Hilbert's Twenty-Fourth Problem

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1. INTRODUCTION. For geometers, Hilbert's influential work on the foundations of geometry is important. For analysts, Hilbert's theory of integral equations is just as important. But the address "Mathematische Probleme" [**37**] that David Hilbert (1862–1943) delivered at the second International Congress of Mathematicians (ICM) in Paris has tremendous importance for all mathematicians. Moreover, a substantial part of Hilbert's fame rests on this address from 1900 (the year after the American Mathematical Society began to publish its *Transactions*). It was by the rapid publication of Hilbert's paper [**37**] that the importance of the problems became quite clear, and it was the American Mathematical Society that very quickly supplied English-language readers with both a report on and a translation of Hilbert's address. (In Paris, the United States and England were represented by seventeen and seven participants, respectively.)

Indeed, this collection of twenty-three unsolved problems, in which Hilbert tried "to lift the veil behind which the future lies hidden" [**37**, p. 437] has occupied much attention since that time, with many mathematicians watching each contribution attentively and directing their research accordingly. Hermann Weyl (1885–1955) once remarked that "We mathematicians have often measured our progress by checking which of Hilbert's questions had been settled in the meantime" [**110**, p. 525]. (See also [**31**] and [**115**].)

Hilbert and his twenty-three problems have become proverbial. As a matter of fact, however, because of time constraints Hilbert presented *only* ten of the problems at the Congress. Charlotte Angas Scott (1858–1931) reported on the Congress and Hilbert's presentation of ten problems in the *Bulletin of the American Mathematical Society* [**91**]. The complete list of twenty-three problems only appeared in the journal *Göttinger Nachrichten* in the fall of 1900 [**37**], and Mary Winston Newson (1869–1959) translated the paper into English for the *Bulletin* in 1901 [**37**]. Already by September 1900, George Bruce Halsted (1853–1922) had written in this MONTHLY that Hilbert's beautiful paper on the problems of mathematics "is epoch-making for the history of mathematics" [**34**, p. 188]. In his report on the International Congress, Halsted devoted about forty of the article's eighty lines to the problems. As to the actual speech, no manuscript was preserved, nor was the text itself ever published.

Recently, Ivor Grattan-Guinness presented an interesting overview of Hilbert's problems in the *Notices of the American Mathematical Society*, discussing the form in which each of the twenty-three problems was published [**30**]. Yet, in dealing with the celebrated problems from this viewpoint, he failed to mention the most interesting problem of Hilbert's collection: the canceled twenty-fourth. Hilbert included it neither in his address nor in any printed version, nor did he communicate it to his friends Adolf Hurwitz (1859–1919) and Hermann Minkowski (1864–1909), who were proofreaders of the paper submitted to the *Göttinger Nachrichten* and, more significantly, were direct participants in the developments surrounding Hilbert's ICM lecture.

So, for a century now, the twenty-fourth problem has been a Sleeping Beauty. This article will try to awaken it, thus giving the reader the chance to be the latter-day Prince (or Princess) Charming who can take it home and solve it. This paper also aims to convince the reader of the utility of the history of mathematics in the sense to which

Constantin Carathéodory (1873–1950) referred in his speech at an MAA meeting in 1936 [**10**, p. 101]: "I will be glad if I have succeeded in impressing the idea that it is not only pleasant and entertaining to read at times the works of the old mathematical authors, but that this may occasionally be of use for the actual advancement of science."

2. THE CANCELED PROBLEM. Let me preface my fairy tale "Looking backward, 2003–1888" (i.e., from the present back to the discovery of a finite ideal basis in invariant theory) with some questions that will serve as a guide for the remainder of my investigation:

i. Why did Hilbert give a talk on unsolved problems and not on new results or methods in general use?
ii. How should the twenty-fourth problem be classified in relation to Hilbert's famous collection of twenty-three problems?
iii. Why and how did Hilbert deal with the canceled twenty-fourth problem later? And what part did this problem play in Hilbert's later research?
iv. Finally, where did I find the canceled problem?

Let me begin by presenting *the problem* itself. The twenty-fourth problem belongs to the realm of foundations of mathematics. In a nutshell, it asks for the simplest proof of any theorem. In his mathematical notebooks [**38**:3, pp. 25–26], Hilbert formulated it as follows (author's translation):

> The 24th problem in my Paris lecture was to be: Criteria of simplicity, or proof of the greatest simplicity of certain proofs. Develop a theory of the method of proof in mathematics in general. Under a given set of conditions there can be but one simplest proof. Quite generally, if there are two proofs for a theorem, you must keep going until you have derived each from the other, or until it becomes quite evident what variant conditions (and aids) have been used in the two proofs. Given two routes, it is not right to take either of these two or to look for a third; it is necessary to investigate the area lying between the two routes. Attempts at judging the simplicity of a proof are in my examination of syzygies and syzygies [Hilbert misspelled the word syzygies] between syzygies [see Hilbert [**42**, lectures XXXII–XXXIX]]. The use or the knowledge of a syzygy simplifies in an essential way a proof that a certain identity is true. Because any process of addition [is] an application of the commutative law of addition etc. [and because] this always corresponds to geometric theorems or logical conclusions, one can count these [processes], and, for instance, in proving certain theorems of elementary geometry (the Pythagoras theorem, [theorems] on remarkable points of triangles), one can very well decide which of the proofs is the simplest. [Author's note: Part of the last sentence is not only barely legible in Hilbert's notebook but also grammatically incorrect. Corrections and insertions that Hilbert made in this entry show that he wrote down the problem in haste.]

In answer to the first question, we begin with a short prehistory of Hilbert's famous speech. As preparation for treating the second question, we next present some remarks on the nature of the proposed problems. Hilbert intended to build up the whole science of mathematics from a system of axioms. However, before one formalizes and axiomatizes, there must be some (meaningful) mathematical substance that can be taken for granted without further analysis [**52**, pp. 171, 190], [**53**, p. 65], [**57**, 7th ed., Anhang 9, p. 288]. For Hilbert, the subject of foundations fell, generally speaking, into two main branches: *proof theory* and *metamathematics*, related to formalization and meaning, respectively [**51**, p. 152], [**58**:3, p. 179]. Proof theory was developed in order to increase certainty and clarity in the axiomatic system, but in the end what really mattered for Hilbert was meaning. The penultimate question will lead us finally to an investigation of how Hilbert carried out his twofold research program on foundations by incorporating into it both proof theory and metamathematics.

3. THE PREHISTORY. In the winter of 1899–1900 Hilbert, though but thirty-eight years old one of the most respected German mathematicians of the day, was invited to present one of the major addresses at the opening session of the forthcoming ICM in Paris. Three years earlier, at the Zürich Congress, Henri Poincaré (1854–1912) had delivered the speech "Sur les rapports d'analyse pure et de la physique mathématique" [On the relationships between pure analysis and mathematical physics]. Hilbert vacillated between replying to Poincaré and choosing another subject, for example, a collection of important, open questions through which at the end of the century he could try to sense the future of mathematics. In January, he asked for Minkowski's opinion. His friend wrote in a letter of 5 January 1900: "Most alluring would be the attempt to look into the future and compile a list of problems on which mathematicians should test themselves during the coming century. With such a subject you could have people talking about your lecture decades later" [**71**] (see also [**87**, p. 69]).

Minkowski was correct, of course, yet Hilbert continued to waiver. At the end of March, he asked Hurwitz for his opinion on the matter. Finally, in the middle of July, Hilbert surprised both Minkowski and Hurwitz with page proofs of the paper "Mathematische Probleme" [**37**], an expanded version of his Paris talk written for publication in the *Göttinger Nachrichten*. However, by that time Hilbert had already missed an ICM deadline, so the program mailed to Congress participants included an announcement of neither a major lecture nor any other contribution by Hilbert himself. One must bear in mind that at this juncture in his life Hilbert was extremely pressed for time; he had prepared the Paris address during a summer term in which he was obliged to lecture ten hours a week. It is noteworthy that in one of these courses, on the theory of surfaces, he developed the celebrated invariant integral that would also play an essential role in the twenty-third problem just a few weeks later [**37**, pp. 472–478] (see as well [**99**, pp. 253–264]).

Both friends advised him to shorten the lecture. Hilbert agreed, presenting only ten of the problems.

