types of structures, ... they might be called the mother structures. ... Beyond this first nucleus, appear the structures which might be called multiple structures. They involve two or more of the great mother structures not in simple juxtaposition (which would not produce anything new) but combined organically by one or more

[15] In the words of Cartan: "That [a final product] can be obtained at all [in Bourbaki's meetings] is a kind of miracle that none of us can explain" (Cartan [1958] 1980, 179).

> axioms which set up a connection between them. . . . Farther along we come finally to the theories properly called particular. In these the elements of the sets under consideration, which in the general structures have remained entirely indeterminate, obtain a more definitely characterized individuality. (Bourbaki 1950, 228–29)

This is the heart of Bourbaki's conception of mathematics as a hierarchy of structures, and it has been quoted and repeated very often. But the interesting point is that this picture has nothing to do with Bourbaki's theory of *structures*! The classification of *structures* according to this scheme is mentioned several times in Bourbaki's volume on set theory, but only as an illustration appearing in scattered examples. Many assertions that were made either explicitly or implicitly by Bourbaki or by its individual members — i.e., that all of mathematical research can be understood as research on structures, that there are mother structures bearing a special significance for mathematics, that there are exactly three, and that these three mother structures are precisely the algebraic, order and topological structures (or *structures*) — all this is by no means a logical consequence of the axioms defining a *structure*. The notion of mother structures and the picture of mathematics as a hierarchy of structures are not results obtained within a mathematical theory of any kind. Rather, they belong strictly to Bourbaki's nonformal images of mathematics; they appear in nontechnical, popular articles, such as in the above-quoted passage, or in the myth that arose around Bourbaki. And yet, because of the blurred mixing of the two terms, *structures* and "structures" in Bourbaki's work, they have been accorded a status of truth similar to that accorded other mathematical results appearing in Bourbaki's treatise — namely, the status of eternal truths.

Closely related to this issue is the relationship between Bourbaki's work and the development of the theory of categories. This theory, first formulated in 1942 by Samuel Eilenberg and Saunders Mac Lane and more vigorously elaborated since the 1960s by a steadily growing community of practitioners, provided a viable alternative to what the theory of *structures* promised: a general framework within which the various mathematical domains and the interrelations among them could be mathematically studied. It would be far beyond the scope of the present article to discuss this issue in detail (see part 2 of Corry 1996). Of direct relevance to the present discussion is that the very existence of such an alternative posed an interesting challenge to the picture presented in the *Eléments* and certainly to the underlying claim of eternal validity for Bourbaki's structural (*structural*?) image of mathematics. As it happened, category theory came to provide a useful language that was fruitfully used in many different mathematical domains; but very much like the theory of structures, it attained rather little significance as an overall organizational scheme for mathematical knowledge.

Another interesting example of the sweeping validity claims associated with Bourbaki's work appears in a book published by Dieudonné in 1977 and translated into English in 1982 under the title *A Panorama of Pure Mathematics: As Seen by Nicolas Bourbaki.* In this book Dieudonné presents an overview of many branches of mathematics and of the main problems addressed in each of them. He puts forward a picture of mathematics as divided into two parts: "classical" and "live." The classical is that part of mathematics embodied in the various volumes of Bourbaki's *Eléments.* No more and no less. The live part is still in the process of being constituted: it is still changing and therefore is unstable; but eventually, as it stabilizes, it will be added to the *Eléments* and thus will become part and parcel of classical mathematics. Moreover, so asserts Dieudonné, the reader interested in knowing the most important parts of this live mathematics will find them by consulting the proceedings of the Seminar Bourbaki, currently held at the University of Paris. This is obviously a very "nonclassical" definition of what classical mathematics is, and also of what live mathematics is. Dieudonné not only endorsed his classification and his whole selection with his professional authority, which was rather well established by then; he also added the suggestion that this way of presenting the core of mathematics, unlike earlier ones, was one that would remain unchanged from then on! In other words, Bourbaki's picture of mathematics included not only a body of mathematics composed of eternal truths but also the concomitant, eternal, images of mathematics. At any rate, one has to take into account that by the time this book was published Bourbaki was well past its heyday and Dieudonné was perhaps trying to reinforce again a point that may already have been less obvious than in the recent past.

I would like to mention here a third and last instance of Bourbaki's way of presenting a certain system of images of knowledge as eternally valid: Bourbaki's historiography. Bourbakian historiography is manifest mainly in the collection of articles published as *Eléments d'histoire des mathématiques* (1969), but also in the many historical writings of individual Bourbaki members, especially Dieudonné. This historiography has received considerable attention and criticism (see e.g. Grattan-Guinness 1979; Spalt 1987), and this is not the place to discuss it in detail except for what pertains to the point at issue here. Bourbaki's historiography is a classical example of the approach according to which the importance of mathematical ideas in the past is judged by pondering their relevance to present conceptions. Bourbaki is not the only representative of this trend in the history of mathematics, but what is singular about Bourbaki is that the frame of reference the group adopted for making historical judgments is one that was meant to remain unchanged *in the future as well!* Thus Bourbaki (especially Dieudonné) interestingly combined in historiography two different aspects: a Whiggish approach to past history and a belief in the eternal character of not only the truth of mathematical theorems and results but also of its present organization.

