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The Hilbert Edition: What is it and what is it good for?

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Résumé : Le Mathematical Institute (MI) de l'université de Göttingen et la Niedersächsische Staats- und Universitätsbibliothek (SUB) possèdent ensemble une impressionnante collection de documents de David Hilbert, qui est peut-être le mathématicien le plus important de la période 1885-1930, un spécialiste notable de physique mathématique. Ce fonds sera appelé ici le *Nachlaß* de Hilbert en un sens étendu. Outre des lettres et bien d'autres documents, le *Nachlaß* contient une grande collection d'autographes et de textes dactylographiés de conférences données par Hilbert tout au long de sa carrière. Les manuscrits des conférences portant sur les fondements des mathématiques et des sciences de la nature forment la base d'une édition (à laquelle nous référons comme l'« Édition Hilbert ») en six volumes : un volume sur les fondements de la géométrie (Vol. 1), deux sur les fondements de la logique et de l'arithmétique (Vol. 2, 3), deux sur les fondements de la physique (Vol. 4, 5), et un volume de sélections des carnets et d'exposés plus généraux de Hilbert. L'Édition contiendra en grande partie des documents inédits, quoique s'y ajoutent par endroits les publications centrales des champs concernés. En publiant ces documents, nous espérons promouvoir une vision renouvelée et plus complète des réalisations scientifiques de Hilbert, comme de son point de vue philosophique. L'article discute les objectifs scientifiques de l'Édition Hilbert, et les critères de sélection ayant présidé au choix des documents.

Abstract: The Mathematical Institute (MI) of the University of Göttingen and the Niedersächsische Staats- und Universitätsbibliothek (SUB) together possess an impressive collection of documents by David Hilbert, perhaps the most important mathematicians of the period 1885-1930, and a very significant mathematical physicist. These holdings will be called here the *Nachlaß* of D. Hilbert in an extended sense. Besides letters and many other documents, the *Nachlaß* contains a vast collection of autographs and typescripts of lectures held by Hilbert across his whole career. The lecture manuscripts concerned with the foundation of mathematics and natural science form the basis of an Edition (henceforth referred to as the "Hilbert Edition") in six volumes, one

* I would like to thank one of my fellow General Editors, Michael Hallett, for his comments, and for helping to put what follows into less stilted English. Nevertheless, as I make clear below, the views represented in this essay are my own.

volume on the foundations of geometry (Vol. 1), two on the foundations of logic and arithmetic (Vols. 2, 3), two on the foundations of physics (Vols. 4, 5) and one volume, containing selections from Hilbert's Notebooks and more general lectures. The Edition will contain largely material previously unpublished, although this is supplemented in a few places by central publications in the respective field. By publishing this material, we hope to foster a new and more complete view of Hilbert's scientific achievements and his philosophical viewpoint. The following paper discusses the scientific aims of the Hilbert Edition, and the selection criteria for the inclusion of documents in it.

Let me begin with a somewhat provocative remark:

The edition of the *Nachlaß* of a scientific author is not a goal in itself, but only a means to an end in achieving a certain goal or collection of goals.

The question consequently is: What was our² goal in deciding to publish a significant part of David Hilbert's *Nachlaß*? In answering this question, I will draw on the many discussions conducted in the formative stages of the project, i.e., before the Hilbert Edition properly speaking was underway, as well as discussions held during the Edition's subsequent evolution. Let me stress at the outset, however, that what is presented here is the author's point of view, and does not necessarily reflect that of the other editors. Two meta-remarks are necessary to prevent obvious misunderstanding of the initial remark above.