4. ON THE ROLE OF PROBLEMS. How should Hilbert's proposed problems be characterized? Time pressures probably explain some of the points commented upon by Grattan-Guinness (personal selection, mixture of distinct kinds of problems that are only partially grouped, overlapping or missing problems) [**30**, pp. 756–757]. (Opinions on this subject differ. See, for example, those of Pavel S. Alexandrov (1896–1982) in his edition of the problems [**37**, German ed., p. 20].) Still, it was Hilbert's aim to present "merely samples of problems" or, more precisely, problems that showed "how rich, how manifold, and how extensive the mathematical science of to-day is" [**37**, p. 478]. Moreover, it was Hilbert's conviction that a branch of science is full of life only as long as it offers an abundance of problems: a lack of problems is a sign of death [**37**, p. 438]. Eight of the twenty-three problems read more like research programs than problems as such; of the remaining fifteen problems, twelve have been completely solved. Also, in a general sense one can regard the twenty-third problem as a program that could not be implemented (the further development of the methods of the calculus of variations) except from a certain vantage point as a (solved) problem (invariant integrals in the calculus of variations). It is of interest to note that Hilbert's notebooks [**38**:3, pp. 69–70] contain an unpublished entry fully anticipating the later general concepts of sufficient conditions (field theories) in the calculus of variations of André Roussel (1904–?), Hermann Boerner (1906–1982), or Rolf Klötzler (see [**99**, pp. 387–402]).

Using words that recall Hilbert's, André Weil (1906–1998) once said, "Great problems furnish the daily bread on which the mathematician thrives" [**105**, p. 324]. As a matter of fact, all of Hilbert's problems have served up beautiful food for thought. Despite their great importance, however, we should not on their account elevate Hilbert to the stature of a prophet of future mathematics, for he himself regarded such prophecies as absolutely impossible. Indeed, quite typical of him, he regarded the impossibility of prophecies as a veritable axiom [**38**:3, inserted pages]. This conviction notwithstanding, Hilbert dared to make some predictions. He considered the seventh problem, which deals with the irrationality and transcendence of certain numbers, extraordinarily difficult and did not expect a speedy solution, but such problems have been treated successfully since the 1920s [**8**:1, pp. 241–268], [**115**, pp. 171–202]. On the other hand, he was quite right in his prognosis that Fermat's Last Theorem would be solved in the twentieth century [**112**]. In 1970, the year Yurij Matiyasevich showed that Hilbert's tenth problem is unsolvable [**70**] (see also [**115**, pp. 85–114]), J. Fang wrote [**18**, p. 123]: "The tenth problem, like the eighth, is not likely to be solved in the near future." The eighth problem (the Riemann Hypothesis), the problem that Hilbert viewed as the most important of mathematics (see [**98**, p. 14]), still shows little sign of yielding to the intense efforts to settle it.

To return to the subject of Hilbert's collection of problems as a whole, the history of mathematics records almost nothing else comparable to what Hilbert single-handedly undertook because, to quote a letter of Hilbert's student John Von Neumann (1903–1957) to Hendrik Kloosterman (1900–1968), chairman of the program committee of the 1954 ICM: "The total subject of mathematics is clearly too broad for any one of us. I do not think that any mathematician since Gauss has covered it uniformly and fully, even Hilbert did not, and all of us are of considerably lesser width (quite apart from the question of depth) than Hilbert" [**85**, p. 8]. Nevertheless, in 1900 Hilbert "dared to chart out the most promising avenues for research in the twentieth century" [**12**]. (During the Millennium Meeting in Paris in May 2000, the Clay Mathematics Institute (CMI) of Cambridge, Massachusetts, identified seven Millennium Prize Problems, for each of which it has put up a one million dollar prize for a solution. The Scientific Advisory Board of the CMI declared that the problems "are not intended to shape the direction of mathematics in the next century" [**12**].) When he received his doctoral degree in 1866, Georg Cantor (1845–1918) followed the custom of the day by defending certain theses that he had advanced. (Hilbert was four years old at the time.) The third of these reads [**9**, p. 8]: "In mathematics the art of asking questions is more valuable than solving problems." Indeed, it is precisely by the identification of concrete problems that mathematics has been able to and will continue to develop. That is the deeper reason why Hilbert took the risk of offering a list of unsolved problems. For the axiomatization of a theory one needs its completion. On the other hand, for the development of a mathematical theory, one needs problems. In addition to the completion of a theory, Hilbert insisted on problems and therefore on the development of a theory. In other words, Hilbert was not at all the pure formalist he is often taken to be.

Hilbert also explained to his audience what the nature of good problems should be, his words echoing what he wrote elsewhere [**38**:1, p. 55]: "The problems must be difficult while being plain—not elementary yet convoluted, because confronted with them we would be helpless, or we would need some exertion of our memory, to bear all the assumptions and conditions in mind." In other words, the formulation of a problem has to be short and to the point. Hilbert himself possessed an uncanny ability to make things simple, to eliminate the unnecessary so that the necessary could be recognized.

The last quotation in the previous paragraph is taken not from the Paris address (see [**37**, p. 438]) but from a parallel remark in Hilbert's mathematical notebook, where I also found the canceled twenty-fourth problem. To the best of my knowledge, the twenty-fourth problem has remained unpublished until now, and I do not know of any responses to this problem, with the exception of Hilbert's own. Surprisingly, although Hilbert was under time pressure when he prepared the Paris address, he made little use of his notebook as a source of inspiration. The notebook (more precisely, the three copybooks [**38**:1–3] in which Hilbert wrote down mathematical remarks, questions, and problems from 1885 onward) is housed in the University of Göttingen Library's Special Collections (Handschriftenabteilung). Unfortunately, no entry in the notebook is dated, which makes it difficult to attach an exact date to the entry concerning the twenty-fourth problem. On the basis of entries (datable to 1901) dealing with results in Werner Boy's dissertation and Otto Blumenthal's habilitation [**38**:3, pp. 29, 33] that appear only a few pages after the one concerning the twenty-fourth problem [**38**:3, pp. 25–26], it is very likely that Hilbert wrote the entry in which he mentioned the cancellation in 1901.

5. SIMPLICITY AND RIGOR. It is widely believed among mathematicians that simplicity is a reliable guideline for judging the beauty (see [**84**, sect. 4–5], [**106**, chap. 6]) or elegance [**113**] of proofs, but like all aesthetic principles, such a criterion is highly subjective. Can one really say that certain mathematical proofs are simpler than others? In other words, the question of what the simplest proof is depends upon interpretation, and interpretation brings nonmathematical concepts into play. As a formal means of overcoming such subjective aspects of nonmathematical reasoning, Hilbert developed a proof theory that deals with formulas and their deduction. In this framework, each mathematical statement (theorem) becomes a deducible formula [**54**, p. 137], [**56**, p. 489]. On the first level, this theory is concerned with the set of provable theorems, but in a broader sense, it also deals with the structure of proofs, for example, with the concept of the simplicity of a proof. In the emerging fields later called proof theory and metamathematics [**51**, p. 152], Hilbert wanted as early as 1900 to have a detailed investigation of the question of simplicity (see also [**42**, lecture 37]). This furnished the rationale for a twenty-fourth problem. Still, an obvious question arises: Is the formula that is (by whatever means) assigned to the simplest proof of a given theorem a derivable formula in proof theory itself?

By 1899 geometric concepts had already been formalized and their relative consistency proved by Hilbert (by assuming the consistency of the theory of real numbers; see [**43**], [**57**]). But Hilbert's ultimate goal was more ambitious: to prove the consistency of mathematics itself. The next step was to address arithmetic (and then, of course, logic proper [**44**, p. 176]). Indeed, shortly after the publication of the *Grundlagen der Geometrie* [**57**] in 1899, Hilbert presented his ideas on the foundations of arithmetic [**43**] in the same way that he had treated geometry. Furthermore, later editions of the *Grundlagen* [**57**] articulated these ideas [**57**, chap. 13]; from the third through the seventh editions of the *Grundlagen* the arithmetic axioms [**43**] appeared as an appendix [**57**, Anhang 6]. In the beginning, however, the necessary task of proving consistency for the proposed system of axioms was only indicated, not actually executed.