An interesting example of the way in which the idea of structure and the importance accorded to it in twentieth-century mathematics enters Dieudonné's historiography as a criterion for retroactive historical judgment is manifest in his account of the history of algebraic geometry (Dieudonné 1985). This account distinguishes seven different periods in the development of the discipline. The first four periods, from 400 B.C. to about 1866, up to and including the works of Riemann and Abel, cover the first twenty-six pages of his book. The fifth period ("Development and Chaos: 1866–1920") is discussed in the next thirty-two pages. Most of the discussion in this chapter (pp. 29–35) is devoted to a classical article by Richard Dedekind and Heinrich Weber (1882), and this is — Dieudonné explains — because of its proximity to modern, structural ideas. As Dieudonné himself writes, however, for this very reason the article had a limited impact on contemporary mathematicians. The sixth period, "New Structures in Algebraic Geometry: 1920–1950," is the one to which the most detailed analysis is accorded. What characterizes this period, in Dieudonné's view, is the fact that "at the beginning of the twentieth century the general idea of the structures underlying diverse mathematical theories became completely conscious" (1985, 59).

But Dieudonné's criteria for historical research, based on choices similar to those that guided his own mathematical research, is not left implicit in the different degree of attention accorded to the various historical stages of his story. Dieudonné stated his historiographical approach explicitly when he wrote that the "algebraic school," although chronologically last, would be treated first because "in the light of future history, it is the algebraic inclination that exercised the most profound influence." Moreover, the alternative approaches to it were at their time just contributing to chaos; they were "attracted to one aspect or another of Riemann's works, and thus are born several schools of algebraic geometry that tend to diverge up to the threshold of mutual incomprehension" (ibid., 27).

In spite of the fact that Bourbaki's theory of *structures* does not help to solve any of the open questions at the level of the images of knowledge, Bourbaki has nevertheless been fond of presenting its own choices as if they were fully justified on purely mathematical grounds, as definite and final, and as unbiased by personal or socially conditioned tastes. Moreover, Bourbaki's selection allegedly does not imply a value judgment. For example, group theory, despite its acknowledged importance, is not included in Bourbaki's treatise because "we cannot say that we have a general method of attack"; the systematic presentation attained in the other theories developed in the treatise cannot therefore be introduced for this one. But the question still remains open: how does the group justify its choices?

This question brings us to an additional important issue. There is usually a high degree of agreement among mathematicians as to what should legitimately be considered part of the body of mathematical knowledge: knowledge has to be endorsed by some kind of proof. The absence of debate characteristic in general of the body of mathematics is often taken, by analogy, to be the desired state of affairs at the level of the images of knowledge as well. From reading texts like the *Eléments,* for instance, one might tend to think that there are not, and should not be, any debate at all at the level of the images of mathematics. If, nevertheless, debate does eventually arise regarding the images of knowledge, it is often dealt with in one of the following ways: (1) a mathematical theory is proposed within which the debate may be safely decided by means of proof; or (2) it is resolved by resort to authority; or (3) it is simply ignored. Had Bourbaki's theory of *structures* had something substantive to say about the hierarchy of structures and related issues, then it would have been covered by the first alternative; but clearly it failed to do so. Lacking a reflexive argument such as the theory of *structures* could have provided, Bourbaki has opted for the second-best alternative: resorting to authority.

As in other aspects of Bourbaki's work, Dieudonné has taken the lead in expressing his opinion on this issue. Although he has also advanced some substantive arguments, authority frequently seems to be his soundest justification for Bourbaki's choice: "No one can understand or criticize the choices made by Bourbaki unless he has a solid and extended background in many mathematical theories, both classical and more recent" (Dieudonné 1982, 623). Sheer authority, however, imparts a taste of arbitrariness to this claim. Since it is generally considered unacceptable to accord the arbitrary an important role in mathematics, some additional claims have been advanced as further justification for Bourbaki's choice criteria. The following quotation is but one example of Dieudonné presenting what he sees, in retrospect, as Bourbaki's selection criteria:

> In spite of its initial aim at universality, the scope of the Bourbaki treatise has finally been greatly reduced (although to a still respectable size) by successive elimination of:
> 1. the end products of theories, which do not constitute new tools;
> 2. the unmotivated abstract developments scorned by the great mathematicians;
> 3. important theories (in the opinion of great mathematicians) that are far from clear descriptions in terms of an interplay of perspicuous structures; examples are finite groups or the analytic theory of numbers. (Ibid., 620)

Like any list of criteria meant to provide a useful guide for choice in either mathematical or historical research, this one can be criticized on many grounds. In particular, it is interesting to notice that these criteria are nothing but a reformulation of the criteria of professional authority. But the really interesting issue from the point of view of the present article is that rather than solving it they only underscore the problem involved in establishing once and for all, as Bourbaki wanted, a comprehensive system of images of mathematical knowledge that would attain the status of eternal truth usually accorded to results belonging to the body of knowledge.

