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THE ORIGIN OF HILBERT'S AXIOMATIC METHOD[1]

1. AXIOMATICS, GEOMETRY AND PHYSICS IN HILBERT'S EARLY LECTURES

This chapter examines how Hilbert's axiomatic approach gradually consolidated over the last decade of the nineteenth century. It goes on to explore the way this approach was actually manifest in its earlier implementations.

Although geometry was not Hilbert's main area of interest before 1900, he did teach several courses on this topic back in Königsberg and then in Göttingen. His lecture notes allow an illuminating foray into the development of Hilbert's ideas and they cast light on how his axiomatic views developed.[2]

1.1 Geometry in Königsberg

Hilbert taught projective geometry for the first time in 1891 (Hilbert 1891). What already characterizes Hilbert's presentation of geometry in 1891, and will remain true later on, is his clearly stated conception of this science as a natural one in which, at variance with other mathematical domains, sensorial intuition— *Anschauung*—plays a fundamental role that cannot be relinquished. In the introduction to the course, Hilbert formulated it in the following words:

> Geometry is the science that deals with the properties of space. It differs essentially from pure mathematical domains such as the theory of numbers, algebra, or the theory of functions. The results of the latter are obtained through pure thinking... The situation is completely different in the case of geometry. I can never penetrate the properties of space by pure reflection, much as I can never recognize the basic laws of mechanics, the law of gravitation or any other physical law in this way. Space is not a product of my reflections. Rather, it is given to me through the senses. I thus need my senses in order to fathom its properties. I need intuition and experiment, just as I need them in order to figure out physical laws, where also matter is added as given through the senses.[3]

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1 This chapter is based on extracts from (Corry 2004), in particular on chapters 2, 3, and 5.

2 An exhaustive analysis of the origins of *Grundlagen der Geometrie* based on these lecture notes and other relevant documents was first published in (Toepell 1986). Here we draw directly from this source.

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

Jürgen Renn (ed.). *The Genesis of General Relativity,* Vol. 4
Theories of Gravitation in the Twilight of Classical Physics:
The Promise of Mathematics and the Dream of a Unified Theory

The most basic propositions related to this intuition concern the properties of incidence, and in order to express them conveniently it is necessary to introduce "ideal elements." Hilbert stressed that these are to be used here only as a shorthand with no metaphysical connotations.

In the closing passage of his lecture, Hilbert briefly discussed the connections between analytic and projective geometry. While the theorems and proofs of the former are more general than those of the latter, he said, the methods of the latter are much purer, self-contained, and necessary.[4] By combining synthetic and axiomatic approaches, Hilbert hinted, it should be possible, perhaps, to establish a clear connection between these two branches of the discipline.

In September of that year, Hilbert attended the *Deutsche Mathematiker- Vereinigung* meeting in Halle, where Hermann Wiener (1857–1939) lectured on the foundations of geometry.[5] The lecture could not fail to attract Hilbert's attention given his current teaching interests. Blumenthal reported in 1935 that Hilbert came out greatly excited by what he had just heard, and made his famous declaration that it must be possible to replace "point, line, and plane" with "table, chair, and beer mug" without thereby changing the validity of the theorems of geometry (Blumenthal 1935, 402–403). Seen from the point of view of later developments and what came to be considered the innovative character of *Grundlagen der Geometrie,* this may have been indeed a reason for Hilbert's enthusiasm following the lecture. If we also recall the main points of interest in his 1891 lectures, however, we can assume that Wiener's claim about the possibility of proving central theorems of projective geometry without continuity considerations exerted no lesser impact, and perhaps even a greater one, on Hilbert at the time. Moreover, the idea of changing names of the central concepts while leaving the deductive structure intact was an idea that Hilbert already knew, if not from other, earlier mathematical sources, then at least from his attentive reading of the relevant passages in Dedekind's *Was sind und was sollen die Zahlen?*,[6] where he may not have failed to see the introductory remarks on the role of continuity in geometry. If Hilbert's famous declaration was actually pronounced for the first time after this lecture, as Blumenthal reported, one can then perhaps conclude that Wiener's ideas were more than just a revelation for Hilbert, but acted as a catalyst binding together several threads that may have already been present in his mind for a while.

Roughly at the time when Hilbert's research efforts started to focus on the theory of algebraic number fields, from 1893 on, his interest regarding the foundations of geometry also became more intensive, at least at the level of teaching. In preparing a course on non-Euclidean geometry to be taught that year, Hilbert was already adopting a more axiomatic perspective. The original manuscript of the course clearly reveals that Hilbert had decided to follow more closely the model put forward by Pasch. As for the latter, using the axiomatic approach was a direct expression of a nat-

4 Cf. (Toepell 1986, 37).

5 He may have also attended Wiener's second lecture in 1893. Cf. (Rowe 1999, 556).

6 As we know from a letter to Paul du Bois-Reymond of March-April, 1888. Cf. (Dugac 1976, 203).

uralistic approach to geometry, rather than a formalistic one: the axioms of geometry—Hilbert wrote—express observations of facts of experience, which are so simple that they need no additional confirmation by physicists in the laboratory.[7] From his correspondence with Felix Klein (1849–1925),[8] however, we learn that Hilbert soon realized certain shortcomings in Pasch's treatment, and in particular, certain redundancies that affected it. Hilbert explicitly stipulated at this early stage that a successful axiomatic analysis should aim to establish the *minimal* set of presuppositions from which the whole of geometry could be deduced. Such a task had not been fully accomplished by Pasch himself, Hilbert pointed out, since his Archimedean axiom, could be derived from others in his system.