This task, however, was brought to the fore a few months later, as the second problem in Hilbert's Paris address (see Kreisel [**66**, pp. 93–130]). This rapid progress in

Hilbert's foundation of arithmetic (axiomatization and the related question of consistency) and logic is quite remarkable. Hilbert believed that such investigations of formalization carried out by an extension of his axiomatic method would not merely be promising but would necessarily be successful, especially if pursued by means of the reduction of proofs to an algebraic calculus or "logical arithmetic" [**44**], [**45**]. That was still his belief in the 1920s ([**48**], [**51**]–[**56**]; see also [**66**]). Hilbert wanted to achieve his objective of establishing the consistency of axiomatized mathematics by proceeding from a purely finitary standpoint, i.e., he accepted only those facts that can be expressed in a finite number of (elementary) symbols and admitted only those operations that can be executed in a finite number of steps. (Haskell Brooks Curry, who took his doctor's degree with Hilbert in 1929, translated Hilbert's technical term "finit" as "finitary.")

In support of the preceding statement, let me quote from Charlotte Angas Scott's interview with Hilbert for her ICM report [**91**, pp. 67–68]: "As to our aim with regard to any problem, there must be a definite result of some kind; it cannot be laid aside until we have obtained either a satisfactory solution or a rigorous demonstration of the impossibility of a solution. The mathematical rigor that is essential in the treatment of a problem does not require complicated demonstrations; it requires only that the result be obtained by a finite number of logical steps from a finite number of hypotheses furnished by the problem itself; in seeking this rigor we may find simplicity."

In his speech, Hilbert did not deal with proving the simplicity of proofs from the finitary perspective as fully as he did in the interview with Scott just cited. Here is what he said to his Paris audience [**37**, p. 441]: "Besides it is an error to believe that rigor in the proof is the enemy of simplicity. On the contrary we find it confirmed ... that the rigorous method is at the same time the simpler and the more easily comprehended. The very effort for rigor forces us to find out simpler methods of proof." At the end of her report, Scott thanked all the speakers for their assistance. This acknowledgment lends credence to the suggestion that Hilbert had explained his idea to her in greater detail. The first part of the quotation, the expression of a belief in the solvability of each well-posed problem, might flatly be called the *Hilbert axiom* (see [**37**, p. 445]). In the second part of the quotation, however, the role of simplicity (from the finitary point of view) is clearly emphasized, much more clearly than in the lecture.

The question as to what constitutes a rigorous proof is a logical question. By examining the language in which the proof is expressed one may ask: What are the conditions under which a strict logical deduction proceeding by a long chain of formal inferences and calculations and leading from link to link by blind calculations can be regarded as simple? As early as 1900, Halsted reported that in Hilbert's opinion "mathematical rigor which we require does not necessitate complicated demonstrations; the most rigorous method is often the simplest and the easiest to comprehend" [**34**, p. 189]. To Hilbert, the qualities of rigor and simplicity are not at odds with each other but go hand in hand. In 1943, in Hilbert's obituary, Weyl echoed that sentiment: "With Hilbert rigor figures no longer as enemy but as promoter of simplicity" [**109**:4, p. 124].

Naturally, it has been an open question ever since Hilbert's time how or to what extent investigations of simplicity might be carried out, if they can be carried out at all.

6. THE PLACE OF THE TWENTY-FOURTH PROBLEM IN PROOF THEORY. Why did Hilbert *cancel* the question of simplicity, as formulated in the twenty-fourth problem? We do not have the necessary historical sources to give a definitive answer to this question, but we can make some conjectures and present evidence in support of them. Moreover, we can look at the progress of Hilbert's proof theory and metamathematics from the perspective of the canceled problem.

I do not think that the main reason was time pressure; rather I believe it was the *status nascendi* of proof theory. In Hilbert's actual speech, the three *problems of foundation* (the first, second, and sixth problems in [**37**]) that he chose to include among the ten presented were already, from his perspective, an acceptable representation of this branch of mathematics, for in the printed version these three were not supplemented by additional problems of this genre. Moreover, one of the three problems on foundations is concerned with the consistency of the axioms of arithmetic (the second problem; see [**66**]), which later played a prominent role in Hilbert's program [**115**, pp. 37–58]. In conjunction with the twenty-fourth problem, this shows that, even at the time, Hilbert had at least a vague idea of metamathematics. Also, Hilbert had already included a decision theory problem on his list: the tenth problem seeks an algorithm to determine whether or not an arbitrary polynomial with integer coefficients has an integer root. (As mentioned earlier, in 1970 Matiyasevich established the nonexistence of such an algorithm [**70**], [**8**:2, pp. 323–378], [**115**, pp. 85–114].)

In his 1917 Zürich talk "Axiomatisches Denken" [Axiomatic thinking], Hilbert surveyed the role of axiomatization and tried to turn the attention of mathematicians to the study of proofs (see [**48**]). He pointed out clearly that the problem of simplicity is among the significant questions of foundations and is closely related to tasks faced in the realm of proving consistency: "When we consider the matter [axiomatization of logic] more closely we soon recognize that the question of consistency of the integers and of sets is not one that stands alone, but that it belongs to a vast domain of difficult epistemological questions which have a specifically mathematical tint: for example (to characterize this domain of questions briefly) the problem of *solvability in principle of every mathematical question* [which we have earlier called the "Hilbert axiom"], the problem of the subsequent *checkability* of the results of a mathematical investigation, the question of a *criterion of the simplicity* for mathematical proofs, the question of the relationship between *content* and *formalism* [Inhaltlichkeit und Formalismus] in mathematics and logic, and finally the problem of *decidability* of a mathematical question by a finite number of operations. We cannot rest content with the axiomatization of logic until all questions of this sort and their interconnections have been understood and cleared up" ([**48**, p. 412], [**58**:3, p. 153], English translation in [**17**, p. 1113]; see also [**18**, p. 195]). Furthermore, near the end of his life, Hilbert created an index for his notebooks by inserting one extra page on which the problem of simplicity appears among the few key words [**38**:3, inserted page]. Indeed, the problem did not let go of him.

7. A LOOK AT HILBERT'S FOUNDATIONS OF MATHEMATICS. From Hilbert's point of view, any part of mathematics can be represented by a deductive system based on a finite set of axioms. Hilbert's idea was to *axiomatize* the branches of mathematics and then to investigate the *consistency* of their axioms, especially those of arithmetic, which underlies all other branches. For this purpose, one must first *formalize* the system (by means of formal logic) in order to make rigorous derivations possible.

In Hilbert's formalistic view, mathematics is to be replaced by mechanical derivations of formulas, without any reasoning concerning their specific content. Recall the words of Griffith [**32**, p. 3]: "A mathematical proof is a formal and logical line of reasoning that begins with a set of axioms and moves through logical steps to a conclusion .... A proof confirms truth for the mathematics." In such a formal system, in proof theory, the subject of research is the mathematical proof itself [**48**, p. 413], [**58**:3, p. 155]. To master this subject (in the object language), one must control the field of proofs (in a metalanguage). However, since one cannot be universal and cannot know everything that is to be known of all proofs, according to Hilbert's ideas one must develop proof theory completely from the *finitary viewpoint* and its intuitively convincing methods (see [**60**:1, secs. 2c–e]). Within this kind of framework (finitism), he hoped to be able to reduce any branch of mathematics to a system that rests on more solid ground. In any area of mathematics, there are questions that by their very nature refer to the infinite. This fact notwithstanding, Hilbert never specified precisely what the finitary, or for that matter what the simplest, proofs were. Clearly, for Hilbert, it had to be possible to convert any proof of a mathematical theorem into a finite one. Hilbert was probably led by certain examples (for instance, syzygies in the algebraic calculus [**40**], [**42**, lectures 47–48]) to the idea of finite (or even simplest) proof, but he had not yet thought to extend these particular results and to express them in a general form as a problem. The idea was still too vague. Nevertheless, since Hilbert's time the question has persisted: What is simplicity?