To summarize, then, one can see how Bourbaki's images of mathematics — especially as formulated and promoted by the group's most active speaker, Jean Dieudonné — put forward the idea that not only is the body of mathematical knowledge a collection of eternal truths, but that this is also the case for the images of knowledge. In particular, the structural image of mathematics is the ultimate stage of a necessary process of historical development, and it is bound to remain unchanged in the future. After the publication of the *Eléments,* suggested Dieudonné, future developments in mathematics would proceed squarely within the basic framework stipulated by the structural image put forward in this treatise: more complex structures would perhaps be developed that would combine in new ways the mother structures and the other, already known, structures built on them. Above all, new and more sophisticated knowledge would be gained about all these structures. But beyond that, our knowledge that mathematics is a hierarchy of structures and that mathematical knowledge advances by further elucidating the individual structures that constitute the hierarchy — this image would remain unchanged and as eternal as any of the specific theorems that have been proved about any of those individual structures.

4. Some Recent Debates

From the 1950s to the late 1970s, Bourbaki's images of knowledge exerted a tremendous influence on mathematical research and teaching, especially in the "pure" branches, all over the world. This influence surely counts as one of the main factors behind the apparently robust status of the belief in the eternal character of mathematical knowledge over this period of time. This belief is far from having disappeared from the mathematical scene (and perhaps this is not without justification), but at the same time interesting debates have arisen around it. In this section, I mention some of the most recent dissenting views.

In 1980 the mathematician Morris Kline published a provocative book entitled *Mathematics. The Loss of Certainty.* The main thesis of the book is that the received view of mathematics, according to which this discipline "is regarded as the acme of exact reasoning, a body of truths in itself, and the truth about the design of nature," is plainly false! Kline presents in his book a historical account of the rise of mathematics to the unparalleled heights of "prestige, respect and glory" that were accorded to it from ancient times and well into the nineteenth century. This development, however, was followed by what Kline sees as a total debacle, in which all certainty about the truth of mathematics, especially concerning the question of which approach to the foundations of mathematics is correct and secure, was lost:

> It is now apparent that the concept of a universally accepted, infallible body of reasoning — the majestic mathematics of 1800 and the pride of man — is a grand illusion. Uncertainty and doubt concerning the future of mathematics have replaced the certainties and complacency of the past. The disagreement about the foundations of the "most certain" science are both surprising and, to put it mildly, disconcerting. The present state of mathematics is a mockery of the hitherto deep-rooted and widely reputed truth and logical perfection of mathematics. (Kline 1980, 6)

"The Age of Reason," Kline concluded in the introductory chapter of his book, "is gone."

The details of Kline's arguments and the question as to whether or not they lead to his sweeping, appalling conclusions will not concern us here. Interested readers can consult the book and judge this for themselves. There is no doubt, however, that Kline's claims, whether well taken or not, whether supported by sound historical evidence or not, were rather uncommon at the time of their publication, especially coming from a prominent mathematician like himself.

Kline's book did not immediately give rise to any kind of open controversy. If one has to judge according to published reactions, then the conclusion is that the book was largely ignored by mathematicians. The few published reviews of the book suggest that the mathematical community may even have been hostile to the kinds of arguments put forward by Kline.[16] At the same time, however, one can see in retrospect that Kline pointed to a direction that was soon to be followed by others, who would undertake a reexamination of the character of eternal truth commonly attributed to mathematical knowledge.

Substantial evidence that such a reexamination was under way appeared in 1985 in a collection of articles edited by Thomas Tymoczko, under the title *New Directions in the Philosophy of Mathematics.* The articles in this collection, written by mathematicians as well as by philosophers and historians, put forward a philosophy of mathematics that Tymoczko calls "quasi-empiricist" and that is opposed to the view which had dominated discourse in this domain at least since the 1920s. Tymoczko called this formerly dominant view "foundationalism," and he characterized it as the search for the true foundations of what is assumed, in the first place, to be a system of certain, unchanging, knowledge. This view includes, of course, three main schools of philosophy of mathematics in the present century: logicism, formalism, and intuitionism.

The quasi-empiricist works that Tymoczko collected in his volume share a common interest in the processes of production, communication, and change of mathematical knowledge, rather than focusing on the finished, and allegedly definitive, versions of it. Also, they coincide in stressing that the nature of mathematics can be elucidated only when this science is considered to be an organic, lively body of knowledge, and that the analysis of its foundations is only a very partial perspective of this more general task. They stress that mathematical knowledge arises as part of a social process in which elements of uncertainty — such as plain mistakes, empirical considerations, heuristic factors, and even tastes and fads — may play some role. This view does not necessarily imply a relativistic account of mathematics; but it does dispute — perhaps from a perspective somewhat different from the one suggested by Kline — some basic, accepted beliefs

[16] See e.g. Corcoran 1980, who describes the book as "important and ambitious," but at the same time, however, expresses regret that the author does not know enough logic, that his historical claims are inaccurate, and that his philosophical arguments are unsound.

concerning the nature of mathematical knowledge as a body of eternal, unshakable truth.[17]

Tymoczko's collection included recent articles but also less recent ones, such as those by Imre Lakatos, who began publishing his idiosyncratic work on the philosophy of mathematics in the late 1960s. But the major motivation behind Tymoczko's publication was the recent rise to prominence of new kinds of proofs that departed from the classical Euclidean paradigm — a long-dominant one on which the classical view of the eternal nature of mathematical truth was based. Among those new kinds of proofs, special attention was accorded to "computer-assisted proofs" (e.g., the four-color problem) but also to "very long proofs" (e.g., the simple, finite groups classification theorem) and to proofs which established that a theorem was true with an "extremely high probability," rather than with absolute, Euclidean or deductive, certainty (e.g., Rabin 1976 on the distribution of prime numbers). This is not the place to describe all these kinds of proofs and the philosophical questions they raise.[18] The point here is simply to make clear that some actual mathematical developments raised pressing questions and posed new challenges that somehow clashed with the received conception of mathematics as a body of eternal truths. These questions were addressed mainly by philosophers and historians of mathematics, and under the influence of their works some observers went so far as to pronounce the classical conception of proof officially dead (Horgan 1993). The reactions of most working mathematicians to these developments were at this stage either indifferent or hostile to the conclusions that some nonmathematicians were deriving from their secondhand knowledge of them (Thurston 1994).

