The Symbolic Universe

Geometry and Physics 1890–1930

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OXFORD
UNIVERSITY PRESS

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6 Hilbert and physics (1900–1915)

Leo Corry[1]

Some mathematical-physical theories look to me like a toy,
that a child has completely messed up and that every three minutes
needs to be fixed again, in order to keep it working.
David Hilbert, Tagebuch[2]

Introduction

The name of David Hilbert (1862–1943) usually arises in connection with the history of physics in three different and rather circumscribed contexts. The first of these is the famous list of twenty three problems that Hilbert presented in 1900 in Paris, on the occasion of the Second International Congress of Mathematicians. The sixth problem of this list deals specifically with physics. Only one year before the Paris Congress Hilbert had published his seminal study on the foundations of geometry, *Die Grundlagen der Geometrie*. The axiomatic approach adopted by Hilbert in this book was to have an enormous influence on the development of twentieth century mathematics and on the way mathematicians looked at their science. Hilbert's sixth problem was the suggestion that an analysis similar to that performed for the case of geometry in the *Grundlagen* should also be applied to individual physical disciplines.

The problems in this list became an object of great mathematical and historical interest over the following decades, with mathematicians of all specialties and of all countries attempting to solve them, and periodically accounts of the current state of research on some or all of them appeared. However, this particular problem, the sixth one, received much less attention than any of the other twenty two in the list. Working mathematicians generally directed relatively little effort to solve it.[3] Nor have historians tried very hard to understand its roots and whatever attempts have been made to address it

1 The first five Sections of this chapter include condensed versions of the contents of Corry (1997a,b).

2 'Manche math-physikalische Theorie erscheint mir wie ein Kinderspielzeug, dass in Unordnung geraten ist und alle 3 Minuten wieder aufgerichtet werden muss, damit es weiter geht.' The date of this quotation is unknown, but may be somewhat before 1900. The

after 1900.[4] This problem seems to have been largely considered as an accessory, that can only artificially be seen as part of Hilbert's otherwise comprehensive and harmonious image of mathematics.

The second context is the kinetic theory of gases. In 1912, at a time when most of Hilbert's energies were directed to his work on the theory of linear integral equations, he solved the so-called Boltzmann equation. Although this represented a major contribution to the development of this particular physical discipline, Hilbert is often considered to have had no real interest in the kinetic theory as such. Rather, his solution of the equation has been considered as an isolated, if important, furtive incursion into this field. In his authoritative account of the development of the kinetic theory, Stephen G. Brush dedicated one short section to describing Hilbert's contribution. Brush's assessment of Hilbert's motivations is expressed in the following passage:

> When Hilbert decided to include a chapter on kinetic theory in his treatise on integral equations, it does not appear that he had any particular interest in the physical problems associated with gases. He did not try to make any detailed calculations of gas properties, and did not discuss the basic issues such as the nature of irreversibility and the validity of mechanical interpretations which had exercised the mathematician Ernst Zermelo in his debate with Boltzmann in 1896–97. A few years later, when Hilbert presented his views on the contemporary problems of physics, he did not even mention kinetic theory. We must therefore conclude that he was simply looking for another possible application of his mathematical theories, and when he had succeeded in finding and characterizing a special class of solutions (later called 'normal')... his interest in the Boltzmann equation and in kinetic theory was exhausted.
>
> (Brush 1976, p. 448)

Brush added that Hilbert did encourage some of his students to work on mathematical problems connected with the theory, and that he seems to have also taught courses on this issue. Yet these qualifications did not change Brush's overall evaluation of Hilbert's motivations. It seems to me that Brush's assessment manifests the widely held beliefs about the nature of Hilbert's contribution to physics.

The third context in which Hilbert's name has been associated with a significant development in physics is general relativity. On November 20, 1915, Hilbert presented to the Royal Scientific Society in Göttingen his version of the field equations of gravitation, in the framework of what he saw as an axiomatically formulated foundation for the whole of physics. During that same month of November, Einstein had been struggling with the final stages of his own effort to formulate the generally covariant equations that lie at the heart of the general theory of relativity. His struggle had spanned at least three years of intense work and included the publication of several previous

versions, each of which Einstein found inadequate for different reasons.[5] In November 1915 he presented three different versions at the weekly meetings of the Prussian Academy of Sciences in Berlin, before attaining his final version, on November 25, that is, five days after Hilbert had presented his theory.

Einstein had visited Göttingen in the summer of 1915, to lecture on the progress and the difficulties encountered in his work. Hilbert was then in the audience and Einstein was greatly impressed by him. He felt that in Göttingen his work had been fully understood down to the details.[6] Hilbert's involvement in the problems associated with general relativity has usually been traced back to no earlier than this visit of Einstein's or, at best, to the years immediately preceding it. Although the axiomatic character of Hilbert's work on relativity has often been stressed—and by implication it has been associated with his work on the foundations of geometry and on mathematics at large—its actual place as part of his overall conception of mathematics and science has not been fully clarified. As in the case of the kinetic theory, this contribution of Hilbert has mainly been seen as a furtive incursion in physics, aimed at illustrating the power and the scope of validity of the 'axiomatic method' (whatever is meant by that) and as a test of Hilbert's mathematical abilities.[7]

A source quoted very often when describing the various aspects of Hilbert's career—and which also refers to his work on physics—is a passage taken from Hermann Weyl's obituary of Hilbert (Weyl, 1944). According to this passage, Hilbert's mathematical career covered five clearly discernible periods during which his attention focused on a single issue each time: (1) theory of invariants (1885–1893); (2) theory of algebraic number fields (1893–1898); (3) foundations, (a) of geometry (1898–1902), (b) of mathematics in general (1922–1930); (4) integral equations (1902–1912); (5) physics (1910–1922) (Weyl, 1944, p. 619). According to Weyl, the passage from one period to the next always signified a sharp departure from Hilbert's past topic of interest in order to move into a completely new one. For instance, concerning his research on the foundations of geometry Weyl wrote (1944, p. 635): '[T]here could not have been a more complete break than the one dividing Hilbert's last paper on the theory of number fields from his classical book *Grundlagen der Geometrie*.' Thus, even if Weyl conceded that Hilbert's scientific interests focused during a relatively long period of time on physics alone (twelve years, two of which overlap with his work on linear integral equations), his account still suggests a clear separation between this period and the rest of his long career.

The periodization manifest in Weyl's account indeed reflects rather faithfully the distribution of Hilbert's published work over the years, and

5 See Norton (1984).
6 As he wrote to Sommerfeld upon returning from Göttingen. See Hermann (1968, p. 30).
7 [illegible]

what constituted his main domains of interest at different times. However, the actual scope of Hilbert's current interests is much broader than such an account may suggest. A clear and balanced perception of Hilbert's mathematical world, and in particular of the actual place he accorded to physics as part of it, necessitates a deeper examination of his teaching and institutional activities in Göttingen: his lectures, his seminars, the doctoral dissertations he advised, the activities of the Göttingen Mathematical Society (GMG).[8] As even the most cursory examination of the lists included in the appendix at the end of this article immediately indicates, these activities included a long-standing interest in physical issues that covered all his years at Göttingen. Physics was always accorded a central place in the scientific agenda implemented at Göttingen by Hilbert, by his colleagues and by his students.

Given the astonishing breadth of Hilbert's scientific interests and knowledge, an examination of his scientific activities other than pure research and standard publishing becomes fundamental to any attempt to understand his mathematical views. Hilbert had the ability to attract extremely gifted students and to communicate to them the kind of deep open questions that in his opinion should be investigated in various fields. He also very often provided the inspiration to solve those problems. Hilbert directed no less than 68 doctoral dissertations, 60 of them in the relatively short period between 1898 and 1914.[9] Four or five of these dissertations deal with issues that can be directly related to physics as well as to Hilbert's current scientific interests. Also his lectures and seminars in Göttingen were never mere systematic presentations of well-established knowledge. On the contrary, Hilbert considered the classroom and the seminar meetings as ideal settings for putting forward new, untried ideas and to benefit from the feedback of his students. Later in life Hilbert described the central place he conceded to his teaching in the following terms:

> The closest conceivable connection between research and teaching became a decisive feature of my mathematical activity. The interchange of scientific ideas, the communication of what one found by onesself and the elaboration of what one had heard, was a pivotal aspect of my scientific work since my early years at Königsberg . . . In my lectures, and above all in the seminars, my guiding principle was not to present material in a standard and as smooth as possible a way, so as to help the student keeping clean and orderly notebooks. Above all, I always tried to illuminate the problems and difficulties and to offer a bridge leading to the present open questions. It often happened that in the course of a semester the program of an advanced lecture was completely changed, because I wanted to discuss issues in which I was currently involved as a researcher and which in no way had yet attained their definite formulation.
>
> (Hilbert, 1971, p. 79)

8 In fact, Weyl himself acknowledged in some occasions the centrality of Hilbert's docent activities and the impact of his influence as a teacher as main traits of his scientific legacy. See Sigurdsson (1994), pp. 356–358.

Thus, the notes that Hilbert prepared for his courses provide an essential source for understanding the development of his ideas.

A detailed and comprehensive account of the contents of Hilbert's unpublished lectures on physics, their historical context and their influence is yet to be done.[10] Such an account will probably bring to light many unfinished ideas that Hilbert presented in the classroom to his students which they further developed in their own works, thus leading to central contributions in the evolution of particular physical disciplines. It will perhaps likewise bring to light many ideas that led nowhere. The present paper surveys in general terms—using both published and unpublished sources—the evolution of Hilbert's interest in various physical domains until 1915, and attempts to explain the place that Hilbert attributed to physical issues within his overall conception of mathematics. I claim that understanding these aspects of his work is essential for understanding his view of mathematics. I will also suggest that, given the centrality of Hilbert in the scientific world of Göttingen, and given the centrality of Göttingen in the scientific world at large, understanding Hilbert's influence on other scientists working at that center may appear as fundamental for better understanding the historical context of many central developments in early twentieth century physics.

Hilbert was undoubtedly among the most influential mathematicians at the beginning of this century, if not indeed the most influential one. His name is associated with many results and concepts that play fundamental roles in several, distinct fields of mathematics such as algebra, number theory, functional analysis, mathematical physics, metamathematics, and foundations of geometry. Even more pervasive is the view that associates his name with the formalist approach that came to dominate a considerable part of mathematical practice throughout the twentieth century. It was only relatively recently, however, that historians of mathematics undertook a careful analysis of his work and of his mathematical world, and a more balanced image of the latter has begun to arise from these studies. The present article makes use of that recent scholarship and is also intended as a further contribution to it.

Physics and geometry in Hilbert's early career

Crucial for understanding the place of physics in Hilbert's overall conception of science is his view of geometry as a natural science, similar in most essential aspects to other physical disciplines. This conception—opposed perhaps to the largely accepted view of Hilbert as the champion of a formalistic interpretation of the essence of mathematics—is consistently manifest throughout his career, in spite of many other changes recognizable in the latter. An interest in physics, in geometry, and in the relationship between them was present in Hilbert's mind from very early on and until well towards the end of his career.

Hilbert studied in his native city of Königsberg and he also spent his first years as a young professor there. The University of Königsberg had a long tradition of experimental physics and of mathematical analysis that went back to the days of Franz Ernst Neumann (1798–1895) and Carl Gustav Jacobi (1804–1851).[11] We have no direct evidence, however, to decide whether Hilbert took courses of physics as a student at all. We do know that Hilbert attended courses given by the versatile mathematician Heinrich Weber (1842–1913),[12] whose fields of interest covered mathematical physics as well, but it is unlikely that Hilbert came under his influence in this respect. Hilbert's doctoral advisor was Ferdinand Lindemann (1852–1939), who introduced him mainly into the study of algebraic invariants; Hilbert's interest in physics apparently developed somewhat later. The main sources of overall influence on Hilbert during his Königsberg years came from his friends Adolf Hurwitz (1859–1919) and Hermann Minkowski (1864–1909); the scientific scope of interest of these three young colleagues focused above all on pure mathematical domains, but gradually it also extended so as to concede physics an important place in it.