Hilbert's correspondence also reveals that he kept thinking about the correct way to implement an axiomatic analysis of geometry. In a further letter to Klein, on 15 November while criticizing Lie's approach to the foundations of geometry, he formulated additional tasks to be accomplished by such an analysis. He thus wrote:

> It seems to me that Lie always introduces into the issue a preconceived one-sidedly analytic viewpoint and forgets completely the principal task of non-Euclidean geometry, namely, that of constructing the various possible geometries by the successive introduction of elementary axioms, up until the final construction of the only remaining one, Euclidean geometry.[9]

The course on non-Euclidean geometry was not taught as planned in 1893, since only one student registered for it.[10] It did take place the following year, announced as "Foundations of Geometry." Hilbert had meanwhile considerably broadened his reading in the field, as indicated by the list of almost forty references mentioned in the notes. This list included most of the recent, relevant foundational works. A clear preference for works that followed an empiricist approach is evident, but also articles presenting the ideas of Grassmann were included.[11] It is not absolutely clear to what extent Hilbert read Italian, but none of the current Italian works were included in his list, except for a translated text of Peano (being the only one by a non-German author).[12] It seems quite certain, at any rate, that Hilbert was unaware of the recent works of Fano, Veronese, and others, works that could have been of great interest for him in the direction he was now following.

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7 "Das Axiom entspricht einer Beobachtung, wie sich leicht durch Kugeln, Lineal und Pappdeckel zeigen lässt. Doch sind diese Erfahrungsthatsachen so einfach, von Jedem so oft beobachtet und daher so bekannt, dass der Physiker sich nicht extra im Laboratorium bestätigen darf." (Hilbert 1893–1894, 10)

8 Hilbert to Klein, 23 May 1893. Quoted in (Frei 1985, 89–90).

9 Hilbert to Klein, 15 November 1893. Quoted in (Frei 1985, 101). On 11 November, he wrote an almost identical letter to Lindemann. Cf. (Toepell 1986, 47).

10 Cf. (Toepell 1986, 51).

11 The full bibliographical list appears in (Toepell 1986, 53–55).

12 At the 1893 annual meeting of the *Deutsche Mathematiker- Vereinigung* in Lübeck (16–20 September), Frege discussed Peano's conceptual language. If not earlier than that, Hilbert certainly heard about Peano's ideas at this opportunity, when he and Minkowski also presented the plans for their expected reports on the theory of numbers. Cf. *Jahresbericht der Deutschen Mathematiker-Vereinigung,* Vol. 4 (1894–1895), p. 8.

Hilbert became acquainted with Hertz's book on the foundations of mechanics, though it was not mentioned in the list. This book seems to have provided a final, significant catalyst for the wholehearted adoption of the axiomatic perspective for geometry. Simultaneously the book established, in Hilbert's view, a direct connection between the latter and the axiomatization of physics in general. Moreover, Hilbert adopted Hertz's more specific, methodological ideas about what is actually involved in axiomatizing a theory. The very fact that Hilbert came to hear about Hertz is not surprising; he would probably have read Hertz's book sooner or later. But that he read it so early was undoubtedly due to Minkowski. During his Bonn years, Minkowski felt closer to Hertz and to his work than to anyone else, and according to Hilbert, his friend had explicitly declared that, had it not been for Hertz's untimely death, he would have dedicated himself exclusively to physics.[13]

Just as with many other aspects of Hilbert's early work, there is every reason to believe that Minkowski's enthusiasm for Hertz was transmitted to his friend. When revising the lecture notes for his course, Hilbert added the following comment:

> Nevertheless the origin [of geometrical knowledge] is in experience. The axioms are, as Hertz would say, pictures 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.[14]

Hilbert defined the task to be pursued as part of the axiomatic analysis, including the need to establish the independence of the axioms of geometry. In doing so, however, he stressed once again the objective and factual character of this science. 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.[15]

But already at this point it is absolutely clear that, for Hilbert, such questions were not just abstract tasks. Rather, he was directly focused on important, open problems of the discipline, and in particular, on the role of the axiom of continuity in the questions of coordinatization and metrization in projective geometry, as well as in the proof of the fundamental theorems. In a passage that was eventually crossed out, Hilbert expressed his doubts about the prospects of actually proving Wiener's assertion that continuity considerations could be circumvented in projective geometry (Toepell

13 See (Hilbert 1932–1935, 3: 355). Unfortunately, there seems to be no independent confirmation of Minkowski's own statement to this effect.

14 "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 wieder Bilder der Folgen sind d.h. was wir aus den Bildern logisch ableiten, stimmt wieder in der Natur." It is worth noting that Hilbert's quotation of Hertz, drawn from memory, was somewhat inaccurate. I am indebted to Ulrich Majer for calling my attention to this passage. (Hilbert 1893–1894, 10)

15 Quoted from the original in (Toepell 1986, 58–59).

1986, 78). Eventually, however, a main achievement of *Grundlagen der Geometrie* would be a detailed realization of this possibility and its consequences, but Hilbert probably decided to follow this direction only after hearing about the result of Friedrich Schur (1856–1932) in 1898. I return to this matter in the next section.