First, note that reference was made quite consciously to the *Nachlaß* of a scientific author, as opposed to the papers and documents of an important literary figure, for example those of a novelist or poet; matters may be very different in cases such as these.³ In case of a poet, for example, it may be a goal in itself to publish the *Nachlaß*, in particular if the author did not have the chance to publish in his lifetime everything he wished to see published, or if there are, say, important works in an incomplete state. But, generally speaking, things are somewhat different where a scientific author is concerned. Normally, a scientific author does during his career publish what he wants to be published, or more precisely, what the scientific community of his time regards as valuable or worthwhile to have published. The rest is kept in a

2. By "our" goal, I mean primarily the goal of the four General Editors, who began the project nearly twenty years ago, though this goal is also shared by all the other editorial collaborators and advisors who have been indispensable in keeping the project alive.

3. Here I use "scientific" in the sense of the German term "wissenschaftlich", a term which is to be applied to any work concerned with the search for truth, including the work of historians, economists, and so on, and not just to works in the natural sciences.

drawer, either deliberately so or, in some cases, necessarily so, perhaps because a paper was rejected by a journal, or a book proposal turned down by a publisher or only partially finished, or where it is a question of work overtaken by subsequent scientific developments. These differences must be born in mind when embarking on an editorial project, particularly one that overrides the intentions of the author or the publishing practices of his time. In other words, there must be particular reasons to ignore these intentions and practices. I will come back to this point shortly, because in Hilbert's case there certainly are such particular reasons, or at least we maintain that there are such.

The second point touches on the distinction between the two notions "goal in itself" and "means to an end". This distinction is, of course, not an absolute one, but depends, among other contextual factors, on the perspectives adopted by the editors of the *Nachlaß* in question. This is a crucial point, and one which deserves to be underlined. A means to an end can become a goal in itself and vice versa, depending on the interests of the editors, the intended readership, the epoch in which the edition is published, and so on. Take for example a historian of science. Almost by the demands of his profession, he is inclined to regard the publication of the *Nachlaß* of a scientific author as a goal in itself. On the other hand, before considering the publication of a *Nachlaß*, the philosopher of science, whose primary interest is in the progress of science, will always ask whether the author has contributed in a significant way to discoveries or in some way to the understanding or clarification of the science in question. If the answer here is clearly negative, then the consideration of that figure will be abandoned or postponed in favour of others who did contribute to the field in question. On the other hand, if the answer is positive, even then there is a significant further question to be addressed, namely are there sufficiently many important unpublished pieces in the *Nachlaß* to justify a publication of a partial or full *Nachlaß* edition, or should just a few individual pieces be published separately, or even dealt with piecemeal in a series of expository articles.⁴

4. Tilman Sauer of the Einstein Edition has criticized my opposition of the historian with the philosopher of science as somewhat "exaggerated". This is certainly true; for the sake of clarity, I have somewhat overdrawn the opposition, though, I contend, not by too much. My real view can be summarized briefly as follows. The historian of science must certainly take into account constraints such as the literary quality of a scientific author, his reputation as a scientist, the originality of his ideas, and so on. In this sense, then, the edition of a *Nachlaß* will never be a "goal in itself", even for the historian of science. Nevertheless, the historian is more inclined than the philosopher of science to investigate authors whose contributions do not lie on the main highway of scientific progress, but rather on a minor road, or even at times on a dead-end street. The philosopher of science, on the other hand, is primarily interested in those authors who have contributed significantly to the actual progress of science and our understanding of nature. Having said this, though, it is not always clear which road represents the main stream, and for this reason the philosopher sometimes might also have to consider authors who pursued by-ways. This can give

The opposition between these points of view is, of course, not a strict one, and is presented here only as a way of pointing to some of the complexities. In many cases, it is a matter of taste and experience whether an author is judged as interesting, or as rather pedestrian and of lesser significance. It often enough transpires subsequently that a previous judgment was quite inappropriate, and that the author in question was either over- or underestimated. In spite of such errors, and other imponderables, it is very important to be clear about the goals that one wants to achieve with the edition of a scientific *Nachlaß* before embarking on the task. This cannot be stated too strongly, and brings us back to the main question, namely what was the goal behind the decision to publish the Hilbert Edition, and why only a part of the Hilbert *Nachlaß*, and why this part and not another?