A noteworthy event in the debate on the eternal character of mathematical truths took place quite recently, when some mathematicians — in fact, some very prominent mathematicians — came forward with their own proposals to change the accepted canons of mathematical publishing. By doing so, they anticipated that a broader spectrum of what constitutes the actual process of mathematical research and knowledge will become public and will be shared by the mathematical community at large. This process will affect the conception of mathematics as a body of eternal truths, and it will contribute, so these mathematicians hope, to the enhanced development of their discipline.

A by now well-known manifestation of this trend was an interchange published in the *Bulletin of the American Mathematical Society* in 1993 and 1994. It started with an article, "Theoretical Mathematics: Toward a Cultural Synthesis of Mathematics and Theoretical Physics," by Arthur Jaffe and Frank Quinn, both distinguished mathematicians, Jaffe, a mathematical physicist from Harvard, being concurrently president of the AMS. According to Jaffe and Quinn, recent

[17] Similar views are also put forward in Kitcher 1988, esp. 294–98.

[18] For a discussion of the philosophical problems raised by computer-assisted proofs, see Tymoczko 1979 and Detlefsen and Luker 1980. On "very long proofs," see Kitcher 1983, 40ff. On proofs based on probabilities, see Kolata 1976.

events in the development of mathematics and physics dictate the need for a redefinition of the relations between the two sciences. In particular, they claim, there has recently been intense activity in physics, which has yielded many new insights into pure mathematical fields. Some of these results were eventually taken over by mathematicians and reworked according to their professional tastes, but originally they were produced by physicists not abiding by the standards set by the mathematical community for themselves. Among the work that Jaffe and Quinn had in mind was Edward Witten's in string theory. They thought that mathematicians should encourage the production of works similar to Witten's. In their view, without an active initiative to do so, the current professional mores would hinder such contributions and thus cut off a vital source of inspiration and insight for mathematics.

Faced with such a situation, the article suggested the need to adopt in mathematics a division of labor accepted in physics throughout the present century — namely, that between theoretical and experimental physics. How is this division translatable into mathematics? Jaffe and Quinn compared the initial stages of mathematical discovery, involving speculation, intuition, and convention, to the work of the theoretical physicists. As with experimentation in physics, rigorous mathematical proof should be introduced only later in order to correct, refine, and validate the results and insights obtained in the earlier stage. Thus while admitting that the terms "theoretical" and "experimental" might at first be somewhat confusing when applied to mathematics, Jaffe and Quinn suggested the following prescription for the healthy future development of the science:

> The mathematical community has evolved strict standards of proof and norms that discourage speculation. These are protective mechanisms that guard against the more destructive consequences of speculation; they embody the collective mathematical experience that the disadvantages outweigh the advantages. On the other hand, we have seen that speculation, if properly undertaken, can be profoundly beneficial. Perhaps a more conscious and controlled approach that would also allow us to reap the benefits but avoid the dangers is possible. The need to find a constructive response to the new influences from theoretical physics presents us with both an important test case and an opportunity.
>
> Mathematicians should be more receptive to theoretical material but with safeguards and a strict honesty. The safeguards we propose are not new; they are essentially the traditional practices associated with conjectures. However a better appreciation of their function and significance is necessary, and they should be applied more widely and more uniformly. Collectively, our proposals could be regarded as measures to ensure "truth in advertising" [for example] "Theoretical work should be explicitly acknowledged as theoretical and incomplete; in particular, a major share of the credit for the final result must be reserved for the rigorous work that validates it." (Jaffe and Quinn 1993, 10)

It was clear to the editors of the *Bulletin* that a proposal of this kind would not pass in silence. Even before publication they asked several leading mathematicians to write their opinions and reactions, to be published in a forthcoming issue of the journal (Atiyah et al. 1994; Jaffe and Quinn 1994). The published reactions ranged from a total rejection (e.g., by Saunders Mac Lane), to a criticism of the general, authoritative tone adopted by the authors when suggesting new standards for publication (e.g., by Michael Atiyah, Armand Borel, and Benoit Mandelbrot), to a general agreement in principle but disagreement in the details of the proposal (by William Thurston and Albert Schwartz), to a disagreement in principle but agreement with some of the details (by René Thom).[19]

But the debate remained open, and one may expect — if only because of the prominence of the mathematicians involved — that the issues raised by it will not soon be forgotten. As it happens, on 12 February 1996 a colloquium was held at Boston University on "Proof and Progress in Mathematics," which was basically a follow-up of this interchange. Jaffe and Mac Lane were again among the discussing parties, together with other mathematicians, such as Gian-Carlo Rota of MIT and the Harvard mathematician Barry Mazur. New issues were raised in this meeting — e.g., of the role of electronic communications among mathematicians, the Internet, etc. — that in retrospect seem inevitable. The pervasiveness of these new media raises the need to redefine some well-established concepts pertaining to the mathematical profession: publishing, definitive versions, authorship of ideas and results, etc.[20]