In 1893 Hilbert wrote his last paper on the theory of invariants and his major research effort shifted now towards a new domain, the theory of algebraic number fields which, however, was not completely distinct from the previous one. Yet, as Michael Toepell's study of the origins of Hilbert's *Grundlagen der Geometrie* has shown (Toepell, 1986), at this time Hilbert was simultaneously pursuing an additional field of interest in a systematic way, namely, the study of the foundations of geometry. This interest was reflected in the courses he taught at that time.[13] Hilbert lectured on geometry in Königsberg for the first time in 1891 and planned to do so again in 1893, but for lack of students registered, however, this latter course was postponed until 1894. A remarkable change occurred in Hilbert's approach to geometry between 1891 and 1893, whereby he gradually moved towards the axiomatic treatment as an appropriate way to address the foundational problems of this discipline. But even in his first lectures of 1891 Hilbert clearly expressed his conception of geometry as a natural science. In its essence, this view was not unlike that of other

11 On the Königsberg school see Klein (1926–27 Vol. 1, pp. 112–115, 216–221); Lorey (1916, pp. 59–64). The workings of the Königsberg physics seminar—initiated in 1834 by Franz Neumann—and its enormous influence on nineteenth-century physics education in Germany are described in great detail in Olesko (1991).

12 For more details on Weber (especially concerning his contributions to algebra) see Corry [illegible]

German geometers—even those who, like Moritz Pasch (1843–1930), had adopted an axiomatic approach in their presentations from very early on (Pasch, 1882).[14] The similarity between geometry and physics, and the different character of the former as compared to other mathematical domains recurrently appears in Hilbert's lectures. In the introduction to his 1891 course, for instance, it is expressed as follows:

> Geometry is the science dealing with the properties of space. It differs essentially from pure mathematical domains such as the theory of numbers, algebra, or the theory of functions. The results of the latter are obtained through pure thinking... The situation is completely different in the case of geometry. I can never penetrate the properties of space by pure reflection, much the same as I can never recognize the basic laws of mechanics, the law of gravitation or any other physical law in this way. Space is not a product of my reflections. Rather, it is given to me through the senses.[15]

When rewriting his lecture notes of 1893 for teaching in 1894, Hilbert added some remarks from which we learn that in the meantime he became acquainted with the ideas put forward in Heinrich Hertz's textbook on *The Principles of Mechanics*, published that same year. Hilbert referred once again to the natural character of geometry and explained the possible role of axioms in elucidating its foundations by making reference to Hertz's characterization of a 'correct' scientific image (*Bild*) or theory. Thus Hilbert wrote:

> Nevertheless the origin [of geometrical knowledge] is in experience. The axioms are, as Hertz would say, images or symbols in our mind, such that consequents of the images are again images of the consequences, i.e., what we can logically deduce from the images is itself valid in nature.[16]

In the same context, Hilbert also pointed out the need for establishing the independence of the axioms of geometry, while alluding to the kind of demand stipulated by Hertz. Stressing once more the objective and factual character of geometry, Hilbert wrote:

> The problem can be formulated as follows: What are the necessary, sufficient, and mutually independent conditions that must be postulated for a system of things, in order that any of their properties correspond to a geometrical fact and, conversely, in order that a complete description and arrangement of all the geometrical facts be possible by means of this system of things.[17]

It is difficult to determine whether Hilbert had indeed mastered the contents of Hertz's book at this stage, and even whether he actually read its

14 See Contro (1976, pp. 284–289). On Pasch, see Gray's essay in this volume.

15 The German original is quoted in Toepell (1986, p. 21). Similar testimonies can be found in many other manuscripts of Hilbert's lectures. Cf., e.g., Toepell (1986, p. 58).

16 Hilbert (1893/94, p. 10): 'Dennoch der Ursprung aus der Erfahrung. Die Axiome sind, wie Herz [sic] sagen würde, Bilde[r] oder Symbole in unserem Geiste, so dass Folgen der Bilder

introduction thoroughly. However, it is likely that whatever acquaintance he made with the basic ideas presented in this book so soon after its publication, it was suggested to Hilbert by his friend Minkowski. Minkowski spent three semesters in Bonn as a student, and he returned to that city after receiving his doctorate in Königsberg in 1885, where he stayed until 1894. While at Bonn, Minkowski became involved in both mathematical and experimental physics.[18] In 1888 he published an article on hydrodynamics that was submitted to the Berlin Academy by Hermann von Helmholtz (Minkowski, 1888). During this period of his life, Minkowski's greatest scientific source of inspiration came from Hertz[19] and he certainly must have communicated this enthusiasm to Hilbert as well.

Hilbert arrived in Göttingen in 1895, at a time when he was completing his major work on algebraic number theory, the *Zahlbericht* (Hilbert, 1897). Algebraic number fields continued to be Hilbert's major field of publication during his first years at this university, but at the same time he organized seminars and lectured on other topics. Between 1895 and 1898 he held joint seminars with Klein on number theory, mechanics and function theory.[20] In 1899 he lectured in Göttingen on the foundations of geometry for the first time. The notes of this course provided the basis for the *Grundlagen der Geometrie*, published in June 1899 as part of a *Festschrift* issued in Göttingen on the occasion of the unveiling of the Gauss–Weber monument. But before that, back in the winter semester of 1898–99, Hilbert taught his first course on a physical topic: mechanics. In the introduction to this course, Hilbert stressed once again the essential affinity between geometry and the natural sciences, and also explained the role that axiomatization should play in the mathematization of the latter. He compared the two domains in the following terms:

> Also geometry [like mechanics] emerges from the observation of nature, from experience. To this extent, it is an *experimental science*... But its experimental foundations are so irrefutably and so *generally acknowledged*, they have been confirmed to such a degree, that no further proof of them is deemed necessary. Moreover, all that is needed is to derive these foundations from a minimal set of *independent axioms* and thus to construct the whole building of geometry by *purely logical means*. In this way [i.e., by means of the axiomatic treatment] geometry is turned into a *pure mathematical* science. In mechanics it is also the case that all physicists recognize its most *basic facts*. But the *arrangement* of the basic concepts is still subject to a change in perception ... and therefore mechanics cannot yet be described today as a *pure mathematical* discipline, at least to the same extent that geometry is. We must strive that it becomes one. We must stretch the limits of pure mathematics ever further, on behalf not only of our mathematical interest, but rather of the interest of science in general.[21]

For Hilbert, then, the difference between geometry and other physical sciences—mechanics in this case—was not one of essence, but rather one of historical stage of development. He saw no reason of principle why an axiomatic analysis of the kind he was then developing for geometry could not eventually be applied to mechanics with similar, useful consequences. Eventually, that is to say, when mechanics had attained a degree of development similar to the present state of geometry, in terms of the quantity and certainty of known results, and in terms of an appreciation of what are really the 'basic facts' on which the theory is based.

Hilbert's first course on mechanics was an elementary one, based on standard presentations, in which he attempted to give a basic overview of the discipline. The long bibliographical list that Hilbert recommended to his students for further reading (Hilbert, 1898–99, pp. 4–5) may be helpful for reconstructing and assessing the degree of his acquaintance with current knowledge of the field. However, one must exercise some care before relying too heavily on these lists as a faithful indicator, given Hilbert's general tendency not to study thoroughly and comprehensively all the existing literature on a topic he was pursuing. On the contrary, Hilbert—as David Rowe has argued—'measured the quality of a mathematician's work by the number of earlier investigations it rendered obsolete.'[22] Still, given the fact that Hilbert had made no specific contributions of his own to mechanics, a detailed account of this bibliographical list, which include several rather uncommon titles, seems to be instructive about his views and his knowledge.

First, Hilbert included in the bibliography four 'classical works': Lagrange's *Méchanique analytique* (1788); Jacobi's *Dynamik* (1843); Kirchhoff's *Mechanik* (1877) and Thomson and Tait's *Theoretische Physik* (in German translations of 1871 and 1886). The textbooks he recommended were the following: *Mechanik* (1880), by someone called Schell, a book he described as 'somewhat antiquated but rich in contents'; *Kinematik, Statik, Dynamik* (1884), by Petersen, 'short and easily comprehensible'; *Cours de Mechanique* 2

21 Hilbert (1898–99, pp. 1–3. Emphasis in the original): 'Auch die Geometrie ist aus der Betrachtung der Natur, aus der Erfahrung hervorgegangen und insofern eine *Experimentalwissenschaft*. ... Aber diese experimentellen Grundlagen sind so unumstösslich und so *allgemein anerkannt*, haben sich so überall bewährt, dass es einer weitere experimentellen Prüfung nicht mehr bedarf und vielmehr alles darauf ankommt diese Grundlagen auf ein geringstes Mass *unabhängiger Axiome* zurückzuführen und hierauf *rein logisch* den ganzen Bau der Geometrie aufzuführen. Also Geometrie ist dadurch eine *rein mathematische* Wiss. geworden. Auch in der Mechanik werden die *Grundthatsachen* von allen Physikern zwar anerkannt. Aber die *Anordnung* der Grundbegriffe ist dennoch dem Wechsel der Auffassungen unterworfen ... so dass die Mechanik auch heute noch nicht, jedenfalls nicht in dem Maasse wie die Geometrie als eine *rein mathematische* Disciplin zu bezeichnen ist. Wir müssen streben, dass sie es wird. Wir

Vols. (1884–86) by Despeyrous and Darboux, 'like Schell's'; and *Analytische Mechanik* (1888), by Otto Rausenberg. In a different section of the bibliographical list, Hilbert mentioned various 'courses': *Elementare Mechanik* (1889), by the Göttingen physicist Woldemar Voigt, 'illuminating from the physical point of view'; Mach's *Principien der Mechanik* (1889); *Mechanik* (1890), by Büdele, 'like Schell and Desperyous–Darboux'; Hertz's *Prinzipien der Mechanik* (1894); 'a memorial (*Denkmal*), in which this young and brilliant physicist presented classical mechanics with Euclidean rigor'; Helmholtz's *Dynamik diskreter Massenpunkte* (1894), reportedly a manuscript of the latter's university lectures; Boltzmann's *Prinzipien der Mechanik* (1897), 'develops the atomistic point of view; opposed to Hertz'; *Dynamik der Systeme starrer Körper* (1897–98), by Routh; and *Traité de méchanique rationelle* 3 Vols. (1893–98), by Appell, 'a comprehensive and systematic handbook'. Hilbert also included a section with 'historical' texts: Düring's *Principien der Mechanik* (1873), and again Mach's book. Finally, his list included three collections of exercises: *Aufgaben aus der analytischen Mechanik* (1879), by Fuhrmann; *Aufgabe aus der theor. Mechanik* (1891), by Zieh; and *Problèmes de méchanique* (1867), by Tullien.

The list is certainly impressive and interesting in itself but, as already said, it is very difficult to know to what extent Hilbert was actually familiar with all the texts and relied on them for his course. In any case, following his lectures of 1899 on the foundations of geometry, Hilbert was to concentrate over the following two years exclusively in this latter topic. Only in the winter semester of 1901–02, did he again teach a course on potential theory. But before retaking that thread, it is relevant to discuss very briefly the contents of the *Grundlagen* and the views put forward in it.

Die Grundlagen der Geometrie

As already said, Hilbert had been involved with questions pertaining the foundations of geometry since 1891. In 1898 Friedrich Schur (1856–1932) proved the so-called theorem of Pappus without using any continuity assumptions. In doing this he reinforced a line of attack initiated in 1891 by Hermann Ludwig Wiener (1857–1939), to which Hilbert had been closely attentive. Wiener had proved that this theorem, together with the so-called theorem of Desargues, suffice to prove the fundamental theorem of projective geometry. The role of continuity assumption in his proofs, however, remained to be fully elucidated. Wiener's ideas had been a main factor in attracting Hilbert's attention to the study of the foundations of geometry in the first place. Schur's proof, on the other hand, seems to have provided the definitive trigger that made him redirect all his efforts to this field of research.[23]

23 For Hilbert's acquaintance with the works of Wiener and Schur, and the events

Thereafter Hilbert undertook a detailed elucidation of the logical interdependence of the various fundamental theorems of projective and Euclidean geometry and, more generally, of the structure of the various kinds of geometries that can be produced under various sets of assumptions.