Three years after the Paris address, Hilbert gave a talk in Göttingen on the foundations of arithmetic in which he promised to work out details of the axiomatic approach. He tried to accomplish this in his Heidelberg lecture [**44**] at the 1904 ICM and in the course "Logische Prinzipien des mathematischen Denkens" [Logical principles of mathematical thought] (see [**45**], [**46**]) that he taught after the Heidelberg congress at the University of Göttingen [**81**, pp. 92–94, 98–101]. Strictly formalistic reasoning cannot prove the consistency of an axiomatic system, because the necessary formulas for proving consistency are not derivable within this system (Gödel's second incompleteness theorem [**26**]). In a different sense, in his metamathematics, Hilbert intended to establish such concepts as consistency by intuitive justifications, i.e., by appealing to self-evident principles of reasoning and by direct methods involving purely finitary, combinatorial inferences. Metamathematics must restrict itself to counting beans, as Hans Freudenthal (1905–1990) sarcastically characterized this attitude [**20**, p. 1056].

After 1904, Hilbert's research took an unexpected turn, and a thirteen-year break from the circle of ideas under discussion here ensued. These were the years during which Hilbert was developing the theory of integral equations (see the survey of Ernst Hellinger (1883–1950) in Hilbert [**58**:3, pp. 94–145] and [**88**, pp. 117–129]). In the latter years of this period, World War I took its toll on his ability to conduct research. In the aforementioned lecture on axiomatic thinking delivered in Zürich in 1917, however, Hilbert picked up the *problem of simplicity* again and presented the same examples we know from the twenty-fourth problem in the notebook [**38**:3, pp. 25–26]. He later based an article, "Axiomatisches Denken" [**48**], on the Zürich talk. Arend Heyting (1898–1980) regarded the discussion of simplicity in this article as the most important, because it demonstrated that Hilbert viewed proofs as mathematical objects in themselves: "In it we perceive the germ of the later 'Proof Theory' " [**36**, p. 36]. (See also [**48**, pp. 412–413], [**58**:3, p. 153].) As noted earlier, Hilbert held this view as early as 1900 (see [**42**, lecture 37]). In the Winter Term of 1917, he returned to it, delivering a course "Principien der Mathematik" [Principles of mathematics], which was worked out by Paul Bernays (1888–1977) [**47**] and which finally led to the book *Grundlagen der Mathematik* [**60**]. The basic idea of Hilbert's proof theory is to ensure the validity of arguments by reducing mathematics to a finite number of rules of inference that govern the manipulation of formulas. The set of derivable formulas is, of course, infinite, but it is "generated" by a finite subset of formulas. Is such an extended set consistent? Or one might ask generally: Is this proof theory itself consistent?

In the 1920s, during which quantum theory was rapidly emerging, Hilbert, too, was occupied with the physical and mathematical problems of quantum mechanics. Still, in this period he launched a major effort, the so-called Hilbert program, to prove that mathematics is consistent (for the mathematical statements, not for Hilbert's philosophical opinion, see the 1922 essay "Neubegründung der Mathematik" [New foundations of mathematics] [**49**]). The theory (metamathematics) that supported this formalism was more or less completed by Hilbert in a 1922 talk, "Logische Grundlagen der Mathematik" [Logical foundations of mathematics], in Leipzig [**51**], although a more readable version is his 1928 paper "Grundlagen der Mathematik" [**53**]. In his 1925 lecture "Über das Unendliche" [On the infinite] [**52**], Hilbert expressed his philosophical thoughts in their mature form. Among other things, in an epistemological aside, he mentioned that in mathematics one finds the philosophical concept of "ideal propositions" (in Kant: regulative device [regulatives Prinzip]) as counterparts of the real or "finite propositions" (in Kant: constitutive device [konstitutives Prinzip]). Such ideal elements do not correspond to anything in reality but serve as regulators in Kant's terminology "if ... one understands as an idea a concept of reason which transcends all experience and by means of which the concrete is to be completed into a totality" [**52**, p. 190]. The reliability of such ideal methods is to be established by finitary means.

Thus, only in his early sixties did Hilbert truly proceed to create proof theory and metamathematics. In this proof theory, he developed his formalistic standpoint in detail and dealt with the question of whether or not a formula can be deduced from the axioms that define a system. Hilbert regarded any derivable formula as an "image" of mental activity, for he was convinced that each mode of thought is accompanied by such symbols [**44**, p. 176]; he expressly formulated this conviction in the "axiom of thinking" [**45**, p. 119]. For Hilbert, both nature and human reflections on nature were *finite*. Therefore, Hilbert's program rested on the 'finite.' Relying upon a purely finite approach, in a system in which the axioms were regarded only as hypotheses for a theory and not as self-evident mathematical truths (see [**57**, chap. 1, sec. 1], [**44**], and [**46**, p. 141, 186]), Hilbert wanted to show that the essential mathematical methods could never lead to contradictions [**52**, pp. 162, 164]. The kind of formal axiomatic method regarded instrumentally by Hilbert is not in itself sufficient for the foundations of mathematics; for such a purpose, the axiomatization must be extended beyond its formal viewpoint. Meaning (inhaltliche Mathematik) is then introduced at the metamathematical level, in the metalanguage. In the end, the details of his program remained to be worked out (for the most refined presentation, see [**51**]).

Hilbert's aim was to secure meaning by using finite concepts. We mention Zermelo's credo as a conviction that is the antithesis of Hilbert's. The following articulation of Zermelo's leading ideas is taken from his five "theses about the infinite in mathematics" (1921): "Each genuine mathematical proposition has an 'infinitary' character, i.e. it ... has to be viewed as a combination of infinitely many 'elementary' sentences. The infinite is neither physically nor psychologically given to us in the real world. It has to be comprehended and 'posited' as an idea in the Platonic sense. Since infinitary propositions can never be derived from finitary ones, also the 'axioms' of all mathematical theories have to be infinitary and the 'consistency' of such a theory can only be proved by exhibiting a corresponding consistent system of infinitely many elementary sentences" (Nachlass Zermelo, quoted in [**15**, pp. 148, 158]). For more about Zermelo's prejudice against a finitary character of genuine mathematical propositions, see [**29**], [**116**]. In light of the fact that in his former Göttingen days Zermelo was on common ground with Hilbert [**80**, pp. 5, 118–122], it is indeed striking how widely divergent Hilbert's and Zermelo's opinions on the foundation of mathematics became.

8. THE HILBERT AXIOM: SOLVABILITY OF EVERY PROBLEM. Modern formalism descends from Hilbert's theory but has evolved into something quite different from it. Metamathematics, a term coined by Hilbert, has over the years come to be regarded as a virtual synonym for proof theory. (Paul Bernays already equated the two in 1934 [**60**].) "Proof theory" is now primarily a name for the study of formal models of mathematical systems. Hilbert's optimistic belief, the Hilbert axiom [**37**, p. 445] that he had preserved from his university days, is summed up by the quotation [**38**:3, p. 95]: "The proof of all proofs: that it must always be possible to arrive at a proof." This stands in remarkable contrast to the fact that in the last decades of the nineteenth century, especially during the time Hilbert was a student, pessimism was widespread. In 1872, the well-known physiologist and physicist Emil Du Bois-Reymond (1818–1896) delivered a famous speech in Leipzig, "Über die Grenzen der Naturerkenntnis" [On the limitations of knowledge in the natural sciences], that was widely read (eight editions of the talk had appeared by 1898). Du Bois-Reymond, a brother of the mathematician Paul Du Bois-Reymond (1831–1889), maintained that certain problems were unsolvable, among them the natures of matter and force or the origin of motion. He concluded the lecture with the then oft-quoted agnostic catchwords "Ignoramus et ignorabimus" [We are ignorant and we shall (always) be ignorant].

Up until the end of his career, Hilbert continued to reject again and again the "foolish ignorabimus" of Du Bois-Reymond and his successors (for Hilbert's last lecture, delivered in 1941, see [**73**, p. 71]). In his notebook, Hilbert phrases his "Noscemus" [We shall know] this way [**38**:3, p. 104]: "That there is no ignorabimus in mathematics can probably be proved by my theory of logical arithmetic." Elsewhere he continued [**38**:3, p. 98]: "Maybe it will turn out that there is no sense in saying there are insoluble problems." In Königsberg, on September 8, 1930, in his famous speech "Naturerkennen und Logik" [Natural philosophy and logic] [**55**], Hilbert pointed out that the positivist philosopher Auguste Comte (1798–1857) had once asserted that science would never succeed in ascertaining the secret of the chemical composition of the bodies in the universe. Then, preparing his optimistic concluding words, Hilbert went on to say: "A few years later this problem was solved" [**55**, p. 963] (see also [**87**, p. 196]).