Parallel to the Jaffe-Quinn proposal for reconsidering the accepted norms of publication in mathematics, the role of rigor in proof and — implicitly at the very least — of the eternal character of mathematical truth, I want to mention here an additional similar debate involving prominent mathematicians. This one was sparked by Doron Zeilberger of Temple University, in an article bearing the provocative title "Theorems for a Price: Tomorrow's Semi-Rigorous Mathematical Culture." Based partly on the innovations implied by his own important mathematical contributions, Zeilberger attempted in this article to attack a conception of mathematics that, although still dominant today, is in his view actually obsolete and bound to be changed by a new mathematical culture:

> The most fundamental precept of the mathematical faith is *thou shalt prove everything rigorously*. While the practitioners of mathematics differ on their views of what constitutes a rigorous proof, and there are fundamentalists who insist on even a more rigorous rigor than the one practiced by the mainstream, the belief in this principle could be taken as the *defining property* of *mathematician*. (Zeilberger 1994, 11; italics in the original)

[19] Kleiner & Movshowitz-Hadar 1997 contains an attempt to look at this recent debate from a broader historical perspective (which is nevertheless rather different from the one intended in the present article).

[20] A selection of the papers presented at this meeting was recently published in *Synthese* 3(2), 1997. On the influence of the new media on mathematical culture, see Jaffe 1997. On the novel role played by "architectural conjectures" in the construction of new mathematical theories, see Mazur 1997.

This conception, Zeilberger promised, will soon be preserved by only a small sect of fringe mathematicians who, in spite of the deep changes expected, will choose to keep abiding by the orthodox conception. To support his claim and make explicit to whom he is referring to with this description, he cites the 1993 article by Jaffe and Quinn. The reader thus understands that Zeilberger is going to present a truly radical proposal for the future of mathematics.

Zeilberger makes his point by referring to the so-called algorithmic proof theory of hypergeometric identities, a field to which he made significant contributions. This theory considers identities involving certain functions, and proves or refutes them by means of algorithms that reduce any given identity of this kind to an auxiliary one, involving only specific polynomials. Today the theory can be successfully applied to a wide range of known identities, but as Zeilberger explains, it is natural to expect that in the future one might construct examples of identities whose reduction using the known algorithms in any computer will involve prohibitive amounts of running time or of memory. Performing the algorithms in this case would lead to absolute certainty concerning the truth or falsity of the identities, but the price (in dollars) would be enormous. On the other hand, it is possible to apply a different kind of algorithm that will enable us to answer the same question, not with full certainty but with a very high probability and for free, or for a very low price in terms of computer resources.

I have already mentioned "probabilistic" proofs — namely, arguments that assign a very high probability to statements of the kind "Theorem X is true." Michael Rabin published one such argument in 1976 concerning the statement that a certain number is prime. Rabin devised an algorithm, each iteration of which raises the probability in question. Thus the idea of a probabilistic proof is not a new one. Still, Rabin never claimed that this should become a mainstream way of supporting mathematical truth. Moreover, it seems quite clear that Rabin would be very pleased to have a deductive argument to prove, in the classical and (by implication) conclusive way, what his probabilistic proof seemed only to support with a very high likelihood. Zeilberger on the other hand is explicitly arguing for the adoption of these kinds of proofs as the standard, mainstream vindication of the truth of a mathematical statement. Zeilberger invokes two arguments to support his position: First, he says, it is likely that few new, nontrivial results might be proved through deductive arguments. Second, the price of the latter will become increasingly high:

> As wider classes of identities, and perhaps even other kinds of classes of theorems, become routinely provable, we might witness many results for which we would know how to find a proof (or refutation); but we would be unable or unwilling to pay for finding such proofs, since "almost certainty" can be bought so much cheaper. I can envision an abstract of a paper, c. 2100, that reads, "We show in a certain precise sense that the Goldbach conjecture is true with probability larger than 0.99999 and that its complete truth could be determined with a budget of $ 10 billion." . . .

> As absolute truth becomes more and more expensive, we would sooner or later come to grips with the fact that few non-trivial results could be known with old-fashioned certainty. Most likely we will wind up abandoning the task of keeping track of price altogether and complete the metamorphosis to nonrigorous mathematics. (Ibid., 14)

A reply to Zeilberger's article came very soon from George E. Andrews. Andrews is a mathematician of no less merit than Zeilberger (in fact the two have collaborated on many occasions). It is instructive to read Andrews in order to realize that, in spite of the strong arguments put forward by Zeilberger, Jaffe, Quinn, and others, the idea of eternal truth in mathematics will not disappear so soon, if only for reasons that touch on the sociology of the profession; but certainly also for reasons deeper than that. In a formulation that might warmly be adopted by many colleagues Andrews disputed Zeilberger's position with the following words:

> Through the summer of 1993 I was desperately clinging to the belief that mathematics was immune from the giddy relativism that has pretty well destroyed a number of disciplines in the university. Then came the October *Scientific American* and John Horgan's article, "The death of proof" [Horgan 1993]. The theme of this article is that computers have changed the world of mathematics forever, in the process making proof an anachronism. Oh well, all my friends said, Horgan is a non-mathematician who got in way over his head. Apart from his irritating comments and obvious slanting of the material, "The death of proof" actually contains interesting descriptions of a number of important mathematics projects. Indeed, as W. Thurston has said [Thurston 1994] "A more appropriate title would have been 'The Life of Proof'."
>
> Then came [Zeilberger's article]. . . . Unlike Horgan, Zeilberger is a first-rate mathematician. Thus one expects that his futurology is based on firm ground. So what is his evidence for this *paradigm shift*? It was at this point that my irritation turned to horror. (Andrews 1994, 16; italics in the original)

Andrews described in some technical detail why in his view Zeilberger's specific arguments do not support his claim. At the same time Andrews stressed an important component of mathematical knowledge that he felt Zeilberger's perspective failed to stress: the role of insight.

Perhaps it is relevant to cite here a further reaction to Zeilberger's and Andrews's articles, which appeared under the title "Making Sense of Experimental Mathematics" (Borwein et al. 1996). This article attempts to put the whole debate raised by such mathematicians as Jaffe, Quinn, and Zeilberger in a broader context and to find a common ground that might be accepted by a larger portion of the mathematical community. The article put forward some arguments that are interesting in themselves. But that is not the point I want to stress here. What I find worthy of special attention is the fact that one of its authors, Jonathan Borwein, works in the Center for Experimental and Constructive Mathematics at Simon Fraser University. This is only one example of a kind of institution active today in many universities around the world. Thus while the debate on new ways to legitimize mathematical truth is still an open one, new institutions are being built that already promote work based on the new principles. One should not be surprised, then, to realize that Zeilberger's vision of "theorems for a price" might become a reality much sooner than he envisaged, though not literally in the sense described in his article: it is not unlikely that financial support for "centers for experimental mathematics" around the world might soon surpass that allocated for more traditionally oriented departments, thus dictating, for a price, what kinds of theorems are going to be proved and in which direction mathematics is going to advance in the foreseeable future. Institutional factors have been decisive throughout history in shaping the course of development of mathematical ideas (through education, grants, appointments, promotion, etc.), and given the present state of academic research, such considerations will become increasingly important.

At any rate, it is not the aim of this article to elucidate the future course of mathematical research. The aim of this section has been merely to indicate an interesting turn that the idea of eternal truth in mathematics has undergone over the last ten years. This makes more perspicuous the relevance of the historical analysis presented in the preceding sections. If this article had been written in the early 1980s it could have started with the following words: "Mathematics is the scientific discipline in which the idea of eternal truths is most deeply entrenched. In fact, unlike other sciences, twentieth-century developments have only strengthened this historically conditioned tendency." However, in view of the developments mentioned above, I must erase the second sentence of the quotation, and instead start as follows: "Mathematics is the scientific discipline in which the idea of eternal truths has historically been most deeply entrenched, although recent developments have modified this to a certain degree, in a direction whose actual significance is still to be definitely evaluated."

5. Summary and Concluding Remarks

This paper has discussed the views of some leading twentieth-century mathematicians concerning the status of truth in their discipline. Neither Bourbaki's nor Hilbert's views in this context is monolithic, yet in general they share a belief in the eternal character of mathematical truth, which has basically been unchallenged throughout history, and still remains so. The interesting debates and nuances that this issue raises pertain to the ways of achieving these truths.

In Bourbaki's conception, the conjunction of the structural, the axiomatic, and the reflexive images of mathematical knowledge together produce an image of mathematics that, besides leading to the discovery of new eternal mathematical truths in an unprecedentedly effective way, itself bears the character of eternal truth: Bourbaki's image of mathematics is bound to remain unchanged as will the truths to which it leads.

Hilbert's views, on which Bourbaki's are supposedly based, were much more multifarious. The eternal truths of mathematics are, in his view, attained in complex ways. The axiomatic method was seen as a very useful, but in no way infallible, tool leading to such truths. It helps mainly in the identification of precisely those places where falsity or contradiction has entered into mathematical reasoning; but even a mindful and able use of the method leaves much room for error, uncertainty, innovation, and need for change. In his published works on physics, for example, Hilbert's axiomatic treatment of theories (e.g., radiation theory or general relativity) suggests an air of definiteness, but in his lectures he put forward a somewhat more tentative approach. And certainly Hilbert did not think that the axiomatic method, or any other mathematical idea for that matter, can lead to a definitive scheme for organizing science.

My discussion of Hilbert, Bourbaki, and eternal truths in mathematics also helps to clarify, I believe, the background to the recent debates mentioned in section 4 above. These are clearly debates about the images of mathematical knowledge. They attempt to establish the disciplinary boundaries of legitimate mathematical knowledge. They do not in general question the eternal character of the truths that currently exist in, and that must be added to, the body of mathematical knowledge. Rather they question whether or not, by accepting new forms of legitimation, new "truths" are going to be accepted that will perhaps bear an essentially different, and therefore undesired, character. On the one side of the debate are those who claim that departing from the established model of proof, basically as embodied in Bourbaki's textbooks, will be detrimental to the future of mathematics as we know it today, precisely because it will cast serious doubt on the character of truth involved in it. The other party involved in this debate does not seem to wish to change the character of mathematical truth as such. Rather, its claim is that introducing additional, legitimate models of proofs will not threaten mathematics as a science of certain knowledge, but will significantly enlarge its scope.