A main question that stood at the centre of Hilbert's investigation was one that had arisen from the attempts of Felix Klein (1849–1925), beginning in 1871, to coordinatize projective geometry using ideas originally formulated by Arthur Cayley (1821–1895).[24] This coordinatization was essential for providing a connecting link between synthetic and analytic geometry, and its realization depended on the same assumptions necessary for proving the already mentioned fundamental theorems. Following the work of Wiener and Schur, Hilbert focused on understanding the specific role of continuity in those proofs. These kinds of questions provided the main motivations behind the investigation undertaken by Hilbert in the *Grundlagen*. The experience gained while thinking on these issues when preparing his lectures on geometry, on the one hand, and his acquaintance with the ideas put forward in Hertz's treatment of physics,[25] on the other hand, indicated to Hilbert that the axiomatic method would provide a powerful and effective tool with which to address these crucial issues in the most effective way.

It would be well beyond the scope of the present article to discuss all the details of the *Grundlagen*, and how the main foundational questions of geometry were addressed therein.[26] Neither can we discuss here the criticisms aroused by its publication and the successive versions of the book, beginning in 1902, which corrected certain gaps of the original edition and added new results. However, in order to understand the basic ideas behind Hilbert's axiomatic approach and its bearing on physical theories, we must nevertheless discuss some of the main features of the analysis put forward in the *Grundlagen*. Of particular interest are the kinds of questions that Hilbert systematically pursued here for the first time, thus establishing a standard for his future work.

The declared aim of the *Grundlagen* was to present a 'simple' and 'complete' system of 'mutually independent' axioms,[27] from which all known theorems of geometry might be deduced. The axioms were defined for three systems of undefined objects ('points', 'lines' and 'planes'), and they postulated relations that should be satisfied by these objects. The axioms are divided into five groups (axioms of incidence, of order, of congruence, of parallels and of continuity), but the groups have no purely logical significance in themselves. Rather they reflect Hilbert's actual conception of the axioms as an expression

24 For an account of Cayley's contributions see Klein (1926–27, Vol. 1, pp. 147–151).
25 In fact, Hertz's was perhaps only one among several sources from within the physical literature that influenced Hilbert's initial inclination to, and then his fully adoption of, the axiomatic method. See Corry (1997a, pp. 92–103).

of our spatial intuition: each of the groups expresses a particular way in which these intuitions manifest themselves.

The requirements imposed by Hilbert on his system of axioms are remarkably similar to the criteria put forward by Hertz as a basis for constructing and assessing physical theories: permissibility, correctness, and appropriateness.[28] Consider, in the first place, Hilbert's requirement for independence of the axioms. This requirement is the most direct manifestation of the kind of foundational concern that motivated Hilbert's research. When analysing independence, Hilbert's interest focused mainly on the axioms of congruence, of continuity and of parallels, since this independence would specifically explain how the various basic theorems of projective and of Euclidean geometry are logically interrelated. In Hilbert's early lectures on geometry, this requirement had already appeared, albeit more vaguely formulated, as a direct echo of Hertz's demand for appropriateness. Now, in the *Grundlagen*, independence of axioms not only appeared as a more clearly formulated requirement, but Hilbert also provided the tools to prove systematically the mutual independence among the individual axioms within the groups and among the various groups of axioms in the system. He did so by introducing the method that has since become standard: he constructed models of geometries which fail to satisfy a given axiom of the system but satisfy all the others.

Also the requirement of simplicity had been explicitly put forward by Hertz and it complements that of independence. It means, roughly, that an axiom should contain 'no more than a single idea'. Beyond mentioning it in the introduction, this requirement was neither explicitly formulated nor otherwise realized in any clearly identifiable way in the *Grundlagen*. It appeared, however, in an implicit way and remained here—as well as in other, later works—an aesthetic desiderata for axiomatic systems, which was not transformed into a mathematically controllable feature.[29]

The 'completeness' that Hilbert demanded for his system of axioms runs parallel to Hertz's demand for correctness.[30] Very much like Hertz's stipulation for correct images, Hilbert required from any adequate axiomatization to allow for a derivation of *all* the known theorems of the discipline in question. The axioms formulated in the *Grundlagen* purportedly allowed to show how all the known results of Euclidean, as well as of certain non-Euclidean,

28 See Jesper Lützen's contribution to this volume.

29 In his 1905 lectures on the axiomatization of physics, Hilbert explicitly demanded the simplicity of the axioms for physical theories. It should also be remarked that in a series of investigations conducted in the USA in the first decade of the present century under the influence of the *Grundlagen*, a workable criterion for simplicity of axioms was still sought after. For instance, Edward Huntington (1904, p. 290) included simplicity among his requirements for axiomatic systems, yet he warned that 'the idea of a simple statement is a very elusive one which

geometries could be elaborated from scratch, depending on which groups of axioms were admitted.[31] Thus, Hilbert discussed in great detail the role of each of the groups of axioms in the proofs of two crucial results: the theorems of Desargues and the theorem of Pappus (also called by Hilbert the theorem of Pascal). Hilbert's analysis allowed a clear understanding of the actual premises necessary for coordinatizing projective geometry, which, as already stressed, was a key step in building the bridge between the latter and other kinds of geometry and a main concern of Hilbert.[32]

Although not explicitly mentioned in the introduction to the *Grundlagen*, the question of the consistency of the various kinds of geometries was an additional concern of Hilbert's analysis, which he addressed in the *Festschrift* right after introducing all the groups of axioms and discussing their immediate consequences. Although, seen from the point of view of Hilbert's later metamathematical research and the developments that followed it, the question of consistency appears as the most important one undertaken in the *Grundlagen*, in the historical context of the evolution of his ideas it certainly was not. In fact, the consistency of the axioms is discussed in barely two pages, and it is not immediately obvious why Hilbert addressed this question at all. It doesn't seem likely that in 1899 Hilbert envisaged the possibility that the body of theorems traditionally associated with Euclidean geometry might contain contradictions. Euclidean geometry, after all, was for Hilbert a natural science whose subject matter is the properties of physical space. Hilbert seems rather to have been echoing here Hertz's demands for scientific theories, in particular his demand for the permissibility of images. In fact, a main point that Hilbert will stress in future opportunities, following Hertz, is that the axiomatic analysis of physical theories was meant to clear away any possible contradictions brought about over time by the gradual addition of new hypotheses into a specific theory.[33] Although this was not likely to be the case for the well-established discipline of geometry, it might still happen that the particular way in which the *axioms* had been formulated in order to account for the theorems of this science led to statements that contradict each other. The recent development of non-Euclidean geometries made this possibility only more patent. Thus, Hilbert believed that in the framework of his system of axioms for

31 Several important changes concerning the derivability of certain theorems appeared in the successive editions of the *Grundlagen*. These are, however, not directly relevant to the main concerns of this article.

32 However, there were many subsequent corrections and additions, by Hilbert as well as by others, that sharpened still further the picture put forward by Hilbert in the first edition of the *Grundlagen*. Toepell (1986, p. 252), presents a table summarizing the interconnections between theorems and groups of axioms as known by 1907. See also Freudenthal (1957) for later developments.

33 See for instance Hertz (1956, pp. 7–8): 'It is not by finding out more and fresh relations and

geometry he could also easily show that no such contradictory statements would appear.

The publication of the *Grundlagen* was followed by many further investigations into Hilbert's technical arguments, as well as by more general methodological and philosophical discussions. One important such discussion appeared in the oft-cited correspondence between Hilbert and Gottlob Frege (1846–1925).[34] This interchange has drawn considerable attention of historians and philosophers, especially for the debate it contains between Hilbert and Frege concerning the nature of mathematical truth. But this frequently emphasized issue is only one side of a more complex picture advanced by Hilbert in his letters. In particular, it is interesting to notice Hilbert's explanation to Frege, concerning the main motivations for undertaking his axiomatic analysis: the latter had arisen, in the first place, from difficulties Hilbert had encountered when dealing with *physical*, rather than mathematical theories. Echoing once more ideas found in the introduction to Hertz's textbook, Hilbert stressed the need to analyze carefully the process whereby physicists continually add new assumptions to existing physical theories, without properly checking whether or not the former contradict the latter, or consequences of the latter. In a letter written on December 29, 1899, Hilbert wrote to Frege:

> After a concept has been fixed completely and unequivocally, it is on my view completely illicit and illogical to add an axiom—a mistake made very frequently, especially by physicists. By setting up one new axiom after another in the course of their investigations, without confronting them with the assumptions they made earlier, and without showing that they do not contradict a fact that follows from the axioms they set up earlier, physicists often allow sheer nonsense to appear in their investigations. One of the main sources of mistakes and misunderstandings in modern physical investigations is precisely the procedure of setting up an axiom, appealing to its truth (?), and inferring from this that it is compatible with the defined concepts. One of the main purposes of my *Festschrift* was to avoid this mistake.[35]

In a different passage of the same letter, Hilbert commented on the possibility of substituting the basic objects of an axiomatically formulated theory by a different system of objects, provided the latter can be put in a one-to-one, invertible relation with the former. In this case, the known theorems of the theory are equally valid for the second system of objects. Concerning physical theories, Hilbert wrote:

> All the statements of the theory of electricity are of course valid for any other system of things which is substituted for the concepts magnetism, electricity, etc., provided only that the requisite axioms are satisfied. But the circumstance I mentioned can never be a defect in a theory [footnote: it is rather a tremendous advantage], and it is in any case unavoidable. However, to my mind, the application of a theory to the world of appearances always requires a certain measure of good will and tactfulness: e.g., that we substitute the smallest possible bodies for points and the longest possible ones, e.g., light-rays, for lines. At the same time, the further a theory has been developed and the more finely articulated its structure, the more obvious the kind of application it has to the world of appearances, and it takes a very large amount of ill will to want to apply the more subtle propositions of [the theory of surfaces] or of Maxwell's theory of electricity to other appearances than the ones for which they were meant . . .[36]

Hilbert's letters to Frege help us understand the important role played in the development of his axiomatic point of view by his conception of the relation between physical and mathematical theories. Hilbert's axiomatic approach clearly did not evolve either as an empty game with arbitrary systems of postulates or as a conceptual break with the classical entities and problems of mathematics and empirical science. Rather it sought to improve the mathematician's understanding of the latter. This motto was to guide much of Hilbert's incursions into several domains of physics over the years to come.

Physics in Hilbert's 1900 list of problems

In 1900, speaking before the Second International Congress of Mathematicians in Paris, Hilbert presented his famous list of twenty three problems. This list implied an overarching research program that Hilbert was suggesting for the entire mathematical community for years to come. Hilbert declared that a wealth of significant open problems is a necessary condition for the healthy development of any mathematical branch and, more generally, of the living organism that mathematics constitutes.[37] Empirical motivations appear in his conception of mathematics as a main source of life for that organism. Stressing once more at this opportunity the close interrelation between mathematics and the physical sciences, Hilbert stated that the quest for rigor in analysis and arithmetic should be extended to cover geometry and the physics, not only because it would perfect our understanding of the latter, but also because it would eventually provide mathematics with ever new and fruitful ideas. Commenting on the opinion that geometry, mechanics and other physical sciences are beyond the possibility of

34 The relevant letters between Hilbert and Frege appear in Gabriel *et al.* (1980), esp. pp. 34–51. For comments on this interchange see Boos (1985); Peckhaus (1990, pp. 40–46); Resnik

36 Quoted in Gabriel *et al.* (1980, p. 41). I have substituted here 'theory of surfaces' for 'Plane

a rigorous treatment, he wrote:

> But what an important nerve, vital to mathematical science, would be cut by the extirpation of geometry and mathematical physics! On the contrary I think that whenever from the side of the theory of knowledge or in geometry, or from the theories of natural or physical science, mathematical ideas come up, the problem arises for mathematical science to investigate the principles underlying these ideas and so to establish them upon a simple and complete system of axioms, that the exactness of the new ideas and their applicability to deduction shall be in no respect inferior to those of the old arithmetic concepts.
>
> (Hilbert, 1902, p. 442)

Hilbert described the development of mathematical ideas as an ongoing dialectical interplay between the two poles of thought and experience; an interplay that brings to light a 'pre-established harmony' between nature and mathematics.[38] Hilbert also expressed here his celebrated opinion that every mathematical problem can indeed be solved: 'In mathematics there is no *ignorabimus*' (1902, p. 445).