Concerning the validity of the parallel axiom, Hilbert adopted in 1893–1894 a thoroughly empirical approach that reminds us very much of Riemann's *Habilitationsschrift*. Hilbert referred also directly to Gauss's experimental measurement of the sum of angles of the triangle described by three Hannoverian mountain peaks.[16] Although Gauss's measurements were convincing enough for Hilbert to indicate the correctness of Euclidean geometry as a true description of physical space, he still saw an open possibility that future measurements would show it to be otherwise. Hilbert also indicated that existing astronomical observations are not decisive in this respect, and therefore the parallel axiom must be taken at least as a limiting case. In his later lectures on physics, Hilbert would return to this example very often to illustrate the use of axiomatics in physics. In the case of geometry, this particular axiom alone might be susceptible to change following possible new experimental discoveries. Thus, what makes geometry especially amenable to a full axiomatic analysis is the very advanced stage of development it has attained, rather than any other specific, essential trait concerning its nature. In all other respects, geometry is like any other natural science. Hilbert thus stated:

> Among the appearances or facts of experience manifest to us in the observation of nature, there is a peculiar type, namely, those facts concerning the outer shape of things. Geometry deals with these facts. ... Geometry is a science whose essentials are developed to such a degree, that all its facts can already be logically deduced from earlier ones. Much different is the case with the theory of electricity or with optics, in which still many new facts are being discovered. Nevertheless, with regards to its origins, geometry is a natural science.[17]

It is the very process of axiomatization that transforms the natural science of geometry, with its factual, empirical content, into a pure mathematical science. There is no apparent reason why a similar process might not be applied to any other natural science. And in fact, from very early on Hilbert made it clear that this should be done. In the manuscript of his lectures we read that "all other sciences—above all mechanics, but subsequently also optics, the theory of electricity, etc.—should be treated according to the model set forth in geometry."[18]

16 The view that Gauss considered his measurement as related to the question of the parallel axiom has been questioned in (Breitenberger 1984) and (Miller 1972). They have argued that this measurement came strictly as a part of Gauss's geodetic investigations. For replies to this argument, see (Scholz 1993, 642–644), and a more recent and comprehensive discussion in (Scholz 2004). Hilbert, at any rate, certainly believed that this had been Gauss's actual intention, and he repeated this opinion on many occasions.

17 Quoted in (Toepell 1986, 58).

18 Quoted in (Toepell 1986, 94).

By 1894, then, Hilbert's interest in foundational issues of geometry had increased considerably, and he had embarked more clearly in an axiomatic direction. His acquaintance with Hertz's ideas helped him conceive the axiomatic treatment of geometry as part of a larger enterprise, relevant also for other physical theories. It also offered methodological guidelines for actually implementing this analysis. However, many of the most important foundational problems remained unsettled for him, and in this sense, even the axiomatic approach did not seem to him to be of great help. At this stage he saw in the axiomatic method no more than an exercise in adding or deleting basic propositions and guessing the consequences that would follow, but certainly not a tool for achieving real new results.[19]

1.2 Geometry in Göttingen

Hilbert moved to Göttingen in 1895 and thereafter he dedicated himself almost exclusively to number theory both in his research and in his teaching. It is worth pointing out, that some of the ideas he developed in this discipline would prove to be essential some years later for his treatment of geometry as presented in *Grundlagen der Geometrie*. In particular, Hilbert's work on the representation of algebraic forms as sums of squares, which had a deep influence on the subsequent development of the theory of real fields,[20] also became essential for Hilbert's own ideas on geometrical constructivity as manifest in *Grundlagen der Geometrie*.

In the summer semester of 1899, Hilbert once again taught a course on the elements of Euclidean geometry. The elaboration of these lectures would soon turn into the famous *Grundlagen der Geometrie*. The very announcement of the course came as a surprise to many in Göttingen, since it signified, on the face of it, a sharp departure from the two fields in which he had excelled since completing his dissertation in 1885: the theory of algebraic invariants and the theory of algebraic number fields. As Blumenthal recalled many years later:

> [The announcement] aroused great excitement among the students, since even the veteran participants of the 'number theoretical walks' (*Zahlkörpersspaziergängen*) had never noticed that Hilbert occupied himself with geometrical questions. He spoke to us only about fields of numbers. (Blumenthal 1935, 402)

Also Hermann Weyl (1855–1955) repeated this view in his 1944 obituary:

> [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*. (Weyl 1944, 635)

As already suggested, however, the break may have been less sharp than it appeared in retrospect to Hilbert's two distinguished students. Not only because of the strong connections of certain, central results of *Grundlagen der Geometrie* to Hil-

19 As expressed in a letter to Hurwitz, 6 June 1894. See (Toepell 1986, 100).
20 Cf. (Sinaceur 1984, 271–274; 1991, 199–254).

bert's number-theoretical works, or because of Hilbert's earlier geometry courses in Königsberg, but also because Hilbert became actively and intensely involved in current discussions on the foundations of projective geometry starting in early 1898. In fact, at that time Hilbert had attended a lecture in Göttingen given by Schoenflies who discussed a result recently communicated by Schur to Klein, according to which Pappus's theorem could be proven starting from the axioms of congruence alone, and therefore without relying on continuity considerations.[21] Encouraged by this result, and returning to questions that had been raised when he taught the topic several years earlier, Hilbert began to elaborate on this idea in various possible alternative directions. At some point, he even thought, erroneously as it turned out, to have proved that it would suffice to assume Desargues's theorem in order to prove Pappus's theorem.[22]

Schur's result provided the definitive motivation that led Hilbert to embark on an effort to elucidate in detail the fine structure 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. The axiomatic method, whose tasks and basic tools Hilbert had been steadily pondering, would now emerge as a powerful and effective instrument for properly addressing these important issues.

The course of 1899 contains much of what will appear in *Grundlagen der Geometrie*. It is worth pointing out here that in the opening lecture Hilbert stated once again the main achievement he expected to obtain from an axiomatic analysis of the foundations of geometry: a complete description, by means of independent statements, of the basic facts from which all known theorems of geometry can be derived. This time he also mentioned the precise source from which this formulation had been taken: the introduction to Hertz's *Principles of Mechanics*.[23] In Hilbert's view, this kind of task was not limited to geometry, and of course also applied, above all, to mechanics. Hilbert had taught seminars on mechanics jointly with Klein in 1897–1898. In the winter semester 1898–1899, he also taught his first full course on a physical topic in Göttingen: mechanics.[24] In the introduction to this course, he explicitly stressed 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:

> Geometry also [like mechanics] emerges from the observation of nature, from experience. To this extent, it is an *experimental science*. ... But its experimental foundations are

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21 Later published as (Schur 1898).