It therefore seems that Bourbaki's ambition of establishing once and for all the images of mathematics according to which mathematical research will have to proceed in the future is being questioned today in directions, and with an intensity, that not even Bourbaki's critics could have envisaged in the past.

References

Andrews, G. E. 1994. "The Death of Proof? Semi-Rigorous Mathematics? You've Got to Be Kidding!" *Math. Int.* 16(4):16–18.

Atiyah, M. et al. 1994. Responses to Jaffe and Quinn 1994. *Bull. AMS* 30:178–207.

Beaulieu, L. 1993. "A Parisian Café and Ten Proto-Bourbaki Meetings (1934–35)." *Math. Int.* 15:27–35.

——. 1994. "Dispelling the Myth: Questions and Answers about Bourbaki's Early Work, 1934–1944." In *The Intersection of History and Mathematics*, edited by C. Sasaki et al., 241–52. Basel: Birkhäuser.

Bernays, P. 1922. "Die Bedeutung Hilberts für die Philosophie der Mathematik." *Die Naturwissenschaften* 10:93–99.

Boos, W. 1985. "'The True' in Gottlob Frege's "Über die Grundlagen der Geometrie" *Arch. Hist. Ex. Sci.* 34:141–92.

Born, M. 1922. "Hilbert und die Physik." *Die Naturwissenschaften* 10:88–93. Reprinted in *Ausgwählte Abhandlungen* 2(1963):584–98. Göttingen: Vandenhoek and Ruprecht.

Borwein, J. et al. 1996. "Making Sense of Experimental Mathematics." *Math. Int.* 18(4):12–18.

Bourbaki, N. 1950. "The Architecture of Mathematics." *Am. Math. Mo.* 67:221–32.

——. 1969. *Eléments d'histoire des mathématiques*, 2nd ed. Paris: Hermann.

Cartan, H. [1958] 1980. "Nicolas Bourbaki and Contemporary Mathematics." Translated by Kevin Lenzen. *MI* 2:175–80. Originally published as "Nicolas Bourbaki und die heutige Mathematik," *Arbeitsg. Forsch. Landes Nordheim* 76:5–18.

Corcoran, J. 1980. Review of Kline 1980. *Math. Rev.* 82e:03013.

Corry, L. 1989. "Linearity and Reflexivity in the Growth of Mathematical Knowledge." *Science in Context* 3:409–40.

——. 1996. *Modern Algebra and the Rise of Mathematical Structures*. Science Network, vol. 17. Boston: Birkhäuser.

——. 1997a. "David Hilbert and the Axiomatization of Physics (1894–1905)." *Arch. Hist. Ex. Sci.* 51:83–198.

——. 1997b "Hermann Minkowksi and the Principle of Relativity." *Arch. Hist. Ex. Sci.* 51:273–314.

——. Forthcoming. "David Hilbert and Physics." In *The Symbolic Universe: Geometry and Physics, 1890–1930*, edited by J. J. Gray. Oxford: Oxford University Press.

Dedekind, R., and H. Weber. 1882. "Theorie der algebraischen Funktionen einer Veränderlichen." *Jour. r. ang. Math.* 92:181–290.

Detlefsen, M., and M. Luker. 1980. "The Four-Color Problem and Mathematical Proof." *Journal of Philosophy* 77:803–24.