The sixth problem of the list was a call to undertake the axiomatization of physical science. Hilbert formulated it as follows:

> The investigations on the foundations of geometry suggest the problem: To treat in the same manner, by means of axioms, those physical sciences in which mathematics plays an important part; in the first rank are the theory of probabilities and mechanics.
>
> (Hilbert, 1902, p. 454)

Hilbert mentioned several existing works as examples of what he had in mind here: the fourth edition of Mach's *Die Mechanik in ihrer Entwicklung*, Hertz's *Principles*, Boltzmann's 1897 *Vorlesungen über die Principien der Mechanik*, and also *Einführung in das Studium der theoretischen Physik* (1900) by the Königsberg physicist Paul Volkmann (1856–1938), with whom Hilbert, while still at his native city, may have had the opportunity to discuss the question of the role of axioms in physics.[39] Together with these well-known works on mechanics, Hilbert also mentioned a recent work by the Göttingen actuarial mathematician Georg Bohlmann (1869–1928) on the foundations of the calculus of probabilities.[40] The latter was important for physics, he said, for its application to the method of mean values and to the kinetic theory of gases.

38 The issue of the 'pre-established harmony' between mathematics and nature was a very central one among Göttingen scientists. This point has been discussed in Pyenson (1975).

39 See Corry (1997, pp. 101–103).

40 Bohlmann (1900). This article reproduced a series of lectures delivered by Bohlmann in a *Ferienkurs* in Göttingen. In his article Bohlmann referred the readers, for more details, to the

Modelling this research on what had already been done for geometry meant that not only theories which are considered as closer to 'describing reality' should be investigated, but also other, logically possible ones. The mathematician undertaking the axiomatization of physical theories should obtain a complete survey of all the results derivable from the accepted premises. Moreover, echoing the concern already found in Hertz and in Hilbert's letters to Frege, a main task of the axiomatization would be to avoid that recurrent situation in physical research, in which new axioms are added to existing theories without properly checking to what extent the former are compatible with the latter. This proof of compatibility, concluded Hilbert, is important not only in itself, but also because it compels us to search for ever more precise formulations of the axioms (1902, p. 445).

Although the sixth problem is the only one in the list to refer directly to physics as such, three additional ones concern mathematical issues intimately connected to the classical problems of mathematical physics. The nineteenth problem concerns the question whether all the solutions of the Lagrangian equations that arise in the context of certain typical variational problems are necessarily analytic. The twentieth, closely related to the former and at the same time to Hilbert's long-standing interest in the domain of validity of the Dirichlet principle, deals with the existence of solutions to partial differential equations with given boundary conditions. Finally, the twenty-third problem of the list is an appeal to extend and refine the existing methods of variational calculus. All these three problems are also strongly connected to physics, and though at variance with the sixth, they are part of mainstream, traditional research concerns.[41] The role of variational principles in Hilbert's program for axiomatizing physics will be further discussed below.

But what has been said above suffices to indicate the natural place of the sixth problem of Hilbert's 1900 list in his overall conception of science. The task of axiomatizing physical theories had arisen organically in conjunction with the very consolidation of Hilbert's view of the centrality of the axiomatic method for studying the foundations of geometry. By 1900 his interest in physical theories had found a natural place among his overall views of mathematics, its main methods and its problems.

Hilbert, Minkowski, and physics in Göttingen: 1900–13

Following the publication of the *Grundlagen*, Hilbert's main focus of attention remained in the study of the foundations of geometry until 1903. That year Bertrand Russell published his discovery of a paradox arising in Frege's logical system. Although contradictory arguments of the kind discovered by Russell had been made known in Göttingen a couple of years earlier by Ernst Zermelo (1871–1953),[42] it seems that Russell's publication led Hilbert to attribute to the axiomatic analysis of logic and of the foundations of set theory a much more central role in establishing the consistency of arithmetic than had been the case until then. Beginning in 1903, intense activity developed in Göttingen in this direction,[43] and the systematic study of logic and set theory as a central issue in the foundations of mathematics was initiated in Hilbert's mathematical circle. Zermelo was then working on the proof of the consistency of arithmetic and on the axiomatization of set theory. After 1905 Hilbert devoted no further efforts to such foundational studies, and Zermelo was left alone at this, going on to publish his well-known paper on the foundations of set-theory in 1908.[44] Meanwhile, as early as 1902 Hilbert had begun publishing in the new domain of research that would concentrate his best efforts until 1912: the theory of linear integral equations.

Still, during all these years, Hilbert's interest in physical issues only deepened. In 1901 and 1902 he lectured on potential theory and in 1902 and 1903 on continuum mechanics. That Hilbert considered these courses to have some original and interesting content, rather than being a simple repetition of existing presentations, is evident from the fact that the only two talks he gave in 1903 at the meetings of the GMG were reports on what he did in those courses.[45] In the winter semester of 1904–05 he taught an exercise course on mechanics and later gave a seminar on mechanics. Then, in the summer semester of 1905, in the framework of a course on the 'Logical Principles of Mathematical Thinking', he gave a long and detailed overview of how the axiomatic approach should be applied in various individual physical disciplines. In the next winter semester (1905–06) he lectured again on mechanics, and then lectured for two more semesters on continuum mechanics.

In 1902 Minkowski arrived in Göttingen, following the creation of a third chair of mathematics in that university under Hilbert's pressure on Klein to convince the Prussian ministry. The renewed encounter between the two old friends was an enormous source of intellectual stimulation for both. As usual, their mathematical walks were an opportunity to discuss a wide variety of mathematical topics. This time, however, physics became a more prominent common interest that it had been in the past. Teaching in Zürich since 1894,

42 Peckhaus (1990, pp. 48–49).
43 Peckhaus (1990, pp. 56–57).
44 Zermelo (1908). For a comprehensive account of the background, development and influence of Zermelo's axioms see Moore (1982). For an account of the years preceding the publication, see esp. pp. 155ff.
45 See the announcements in the *Jahresbericht der Deutschen Mathematiker-Vereinigung* [illegible]

Minkowski had kept alive his interest in mathematical physics, and in particular in thermodynamics.[46] Now at Göttingen he developed this interest further. In 1906 Minkowski published an article on capillarity, commissioned for the physics volume of the *Encyclopädie der mathematischen Wissenschaften*, edited by Arnold Sommerfeld (Minkowski, 1906). At several meetings of the GMG he lectured on this, as well as on other physical issues such as Euler's equations of hydrodynamics and Nernst's work on thermodynamics.[47] He also taught advanced seminars on physical topics and more basic courses on continuum mechanics, exercises on mechanics and heat radiation.[48] In 1905 Hilbert and Minkowski organized, together with other Göttingen professors, an advanced seminar that studied recent progress in the theories of the electron. This seminar – reconstructed in detail by Lewis Pyenson[49] – exemplified the vitality of physical research at that university, and the role Hilbert and Minkowski played in fostering it. Again in 1907, the two conducted a joint seminar on the equations of electrodynamics.[50] Finally, as is well known, during the last years of his life, Minkowski's efforts were intensively dedicated to electrodynamics and the principle of relativity. In order to gain a deeper understanding of Hilbert's views on physical issues during these years it is useful to discuss his 1905 course on the axiomatic method and the work of Minkowski in electrodynamics.

In order to understand the context of Hilbert's course of 1905 correctly it is important to stress that it started with a detailed treatment of the axioms of arithmetic and of geometry, and that in its last section it attempted to develop a formalized calculus for propositional logic.[51] At this time the reconstruction of the logical foundations of mathematics was increasingly drawing Hilbert's attention, and the overall aim of the course was to discuss issues related to it. In exactly what fashion Hilbert conceived the possible role of the axiomatic approach as part of this reconstruction, and as part of his general views on physics and on mathematics at this time, is illuminatingly condensed in the following passage taken from one of the lectures:

> The edifice of science is not raised like a dwelling, in which the foundations are first firmly laid and only then one proceeds to construct and to enlarge the rooms. Science prefers to secure as soon as possible comfortable spaces to wander around and only subsequently, when signs appear here and there that the loose foundations are not able to sustain the expansion of the

46 See Rüdenberg and Zassenhaus (1973, pp. 110–114).
47 As registered in the *JDMV*, Vol. 12 (1903), pp. 445, 447; Vol. 15 (1906), p. 407.
48 See the announcement of his courses in *JDMV* Vol. 13 (1904), p. 492; Vol. 16 (1907), p. 171; Vol. 17 (1908), p. 116.
49 Pyenson (1979).

rooms, it sets to support and fortify them. This is not a weakness, but rather the right and healthy path of development.[52]

Hilbert's discussion of the axioms of physical science covered a surprisingly varied range of domains: mechanics, thermodynamics, probability calculus, kinetic theory of gases, insurance mathematics, electrodynamics, psychophysics. Without entering into a detailed account of what Hilbert did for each and every domain he considered, and of the background of his treatment, it is nevertheless convenient to describe the general aims he pursued in his presentation and what he did for certain domains.[53] The first domain of physics that Hilbert discussed was mechanics. He started with the axioms defining the addition of vectors, the main ideas of which he took from recent works of Gaston Darboux (1842–1917), of Georg Hamel (1877–1954), and of the Göttingen student Rudolf Schimmack (1881–1912).[54] The first three of the axioms adopted by Hilbert stipulate the existence of a sum of any two vectors, its commutativity and its associativity. The fourth axiom connects the sum-vector with the directions of the factors as follows:

> 4. Let aA denote the vector (aA_x, aA_y, aA_z), having the same direction as A. Then every real number a defines the sum:
>
> $$A + \alpha A = (1+\alpha)A$$
>
> i.e., the addition of two vectors having the same direction is defined as the algebraic addition of the longitudes along the straight line on which both vectors lie.[55]

The fifth one establishes the interchangeability of addition and rotation of vectors. The sixth axiom concerns continuity:

> 6. Addition is a continuous operation, i.e., given a sufficiently small domain G around the end-point of $A + B$ one can always find domains G_1 and G_2, around A and B respectively, such that the end-point of the sum of any two vectors belonging to each of these domains will always fall inside G. (p. 124)

This last axiom of continuity plays a very central role in Hilbert's overall conception of the axiomatization of natural science—geometry, of course, included. It is part of the essence of things—said Hilbert in his lecture—that

52 Hilbert (1905, p. 102): 'Das Gebäude der Wissenschaft wird nicht aufgerichtet wie ein Wohnhaus, wo zuerst die Grundmauern fest fundiert werden und man dann erst zum Auf- und Ausbau der Wohnräume schreitet; die Wissenschaft zieht es vor, sich möglichst schnell wohnliche Räume zu verschaffen, in denen sie schalten kann, und erst nachträglich, wenn es sich zeigt, dass hier und da die locker gefügten Fundamente den Ausbau der Wohnräume nicht zu tragen vermögen, geht sie daran, dieselben zu stützen und zu befestigen. Das ist kein Mangel, sondern die richtige und gesunde Entwicklung.' Other places where Hilbert uses the 'building metaphor' are Hilbert (1897, p. 67); Hilbert (1918, p. 148).