22 Cf. (Toepell 1986, 114–122). Hessenberg (1905) proves that, in fact, it is Pappus's theorem that implies Desargues's, and not the other way round.

23 Cf. (Toepell 1986, 204).

24 According to the *Nachlass* David Hilbert (Niedersächsische Staats- und Universitätsbibliothek Göttingen, Abteilung Handschriften und Seltene Drucke), (Cod. Ms. D. Hilbert, 520), which contains a list of Hilbert's lectures between 1886 and 1932 (handwritten by Hilbert himself up until 1917–1918), among the earliest courses taught by Hilbert in Königsberg was one in Hydrodynamics (summer semester, 1887).

> 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 edifice 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 ever stretch the limits of pure mathematics wider, on behalf not only of our mathematical interest, but rather of the interest of science in general.[25]

This is perhaps the first explicit presentation of Hilbert's program for axiomatizing natural science in general. The more definitive status of the results of geometry, as compared to the relatively uncertain one of our knowledge of mechanics, clearly recalls similar claims made by Hertz. The difference between geometry and other physical sciences—mechanics in this case—was not for Hilbert one of essence, but rather one of historical stage of development. He saw no reason in 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 would attain a degree of development equal to geometry, in terms of the quantity and certainty of known results, and in terms of an appreciation of what really are the "basic facts" on which the theory is based.

2. GRUNDLAGEN DER GEOMETRIE

When Hilbert published his 1899 *Festschrift* (Hilbert 1899) he was actually contributing a further link to a long chain of developments in the foundations of geometry that spanned several decades over the nineteenth century. His works on invariant theory and number theory can be described in similar terms, each within its own field of relevance. In these two fields, as in the foundations of geometry, Hilbert's contribution can be characterized as the "critical" phase in the development of the discipline: a phase in which the basic assumptions and their specific roles are meticulously inspected in order to revamp the whole structure of the theory on a logically sound

25 "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 weiteren 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 Masse wie die Geometrie als eine *rein mathematische* Disciplin zu bezeichnen ist. Wir müssen streben, dass sie es wird. Wir müssen die Grenzen echter Math. immer weiter ziehen nicht nur in unserem math. Interesse sondern im Interesse der Wissenschaft überhaupt." (Hilbert 1898–1899, 1–3)

basis and within a logically transparent deductive structure. This time, however, Hilbert had consolidated the critical point of view into an elaborate approach with clearly formulated aims, and affording the proper tools to achieve those aims, at least partly. This was the axiomatic approach that characterizes *Grundlagen der Geometrie* and much of his work thereafter, particularly his research on the foundations of physical theories. However, *Grundlagen der Geometrie* was innovative not only at the methodological level. It was, in fact, a seminal contribution to the discipline, based on a purely synthetic, completely new approach to arithmetizing the various kinds of geometries. And again, as in his two previous fields of research, Hilbert's in-depth acquaintance with the arithmetic of fields of algebraic numbers played a fundamental role in his achievement.

It is important to bear in mind that, in spite of the rigor required for the axiomatic analysis underlying Grundlagen der Geometrie, many additions, corrections and improvements—by Hilbert himself, by some of his collaborators and by other mathematicians as well— were still needed over the following years before the goals of this demanding project could be fully attained. Still most of these changes, however important, concerned only the details. The basic structure, the groups of axioms, the theorems considered, and above all, the innovative methodological approach implied by the treatment, all these remained unchanged through the many editions of *Grundlagen der Geometrie*.

The motto of the book was a quotation taken from Kant's *Critique of Pure Reason*: "All human knowledge thus begins with intuitions, proceeds thence to concepts and ends with ideas." If he had to make a choice, Kant appears an almost obvious one for Hilbert in this context. It is hard to state precisely, however, to what extent he had had the patience to become really acquainted with the details of Kant's exacting works. Beyond the well-deserved tribute to his most distinguished fellow Königsberger, this quotation does not seem to offer a reference point for better understanding Hilbert's ideas on geometry.

Hilbert described the aim of his *Festschrift* as an attempt to lay down a "simple" and "complete" system of "mutually independent" axioms, from which all known theorems of geometry might be deduced. His axioms are formulated for three systems of undefined objects named "points," "lines," and "planes," and they establish mutual relations that these objects must satisfy. The axioms are divided into five groups: axioms of incidence, of order, of congruence, of parallels, and of continuity. From a purely logical point of view, the groups have no real significance in themselves. However, from the geometrical point of view they are highly significant, for they reflect Hilbert's actual conception of the axioms as an expression of spatial intuition: each group expresses a particular way that these intuitions manifest themselves in our understanding.