Unfortunately, this simple question cannot be answered directly because the answer is, in a certain sense, context dependent, and this dependence must first be outlined in order to make the answer intelligible. The context has several dimensions: there were several different circumstances which, taken together, influenced the Editors, and which were behind the decision to publish only a certain part of Hilbert's *Nachlaß*. These circumstances were: (1) the time at which the publication was being considered by the initiators of the project; (2) the possible sources of organizational and of financial support; (3) the different education and professional interests of the Editors; (4) the question of the possible readership, and (5) lastly, the interdisciplinary dimension of Hilbert's work. Of course, not all these circumstances had the same weight, and I will sketch only the three most important ones and their interrelations. Let me begin with the time factor.

The idea of publishing part of Hilbert's *Nachlaß* was conceived by myself, Michael Hallett and William Ewald, all rather independently, in the second part of the 1980s. We each realized that there was a wealth of interesting and potentially important unpublished material in Hilbert's *Nachlaß*, which deserved consideration by the broader scientific community. The loose ideas then received more concrete form in discussions and plans between Hallett and myself in 1991. This period of deliberation is notable, because Hilbert's fame as a leading mathematician, one who had "impressed the seal of his spirit" on the mathematics of the twentieth century, had "sunken below the horizon" (to use a formulation of Hermann Weyl [Weyl 1944]), while at the same time a new interest in Hilbert as a truly universal scientist had begun to glimmer as a new dawn. More detail surrounding this complex situation will follow later; for the time being, it suffices to keep in mind the change of Hilbert's reputation as being similar to a sunset at the polar circle.

Certainly, the most important circumstance regarding the edition of Hilbert's *Nachlaß* was the fact that the Editors initiating the project are by profession philosophers and not, as one might perhaps expect, mathemati-

birth to editions of material by secondary authors. The edition of such material is then a quasi "goal in itself".

cians or historians of science proper. This was not only crucial for the decision to publish only a *part* of Hilbert's *Nachlaß*, namely his writings on the foundations of mathematics and science, and thus to ignore his writings on pure mathematics, but it also determined the particular goals of the Edition. (See below.) The second and equally important circumstance is the interdisciplinary character of Hilbert's work. This important aspect is often overlooked; it was, for the Editors, one of the main reasons for publishing precisely that part of Hilbert's *Nachlaß* which showed most vividly his simultaneous interest in the foundations of mathematics and physics, as well as in the attendant philosophical questions. In fact, in our view, Hilbert reveals himself in the *Nachlaß* as an important philosopher as well as a scientist and an outstanding mathematician.

Let me now address some important features of the intellectual background of the 1990s and outline the more specific reasons, which made it important, at least from a philosophical point of view, to edit a considerable selection of Hilbert's *Nachlaß*.

There are two central points, which have to be outlined. The first touches on a rather general aspect of philosophy towards the end of the twentieth century; the second concerns a specific difficulty in orthodox philosophy of science following the appearance of Kuhn's work *The Structure of Scientific Revolutions*.

"Analytical philosophy" in the tradition of Frege, Russell, Carnap, and Quine was the dominant school in the second half of the twentieth century. Those who distrusted either the empiricist or the logicist dogmas of this school of philosophy were in the typical position of a scientific minority. Here is not the place to go into all the reasons for distrust; but the most important and serious difficulty was that there seemed to be no reasonable and feasible alternative to Frege's and Russell's views on the philosophy of language in general, and the philosophy of logic and mathematics in particular grounded as these are in "truth-value semantics". This is not to say that there were no *alternative* foundations around. There were too many, in fact; but most of these were fabrications by philosophers and had little to do with the practice of mathematics and almost no contact with the real sciences. In this situation, it seemed to us utterly desirable to highlight one of the "genuine" alternatives, one, which had withstood the tests of science, and which also stood in close contact with the practice of modern mathematics. This was Hilbert's conception of a "Be-weistheorie", which aimed at a "New Foundation of Mathematics".⁵

There are two serious objections, which arise at this point. First, wasn't this program already well known from Hilbert's own publications? Second, and more importantly, wasn't the program a complete failure in the light of Gödel's famous proof that there can be no "finitist" proof of the consistency even of

5. This is a translation of the German expression "Neubegründung der Mathematik", which was used as the title of the essay in which Hilbert informed the mathematical world for the first time of his program for a new foundation of mathematics.