Indeed, in the course of time Hilbert's group, the formalists, achieved large parts of their goals in the foundations of mathematics (Wilhelm Ackermann, John Von Neumann, see [**54**, p. 137]; Paul Bernays [**60**:1, p. v]). It seemed then that the proof of the consistency of number theory was more or less a matter of finding the proper mathematical technique. Consequently, at the 1928 ICM in Bologna, Hilbert's talk concerned problems of foundations [**54**, pp. 139–140]. In his program, Hilbert added to the old problem of consistency the new problem of the completeness of formal systems (first mentioned in [**57**, introduction], then in the second problem of the Paris address [**37**], and also in [**45**, chap. 1, sec. 3]). Hans Hahn (1879–1934) communicated Hilbert's extended program to the Vienna Circle, and in 1930 a young Austrian mathematician named Kurt Gödel (1906–1978) demonstrated in his Ph.D. dissertation [**24**] that first-order predicate logic is complete, i.e., every true statement in it can be derived from its axioms (see [**93**]).

Even the things one most confidently expects do not always come to pass. In one of the great ironies in the history of mathematics, it was coincident with Hilbert's great 1930 speech in Königsberg that the same mathematician Gödel again entered the scene, unnoticed by Hilbert; Gödel was in Königsberg attending a philosophical congress [**93**]. Indeed, it was only in the discussion that took place on September 7 (one day before Hilbert's famous speech) that Gödel made an offhand remark on a work in progress, which is now known as Gödel's incompleteness theorem ([**33**, pp. 147–148], see also [**25**]). Already in November 1930 the Leipzig journal *Monatshefte für Mathematik und Physik* received the epoch-making, 25-page article "Über formal unentscheidbare Sätze" [On formally undecidable propositions] [**26**], in which the 25-year-old author proved striking results in a way that Hilbert had not anticipated.

To be specific, Gödel gave negative answers to the remaining problems of foundations posed by Hilbert at the 1928 ICM [**54**]. Over Hilbert's great expectations fell the shadow of harsh reality represented by Gödel's results: (axiomatic) mathematical knowledge is always imperfect; we cannot prove all that is to be known. (In 1781, Königsberg's other great son Immanuel Kant (1724–1804) had stated this result philosophically in *The Critique of Pure Reason*: "What the things-in-themselves [Dinge an sich] may [finally] be I do not know, nor do I need to know, since a thing can never come before me except in appearance" [Critique, version A, p. 276; trans. N. Kemp Smith].) Gödel's results confirmed Cantor's belief that there are no foundations of mathematics without metaphysics, i.e., without infinite methods. And thus Hilbert's prediction, the sketched but never completely established finitary program, met the fate that the Bible so eloquently ordains: "The wind bloweth where it listeth, and thou hearest the sound thereof, but canst not tell whence it cometh, and whither it goeth: so is every one that is born of the spirit" (John 3:8).

If we wished to continue on this theme, we would soon encounter the names of Ernst Zermelo (1871–1953), Thoralf Skolem (1887–1963), Adolf Fraenkel (1891–1965), Rudolf Carnap (1891–1970), Alonzo Church (1903–1995), Stephen Cole Kleene (1909–1994), Alan Turing (1912–1954), and others. That, however, would be another story (see Jean-Yves Girard's contribution to [**82**:2, pp. 515–545]). Hans Freudenthal provided a rather sad postscript to the foregoing discussion [**20**, p. 1057]: "At a closer look, 1931 is not the turning point but the starting point of foundations of mathematics as it has developed since. But then Hilbert can hardly be counted among the predecessors." Still, of those who insisted on the importance of mathematical proofs themselves and regarded proofs as mathematical objects, Hilbert obviously must be counted among the first.

Furthermore, Hilbert's work on metamathematics has greatly improved our understanding of the nature of mathematical reasoning. Despite the fact that Hilbert's program was largely discredited by Gödel's theorems, Hilbert's ideas concerning foundations are not without value for certain areas. In a letter to Constance Reid dated March 1966, Gödel himself underscored this point: "I would like to call your attention to a frequently neglected point, namely the fact that Hilbert's scheme for the foundation of mathematics remains highly interesting and important in spite of my negative results. What has been proved is only that the specific epistemological objective which Hilbert had in mind cannot be obtained .... However, viewing the situation from a purely mathematical point of view, consistency proofs on the basis of suitably chosen stronger metamathematical presuppositions (as have been given by Gerhard Gentzen (1909–1945) and others) are just as interesting, and they lead to highly important insights into the proof theoretic structures of mathematics .... As far as my negative results are concerned, apart from the philosophical consequences mentioned before, I would see their importance primarily in the fact that in many cases they make it possible to judge, or to guess, whether some specific part of Hilbert's program can be carried through on the basis of given metamathematical presuppositions" ([**28**]; see also [**87**, pp. 217–218]).

Indeed, there is a surprising amount of work that can be carried out along the lines of the Hilbert program. In some sense, part of it was already done by Hilbert and his collaborators, particularly Wilhelm Ackermann (1896–1962) and Paul Bernays (1888–1977). The first major variation of Hilbert's program was due to Gerhard Gentzen, who in the 1934 paper "Die Widerspruchsfreiheit der Zahlentheorie" [**21**] established the consistency of number theory (see [**72**]), which Ackermann did independently in 1940 [**2**]. In accordance with Gödel's results, such investigations must resort to principles that lie outside pure number theory. For example, transfinite induction is used, but apart from this "transcendental" element, the proofs are carried out completely within the framework of number theory. (Noteworthy in this connection is the fact established by Church that Gödel's results cannot be obtained by finite means [**11**].)

In the axiomatic approach, the "tree" of all possible mathematical formulas growing from axioms is not only extremely expansive but, as Gödel pointed out, even disconnected. However, from a finitary standpoint, the restriction to "meaningful questions" (the Hilbert tree) would lead to a "human mathematics" (see the interview with Mikhael Gromov in [**82**:2, pp. 1213]). True, Gödel's results dictate certain limits to Hilbert's foundations of mathematics, but there remains much of value in continuing Hilbert-like programs. To give one important example: nonstandard analysis, in the spirit of Abraham Robinson (1918–1974), is a new branch of mathematics that grew out of model theory around 1960 and owes much to the ideas of Hilbert. In addition, the advent of computer science led to a rebirth of Hilbert's proof theory.

9. SIMPLICITY VERSUS COMPLEXITY. Hilbert was not alone in his desire for maximal simplicity in mathematical proofs. This issue was of importance, for example, to the French mathematician Émile Lemoine (1840–1912), who showed great interest in simplifying geometric constructions. In 1888, Lemoine reduced all geometric constructions by ruler and compass to five basic operations. One of them, for instance, was simply placing an end of the compass at a given point. Lemoine quantified the *simplicity of a construction* as the total number of times these five basic operations were used in it. In this way, he was able assign a numerical value to the complexity of a geometric construction [**68**].

It is probable that in some analogous way Hilbert wanted to make proofs in general a measurable object of another theory ("logical arithmetic") in which only finite methods were acceptable. The reduction of proofs to an algebraic calculus by means of formal logic would allow one to decide which of two given proofs is simpler merely by comparing the number of operations involved in each. In invariant theory [**1**], Hilbert had already touched upon the question of how to express relations in terms of a finite basis (see also his 1897 lecture [**42**]). From such a viewpoint, it would seem possible to arrange mathematical proofs into strata characterized by their degree of simplicity. Mathematics would then assume the appearance of a neatly organized warehouse in which formulas would be stacked in hierarchical order: the lower the stratum, the simpler the proof.

As to the *complexity* of technical details for proofs that sit in this mathematical storage facility, I would remind the reader of four well-known problems. The proof of the Burnside conjectures (see [**61**, p. 106]) occupies about one thousand pages, while a complete proof of Ramanujan's conjectures is estimated to require at least two thousand pages. Nevertheless, such proofs, including those of Fermat's Last Theorem by Andrew Wiles [**97**], [**112**] or of the "Four-color Problem" by Appel and Haken [**3**], are examples of the kind of finite mathematics that Hilbert hoped to establish by his program (see [**101**]). Wiles's proof can be carried out by hand; by contrast, no proof unaided by technology is yet known for the four-color problem. Of course, in general, computer-aided proofs have too many cases for any human being to check them step by step. However, even for finite proofs created solely by the human brain, there remains Hilbert's practical question [**38**:1, p. 53]: "whether in mathematics problems exist that cannot be dealt with in a prescribed short time?" His example (see [**38**:2, p. 1], [**38**:3, inserted index]; see also [**48**, p. 414] or [**58**:3, p. 155]): compute the nth digit in the decimal expansion of π, where n is equal to 10 raised to the 10th power to the 10th power, i.e., $n = 10^{10^{10}}$. (In the theory of invariants one is faced with similar questions. Hilbert's first papers settled the finiteness question only in principle, without any indication that we can actually calculate certain numbers; [**42**, lecture 37], [**74**], [**89**].)