2.1 Independence, Simplicity, Completeness, Consistency

Hilbert's first requirement, that the axioms be independent, is the direct manifestation of the foundational concerns that directed his research. When analyzing independence, his interest focused mainly on the axioms of congruence, continuity and of parallels, since this independence would specifically explain how the various basic theorems of Euclidean and projective geometry are logically interrelated. But as we have seen, this requirement had already appeared—albeit more vaguely formulated—in Hilbert's early lectures on geometry, as a direct echo of Hertz's demand for appropriateness. In *Grundlagen der Geometrie*, the requirement of independence not only appeared more clearly formulated, 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 that fail to satisfy a given axiom of the system but satisfy all the others. However, this was not for Hilbert an exercise in analyzing abstract relations among systems of axioms and their possible models. The motivation for enquiring about the mutual independence of the axioms remained, essentially, a geometrical one. For this reason, Hilbert's original system of axioms was not the most economical one from the logical point of view. Indeed, several mathematicians noticed quite soon that Hilbert's system of axioms, seen as a single collection rather than as a collection of five groups, contained a certain degree of redundancy.[26] Hilbert's own aim was to establish the interrelations among the groups of axioms, embodying the various manifestations of special intuition, rather than among individual axioms belonging to different groups.

The second requirement, simplicity, complements that of independence. It means, roughly, that an axiom should contain "no more than a single idea." This is a requirement that Hertz also had explicitly formulated, and Hilbert seemed to be repeating it in the introduction to his own book. Nevertheless, it was neither formally defined nor otherwise realized in any clearly identifiable way within *Grundlagen der Geometrie*. The ideal of formulating "simple" axioms as part of this system was present implicitly as an aesthetic desideratum that was not transformed into a mathematically controllable feature.[27]

The "completeness" that Hilbert demanded for his system of axioms should not be confused with the later, model-theoretical notion that bears the same name, a

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26 Cf., for instance, (Schur 1901). For a more detailed analysis of this issue, see (Schmidt 1933, 406–408). It is worth pointing out that in the first edition of *Grundlagen der Geometrie* Hilbert stated that he intended to provide an independent system of axioms for geometry. In the second edition, however, this statement no longer appeared, following a correction by E. H. Moore (1902) who showed that one of the axioms might be derived from the others. See also (Corry 2003, § 3.5; Torretti 1978, 239 ff.).

27 In a series of articles published in the USA over the first decade of the twentieth century under the influence of *Grundlagen der Geometrie*, see (Corry 2003, § 3.5), a workable criterion for simplicity of axioms was systematically 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 has not been satisfactorily defined, much less attained."

notion that is totally foreign to Hilbert's axiomatic approach at this early stage. Rather it is an idea that runs parallel to Hertz's demand for "correctness." Thus, Hilbert demanded from any adequate axiomatization that it should allow for a derivation of all the known theorems of the discipline in question. The axioms formulated in *Grundlagen der Geometrie*, Hilbert claimed, would indeed yield all the known results of Euclidean geometry or of the so-called absolute geometry, namely that valid independently of the parallel postulate, if the corresponding group of axioms is ignored. Thus, reconstructing the very ideas that had given rise to his own conception, Hilbert discussed in great detail the role of each of the groups of axioms in the proofs of two crucial results: the theorem of Desargues and the theorem of Pappus. Unlike independence, however, the completeness of the system of axioms is not a property that Hilbert knew how to verify formally, except to the extent that, starting from the given axioms, he could prove all the theorems he was interested in.

The question of consistency of the various kinds of geometries was an additional concern of Hilbert's analysis, though, perhaps somewhat surprisingly, one that was not even explicitly mentioned in the introduction to *Grundlagen der Geometrie*. He addressed this issue in the *Festschrift* right after introducing all the groups of axioms and after discussing their immediate consequences. Seen from the point of view of Hilbert's later metamathematical research and the developments that followed it, the question of consistency might appear as the most important one undertaken back in 1899; but in the historical context of the evolution of his ideas it certainly was not. In fact, consistency of the axioms is discussed in barely two pages, and it is not immediately obvious why Hilbert addressed it at all. It doesn't seem likely that in 1899 Hilbert would have envisaged the possibility that the body of theorems traditionally associated with Euclidean geometry might contain contradictions. After all, he conceived Euclidean geometry as an empirically motivated discipline, turned into a purely mathematical science after a long, historical process of evolution and depuration. Moreover, and more importantly, Hilbert had presented a model of Euclidean geometry over certain, special types of algebraic number fields. If with the real numbers the issue of continuity might be thought to raise difficulties that called for particular care, in this case Hilbert would have no real reason to call into question the possible consistency of these fields of numbers. Thus, to the extent that Hilbert referred here to the problem of consistency, he seems in fact to be echoing here Hertz's demand for the permissibility of images. As seen above, a main motivation leading Hertz to introduce this requirement was the concern about possible contradictions brought about over time by the gradual addition of ever new hypotheses to a given theory. Although this was not likely to be the case for the well-established discipline of geometry, it might still have happened that the particular way in which the axioms had been formulated in order to account for the theorems of this science would have led to statements that contradict each other. The recent development of non-Euclidean geometries made this possibility only more patent. Thus, Hilbert believed that, although contradictions might in principle possibly occur within his own system, he could also easily show that this was actually not the case.

The relatively minor importance conceded by Hilbert in 1899 to the problem of the consistency of his system of axioms for Euclidean geometry is manifest not only in the fact that he devoted just two pages to it. Of course, Hilbert could not have in mind a direct proof of consistency here, but rather an indirect one, namely, a proof that any contradiction existing in Euclidean geometry must manifest itself in the arithmetic system of real numbers. This would still leave open the question of the consistency of the latter, a problem difficult enough in itself. However, even an indirect proof of this kind does not appear in explicit form in *Grundlagen der Geometrie*. Hilbert only suggested that it would suffice to show that the specific kind of synthetic geometry derivable from his axioms could be translated into the standard Cartesian geometry, taking the axes as representing the whole field of real numbers.[28] More generally stated, in this first edition of *Grundlagen der Geometrie*, Hilbert preferred to bypass a systematic treatment of the questions related to the structure of the system of real numbers. Rather, he contented himself with constructing a model of his system based on a countable, proper sub-field—of whose consistency he may have been confident—and not the whole field of real numbers (Hilbert 1899, 21). It was only in the second edition of *Grundlagen der Geometrie*, published in 1903, that he added an additional axiom, the so-called "axiom of completeness" (*Vollständigkeitsaxiom*), meant to ensure that, although infinitely many incomplete models satisfy all the other axioms, there is only one complete model that satisfies this last axiom as well, namely, the usual Cartesian geometry, obtained when the whole field of real numbers is used in the model (Hilbert 1903a, 22–24). As Hilbert took pains to stress, this axiom cannot be derived from the Archimedean axiom, which was the only one included in the continuity group in the first edition.[29] It is important to notice, however, that the property referred to by this axiom bears no relation whatsoever to Hilbert's general requirement of "completeness" for any system of axioms. Thus his choice of the term "*Vollständigkeit*" in this context seems somewhat unfortunate.