Peano arithmetic? Before addressing these questions, it should be stressed that elucidating Hilbert's program alone, no matter how successful, would itself not be a sufficient reason to publish a significant part of Hilbert's *Nachlaß*. More delicate questions had to be taken into account, questions which centre on the relationship between the work in the *Nachlaß* and the already published work. For example, did Hilbert not have sufficient opportunity to promote his new proof theory? This would seem highly unlikely, given his preeminence and influence. Was his "formalist" point of view misconceived, at least in the eyes of the philosophers, or only misunderstood? Was his "finitist point of view [finite Einstellung]" of a combined foundation for logic and mathematics in fact an untenable position, or did his contemporaries only misconstrue it? Are there genuinely significant differences between the way matters are presented in the *Nachlaß*, and are these differences enough to justify a publication of material from the latter? Would this suffice to correct the most important misunderstandings? And lastly, but by no means least, is there a genuine chance of a reassessment of Hilbert's work and his program, or did developments after the nineteen thirties leave Hilbert's work hopelessly behind?

The extensive Hilbert Edition is well underway [Hilbert 2004a], and from this fact, it is easy to imagine what were concluded as answers to these delicate questions. Nonetheless, let me stress one principal consideration, which was highly important in influencing the decision to pursue an Edition. If the Editors had not been convinced that most of the misgivings about Hilbert's program rested on a set of misunderstandings, most of them crude, although some subtle, our decision would have been different. Only the firm conviction that there are genuine misunderstandings of Hilbert's work and that the publication of a considerable part of the *Nachlaß* offered a real chance of correcting these misunderstandings tipped the balance in favour of an edition. Now, let me turn to the second, more specifically philosophical consideration.

Orthodox philosophy of science with its standard view of the progress of science as a cumulative enterprise had its beginnings in the nineteen twenties, when Schlick, Carnap, and Neurath founded the *Vienna Circle*, and Reichenbach started a similar group in Berlin. Whatever the internal differences, their common goal can be summed up in the slogan "The Overcoming of Metaphysics by Logical Analysis of Language",⁶ or more specifically, the logical analysis of scientific languages and theories. This compelling program never really got off the ground, but turned into a debate about different methodological principles and antithetical methodologies that finally collapsed under the assault of Kuhn's *The Structure of Scientific Revolutions*. Following this, concepts like "meaning-change", "incommensurability of theories" and "shift of paradigms" through scientific revolutions became the new slogans of philosophy of science. But what if one retained sympathy with some of the goals of the old approach,

6. This is the translation of the German "Die Überwindung der Metaphysik durch logische Analyse der Sprache", which was the title of a programmatic essay by Rudolf Carnap [Carnap 1931].

or with the principle of rational continuity of the Vienna Circle, given that the program of the Circle as originally conceived is not viable? This is where Hilbert again becomes of central importance, since his *Nachlaß* contains many lectures (both individual lectures and lecture courses), which provide a logical analysis of physical theories of the highest quality, comparable only with the axiomatic presentation of geometry and its logical analysis given in Hilbert's own book *Foundations of Geometry* of 1899 and later. Indeed, these very different works of Hilbert have much in common; they are tied together by the same methodical procedure, the procedure that Hilbert called the "axiomatic method". To make these lectures public is the second main reason for publishing Hilbert's *Nachlaß*.