The solvability of this problem in finitely many operations is evident, but from an epistemological viewpoint such computing tasks, which have been undertaken by Yasumasa Kanada, Takahashi, Fabrice Bellard, the brothers Gregory and David Chudnovsky, and others and which have provided millions of digits of π (as of April 1999 the number was up to 68 billion), are problematic. Imagine a supercomputer of the largest size possible, that of the universe, and imagine also that this computer has been calculating since the Big Bang. On the basis of the standard model of cosmology it is obvious that the number N of steps that this computer would have been able to execute must be finite. (It has been assumed that the total number of long-lived particles (electrons and protons) in the universe is about 10^{80} (Sir Arthur Eddington, 1931). This empirical assumption provides the maximal number of components of the central processing unit. On the other hand, quantum mechanics requires a minimal time for an operation. Coupled with the elapsed time due to the cosmological standard model the total number N of operations this computer would be able to carry out is somewhere between 10^{120} and 10^{160}; see [**22**, pp. 44–46], [**104**, pp. 64–65]). For the given n (or, more generally, for any given natural number), one can choose a natural number g for which $n^g > N$. Even at the rate of one thousand operations for the calculation of one digit of π, the "π-hunters" will likely remain below 10^{15} for the foreseeable future. The upshot of this discussion is that there is and will always be a limit to our knowledge; in the end, it is the complexity of the required operations that determines our access to the (presumed) realm of Platonic knowledge. (Incidentally, dealing with the prime-number theorem G. H. Hardy (1877–1947) looked upon n^{34}, the Skewes number, as the largest number in mathematics of any practical significance.)

Therefore, the complexity of computations (or, more precisely, of algorithms) is of the utmost importance (see the concise survey [**90**]). Suppose that a problem can be solved by means of any of several algorithms. For various reasons, it might be desirable to compare the complexities of the algorithms at hand. Such complexities are formulated in terms of Turing machines. Among the classes of complexity there are those of practical interest that can be solved by a deterministic and a nondeterministic Turing machine, respectively, whose time complexity is bounded by a polynomial time function (P-problem and NP-problem, respectively). Obviously, the class of NP-problems contains that of P-problems. In 1971, Stephen Cook formulated the P versus NP problem (now among the Millennium Prize Problems [**12**]): $N = NP$? The strong connections to computer science become obvious here. This rapidly increasing field of research has developed natural links with the search for the most effective and shortest algorithms, an endeavor with clear ties to Hilbert's vision of proof simplicity (see [**66**], [**83**], [**116**]).

One might ask: How many proofs do mathematicians publish each year? A back-of-the-envelope calculation yields a rough approximation: multiplying the number of journals by the number of yearly issues by the number of papers per issue by the average number of theorems per paper, someone has arrived at an estimated lower bound of two hundred thousand theorems a year! Who could conceivably judge which of them are established via the simplest possible proof? Nevertheless, we read in Hilbert's notebook: "All our effort, investigation, and thinking is based on the belief that there can be but *one* valid view" [**38**:3, p. 96]. That means also there must be a *simplest proof*. In a more colloquial spirit, Hilbert added [**38**:3, inserted page]: "Apply always the strictest proof! Philological-historic import must be wiped out. Given 15-inch guns, we don't shoot with the crossbow." He resorted to the same metaphor a second time with a reference to the Franco-Prussian war (1870–1871) [**38**:2, p. 99]: "We did not go to war against France with bows and arrows, although they too might have produced their effect."

Regarding the diversity of proofs, I point to the history of the fundamental theorem of algebra as a celebrated example. There are two basic ideas: for an algebraic proof, the approach espoused by Leonhard Euler (1707–1783), and for an analytic one, the mode of attacked favored by Jean-Baptist Le Rond D'Alembert (1717–1783). Of course, there is the research of Carl Friedrich Gauss (1777–1855) that forms a link between the two. Despite his motto "Pauca, sed matura" [Few, but ripe], Gauss returned to this subject many times and altogether gave four proofs of this important theorem. For Gauss, the simpler and more elegant he could make the proof, the better.

Frequently, a mathematical theorem is regarded as "deep" if its proof is difficult. The opposite of "deep" is "trivial," a term suggesting that little or no proof is necessary. Nevertheless, there is a constant movement in mathematics toward simplification, toward finding ways of looking at a matter from an easier, more "trivial," and hopefully more revealing vantage point. The simplicity of a proof depends on a multitude of factors: the length of its presentation, the techniques used, one's familiarity with the concepts involved, the proof's abstract generality, the novelty of ideas, and so forth. We read these words in Hilbert's notebook [**38**:3, p. 101]: "Always endeavor to make a proof with the least elementary means, for that way mastery of the subject comes best to the fore (the opposite of Weierstrass and Kronecker and their imitators). 'Elementary' is the designation only for what is known and familiar."

As to the mathematician's temptation to undertake generalization for generalization's sake, Hilbert declared [**38**:1, p. 45]: "The mathematicians' function should be to simplify the intricate. Instead they do just the opposite, and complicate what is simple, and call it 'generalizing'. Even if a method or an elaboration achieves no more than half, yet is two times simpler, I find that a great advantage." A simpler proof makes us wiser; the simpler its premises and deductions are, the more convincing a theory is. A proof is the most straightforward way to justify mathematical reasoning. "A mathematical proof," to quote G. H. Hardy, "should resemble a simple and clear-cut constellation, not a scattered cluster in the Milky Way" [**35**, p. 113].

In contrast to such views, Solomon Feferman remarked [**19**, p. 20]: "A proof becomes a proof after the social act of 'accepting it as a proof.' " The social perspective thus becomes relevant to the informal concept of the simplest proof. From this viewpoint complicated and long proofs, having features that might hamper or retard acceptance, would fail the test of simplicity. A famous historical example is the work of Girard Desargues (1591–1661) that was not well received in his time because Desargues invented too many "strange" new technical terms.

10. THE BACKGROUND OF THE TWENTY-FOURTH PROBLEM. Instead of proving mathematical theorems, we can examine deductive systems themselves and explore their properties. In this setting we investigate proofs themselves as mathematical objects. In order to be able to manipulate objects, however, we must first learn to distinguish them. This process begins by attributing certain properties (e.g., simplicity, complexity, shortness) to proofs as distinguishing markers.

It is evident that the question of simplicity is connected with the internal structure of such theories, extending all the way back to the choice of axioms and their particular formulation. In other words, the simplicity of axiom systems is also involved in the twenty-fourth problem. Here again, one is confronted by subjective viewpoints that lead into controversial areas of the foundations of mathematics.

However, we ought to remember that in no way is the equivalence of axiom systems affected by such requirements of efficiency, nor do we intend to suggest that well-constructed axiom systems with "shortest axioms" (whatever that might mean) will automatically give rise to the shortest or simplest proofs (see [**95**]). Although some research has been done with a view toward simplifying certain axiomatic systems (above all by Leśniewski (1886–1936) and his school), the problem has not yet been fully discussed. (I am indebted to Fred Rickey for bringing these developments to my attention.)

The final goal of presenting a branch of mathematics is to express it as a formalized system. However, the mathematical research that precedes the creation of such formal systems has need of its problems (for research) and examples (for representation). As we emphasized earlier, important problems appeared to Hilbert "as the life nerve of mathematics" [**109**:4, p. 123]. It is of consequence that he always began a project with clear but extremely simple examples and that very special results led him eventually to general ideas. As a case in point, he usually started lectures on ordinary differential equations with a thorough investigation of the elementary but instructive examples

$$y''(x) = 0, \quad y''(x) + y(x) = 0$$

[**87**, p. 104] (see also [**41**, p. 1 (insertion)]). However, "The detailed work will not receive the highest sacred fire unless the look is turned to generality and to the understanding of foundations" [**38**:3, insertion].