53 For a fully detailed account see Corry (1997a). The following passages condensate the arguments put forward in that article.

54 Darboux (1875), Hamel (1905), Schimmack (1903). An additional related work, also mentioned by Hilbert in the manuscript is Schur (1903).

the axiom of continuity should appear in every geometrical or physical system. Therefore it can be formulated not just with reference to a specific domain, as was the case here for vector addition, but in a much more general way. A very similar opinion had been advanced by Hertz, who in the introduction to his textbook described the continuity of nature as 'an experience of the most general kind'. . .'an experience which has crystallized into firm conviction in the old proposition—*Natura non facit saltus*' (Hertz, 1956, pp. 36–37). Hilbert formulated a general principle of continuity in the following terms:

> If a sufficiently small degree of accuracy is prescribed in advance as our condition for the fulfillment of a certain statement, then an adequate domain may be determined, within which one can freely choose the arguments [of the function defining the statement], without however deviating from the statement, more than allowed by the prescribed degree. (p. 125)

Experiment—continued Hilbert—compels us to place this axiom on the top of every natural science, since it allows to assert the validity of our assumptions and claims. In every special case, this general axiom must be given the appropriate version, as was done in an earlier part of the lectures for geometry and here for vector addition.

There is no direct evidence by which we can judge the reaction of the students who attended these lectures of Hilbert, which were listed among the elementary courses offered in Göttingen that semester. Before those students stood the great Hilbert, quickly surveying many different physical theories, together with arithmetic, geometry, and even logic, all in the framework of a single course. Hilbert moved from one theory to the other, and from one discipline to the next, without providing motivations or explaining the historical background to the specific topics addressed, without giving explicit references to the sources, without stopping to work out any particular idea, without proving any assertion in detail, but claiming all the while to have a unified view of all these matters. The impression must have been thrilling, but one wonders to what extent his students could really appreciate the ideas presented to them. In fact, it is hard to determine with exactitude to what extent Hilbert himself dominated the physical subtleties of the issues he discussed, though there can be no doubt that his unusual abilities helped him overcoming very easily what perhaps presented great mathematical difficulties to others. Still, the picture of Hilbert's knowledge of physics that arises from these lectures is impressive both in its breadth and its incisiveness.

In Hilbert's treatment of physical theories of 1905 we come across diverse kinds of axioms. In the first place, every theory is assumed to be governed by specific axioms that characterize it. These axioms usually express mathematical properties establishing relations among the basic magnitudes involved in the theory. Then, there are certain general mathematical principles that stressed above all the 'continuity axiom', providing both a general formulation and more specific ones for each theory. As an additional general principle of this kind he suggested the assumption that all functions appearing in the natural sciences should have at least one continuous derivative. Furthermore, the universal validity of variational principles as the key to deriving the main equations of physics, especially in mechanics and electrodynamics (which were not described in detail in the course) was a central underlying assumption of all of Hilbert's work on physics, and they appear throughout these lectures as well. In each of the theories he considered in his 1905 lectures, Hilbert attempted to show how the exact analytic expression of a particular function that condenses the contents of the theory in question could be effectively derived from the specific axioms of the theory, together with more general principles. On some occasions he elaborated this more thoroughly, while on others he simply declared that such a derivation should be possible.

There is yet a third type of axiom for physical theories, however, which Hilbert avoided addressing in his 1905 lectures. They comprise claims about the ultimate nature of physical phenomena, an issue which was particularly controversial during the years preceding these lectures. Although Hilbert's sympathy for the mechanistic world-view is apparent throughout the manuscript of the lectures, his axiomatic analyses of physical theories contain no direct reference to it. The logical structure of the theories should be fully understood independently of any particular position one would assume in this debate. As will be seen below, this position would change around 1913, when Hilbert wholeheartedly adopted the view that all physical phenomena should be explained in terms of electrodynamic processes. I discuss this issue below.

After the summer semester of 1905, Hilbert lectured again on mechanics (WS 1905–06)[56] and continuum mechanics (SS 1906 and WS 1906–07). Between then and until 1910 he taught no additional courses on physics. But at the same time, from 1907 and until his death in 1909, Hilbert's colleague Minkowski devoted much effort to the study of electrodynamics and the principle of relativity. It would seem as if Hilbert had left the stage to Minkowski alone to work out his ideas on these issues. As a matter of fact, however, Minkowski's work can be seen to a large extent as a realization of Hilbert's program of axiomatization, in which the specific role of the newly adopted principle of relativity in various physical theories was thoroughly investigated for the first time. Moreover, Hilbert's lectures on physical issues over the years following Minkowski's death indicate that the former fully adopted the point of view, the notations, and the concepts introduced by the latter. Thus a description of Minkowski's work on electrodynamics may also serve as a parallel description of how Hilbert's conceptions applied to the new situation created in physics by the introduction of the principle of relativity,

and at the same time, as evidence of what Hilbert's own ideas on physical issues looked like over that period of time of which we have no additional, direct evidence.

Elsewhere I have presented a comprehensive picture of Minkowski's work on electrodynamics, in which his motivations and the details of his work are interpreted in the lines suggested in the preceding paragraph.[57] For the purposes of the present article it may suffice to discuss briefly only one of Minkowski's articles, dealing with 'the basic equations of electromagnetic processes in moving bodies' (Minkowski, 1908). This was Minkowski's only publication on this issue that appeared in print during his lifetime. This was also the article that Hilbert considered as his friend's most significant contribution to this field. In his obituary of Minkowski, Hilbert stressed the importance and the innovative character of the axiomatic analysis presented in that article, as well as of Minkowski's derivation of the equations for moving matter starting from the so-called 'World postulate', and from three additional axioms. The correct form of these equations had been theretofore an extremely controversial issue among physicists, but this situation had totally changed—so Hilbert believed—thanks to Minkowski's work (Hilbert, 1909, pp. 93–94).

Minkowski's article opened with an analysis of current developments in the theories of the electron and of the role played by the principle of relativity in them. Minkowski distinguished three possible different meanings of this principle. First, one has the plain mathematical fact that the Maxwell equations, as formulated in Lorentz's theory of electrodynamics, are invariant under the Lorentz transformations. Minkowski called this latter fact the 'theorem of relativity'. The 'postulate of relativity' differs from the theorem in that it expresses more of a confidence (*Zuversicht*) than an objective assessment concerning some actual state of affairs: a confidence that the domain of validity of the theorem may be extended to cover *all* laws governing ponderable bodies, including laws that are still unknown. He compared this postulate to the postulation of the validity of the principle of conservation of energy, which we assume even for forms of energy that are not yet known. Finally, there is the 'principle of relativity' which expresses the assertion that the expected Lorentz covariance actually holds as a relation between purely observable magnitudes relating to a moving body (Minkowski, 1908, p. 353).

Minkowski's declared aim was to deduce the exact expression of the equations for moving bodies from the principle of relativity. He claimed that his formulation of the principle had never before been articulated the way he did and that under earlier formulations the equations were not truly invariant. Minkowski believed that his axiomatic analysis of the principle of relativity and of the electrodynamical theories of moving bodies was the best approach for unequivocally obtaining the correct equations. Minkowski started by presenting the equations for pure ether. Then, in the second part of his article, he discussed how the equations change when matter is added to the ether. For the case of a body at rest in the ether, Minkowski relied on Lorentz's version of Maxwell's equations, and analyzed the symmetry properties of the latter. He formulated the equations as follows:

(I)
$$\operatorname{curl} \boldsymbol{m} - \frac{\partial \boldsymbol{e}}{\partial t} = \boldsymbol{s}$$

(II)
$$\operatorname{div} \boldsymbol{e} = \rho$$

(III)
$$\operatorname{curl} \boldsymbol{E} + \frac{\partial \boldsymbol{M}}{\partial t} = 0$$

(IV)
$$\operatorname{div} \boldsymbol{M} = 0$$

Here $\boldsymbol{M}$ and $\boldsymbol{e}$ are called the magnetic and electric intensities (*Erregung*) respectively, $\boldsymbol{E}$ and $\boldsymbol{m}$ are called the electric and magnetic forces, ρ is the electric density, $\boldsymbol{s}$ is the electric current vector (*elektrischer Strom*). The properties of matter, in the case of isotropic bodies, are characterized by the following equations:

(V)
$$\boldsymbol{e} = \varepsilon \boldsymbol{E}, \qquad \boldsymbol{M} = \mu \boldsymbol{m}, \qquad \boldsymbol{s} = \sigma \boldsymbol{E},$$

where ε is the dielectric constant, μ is the magnetic permeability, and σ is the conductivity of matter.

From the basic properties of the equations for bodies at rest, Minkowski deduced the fundamental equations for the case of a body in motion. This deduction is where the detailed axiomatic derivation is realized: Minkowski assumed the validity of the previously discussed equations for matter at rest and then added three more axioms to them. He then sought to derive the equations for matter in motion exclusively from these axioms, together with the equations for rest. Minkowski's axioms are:

1. Whenever the velocity **v** of a particle of matter equals 0 at x, y, z, it in some reference system, then equations (I)–(V) also represent, in that system, the relations among all the magnitudes: ρ, the vectors $\boldsymbol{s}, \boldsymbol{m}, \boldsymbol{e}, \boldsymbol{M}, \boldsymbol{E}$, and their derivatives with respect to x, y, z, it.

3. If a Lorentz transformation acting on the variables x, y, z, it, transforms both $\boldsymbol{m}, -i\boldsymbol{e}$ and $\boldsymbol{M}, -i\boldsymbol{E}$ as space–time vectors of type II, and $\boldsymbol{s}, i\boldsymbol{e}$ as a space–time vector of type I, then it transforms the original equations exactly into the same equations written for the transformed magnitudes.[58]

This last axiom is what Minkowski called the principle of relativity.

Minkowski deduced the equations for moving bodies in a section where he showed in detail how every step of the deduction is allowed by one of the axioms. On first reading, his straightforward argument may seem somewhat out of place amidst all the elaborate mathematical and physical arguments displayed throughout his long article. But when seen in the light of the kind of axiomatic conceptual clarification promoted by Hilbert in his lectures on physics, it becomes clear that Minkowski was simply stressing this deduction as a central task of his exposition of the theory. Moreover, Minkowski went on to check to what extent different existing versions of the equations satisfy the principle as stated in his axioms. Minkowski's implicit assumption was that only such equations can be accepted as correct as comply with his own version of the principle. Thus Minkowski showed that the equations for moving media formulated by Lorentz in his *Encyclopädie* article (Lorentz, 1904) are in certain cases incompatible with his principle. Minkowski also discussed the equations formulated in 1902 by Emil Cohn, pointing out that they agree with his own ones up to terms of first order in the velocity (Minkowski, 1908, p. 372). After having formulated the equations and discussed their invariance properties, Minkowski dealt in detail, in three additional sections, with the properties of electromagnetic processes in the presence of matter.

Minkowski's article also contained an appendix discussing the relations between mechanics and the postulate of relativity. In this appendix the similarity of Minkowski's and Hilbert's treatment of physical theories is even more clearly manifest: it explores the consequences of adding the postulate of relativity to the existing building of mechanics, and the compatibility of the postulate with the already established principles of this discipline. The extent to which this addition can be successfully realized provides, in Minkowski's view, a standard for assessing the status of Lorentz covariance as a truly universal postulate of all physical science.