3. THE 1900 LIST OF PROBLEMS

Soon after the publication of *Grundlagen der Geometrie*, Hilbert had a unique opportunity to present his views on mathematics in general and on axiomatics in particular, when he was invited to address the Second International Congress of Mathematicians

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28 And the same is true for Hilbert's treatment of "completeness" (in his current terminology) at that time.

29 The axiom is formulated in (Hilbert 1903a, 16). Toepell (1986, 254–256) briefly describes the relationship between Hilbert's Vollständigkeitsaxiom and related works of other mathematicians. The axiom underwent several changes throughout the various later editions of the Grundlagen, but it remained central to this part of the argument. Cf. (Peckhaus 1990, 29–35). The role of this particular axiom within Hilbert's axiomatics and its importance for later developments in mathematical logic is discussed in (Moore 1987, 109–122). In 1904 Oswald Veblen introduced the term "categorical" (Veblen 1904, 346) to denote a system to which no irredundant axioms may be added. He believed that Hilbert had checked this property in his own system of axioms. See (Scanlan 1991, 994).

held in Paris in August of 1900. The invitation was a definite sign of the reputation that Hilbert had acquired by then within the international mathematics community. Following a suggestion of Minkowski, Hilbert decided to use the opportunity to provide a glimpse into what, in his view, the new century would bring for mathematics. Thus he posed a list of problems that he considered significant challenges that could lead to fruitful research and to new and illuminating ideas for mathematicians involved in solving them.

In many ways, Hilbert's talk embodied his overall vision of mathematics and science, and he built the list of problems to a large extent according to his own mathematical horizons.[30] Some of the problems belonged to number theory and the theory of invariants, the domains that his published work had placed him in among the leading world experts. Some others belonged to domains with which he was closely acquainted, even though he had not by then published anything of the same level of importance, such as variational calculus. It further included topics that Hilbert simply considered should be given a significant push within contemporary research, such as Cantorian set theory. The list reflected Hilbert's mathematical horizon also in the sense that a very significant portion of the works he cited in reference to the various problems had been published in either of the two main Göttingen mathematical venues: the *Mathematische Annalen* and the Proceedings of the Göttingen Academy of Sciences. And although Hilbert's mathematical horizons were unusually broad, they were nonetheless clearly delimited and thus, naturally, several important, contemporary fields of research were left out of the list.[31] Likewise, important contemporary Italian works on geometry, and the problems related to them, were not referred to at all in the geometrical topics that Hilbert did consider in his list. Moreover, two major contemporary open problems, Fermat's theorem and Poincaré's three-body problem, though mentioned in the introduction, were not counted among the twenty-three problems.

The talk also reflected three other important aspects of Hilbert's scientific personality. Above all is his incurable scientific optimism, embodied in the celebrated and often quoted statement that every mathematical problem can indeed be solved: "There is the problem. Seek its solution. You can find it by pure reason, for in mathematics there is no *ignorabimus*." This was meant primarily as a reaction to a well-known pronouncement of the physiologist Emil du Bois Reymond (1818–1896) on the inherent limitations of science as a system able to provide us with knowledge about the world.[32] Second, is the centrality of challenging problems in mathematics as a main, necessary condition for the healthy development of any branch of the discipline and, more generally, of that living organism that Hilbert took mathematics to be. And third, is the central role accorded to empirical motivations as a fundamental source of nourishment for that organism, in which mathematics and the physical sci-

30 Several versions of the talk appeared in print and they were all longer and more detailed than the actual talk. Cf. (Grattan-Guinness 2000).
31 Cf. (Gray 2000, 78–88).

ences appear tightly interrelated. But stressing the empirical motivations underlying mathematical ideas should by no means be taken as opposed to rigor. On the contrary, contrasting an "opinion occasionally advocated by eminent men," Hilbert insisted that the contemporary quest for rigor in analysis and arithmetic should in fact be *extended to both geometry and the physical sciences*. He was alluding here, most probably, to Kronecker and Weierstrass, and the Berlin purist tendencies that kept geometry and applications out of their scope of interest. Rigorous methods are often simpler and easier to understand, Hilbert said, and therefore, a more rigorous treatment would only perfect our understanding of these topics, and at the same time would provide mathematics with ever new and fruitful ideas. Explaining why rigor should not be sought only within analysis, Hilbert actually implied that this rigor should actually be pursued in axiomatic terms. He thus wrote:

> Such a one-sided interpretation of the requirement of rigor would soon lead to the ignoring of all concepts arising form geometry, mechanics and physics, to a stoppage of the flow of new material from the outside world, and finally, indeed, as a last consequence, to the rejection of the ideas of the continuum and of irrational numbers. But what an important nerve, vital to mathematical science, would be cut by rooting out geometry and mathematical physics! On the contrary I think that wherever mathematical ideas come up, whether from the side of the theory of knowledge or in geometry, or from the theories of natural or physical science, the problem arises for mathematics to investigate the principles underlying these ideas and to establish them upon a simple and complete system of axioms, so that the exactness of the new ideas and their applicability to deduction shall be in no respect inferior to those of the old arithmetical concepts.[33]