This seems to be the right place to enumerate what the *Nachlaß* contains; after this I will discuss briefly why we believe that we can achieve our goals. As already mentioned in the abstract, there exist two important collections of Hilbert documents in Göttingen which together we referred to as Hilbert's *Nachlaß*: a collection of lecture notes (the majority of them typescripts) held in the library of the Mathematical Institute (MI), and then a much more diverse and inhomogeneous set of papers and documents held in the Niedersächsische Staats- und Universitätsbibliothek (SUB). The latter could be called Hilbert's *Nachlaß* in the proper sense, because it contains all the extant autographs, some of Hilbert's own copies of lecture notes prepared for, or from, his lectures, and many other kinds of documents written in Hilbert's hand (e.g., complete lectures delivered to scholarly societies, or to mathematical or philosophical seminars, or fragments and sketches of such), and additionally a great number of letters to Hilbert. There is some overlap between the two collections, since some of the lecture notes in the MI (but by no means all) are also present in the SUB; where there are duplicates, we have relied in most cases on the copies from the SUB, since these contain usually more substantial annotations in Hilbert's hand; where there is no duplication we have, of course, chosen the manuscripts or typescripts from the MI.

There is another, presumably rather uncommon, characteristic of Hilbert's *Nachlaß*, which must be mentioned. The *Nachlaß* contains in all more than 120 distinct sets of lecture notes, most of them well worked out (with the exception of some of Hilbert's early autographs) either by Hilbert himself or by one of his assistants, or sometimes by an advanced student. Indeed, it is no exaggeration to say that these lecture notes make up the main bulk of the significant material in Hilbert's *Nachlaß*. The content of these lecture notes ranges from topics in pure mathematics (e.g., number theory, function theory, etc.), geometry, logic and arithmetic (including proof theory), and furthermore many different branches of classical and modern physics (mechanics, electrodynamics, thermodynamics and statistics, relativity and quantum theory), and in addition some more popular lectures on philosophical questions and problems concerning mathematics and the natural sciences. Given the enormous extent of all this material we had to make a selection, and we decided from

the very beginning that we would only publish works on the foundations of mathematics and physics. This meant three things:

- (i) we had to ignore all the purely mathematical writings (which form, of course, the bulk of Hilbert's *Nachlaß*);
- (ii) we decided not to edit the correspondence, although we make use of it in the annotations, not least because we possess very little of Hilbert's side of the exchanges;
- (iii) we decided to omit all private and/or administrative material, even though some of this is very interesting, e.g., the material concerning Brouwer, or that concerning Husserl, Nelson, etc.

These decisions were, of course, not sufficient by themselves to narrow down the material to a wise and useful selection; further criteria had to be brought to bear. The main problem to be solved is encapsulated in the questions: What counts as “foundational”? And which works count as a contribution to the foundations of a discipline and which do not? There is, of course, no sharp answer to these questions, and they had to be dealt with differently with respect to the different fields. In geometry, the issue is relatively easy, because in this area, since Euclid, we have had an intimate relation between foundational study and the axiomatic approach.⁷ The distinction is already more difficult in the area of pure mathematics because here there is no general agreement as to which theory is fundamental, for example, logic, algebra, set theory or elementary arithmetic. And Hilbert actually occupied himself rather intensively with this question, certainly up until 1920. Consequently, we handled the “criteria of choice” here in a rather liberal way, and have included beside lecture courses in logic (and “logical principles of mathematical thought”), arithmetic and proof theory, courses covering set theory, as well as more general series such as the ones focusing on the “Quadratur des Kreises” and related topics. Moreover, it is important to realize that Hilbert himself changed the nature of “foundational” investigation so that this included the logical analysis of central (i.e., fundamental) theories. Important in this is Hilbert's conception of the “axiomatic method”, first expounded and wonderfully exploited, in his work on geometry in the 1890s. This new kind of foundational investigation was pursued, not just with respect to geometry, analysis, set theory and other central areas of mathematics, but also with respect to physics. This is the most problematic area, since the growth and development of physics in the first half of the twentieth century was so rapid, and consequently no sharp distinction can be drawn between the most advanced research fields and the