Using the four-vector formalism that he had introduced in the earlier sections of his article, Minkowski showed that the equations of motion of classical mechanics are invariant under the Lorentz group only under the assumption that $c = \infty$. It would be embarrassing or perplexing (*verwirrend*), he said, that the laws of transformation of the basic expression

$$-x^2 - y^2 - z^2 + c^2t^2$$

into itself would necessitate a certain finite value of c in a certain domain of physics and a different, infinite one, in a second domain. In view of this situation, the postulate of relativity (i.e., our confidence in the universal validity of the theorem) compels us to see Newtonian mechanics as only a tentative approximation initially suggested by experience, which however must be corrected to make it invariant for a finite value of c. Minkowski thought that reformulating mechanics in this direction was possible, and moreover—expressing himself in terms that could have been equally found in Hilbert's lecture notes—he asserted that such a reformulation would seem only to perfect, to a considerable extent, the axiomatic structure of mechanics as currently conceived.[59]

Clearly, the universal validity of the postulate of relativity could only be asserted if one could show that it does not contradict the observable phenomena related to gravitation. To this effect, in the last section of his article, Minkowski sketched a proposal for a theory of gravitation that would also be Lorentz covariant. This sketch was no more than a preliminary attempt in this direction, that neither Minkowski himself nor Hilbert went on to pursue.

A main motivation of Minkowski's work in electrodynamics, then, is a systematic investigation of the logical consequences of assuming the universal validity of Lorentz covariance for all physical disciplines. This is exactly the formulation used by Hilbert in his future lectures to describe the contents of the 'new mechanics.' The postulate of relativity had been strongly suggested by experimental results obtained during the late nineteenth century, and its theoretical implications had been investigated from different perspectives in recent works, notably those of Lorentz, Poincaré, and Einstein. Yet, in a spirit similar to that underlying Hilbert's program, Minkowski believed that the logical structure of the physical theories built on the principle of relativity had not yet been satisfactorily elucidated. The postulate of relativity should be taken as a further axiom appearing at the basis of each and every physical theory, together with the particular axioms of that theory. In his work Minkowski was able to prove for certain domains of physics that the ensuing theory indeed produced a consistent logical building. Concerning gravitation, he was less successful, but always declared his conviction to have indicated in principle how a consistent, Lorentz covariant theory of gravitation could eventually be elaborated in detail.

For Minkowski the postulate of relativity was not simply an additional axiom, with perhaps a wider domain of validity in physics than others. It was an axiom of a different nature: a principle that should be valid for every

59 Minkowski (1908, p. 393) (Italics in the original.): 'Ich möchte ausführen, daß durch eine *Reformierung der Mechanik*, wobei an Stelle des Newtonschen Relativitätspostulates [illegible]

conceivable physical theory, even those theories that were yet to be discovered or formulated. As I mentioned above, also Hilbert laid a great stress on the importance of this kind of universal physical principle, and focused especially on the 'principle of continuity.' Minkowski's comparison, in this context, of the status of the postulate of relativity with that of the principle of conservation of energy, had been drawn earlier by Einstein.[60] But Einstein's and Minkowski's comparisons were basically different. Einstein spoke in his article of two 'open' principles of physics with a strong heuristic character. Unlike Minkowski and Hilbert, Einstein did not see the principle of relativity and the principle of energy conservation as parts of strictly deductive systems from which the particular laws of a given domain could be derived.[61] More generally, although Einstein introduced the principle of relativity together with the constancy of the speed of light at the beginning of his 1905 article as 'postulates' of the theory (in some sense of the word), it is necessary to draw a clear distinction between what he did and what Minkowski and Hilbert had in mind when speaking of an axiomatic analysis of the postulate of relativity. In fact, one of the main explicit aims of Hilbert's program was to address situations similar to the one created here by Einstein, which had the potential of turning out to be problematic, namely, that faced with conflict between an existing theory and new empirical findings, it was common for physicists to add new hypotheses that apparently settle the disagreement in question but which perhaps contradict some other consequences of the existing theory. Hilbert thought that an adequate axiomatic analysis of the principles of a given theory would help clear away possible contradictions and superfluous additional premises created by the gradual introduction of new hypotheses into existing theories. This was also what Minkowski was pursuing with his analysis: to verify that the introduction of the principle of relativity will not create such a problematic situation.

Minkowski's analysis of the place of the postulate of relativity in physics avoided any assumption about, and certainly any commitment to, any particular conception of the ultimate nature of physical phenomena.[62] Perhaps Minkowski had some kind of clear position of his own on these issues, though we have no direct evidence of it. Minkowski's admiration for Hertz and the fact that in 1910 Hilbert sided with the mechanistic world-view when lecturing

60 Einstein (1907). Minkowski was very likely aware of this specific article of Einstein, if only because it appeared in the *Annalen der Physik* as a reply to an earlier article of Paul Ehrenfest who at that time was in Göttingen.

61 Cf. (Einstein 1907, p. 411): 'Es handelt sich hier also keineswegs um ein 'System', in welchem implizite die einzelnen Gesetze enthalten wären, und nur durch Deduktion daraus gefunden werden könnten, sondern nur um ein Prinzip, das (ähnlich wie der zweite Hauptsatz

on mechanics under the declared influence of Minkowski's ideas[63] might seem to suggest that this was also the latter's view. Hilbert was initially very attracted to the mechanistic reductionism of Hertz or Boltzmann until around 1913 when, as will be seen below, he changed his position diametrically and adopted an electromagnetic, reductionistic world-view based on Gustav Mie's theory of matter. Yet both Minkowski and Hilbert, at least around 1910, considered that the much needed task of axiomatically analysing the logical building of physical theories should be carried out while leaving aside this kind of unresolved physical issue.

Until 1912 Hilbert's mathematical efforts were concentrated on the theory of linear integral equations, where he published the successive instalments of what finally constituted his classical treatise on this topic (Hilbert, 1912a). At the same time, however, after Minkowski's death, Hilbert returned to teach courses on physical issues. Hilbert now moved into disciplines that he had never taught in the past. Thus, after teaching mechanics and continuum mechanics in 1910 and 1911, Hilbert taught statistical mechanics for the first time in the winter semester of 1910–11. This course marked the starting point of Hilbert's definitive involvement with a wider variety of physical theories. In December of 1911 he presented to the GMG an overview of his recent investigations on the kinetic theory of gases, which were soon to be published.[64] The kinetic theory was also the topic of his course during the winter semester of 1911–12, and in the following semester he taught a course on radiation theory (Hilbert, 1911–12). In 1912 Hilbert enrolled an assistant for physics, who was commissioned to keep him abreast of current developments in the various branches of physics. Paul P. Ewald (1888–1985), who had recently finished his dissertation in Munich, was the first to hold this position, which Hilbert maintained for many years to come. In April 1912, Hilbert was among the lecturers that took part in a twelve-day seminar on the axiomatic foundations of physics, opened for high-school teachers.[65]

Hilbert's increasingly deep involvement in physics led him to ponder again, now from a wider perspective, some basic questions concerning the foundations of this discipline. By 1910 Hilbert's approach, as already said, was dominated by the view that all physical phenomena could be reduced to mechanics. This view was clearly manifest in the courses he taught. Between 1910 and 1913, however, although his reductionistic inclinations did not change, he moved from the mechanistic to the electromagnetic point of view. Electromagnetic reductionism became the basis of all of Hilbert's work in physics thereafter.

From the manuscripts of Hilbert's courses between 1911 and 1913, as well as from his publications, it becomes evident that Hilbert was very much

63 As manifest, i.e., in Hilbert's lecture notes: see Hilbert (1910–11, p. 295).
64 See the announcement in the *JDMV* Vol. 21, p. 58.

impressed by recent developments in quantum theory. The importance of these developments had particularly been discussed and highlighted during the First Solvay Conference, held in Brussels in October 1911,[66] and its echoes must have reached Hilbert through his physicist colleagues. 'There has never has been a more proper and challenging time than now'—said Hilbert in the opening lecture of a course taught in 1912—'to undertake research into the foundations of physics.' What seems to have impressed him more than anything else were the recently discovered deep interconnections, 'of which formerly no one could have even dreamed, namely, that optics is nothing but a chapter of the theory of electricity, that electrodynamics and thermodynamics are one and the same, that energy also possesses inertial properties, that physical methods have been introduced into chemistry as well.'[67] And above all, the 'atomic theory', the 'principle of discontinuity', as Hilbert said, which today is not a hypothesis any more, but rather, 'like Copernicus's theory, a fact confirmed by experiment'.[68] Very much like the unification of apparently distant mathematical domains, which played a leading role throughout his career, the unity of physical laws exerted a strong attraction on Hilbert.

Electrodynamics and general relativity: 1913–15

By the year 1913 Hilbert's interest in a wide variety of physical disciplines had become a truly central feature of his current research and teaching concerns. In 1912 Hilbert solved the Boltzmann equation, which was intimately connected to his current research in the theory of linear integral equations, but which at the same time raised intriguing physical problems that directly attracted his attention. Hilbert's student Ludwig Föppl completed his dissertation on atomic stability in March 1912 (Föppl, 1912), as did Hans Bolza with his on the theory of gases. Hilbert also gave public lectures on Maxwell's theory of gases, statistical mechanics and Nernst's law of heat. The meetings of the GMG, clearly with Hilbert's approval, if not under his direct initiative, discussed Gustav Mie's recent work on an electromagnetic theory of matter (in December 1912) as well as Albert Einstein and Marcel Grossmann's first attempt to formulate a relativistic theory of gravitation (in December 1913), the famous *Entwurf* paper.[69]

66 See Kormos Barkan (1993).
67 Hilbert (1912d, p. 2): 'Nun kommen wir aber zu eigentlicher Physik, welche sich auf der Standpunkt der Atomistik stellt und da kann man sagen, dass keine Zeit günstiger ist und keine mehr dazu herausfordert, die Grundlagen dieser Disziplin zu untersuchen, wie die heutige. Zunächst wegen der Zusammenhänge, die man heute in der Physik entdeckt hat, wovon man sich früher nichts hätte traumen lassen, dass die Optik nur ein Kapitel der Elektrizitätslehre ist, dass Elektrodynamik und Thermodynamik dasselbe sind, dass auch die Energie Trägheit besitzt, dann dass auch in der Chemie (Metalchemie, Radioaktivität) physikalische Methoden in der Vordergrund haben.'
68 Hilbert (1912d, p. 2): '...wie die Lehre des Kopernikus, eine durch das Experimente bewiesene Tatsache.'

This increased interest in physics is also manifest in Hilbert's lectures of 1913. The topics of his lectures on physics had expanded way beyond the more traditional ones of classical mechanics and continuum mechanics and now covered also statistical mechanics, radiation theory and the molecular theory of matter. In the summer of 1913 Hilbert returned to an old field of interest that would occupy his thoughts for several years now: electromagnetism. He taught a course on electron theory and, at the same time, a second course on the principles of mathematics, very similar to the earlier one of 1905, where he included again a long section on the axiomatization of physics. Over the following years he would also lecture on electromagnetic oscillations, statistical mechanics, the structure of matter, and, in 1916–17, on the general theory of relativity and the theory of the electron. These years were also characterized by Hilbert's unqualified adoption of an electromagnetic, reductionist view of physical phenomena. More generally, his views on physical issues during this period became more articulate and, in many respects, much more dogmatic.

Hilbert's 1913 course on the theory of the electron was conceived as an axiomatic treatment of electrodynamics. Following Minkowski's work, Hilbert laid special stress on the role of the (special) relativity principle, i.e., on the assumption that all laws of nature are expressible as formulas that are invariant with respect to simultaneous, homogenous (orthogonal) transformations of the four variables x, y, z, t.[70] Hilbert devoted some effort to highlight the importance of the specific contributions of his Göttingen colleagues: Minkowski's 'world vector analysis', and Born's rigid body. The Maxwell equations and the concept of energy, he explained, do not suffice to provide a complete foundation of electrodynamics. An additional concept is thus needed: the concept of rigidity. Electricity must be attached to a steady scaffold. This scaffold is what we call an electron. The electron, he explained, embodies the concept of a rigid body in Hertz's mechanics. All of the laws of mechanics can be derived, in principle at least, from these three ideas: Maxwell's equations, the concept of energy, and rigidity. From them also all the forces of physics can be derived, in particular the molecular forces. Only gravitation, he concluded, has evaded until now every attempt at an electrodynamic explanation.[71]

Hilbert also heavily stressed the mathematical difficulty involved in solving the n-electron problem. This difficulty, he asserted, provides additional

70 Hilbert (1913c, p. 1. Emphasis in the original): 'Der Inhalt des Relativitätsprinzip ist nun die Behauptung, *dass die gesamten Naturgesetze mathematischen Formal* ihren Ausdruck finden, die invarianten gegen einen simultanen homogenen (orthogonale) Transformation der vier Variablen x, y, z, t sind.'