Using rhetoric reminiscent of Paul Volkmann's 1900 book, 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.[34] The "edifice metaphor" was invoked to help stress the importance of investigating the foundations of mathematics not as an isolated concern, but rather as an organic part of the manifold growth of the discipline in several directions. Hilbert thus said:

> Indeed, the study of the foundations of a science is always particularly attractive, and the testing of these foundations will always be among the foremost problems of the investi-

32 See (Du Bois-Reymond 1872). Hilbert would repeat this claim several times later in his career, notably in (Hilbert 1930). Although the basic idea behind the pronouncement was the same on all occasions, and it always reflected his optimistic approach to the capabilities of mathematics, it would nevertheless be important to consider the specific, historical framework in which the pronouncement came and the specific meaning that the situation conveys in one and the same sentence. If in 1900 it came, partly at least, as a reaction to Du Bois-Reymond's sweeping claim about the limitation of science, in 1930 it came after the intense debate against constructivist views about the foundations of arithmetic.

33 The classical locus for the English version of the talk is (Hilbert 1902a). Here I have preferred to quote, where different, from the updated translation appearing in (Gray 2000, 240–282). This passage appears there on p. 245.

34 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 1982).

> gator ... [But] a thorough understanding of its special theories is necessary for the successful treatment of the foundations of the science. Only that architect is in the position to lay a sure foundation for a structure who knows its purpose thoroughly and in detail.[35]

Speaking more specifically about the importance of problems for the healthy growth of mathematics, Hilbert characterized an interesting problem as one that is "difficult in order to entice us, yet not completely inaccessible, lest it mock our efforts." But perhaps more important was the criterion he formulated for the solution of one such problem: it must be possible "to establish the correctness of the solution by a finite number of steps based upon a finite number of hypotheses which are implied in the statement of the problem and which must always be exactly formulated."

3.1 Foundational Problems

This is not the place to discuss in detail the list of problems and their historical background and development.[36] Our main concern here is with the sixth problem— Hilbert's call for the axiomatization of physical sciences—and those other problems on the list more directly connected with it. The sixth problem is indeed the last of a well-defined group within the list, to which other "foundational" problems also belong. Beyond this group, the list can be said roughly to contain three other main areas of interest: number theory, algebraic-geometrical problems, and analysis (mainly variational calculus) and its applications in physics.

The first two foundational problems, appearing at the head of Hilbert's list, are Cantor's continuum hypothesis and the compatibility of the axioms of arithmetic. In formulating the second problem on his list, Hilbert stated more explicitly than ever before, that among the tasks related to investigating an axiomatic system, proving its consistency would be the most important one. Eventually this turned into a main motto of his later program for the foundations of arithmetic beginning in the 1920s, but many years and important developments still separated this early declaration, diluted among a long list of other important mathematical tasks for the new century, from an understanding of the actual implications of such an attempt and from an actual implementation of a program to pursue it. In the years to come, as we will see below, Hilbert did many things with axiomatic systems other than attempting a proof of consistency for arithmetic.

Hilbert stated that proving the consistency of geometry could be reduced to proving that of arithmetic, and that the axioms of the latter were those presented by him in "Über den Zahlbegriff" several months prior to this talk. Yet, Hilbert was still confident that this would be a rather straightforward task, easily achievable "by means of a careful study and suitable modification of the known methods of reasoning in the theory of irrational numbers" (Hilbert 1902a, 448).Hilbert did not specify the exact

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35 Quoted from (Gray 2000, 258).

36 Cf. (Rowe 1996), and a more detailed, recent, discussion in (Gray 2000).

meaning of this latter statement, but its wording would seem to indicate that in the system of axioms proposed for arithmetic, the difficulty in dealing with consistency would come from the assumption of continuity. Thus the consistency of Euclidean geometry would depend on proving the consistency of arithmetic as defined by Hilbert through his system of axioms. This would, moreover, provide a proof for the very existence of the continuum of real numbers as well. Clearly Hilbert meant his remarks in this regard to serve as an argument against Kronecker's negative reactions to unrestricted use of infinite collections in mathematics, and therefore he explicitly asserted that a consistent system of axioms could prove the existence of higher Cantorian cardinals and ordinals.[37] He thus established a clear connection between the two first problems on his list through the axiomatic approach. Still, Hilbert was evidently unaware of the difficulties involved in realizing this point of view, and, more generally, he most likely had no precise idea of what an elaborate theory of systems of axioms would involve. On reading the first draft of the Paris talk, several weeks earlier, Minkowski understood at once the challenging implications of Hilbert's view, and he hastened to write to his friend:

> In any case, it is highly original to proclaim as a problem for the future, one that mathematicians would think they had already completely possessed for a long time, such as the axioms for arithmetic. What might the many laymen in the auditorium say? Will their respect for us grow? And you will also have a though fight on your hands with the philosophers.[38]

Minkowski turned out to be right to a large extent, and among the ideas that produced the strongest reactions were those related with the status of axioms as implicit definitions, such as Hilbert introduced in formulating the second problem. He thus wrote:

> When we are engaged in investigating the foundations of a science, we must set up a system of axioms which contains an exact and complete description of the relations subsisting between the elementary ideas of the science. The axioms so set up are at the same time the definitions of those elementary ideas, and no statement within the realm of the science whose foundation we are testing is held to be correct unless it can be derived from those axioms by means of a finite number of logical steps. (Hilbert 1902a,447)[39]

The next three problems in the list are directly related with geometry and, although not explicitly formulated in axiomatic terms, they address the question of finding the correct relationship between specific assumptions and specific, significant geometrical facts. Of particular interest for the present account is the fifth. The question of the foundations of geometry had evolved over the last third of the nineteenth century along two parallel paths. First was the age-old tradition of elementary synthetic

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37 Hilbert also pointed out that no consistent set of axioms could be similarly set up for all cardinals and all alephs. Commenting on this, Ferreirós (1999, 301), has remarked: "This is actually the first published mention of the paradoxes of Cantorian set theory — without making any fuss of it." See also (Peckhaus and Kahle 2002).