7. Let me stress that this is not meant to suggest that progress in geometry can only be achieved via an axiomatic approach; on the contrary, progress in geometry has come in many different and quite unexpected ways. Rather, it means that the search for, and analysis of, the “fundamental” principles is most properly served by the axiomatic approach.

question of the “foundations” of the various disciplines.⁸ For this reason, the distinction between pure and foundational research was in effect ignored and we selected those lectures that, with the wisdom of hindsight, appeared the most progressive and epistemologically interesting.

Quite apart from the general issue of finding reasonable selection criteria, there were special, local problems; I will only mention two of them here. Sometimes the *Nachlaß* contains more than one exemplar of notes of lectures prepared from the very same series by two people apparently working independently. One striking example is the lecture series from 1905 entitled “Logische Principien des mathematischen Denkens”, for which there are two different scripts, one prepared by Max Born and the other by Hilbert’s assistant Ernst Hellinger. This raises the question as to which of the two sets of notes were to be selected for inclusion in the Edition. In this case, the decision was taken to include the Hellinger’s script and not Born’s, since the Hellinger was clearly in Hilbert’s possession, and later in the MI’s, whereas Born’s script was first added to the Hilbert *Nachlaß* long after Hilbert’s death, there being also no evidence that Hilbert was aware of its existence. Moreover, Hellinger’s script is slightly more extensive than Born’s, although they do not disagree radically in content. Note, though, that the Born’s script is to be referred to in the editorial apparatus whenever it deviates from Hellinger’s in a significant way.

The second problem is more delicate. Frequently, Hilbert annotated a set of lecture notes twice and sometimes even three times. When the changes involve only relatively minor alterations and corrections, this poses no great problem. But often the changes are quite extensive in the sense that they imply a conceptual reconstruction of certain sections or chapters, or even of the whole tenor of the lectures. This means that there is not one but several layers of text and, hence, the question arises as to which layer is to be given priority: the first, the last, or all of them? (There is, of course, also a further delicate question about the determination of the “layer” to which a particular annotation belongs, but we will leave this aside here.) This problem becomes particularly pressing if the first (i.e., the original) layer is written in Hilbert’s own hand, because in these cases it is often extremely difficult to decide whether an annotation properly belongs to the original exposition, or is a later addition. Frequently the question can only be answered conjecturally, and even then, only by recourse to the respective contents, which, of course, involves a certain methodological circularity.

Let me return to our particular goals in the different fields of Hilbert’s foundational work, and explain what the main point of view is, and why we are convinced that the goals are attainable, certainly in the long run. We begin with geometry and with the *prima facie* simple question as to what Hilbert’s new axiomatic approach to geometry, the so-called “axiomatic method”, is.

8. Indeed, Hilbert’s best known contributions to the initial development of general relativity was entitled “Die Grundlagen der Physik” (Erste und Zweite Mitteilung).

Following Hilbert's epoch-making book *The Foundations of Geometry* (first published in 1899 as a *Festschrift* to celebrate the unveiling of a monument in commemorating Weber's and Gauss's work on the electric telegraph), the logical positivists propounded a view of Hilbert's new axiomatic method, a view which is at best a caricature of Hilbert's own. This is not the place to recapitulate the position presented by the positivists, but it is important to stress one point. The positivists saw a sharp contrast between Hilbert's "new" axiomatic approach and Euclid's "old" axiomatic procedure, a view that was definitely not Hilbert's. On the contrary, he saw his own approach as a continuation and an improvement of Euclid's way of proceeding in the *Elements*. And this leads to the decisive point regarding the material on geometry from Hilbert's *Nachlaß* which the Edition has published [Hilbert 2004b]: If you consult only Hilbert's published work in geometry, i.e., the *Festschrift* and four or five essays, then it is not possible to see immediately that, and why, the "caricature" given by the logical empiricists is not Hilbert's own view. But this recognition is fairly clear once all the lecture courses and lecture notes leading up to the publication of the *Festschrift* are taken together. With these one can see how Hilbert developed his axiomatic method step by step, until he could show, by means of this method, the logical independence of every axiom of his system of Euclidean geometry and finally the system's "completeness", as Hilbert called the categoricity of Euclidean geometry. Important here, too, is the way Hilbert analyzed the logical role of many of the central elementary propositions of the Euclidean system, often adding to this a refined analysis of the geometrical "intuition" behind both these propositions and their proofs. We believe that the reception and understanding of Hilbert's work on geometry will be substantially altered in the desired direction by the publication of this work on geometry.