So, it is not surprising that the twenty-fourth problem consists of a general program that is illustrated by two specific examples. For Hilbert, the simplest mathematical operation was addition, so he used this operation in one of the examples to motivate the twenty-fourth problem. To each geometric or logical process, there corresponded for Hilbert an "adding together." In this way, he was able to formalize certain geometric concepts arithmetically, especially calculations with straight lines (Streckenrechnung) [**57**, sec. 15, 28]. Hilbert went into some detail about this subject in his address at the Heidelberg ICM [**44**]. He also investigated the possibility of correspondences of this kind for geometric constructions. "The geometrical figures are graphic formulas," he said in his Paris talk [**37**, p. 443]. There is no doubt that certain constructions or proofs rely completely on finite processes, i.e., their truth can be proved in a finite number of steps. And it was exactly this finitary point of view that Hilbert advocated.

"Simplicity ... is simplicity of ideas, not simplicity of a mechanical sort that can be measured by counting equations or symbols," declared the Nobel Laureate physicist Steven Weinberg [**106**, p. 107]. He went on to say: "Any symmetry principle is at the same time a principle of simplicity" [**106**, p. 110]. Weinberg's rejection of mere counting does not clash with Hilbert's finite notion of simplicity; rather, it supports Hilbert's belief because Hilbert was deeply convinced that proofs as shown in Riemann (1826–1866) are better achieved through ideas than through long calculations (this was Hilbert's "Riemann principle"). Weinberg's statement that "We demand a simplicity and rigidity in our principles before we are willing to take them seriously" [**106**, p. 118] coincides exactly with Hilbert's intention to use the simplest possible, yet rigorous concept.

In the end, Hilbert's aim was to justify classical mathematics by finite methods. "To preserve the simple formal rules of Aristotelian logic [in light of finite methods] we must supplement the finitary statements with ideal statements," Hilbert had declared in a lecture "On the infinite" delivered in Münster on June 4, 1925 [**52**, p. 174] (see also [**87**, p. 177]). However, such "adjoint" [adjungierte] ideal statements (for example, the existence of the infinite, of the continuum, and of ideals in algebra) obviously depart from the finite viewpoint. Nevertheless, such extended finite systems are indispensable for the development of mathematics. But such an extension of a finite domain that was taken to consist of meaningful true propositions of mathematics and their justifying proofs (termed "real mathematics" by Hilbert) is legitimate, provided this extension (termed "ideal" by Hilbert) does not cause any contradictions. Such accommodation was strictly rejected by Leopold Kronecker (1823–1891), and in the beginning Hilbert refused to accept Kronecker's views [**38**:1, p. 53, 79, 91]. From Hilbert's point of view any branch of mathematics dealing with ideal concepts could be accepted, as long as there was a proof that such an extended system was consistent.

Hilbert's justification rested on a division of mathematics into two parts: the real mathematics to be regulated and the ideal mathematics serving as regulator. Therefore, in Hilbert's reasoning, the question of how to establish the reliability of the ideal methods (the regulators) by finite means is given great weight, and the consistency of extended systems proves to be an item of central importance. Hilbert attempted to secure the ideal (i.e., infinitary) parts of mathematics by formalizing them, then calling for a proof of their reliability. He believed that this could be done by finitary means alone [**52**, pp. 170–171]. In pursuing this objective (i.e., the justification of infinite deductions), Hilbert finally approached Kronecker's finitism to a certain extent: once metamathematics was taken to be a weak part of arithmetic, it closely corresponded to finitary mathematics à la Kronecker [**56**, p. 487].

11. SYZYGIES AS A POSSIBLE PARADIGM FOR SIMPLICITY. It may be very hard to define the simplicity of proofs, but we nevertheless recognize "simplicity" in proofs when we see it. Different criteria for assessing simplicity (for example, proof length [**27**], total number of symbols in the proof, the absence of certain terms, the number of basic operations) may become relevant as the context changes so that different facets of simplicity may be emphasized by some criteria, downplayed by others (see [**75**:6, chap. 7]). In an interesting historical remark on simplicity in geometry, René Descartes (1596–1650) discussed in his *La Géométrie* (1637) the question of when one curve is simpler than another (see [**6**, chap. 25]). Keeping all these factors in mind, what *features of simplicity* can we define precisely?

The introduction of measures of simplicity for proofs is, to be sure, a delicate business. As mentioned earlier, Hilbert was guided by his investigations of invariants [**1**], in which he made use of special algebraic objects known as *syzygies* ([**74**, pp. 163–183] gives a comprehensive survey of the old theory). Geometric facts that are independent of the coordinate system—in other words, invariants—can frequently be expressed through the requirement that some related algebraic condition be satisfied. For example, in Euclidean geometry, the relevant invariants are embodied in quantities that are not altered by geometric transformations such as rotations, dilations, and reflections. In analytical terms, the invariants in question are invariants of tensors or, to employ the terminology of Hilbert's time, invariants of an n-ary form of degree m under linear transformations ([**39**], [**42**]; see also [**74**], [**79**], [**89**]). Old papers in invariant theory typically consisted of masses of endless algorithmic computations, whereas by viewing invariants in a broader framework Hilbert proved his theorems in a few pages, almost without calculations (see [**40**], [**74**], [**79**], [**89**]).

In algebra, the term "syzygy" is used to signify a relationship. Starting with a polynomial ring $R = K[x_1, x_2, \ldots, x_n]$ over a field K, Hilbert had shown—we use modern terminology (see [**61**, chap. 8, sec. 4], [**111**, p. 251])—that the set of all invariants i form an ideal I of R and any invariant i can be represented by a *finite basis* $i_1, i_2, \ldots, i_k$ so that all invariants are integral rational functions of the generators (basic invariants). However, at the same time these basic invariants are not algebraically independent: they belong to the zero-sets of certain polynomial relations, the syzygies of the preceding paragraph. The collection of syzygies is closed under the operations of addition and multiplication. Moreover, syzygies constitute an ideal in the relevant polynomial ring. The syzygy ideal itself has a finite basis, the elements of which are not always algebraically independent. Thus, one obtains second-order syzygies, and so on.

In the theory it is proved that the foregoing cascade of syzygies comes to a halt in a finite basis whose members are algebraically independent after at most k steps, where k is the number of invariants of the basis [**13**, chap. 3], [**74**], [**79**]. (In his papers [**1**], Hilbert actually proved a stop in "at most $k+1$" steps; see also [**39**], [**42**, lecture 47].) Hilbert extended this result by establishing that any ideal I in a polynomial ring $R = K[x_1, x_2, \ldots, x_n]$ over a field K (or over a ring of integers) is finitely generated, a fact now known as Hilbert's Basis Theorem [**1**], [**42**, lecture 35], [**58**:2, no. 16, pp. 199–270] (for generalizations, see [**88**], [**8**:2, pp. 431–444], [**61**, pp. 387, 391], and [**92**]).

Accordingly, this special part of algebra can be placed in the kind of finitary framework we have been discussing. As to the reduction of proofs to an algebraic calculus, it may well be that what Hilbert had in mind was to generalize the situation described by the basis theorem (see [**48**, p. 413], [**17**, p. 1113], and [**18**, p. 196]). In a lecture "Theorie der algebraischen Invarianten nebst Anwendungen auf Geometrie" [Theory of algebraic invariants together with applications to geometry] [**42**] that Hilbert delivered in 1897 when his research in invariant theory had been completed, he remarked: "With each mathematical theorem, three things are to be distinguished. First, one needs to settle the basic question of whether the theorem is valid .... Second, one can ask whether there is any way to determine how many operations are needed at most to carry out the assertion of the theorem. Kronecker has particularly emphasized the question of whether one can carry it out in a finite number of steps" [**42**, lecture 37]. Consistent with this statement is the following quotation taken from the same collection of lectures: "It can also happen that a given invariant has several different symbolic representations. When making symbolic calculations, one of course chooses the simplest one" [**42**, lecture 31] (for the symbolic representation see [**89**, p. 22]).