71 Hilbert (1913c, pp. 61–62. Emphasis in the original): 'Auf die Maxwellschen Gleichungen und den Energiebegriff allein kann man die Elektrodynamik nicht gründen. Es muss noch der Begriff der *Starrheit* hinzukommen; die Elektrizität muss an ein festes Gerüst angeheftet sein. Dies Gerüst bezeichen wir als Elektron. In ihm ist der Begriff der starrer Verbindung der Hertzschen Mechanik verwirklicht. Aus den Maxwellschen Gleichungen, dem Energiebegriff

justification for studying the movements of the electron based on the principles of statistical mechanics.[72] It is interesting to notice that Gustav Mie and his electromagnetic theory of matter were not mentioned explicitly in the lectures, despite the fact that back in December 1912 this theory had been discussed in the meeting of the GMG.

Parallel to this course, Hilbert also lectured in the summer of 1913 on the 'Elements and principles of mathematical thinking'. A glance at the contents of the course indicates the extent to which Hilbert still considered physics and mathematics as tightly interconnected in their most fundamental and essential aspects, and how he thought that the axiomatic method should be similarly applied for the benefit of both domains. The contents of the course also illustrate, as was the case with the similar 1905 course, how a typical Hilbert lecture might look: several, not obviously interconnected, mathematical issues could be discussed in successive lectures, in very general terms and without entering into any kind of details. The initial program of the course could always be changed, according to the way in which it developed.

One innovation found in these lectures, compared to those of 1905, is an elaborate treatment of special relativity or, as Hilbert called it, of the new conceptions of space and time.

In the winter semester of 1913–14 Hilbert taught his last course on a physical issue before eventually turning to general relativity. It dealt with electromagnetic oscillations. In the introductory lecture of this course he referred once again to the example of geometry as a model of an *experimental* science, our thorough knowledge of which has transformed into a mathematical, and therefore a 'theoretical science'. In a passage that characterizes very aptly his conception of the relation between physical disciplines and mathematics, Hilbert said:

> From antiquity the discipline of geometry is a part of mathematics. The experimental grounds necessary to build it are so suggestive and generally acknowledged, that from the outset it has immediately appeared as a theoretical science. I believe that the highest glory that such a science can attain is to be assimilated by mathematics, and that theoretical physics is presently on the verge of attaining this glory. This is valid, in the first place for relativistic mechanics, or four-dimensional electrodynamics, which I have been convinced for a long time belongs to mathematics.[73]

72 Hilbert (1913c, p. 83): 'Je komplizierte aber das Problem, mit umsomehr Recht wenden wir später das Grundprinzip der statistischen Mechanik auf die Elektronenbewegung an.'

73 Hilbert (1913–14, p. 1) (Emphasis in the original): 'Seit Alters her ist die Geometrie eine Teildisziplin der Mathematik; die experimentelle Grundlagen, die sie benutzen muss, sind so naheliegend und allgemein anerkannt, dass sie von vornherein und unmittelbar als theoretische Wissenschaft auftrat. Nun glaube ich aber, dass es der höchste Ruhm einer jeden Wissenschaft

But, here, for the first time in Hilbert's lectures, we come across an explicit suggestion that electrodynamics is the field that might provide the correct foundation for *all* of physics. In fact, Hilbert seems to have conceived a much more comprehensive, unified picture of science that also covered all of mathematics and physics. In a somewhat unclear passage, Hilbert claimed:

> It appears, however, as if theoretical physics has finally and totally been absorbed by electrodynamics, to the extent that every special question should be solved, in the last instance, by appealing to electrodynamics. Following the methods used prevailingly in the individual mathematical disciplines, one could also classify mathematics—more from the point of view of contents than from a formal one—into one-dimensional mathematics (i.e., arithmetic), then function theory (which in essence is limited to two dimensions), geometry, and finally four-dimensional mechanics.[74]

This more general, unified conception would remain basic to Hilbert's subsequent dealings with physics, and particularly to his involvement with the problems associated with the general theory of relativity.

While teaching his 1913–14 course, Hilbert's interest in the work of Gustav Mie must have already become evident. As already mentioned, Max Born lectured before the GMG on this theory in December 1912. On October 22, 1913, Mie wrote a letter to Hilbert expressing his satisfaction at the interest that Hilbert had manifested in Mie's recent work (presumably in an earlier letter which has not been preserved).[75] Then, on December 12, 1913, Born lectured again at the meeting of the GMG, this time on his own contributions to Mie's theory. The reformulation of Mie's theory at the hands of Born was to become a crucial turning-point in Hilbert's route to the problems of general relativity.[76] Incidentally, Born's lecture was communicated to the society by Hilbert himself.[77] At the same time, recent work on gravitation and relativity began to be discussed more intensely in Göttingen. Einstein and Grossmann's *Entwurf* paper, as already mentioned, was discussed in the GMG on December 12, 1913, and the works of Max Abraham and Gunnar Nordström were also studied with great interest.[78]

74 Hilbert (1913–14, p. 1): 'Es scheint indessen, als ob die theoretische Physik schliesslich ganz und gar in der Elektrodynamik aufgeht, insofern jede einzele noch so spezielle Frage in letzter Instanz an die Elektrodynamik appellieren muss. Nach den Methoden, die die einzelnen mathematischen Disziplinen vorwiegend benutzen, könnte man alsdann mehr inthaltlich als formel—die Mathematik einteilen in die eindimensionale Mathematik, die Arithmetik, ferner in die Funktionentheorie, die sich im wesentlichen auf zwei Dimensionen beschränkt, in die Geometrie, und schliesslich in die vierdimensionale Mechanik.'

75 Mie's letter is in Hilbert's Nachlass, NSUB Göttingen - Cod Ms David Hilbert 254 1.

76 For more details see Corry (1999a, §3).

77 See the announcement in *Jahrb. DMV* Vol. 22, p. 207. Born's work appeared later as Born (1913).

Hilbert's physical activities in 1914 were less intense than in previous years. He published the third instalment of his work on the foundations of the theory of radiation, and lectured once again on statistical mechanics. Also, in June 1914, his student Kurt Schellenberg presented a dissertation dealing with the applications of integral equations to the theories of electrolysis (Schellenberg, 1915). The beginning of the war obviously altered the normal course of activities in Göttingen, and in particular the presence of students and young docents there over the next years declined steeply. As early as November 3, 1914, Hilbert discussed the consequences of the war on the GMG's activities in the meeting of the society.[79]

In the summer semester of 1915 Hilbert lectured on the structure of matter. The notes of this course illuminate yet another significant aspect of Hilbert's wide range of physical knowledge and interests. The course focused, in fact, on Born's theory of crystals, which was based on the study of the potential energy of a lattice of particles. For Hilbert, this theory and the theory of dilute gases were complementary to each other in accounting for the properties of matter (Hilbert, 1915, p. 1). Hilbert discussed briefly the mathematical aspects of the theory (crystallographic groups), and also, in much more detail, its physical aspects: wave displacement inside the lattice, crystal elasticity, specific heat in the lattice, the piezo-electric effect, etc.

The announcement of meetings at the GMG for the summer semester of 1915 record a lecture on structure of crystals by Felix Klein (together with Hilbert and Mügge) in May, and a lecture by Sommerfeld 'On modern physics' in June. Then from June 29 to July 7, 1915, Einstein gave a series of lectures on the current state of his research on gravitation and relativity. Unfortunately not much is known about the visit itself, except that, as I mentioned in the introduction to this article, Einstein felt that his work had been understood down to the details.[80] In the months following that visit, and especially in October and November, Hilbert devoted most of his efforts to what he later called 'the foundations of physics', namely, the formulation of a unified field theory, based on Mie's electromagnetic theory of matter.[81] He presented the results of his investigations in the meeting of the Göttingen Scientific Society on November 20, 1915.[82]

Hilbert's work during these months and the publication of his field equations of gravitation opened a new phase in his career, during which he and his colleagues in Göttingen would dedicate much efforts to the general

79 See the announcements of the meetings of the GMG for that year in the *JDMV* Vol. 23 (1914).

80 I have made some efforts to gather documents related to this visit, so far without much success. What I did find in Hilbert's Nachlass in Göttingen, nevertheless, are the handwritten notes taken from the first of Einstein's lectures (Staats- und Universitsbibliothek Göttingen, Cod Ms D Hilbert 724). These notes have been published meanwhile in Kox *et al.*, (1996), App.

theory of relativity.[83] From that time on, Hilbert became increasingly enthusiastic about the significance of general covariance. In a lecture held in 1921, for instance, he asserted that no other discovery in history had sparked so much interest and excitement as had Einstein's relativity theory ('the highest achievement of human spirit'). This excitement was indeed justified in Hilbert's view since, whereas all former laws of physics were provisory, inexact and special, the principle of relativity (and Hilbert meant by this the general covariance of physical laws) signified 'for the first time since the world exists, a definitive, exact and general expression of the natural laws that hold in reality.'[84] At the same time, Hilbert's involvement with general relativity provided additional support to his empiricist conception of geometry. Although a detailed analysis of the place of geometry in Hilbert's work on relativity would be much beyond the scope of the present account, it is pertinent to quote here from the manuscript of his course on this issue, taught in the winter semester of 1916–17, where Hilbert expressed this connection as part of a more general, unified view of all branches of human knowledge. Hilbert thus said:

> In the past, physics adopted the conclusions of geometry without further ado. This was justified insofar as not only the rough, but also the finest physical facts confirmed those conclusions. This was also the case when Gauss measured the sum of angles in a triangle and found that it equals two right ones. That is no longer the case for the new physics. *Modern physics must draw geometry into the realm of its investigations.* This is logical and natural: every science grows like a tree, of which not only the branches continually expand, but also the roots penetrate deeper.
>
> Some decades ago one could observe a similar development in mathematics. A theorem was considered to have been proved according to Weierstrass if it could be reduced to relations among integer numbers, whose laws were assumed to be given. Any further dealings with the latter were laid aside and entrusted to the philosophers. Kronecker said once: 'The good Lord created the integer numbers.' These were at that time a touch-me-not (*noli me tangere*) of mathematics. That was the case until the logical foundations of this science began to stagger. The integer numbers turned then into one of the most fruitful research domains of mathematics, and especially of set theory (Dedekind). The mathematician was thus compelled to become a philosopher, for otherwise he ceased to be a mathematician.
>
> The same happens now: *the physicist must become a geometer*, for otherwise he runs the risk of ceasing to be a physicist and viceversa. The separation of the sciences into professions and faculties is an anthropological one, and it is thus foreign to reality as such. For a natural phenomenon does not ask about itself whether it is the business of a mathematician or of a physicist. On these grounds we should not be allowed to simply accept the axioms of geometry. The latter may be the expression of certain facts of experience that further experiments would contradict.[85]

But as was the case with his earlier contributions to physics, Hilbert's work in general relativity did not attain wide recognition. Einstein himself, like many other physicists, disliked both Hilbert's derivation of the equations from a variational principle and his excessive reliance on Mie's theory of matter. Nevertheless, seen in the context of his other contributions to physics, there can be no doubt that, as in other disciplines, Hilbert's involvement in general relativity was far from being a short-range, opportunistically motivated endeavour.