Dieudonné, J. 1962. "Les méthodes axiomatiques modernes et les fondements des

mathématiques." In *Les grands Courants de la Pensée Mathématique,* 2nd enl. ed., edited by F. Le Lionnais, 443–555. Paris: Blanchard.
——. [1977] 1982. *A Panorama of Pure Mathematics: As Seen by Nicolas Bourbaki.* Translated by I. G. MacDonald. New York: Academic Press. Originally published as *Panorama des mathématiques pures. Le choix bourbachique.*
——. 1982. "The Work of Bourbaki in the Last Thirty Years." *Notices AMS* 29:618–23.
——. 1985. *History of Algebraic Geometry. An Outline of the Historical Development of Algebraic Geometry.* Monterey, Calif.: Wadsworth.
Fang, J. 1970. *Bourbaki: Towards a Philosophy of Modern Mathematics I.* New York: Paideia Press.
Gabriel, G. 1980. et al., eds. 1980. *Gottlob Frege — Philosophical and Mathematical Correspondence.* Chicago: The University of Chicago Press.
Grattan-Guinness, I. 1979. Review of Dieudonné, ed., *Abrégé d'histoire des mathématiques, 1700–1900. Annals of Science* 36:653–55.
Hertz, H., [1894] 1956. *The Principles of Mechanics Presented in a New Form.* New York: Dover. Originally published as *Die Prinzipien der Mechanik in neuem Zusammenhange dargestellt.* Leipzig.
Hilbert, D. 1899. *Grundlagen der Geometrie (Festschrift zur Feier der Enthüllung des Gauss-Weber-Denkmals in Göttingen).* Leipzig: Teubner.
——. 1902. "Mathematical Problems." *Bulletin AMS* 8: 437–79.
——. 1903. *Grundlagen der Geometrie,* 2nd rev. ed., with five supplements. Leipzig: Teubner.
——. 1916–17. *Die Grundlagen der Physik, II,* Ms. Vorlesung WS 1916-17. Annotated by R. Bär. Bibliothek des Mathematischen Seminars: Universität Göttingen.
——. 1918. "Axiomatisches Denken." *Math. Ann.* 78:405–15. Reprinted in Hilbert 1932–35, 3:146–56.
——. [1919–20] 1992. *Natur und Mathematisches Erkennen: Vorlesungen, gehalten 1919–1920 in Göttingen. Nach der Ausarbeitung von Paul Bernays.* Edited and with an English introduction by David E. Rowe. Basel: Birkhäuser.
——. 1922–23. "Wissen und mathematisches Denken." WS 1922/23, Ausgearbeitet von W. Ackerman, Mathematisches Institut Universität Göttingen. Revised version.
——. 1932–35. *Gesammelte Abhandlungen,* vol. 3. Berlin: Springer Verlag.
Horgan, J. 1993. "The Death of Proof." *Scientific American* 269(4):74–103.
Huntington, E. 1902. "Simplified Definition of a Group." *Bulletin AMS* 8: 296–300.
Jaffe, A. 1997. "Proof and the Evolution of Mathematics." *Synthese* 111(2):133–46.
Jaffe, A., and F. Quinn. 1993. "Theoretical Mathematics: Towards a Cultural Synthesis of Mathematics and Theoretical Physics." *Bull. AMS* 29:1–13.
——. 1994. Responses to M. Atiyah et al., 1994. *Bull. AMS* 30:208–11.

Kitcher, P. 1983. *The Nature of Mathematical Knowledge*. New York: Oxford University Press.

——. 1988. "Mathematical Naturalism." In *History and Philosophy of Modern Mathematics*, Vol. 11 of Minnesota Studies in the Philosophy of Science, edited by R. Aspray and P. Kitcher, 293–323. Minneapolis: University of Minnesota Press.

Kleiner, I., and N. Movshowitz-Hadar. 1997. "Proof: A Many-Splendored Thing," *Mathematical Intelligencer* 19(3):16–32.

Kline, M. 1980. *Mathematics. The Loss of Certainty*. New York: Oxford University Press.

Kolata, G. 1976. "Mathematical Proofs: The Genesis of Reasonable Doubt." *Science* 192:989–90.

Mazur, B. 1997. "Conjecture." *Synthese* 111:197–210.

Moore, E. H. 1902. "A Definition of Abstract Groups." *Transactions AMS* 3:485–92.

Moore, G. H. 1987. "A House Divided against Itself: The Emergence of First-Order Logic as the Basis for Mathematics." In *Studies in the History of Mathematics*. MAA Studies in Mathematics, edited by E. R. Phillips, 98–136.

Pasch, M. 1882. *Vorlesungen über neuere Geometrie*. Leipzig: Teubner.

Peckhaus, V. 1990. *Hilbertprogramm und Kritische Philosophie. Des Göttinger Modell interdisziplinärer Zusammenarbeit zwischen Mathematik und Philosophie*. Göttingen: Vandenhoeck & Ruprecht.

Rabin, M. 1976. "Probabilistic Algorithms." In *Algorithms and Complexity: New Directions and Recent Results*, edited by J. F. Traub, 21–40. New York: Academic Press.

Rowe, D. Forthcoming. "Perspectives on Hilbert — A Review Essay." In *Perspectives on Science*.

Scanlan, M. 1991. "Who Were the American Postulate Theorists?" *Jour. Symb. Logic* 50: 981–1002.

Spalt, D. 1987. *Vom Mythos der mathematischen Vernunft*. Darmstadt: Wissenschaftliche Buchgesellschaft.

Stegmüller, W. 1979. *The Structuralist View of Theories: A Possible Analogue to the Bourbaki Programme in Physical Sciences*. Berlin: Springer.

Thurston, W. 1994. "Letter to the Editor." *Scientific American* 270(1):9.

Toepell, M. 1986. *Über die Entstehung von David Hilberts "Grundlagen der Geometrie."* Göttingen: Vandenhoeck & Ruprecht.

Tymoczko, T. 1979. "The Four-Color Problem and Its Philosophical Significance." *Journal of Philosophy* 76:67–114.

——, ed. 1985. *New Directions in the Philosophy of Mathematics*. Boston: Birkhäuser.

van der Waerden, B. L. 1930. *Moderne Algebra*, 2 vols. Berlin: Springer.

Veblen, O. 1904. "A System of Axioms for Geometry." *Transactions AMS* 5:343–84.

Weyl, H. 1944. "David Hilbert and His Mathematical Work." *Bulletin AMS* 50:612–54.

Zeilberger, D. 1994. "Theorems for a Price: Tomorrow's Semi-Rigorous Mathematical Culture." *Notices of the AMS* 40:978–81 (Reprinted *Math Int.* 16(4):11–14).

Cohn Institute for the History and Philosophy of Science and Ideas
Tel Aviv University

Note:
Original manuscripts of Hilbert's courses are quoted by permission of the library of the Mathematisches Institut, Universität Göttingen (see Hilbert 1916–17).