Another field about which we aim to correct views concerns Hilbert's alleged formalist position in the foundations of mathematics. Three positions are usually distinguished in the philosophical debate about the epistemological foundations of mathematics, the Logicism of Frege and Russell, Brouwer's Intuitionism, and Hilbert's Formalism. As the last "-ism" indicates, Hilbert is usually regarded as the father of the modern formalistic school in the foundations of mathematics that stands in strong opposition to Logicism and Intuitionism. Here again we encounter a certain misunderstanding, but this time a subtler one, because Hilbert indeed criticized both alternative approaches. But from this it does not follow that Hilbert himself was a "formalist", at least not in the way in which his opponents tried to characterize his position. Unfortunately, Hilbert himself fostered this misunderstanding by occasional remarks in his published papers, in which he tried to explain the idea behind his new proof theory, namely the elimination of all questions of meaning by a complete "transformation" of the semantic dimension of a mathematical theory and its language into a set of well-defined syntactical rules. But once the

relevant lecture notes from the *Nachlaß* are published,⁹ it will be recognized quite clearly that Hilbert's idea of a proof theory has nothing to do with the kind of mindless formalism that his critics allege against his "New Foundations of Mathematics", this quite aside from the matter of whether this program was successful or not.

Finally, the third area which we hope will be illuminated by the publication of part of the *Nachlaß* is the vast area of the foundations of natural science, or more properly speaking, the different fields of physics and their respective foundations. Here, it is not so much that Hilbert's contributions are misunderstood, but more that they are not known. The first reason for this is Hilbert's own restrictive policy concerning publications in physics, and the second reason is to be found in an undeniable antipathy on the part of some physicists against Hilbert, who was seen as a pure mathematician, merely demonstrating his abilities in the playground of physics.¹⁰ Hilbert published just a handful of papers in physics (if we count only the original papers and not their reissues), and this moreover well into the second half of his research career. From the *Nachlaß*, however, we know that he gave his first lecture in physics (actually on classical mechanics) as early as 1898, and from that time continued to lecture on physical subjects almost every semester, covering all the principal branches of modern physics, including classical mechanics, thermodynamics, statistical mechanics, electrodynamics, the general theory of relativity and modern quantum theory.¹¹ This wealth of material has meant that we have had to make a serious selection, a selection which we hope will nevertheless lift the veil of ignorance surrounding Hilbert's contributions to, and logical clarifications of, the foundations of physics. This material has been put together in Volumes 4 and 5 of the Edition; the latter has recently been published as *David Hilbert's Lectures on the Foundations of Physics, 1915-1927* [Hilbert 2009].

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9. All these lectures are contained in Volume 3 of the Hilbert Edition; this will appear in early 2010.

10. In 1922, Blumenthal referred to Hilbert's "paradox": "Physics is far too difficult for the physicist", meaning above all that foundational analysis is too difficult.

11. For a complete list of all lecture courses Hilbert held throughout his career at both Königsberg and Göttingen, see "Hilbert Lecture Courses 1886-1934", in either Volume 1 or Volume 5 of the Hilbert Edition. This list will be published in each of the forthcoming volumes.

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