Concluding remarks

Hilbert had a sustained interest in physics that can be traced throughout his career. In the present article I have shown how this interest is manifest from his early dealings with geometry, around 1894, until 1915. This interest, however, continued to occupy a central place in Hilbert's overall conception of science until the end of his career. Although Hilbert's published work on physical issues covers only a small portion of his overall output, he actually dedicated a much more significant part of his efforts to various physical

83 In Corry (1998a,b), I give detailed historical accounts of Hilbert's way of general relativity, as well as the more immediate context of his publications. David Rowe's contribution to the present volume (Chapter 7) discusses the developments in Göttingen around general relativity between 1916 and 1918.

84 Hilbert (1921 [illegible]

85 Hilbert (1916–17, pp. 2–3) (Emphasis in the original): 'Früher übernahm die Physik die Lehren der Geometrie ohne weiteres. Dies war berechtigt, solange nicht nur die groben, sondern auch die feinsten physikalischen Tatsachen die Lehren der Geometrie bestätigen. Dies war noch der Fall, als Gauss die Winkelsumme im Dreieck experimentell mass und fand, dass sie zwei Rechte beträgt. Dies gilt aber nicht mehr von der neuesten Physik. *Die heutige Physik muss vielmehr die Geometrie mit in den Bereich ihrer Untersuchungen ziehen.* Das ist logish und naturgemäss; jede Wissenschaft wächst wie ein Baum, nicht nur die Zweige greifen weiter aus, sondern auch die Wurzeln dringen teifer.

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

So ist es auch jetzt wieder: *der Physiker muss Geometer werden*, weil sonst Gefahr läuft, aufzuhren, Physiker zusein und umgekehrt. Die Trennung der Wissenschaften in Fächer und Fakultäten ist eben etwas Antropologisches, und der Wirklichkeit Fremdes; denn eine Naturerscheinung frägt nicht danach, ob sie es mit einem Physiker oder mit einem Mathematiker zu tun hat. Aus diesem Grunde dürfen wir die Axiome der Geometrie nicht disciplines than the amount of his publications might indicate. This effort can properly be understood only by examining Hilbert's teaching and organizational activities in Göttingen, as I have done in the preceding pages.

In order to determine whether Hilbert's ideas had any actual influence on the development of twentieth century physics it is necessary to undertake a more detailed examination of his published and unpublished writings in each individual discipline, and of the people who were exposed to these ideas. I have suggested above that this influence may have been instrumental, in one way or another, in shaping the ideas of several persons who contributed significant work in physics: Max Born, and Hermann Minkowski, as well as other, relatively minor figures. Other names come to mind which were not mentioned in this article but which certainly were influenced by Hilbert in different domains of physics, such as Hermann Weyl in relativity and John von Neumann in quantum theory.

Many physicist reacted with lack of enthusiasm, to say the least, to Hilbert's incursions into physics. Einstein expressed many reservations concerning Hilbert's approach to relativity. In a letter to Hermann Weyl, he criticized Hilbert's use of the variational principle in this context and judged his approach to the general theory of relativity to be 'childish ... in the sense of a child that recognizes no malice in the external world.'[86] Weyl himself considered that Hilbert's work in physics was of rather limited value, especially when compared to his work in pure mathematics. In particular Weyl considered that Hilbert's application of the axiomatic method was of little significance for physics. Weyl thought that a valuable contribution to physics required a different kind of skill than those in which Hilbert excelled. In an obituary of Hilbert, Weyl wrote:

> The maze of experimental facts which the physicist has to take in account is too manifold, their expansion too fast, and their aspect and relative weight too changeable for the axiomatic method to find a firm enough foothold, except in the thoroughly consolidated parts of our physical knowledge. Men like Einstein and Niels Bohr grope their way in the dark toward their conceptions of general relativity or atomic structure by another type of experience and imagination than those of the mathematician, although no doubt mathematics is an essential ingredient.[87]

Max Born was perhaps the physicist that expressed the most consistent enthusiasm for Hilbert's physics. He seems also to have truly appreciated the exact nature of Hilbert's program for axiomatizing physical theories and the potential contribution that the realization of that program could bear. On the occasion of Hilbert's sixtieth birthday, the journal *Die Naturwissenschaften* dedicated one of its issues to celebrate the achievements of the master. Several

of his students were commissioned with articles summarizing Hilbert's contributions in different fields. Born, who as a young student in Göttingen attended many of Hilbert's courses, and later on as a colleague continued to participate in his seminars, wrote about Hilbert's physics. Besides his laudatory summary of Hilbert's achievements, Born also clarified, in a very succinct formulation, the nature of Hilbert's axiomatic treatment and why, in general, physicists tended not to appreciate it. Born put it in the following words:

> The physicist set outs to explore how things are in nature; experiment and theory are thus for him only a means to attain an aim. Conscious of the infinite complexities of the phenomena with which he is confronted in every experiment, he resists the idea of considering a theory as something definitive. He therefore abhors the word 'axiom', which in its usual usage evokes the idea of definitive truth. The physicist is thus acting in accordance with his healthy instinct, that dogmatism is the worst enemy of natural science. The mathematician, on the contrary, has no business with factual phenomena, but rather with logic interrelations. In Hilbert's language the axiomatic treatment of a discipline implies in no sense a definitive formulation of specific axioms as eternal truths, but rather the following methodological demand: specify the assumptions at the beginning of your deliberation, stop for a moment and investigate whether or not these assumptions are partly superfluous or contradict each other.
>
> (Born, 1922, p. 591)

In fact Hilbert never performed for a physical theory exactly the same kind of axiomatic analysis he had done for geometry, though he very often declared this to be the case. In no case in the setting of his lectures did Hilbert actually prove the independence, consistency or completeness of the axiomatic systems he introduced. In certain cases, like vector addition, he quoted works in which such proofs could be found. In other cases there were no such works to mention, and Hilbert limited himself to *stating* that his axioms are indeed independent. In yet other cases he barely mentioned anything about the independence or other properties of his axioms. His derivations of the basic laws of the various disciplines from the axioms are also rather sketchy, when they appear at all. Many times Hilbert simply declared that such a derivation was possible. Among his published works, his last article on radiation theory contains – perhaps under the pressure of criticism – a more detailed attempt to prove independence and consistency of a system of axioms for a physical theory. But what is clear in all cases is that Hilbert always considered that an axiomatization along the lines he suggested was plausible and could eventually be fully performed following the standards established in the *Grundlagen*. Be that as it may, there can be no doubt that the kind of conceptual clarification attained in Hilbert's examination of physical theories, as well as in works of those who followed

Whether or not physicists should have looked more closely at Hilbert's ideas than they actually did, and whether or not Hilbert's program for the axiomatization of physics had any influence on subsequent developments in this discipline, it is nevertheless important to stress that a full picture of Hilbert's own conception of *mathematics* cannot be complete without taking into account his views on physical issues and on the relationship between mathematics and physics. More specifically, a proper understanding of Hilbert's conception of the role of axioms in physical theories—a conception condensed in the passage by Born quoted above, and illustrated throughout this article—helps us understand his conception of the role of axioms in mathematical theories as well. The picture that arises from such an understanding is obviously very different from the somewhat widespread image of Hilbert as the champion of a formalistic conception of the nature of mathematics.

Acknowledgments

The research and archival work that preceded the writing of this article was conducted as part of a larger project on the historical context of the rise of the general theory of relativity at the Max-Planck-Institut für Wissenschaftsgeschichte in Berlin, where I worked during the academic year 1994 95. I would like to thank the staff for their warm hospitality and diligent cooperation, and especially Jürgen Renn for inviting me to participate in the project and for his constant encouragement. During my stay in Berlin, I benefited very much from illuminating discussions on the history of modern physics with Jürgen Renn, Tilman Sauer and John Stachel.

This article was partly written at the Dibner Institute during the academic year 1995–96. I wish to thank the Directors and staff of the DI, as well as the other fellows with whom I was fortunate to share my time. Special thanks to Ulrich Majer and David Rowe for long and interesting discussions on the issues analyzed in this article.

I also thank Jeremy Gray and the participants of the meeting on Geometry and Physics, 1900–1930, at the Open University, Milton Keynes (March 14–19, 1996) for their illuminating lectures and for their fruitful remarks and criticism following my own talks.

Arne Schirrmacher read an earlier version of this article and provided helpful corrections and suggestions for improvement. I thank him very much.

Original manuscripts are quoted in the text by permission of the Staats- und Universitätsbibliothek Göttingen (*Handschriftenabteilung*), the library of the Mathematisches Institut Universitt Göttingen. I thank Ralf Haubrich and the Hilbert Edition staff, for allowing me to quote the *motto* of this article from their provisory typescript draft of the *Tagebuch*. I owe special thanks to Peter

Appendix

This list has been compiled from the manuscripts of the lecture notes in the *Lesezimmer* of the mathematical institute in Göttingen. It is conceivable that additional sources may add further items this list.

Hilbert's Courses on Physics (1898–1927)

1. Mechanics	SS 1898
2. Lectures on Potential Theory	WS 1901 02
3. Selected Chapters of Potential Theory	SS 1902
4. Continuum Mechanics—Part I	WS 1902–03
5. Continuum Mechanics—Part II	SS 1903
6. Exercises on Mechanics	WS 1904 05
7. Logical Principles of Mathematical Thinking (and of Physics)	SS 1905
8. Mechanics	WS 1905 06
9. Continuum Mechanics	SS 1906
10. Lectures on Continuum Mechanics	WS 1906 07
11. Mechanics	WS 1910 11
12. Continuum Mechanics	SS 1911
13. Statistical Mechanics	WS 1911 12
14. Radiation Theory	SS 1912
15. Molecular Theory of Matter	WS 1912 13
16. Foundations of Mathematics (and the axiomatization of Physics)	SS 1913
17. Electron Theory	SS 1913
18. Electromagnetic Oscillations	WS 1913 14
19. Statistical Mechanics	SS 1914
20. Lectures on the Structure of Matter	SS 1915
21. The Foundations of Physics I (General Relativity)	SS 1916
22. The Foundations of Physics II (General Relativity)	WS 1916 17
23. Electron Theory	SS 1917
24. Space and Time	WS 1917 18
25. Space and Time	WS 1918 19
26. Mechanics and the New Theory of Gravitation	SS 1920
27. Basic Principles of the Theory of Relativity	SS 1921
28. Statistical Mechanics	SS 1922

30. On the Unity of Science	WS 1923–24
31. Mechanics and Relativity Theory	SS 1924
32. Mathematical Methods of Quantum Theory	WS 1926/27

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7 The Göttingen response to general relativity and Emmy Noether's theorems

David Rowe

Emmy Noether's name is usually connected with the emergence of modern algebra at Göttingen during the 1920s.[1] Although widely regarded by contemporary algebraists as the leading figure in the field, she was also known to both mathematicians and physicists—including Klein, Hilbert, Weyl, and Einstein—for her work on differential invariants and the connections between variational principles and conservation laws in physics. During the war years in Göttingen, Noether served as an assistant to Felix Klein and David Hilbert when both were immersed in work on general relativity theory. In this paper, I will not enter deeply into Noether's published work nor will I discuss its impact on subsequent research. My purpose instead is to show the central part she played in the context of the overall response to general relativity by Göttingen mathematicians beginning with the 'codiscovery' of the gravitational field equations by Einstein and Hilbert in November 1915.

In speaking of the Göttingen response to general relativity, I will concentrate primarily on the mathematicians within the Göttingen scientific community or those, like Zürich's Hermann Weyl, with close ties to this core group.[2] In particular, I wish to focus on their efforts to come to grips with fundamental questions concerning the role of conservation laws derived from variational principles in general relativity. These and related issues were already raised by Hilbert in his first note of November 1915 on 'Die Grundlagen der Physik', and subsequently addressed by Lorentz, Einstein, Weyl, Klein, and Emmy Noether.[3] Beginning in early 1917, Klein carried on a

1 For a global survey of Noether's influence on the 'image' of algebra, see Corry (1996).
2 For further information on physics in Göttingen, see Pauli (1921); Vizgin (1994); Sigurdsson (1991); Sigurdsson (1994).
3 Aside from Lorentz's work, the others are discussed and cited below. On Lorentz's views on