Moreover, Hilbert's theory of algebraic number fields was partly foreshadowed by certain finite investigations of so-called algebraic modular systems (Modulsysteme) by Kronecker [**13**, p. 147]. Hilbert might have felt that these investigations could serve as an example of a finite theory [**56**, p. 487]. In view of the twenty-fourth problem, it could well be that Hilbert's general outline had its roots in the structure of the aforementioned systems.

12. CONCLUSIONS. His biographer Otto Blumenthal (1876–1944) stressed that Hilbert was a man of problems [**58**:3, p. 405]. This meant that Hilbert's starting points were always simple, but important problems. I believe that this way to create a theory will salvage essential parts of his proof theory and metamathematics. Indeed, the methods of proof theory are now playing, not surprisingly, a significant role in computer science. Moreover, despite Gödel's results that reveal the goal of the original Hilbert program to be unattainable, a modified Hilbert program did lead to the development of proof theory, metamathematics, and decision theory (or, as it is sometimes called, computability theory).

In the end, neither Hilbert nor his staunchest adversary on foundational issues, Luitzen Egbert Brouwer (1881–1966), felt any more obliged than most mathematicians today to adhere to restrictive philosophical doctrines in their "everyday" mathematical research, despite the caveat of Gödel's triumph. A striking example is found in Brouwer's topological research—at least until 1917—in which the use of geometric intuition plays a vital role (as it does in every contemporary mathematician's geometrical paper) [**64**, pp. 145, 148–156].

Hilbert believed in the ultimate efficacy of (finite) mathematics. His writings, lectures, letters, and discussions display the full conviction that well-posed mathematical problems are always questions with meaningful answers in the same way that questions about physical reality have answers. As to parallels between nature and thought, Hilbert stated in his mathematical notebook: "Between thought [Denken] and event [Geschehen] there is no fundamental and no quantitative difference. This explains the pre-established harmony [between thought and reality] and the fact that simple experimental laws generate ever simpler theories" [**38**:3, p. 95] (see also [**56**, p. 485]). We have mentioned earlier that Hilbert believed thought and nature to be finite. Later on, Hilbert became convinced of this metaphysical principle: there is a realm beyond phenomena, and the universe is governed in such a way that a maximum of simplicity and perfection is realized (compare also Einstein's view in [**78**]).

The idea of the infinite had stirred men's emotion like no other subject ([**52**, p. 163]; see also [**87**, p. 176]). Hilbert broached the old question of the limits of human ability to handle the infinite. Can we grasp the mathematical infinite in finite terms? Is it possible for a special problem or, beyond that, for a whole theory? Or is it a contradiction in itself? Suppose that in mathematics we could eliminate ideal conceptions (for example, the continuum), i.e., express them in finite form. Would it then be possible to exploit this finiteness to establish a framework for gauging the simplicity of proofs? Or, if we insist upon remaining in the realm of the finite, could we then carry out all the necessary mathematical reasoning? Can the limited human brain even begin to grasp in finite terms all the theorems in mathematics that stand in need of proof?

How could this great mathematician ever have believed that such dreams would be realized [**20**, p. 1056]? Does it appear that after Hilbert's discovery of the extremely general finiteness principle upon which his proof of the basis theorem was based, Hilbert was overly optimistic about finiteness results in other algebraic and even in foundational contexts? Was Hilbert's unwavering belief in the power of thought just naive? Was it indebted to the widespread belief in irresistible progress that prevailed at the turn of the twentieth century, part of the Zeitgeist? David Mumford, Fields medalist in 1974, remarked in a paper "The Finite Generation of Subrings Such As Rings of Invariants" dealing with Hilbert's fourteenth problem [**8**:2, pp. 431–432]: "However my belief is that it [Hilbert's belief in finiteness] was not at all a blind alley: that on the one hand its failure reveals some very significant and far-reaching subtleties in the category of varieties .... In fact, my guess is that it was Hilbert's idea to take a question that heretofore had been considered only in the narrow context of invariant theory and thrust it out into a much broader context where it invited geometric analysis and where its success or failure had to have far-reaching algebro-geometric significance." We can add this to it: Hilbert's philosophical ideas led him to believe that through an extension of these finite properties he had first become aware of in invariant theory it could be possible to establish the foundations of mathematics by finitary means. The price one normally pays for aiming impossibly high but, like Sisyphus, repeatedly falling short of one's objective is despair. Yet, even after Gödel's results, which set up impassable roadblocks to the achievement of Hilbert's goal, the aging Hilbert was full of hope that these foundational objectives were attainable in a modified sense. From our viewpoint, however, a distinction is to be made between the program Hilbert intended to carry out and its realizable part [**94**].

We extract another quotation from Hilbert's Paris lecture [**37**, p. 444]: "Occasionally it happens that we seek the solution under insufficient presuppositions or in an incorrect sense, and for this reason do not succeed. The problem then arises: to show the impossibility of the solution under the given hypotheses." What does simplest proof mean in this context—the simplest counterexample? If we consider the last quotation in relation to the second problem on the Hilbert list, the Continuum Problem, then a remark of Gödel, who had profound insights into the problem, is most interesting. I quote from a letter of Gödel to Constance Reid [**28**, 4 June 1969], [**28**, 22 March 1969] (see also [**87**, p. 218]): "It is frequently overlooked that, disregarding questions of detail, one quite important *general* idea of his has proved perfectly correct, namely that the Continuum Problem will require for its solution entirely new methods deriving from the foundation of mathematics. This, in particular, would seem to imply (although Hilbert did not say so explicitly) that the Continuum Hypothesis is undecidable from the usual axioms of set theory." Evidently, the open-minded Hilbert was not so misguided by preconceived notions to think that something he wished to be true had to be true (as is sometime maintained; see [**20**, p. 1057]).

Despite the fact that problems form the basis of mathematics and determine its progress, for Hilbert they were not the be-all and end-all. Mathematics is more than a collection of isolated problems. It is only the mathematical method that prepares the ground for mathematics, highlights the basic ideas, and finally makes mathematics more than a hodgepodge of problems. On the other hand, Hilbert was fully aware of the vital part problems play [**37**, p. 444]: "He who seeks for methods without having a definite problem in mind seeks for the most part in vain."

Mathematics is not unlimited in scope, true, but for all its limitations it does continue to make progress. Despite an ever-growing diversity of mathematical areas and a rapidly expanding body of mathematical knowledge, the simplification of proofs by axiomatic methods has made mathematics as a whole a more efficient and unified enterprise. "Axiomatics is the rhythm that makes music of the method, the magic wand that directs all individual efforts to a common goal" [**38**:3, p. 93]. From the beginning, for Hilbert such a common goal was the unity of mathematics ("the science of mathematics ... is an indivisible whole, an organism whose ability to survive rests on the connection between its parts" (translation by Weyl [**109**:4, p. 123]) in which axiomatics, proof theory, and metamathematics are but distinct parts.

No one will dispute the fact that Hilbert's spirit and influence have played important roles in mathematics. Although a naive assumption that progress is inevitable (in mathematics or otherwise) no longer prevails and, with the passage of time, Hilbert's once highly acclaimed work on the foundations of mathematics is looked upon more critically, we should respect the past—not play the tempting role of "a backward-looking prophet" (Friedrich Wilhelm Schelling, 1775–1806) and demean the past by making unfair comparisons with the present.

Hermann Weyl's oft-cited poetic remark sums up the impact of Hilbert's contributions [**108**:4, p. 132]: "I seem to hear in them from afar the sweet flute of the Pied Piper that Hilbert was, seducing so many rats to follow him into the deep river of mathematics." By introducing his problems, Hilbert himself stoked the fires of our mathematical enthusiasm. In a lecture, he once pointed out to the audience that in mathematics we cannot use and furthermore do not need such lame excuses as "the ladder is too short" or "the experiments are too expensive" [**50**, p. 24]. On another occasion, he stirred his listeners with the words: "We hear within us the perpetual call: There is the problem. Seek its solution. You can find it by pure reason, for in mathematics there is no *ignorabimus*" [**37**, p. 445].

That is what Hilbert believed. Clearly, because of Gödel's results, there are some objections to Hilbert's profession of faith in mathematics. Nevertheless, mathematicians will forever find inspiration in the optimistic tone sounded in the famous line from his

Paris address [**37**, p. 445] that was engraved on his tombstone (see [**87**, p. 220]):

> "Wir müssen wissen, wir werden wissen" [We must know, we shall know].

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