38 On 17 July 1900, (Rüdenberg and Zassenhaus 1973, 129).

39 And also quoted in (Gray 2000, 250).

geometry, where the question of foundations more naturally arises in axiomatic terms. A second, alternative, path, that came to be associated with the Helmholtz-Lie problem, had derived directly from the work of Riemann and it had a more physically-grounded orientation connected with the question of spaces that admit the free mobility of rigid bodies. Whereas Helmholtz had only assumed continuity as underlying the motion of rigid bodies, in applying his theory of group of transformations to this problem, Lie was also assuming the differentiability of the functions involved. Hilbert's work on the foundations of geometry, especially in the context that led to *Grundlagen der Geometrie*, had so far been connected with the first of these two approaches, while devoting much less attention to the second one. Now in his fifth problem, he asked whether Lie's conditions, rather than assumed, could actually be deduced from the group concept together with other geometrical axioms.

As a mathematical problem, the fifth one led to interesting, subsequent developments. Not long after his talk, on 18 November 1901, Hilbert himself proved that, in the plane, the answer is positive, and he did so with the help of a then innovative, essentially topological, approach (Hilbert 1902b). That the answer is positive in the general case was satisfactorily proved only in 1952.[40] What concerns us here more directly, however, is that the inclusion of this problem in the list underscores the actual scope of Hilbert's views over the question of the foundations of geometry and over the role of axiomatics. Hilbert suggested here the pursuit of an intricate kind of conceptual clarification involving our assumptions about motion, differentiability and symmetry, such as they appear intimately interrelated in the framework of a well-elaborate mathematical theory, namely, that of Lie. This quest is typical of the spirit of Hilbert's axiomatic involvement with physical theories. At this point, it also clearly suggests that his foundational views on geometry were much broader and open-ended than an exclusive focusing on *Grundlagen der Geometrie*— with a possible overemphasizing of certain, formalist aspects—might seem to imply. In particular, the fifth problem emphasizes, once again and from a different perspective, the prominent role that Hilbert assigned to physicalist considerations in his approach to geometry. In the long run, one can also see this aspect of Hilbert's view resurfacing at the time of his involvement with general theory of relativity. In its more immediate context, however, it makes the passage from geometry to the sixth problem appear as a natural one within the list.

Indeed, if the first two problems in the list show how the ideas deployed in *Grundlagen der Geometrie* led in one direction towards foundational questions in arithmetic, then the fifth problem suggests how they also naturally led, in a different direction, to Hilbert's call for the axiomatization of physical science in the sixth problem. The problem was thus formulated 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

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40 This was done, simultaneously, in (Gleason 1952) and (Montgomery and Zippin 1952).

> an important part; in the first rank are the theory of probabilities and mechanics. (Hilbert 1902a, 454)[41]

As examples of what he had in mind Hilbert mentioned several existing and well-known works: 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 Volkmann's 1900 *Einführung in das Studium der theoretischen Physik*. Boltzmann's work offered a good example of what axiomatization would offer, as he had indicated, though only schematically, that limiting processes could be applied, starting from an atomistic model, to obtain the laws of motion of continua. Hilbert thought it convenient to go in the opposite direction also, i.e., to derive the laws of motions of rigid bodies by limiting processes, starting from a system of axioms that describe space as filled with continuous matter in varying conditions. Thus one could investigate the equivalence of different systems of axioms, an investigation that Hilbert considered to be of the highest theoretical importance.

This is one of the few places where Hilbert emphasized Boltzmann's work over Hertz's in this regard, and this may give us the clue to the most immediate trigger that was in the back of Hilbert's mind when he decided to include this problem in the list. Hilbert had met Boltzmann several months earlier in Munich, where he heard his talk on recent developments in physics. Boltzmann had not only discussed ideas connected to the task that Hilbert was now calling for, but he also adopted a rhetoric that Hilbert seems to have found very much to the point. In fact, Boltzmann had suggested that one could follow up the recent history of physics with a look at future developments. Nevertheless, he said, "I will not be so rash as to lift the veil that conceals the future" (Boltzmann 1899, 79). Hilbert, on the contrary, opened the lecture by asking precisely, "who among us would not be glad to lift the veil behind which the future lies hidden" and the whole trust of his talk implied that he, the optimistic Hilbert, was helping the mathematical community to do so.

Together with the well-known works on mechanics referred to above, Hilbert also mentioned a recent work by the Göttingen actuarial mathematician Georg Bohlmann (1869–1928) on the foundations of the calculus of probabilities.[42] The latter was important for physics, Hilbert said, for its application to the method of mean values and to the kinetic theory of gases. Hilbert's inclusion of the theory of probabilities among the main *physical* theories whose axiomatization should be pursued has often puzzled readers of this passage. It is also remarkable that Hilbert did not mention electrodynamics among the physical disciplines to be axiomatized, even though the second half of the Gauss-Weber *Festschrift*, where Hilbert's *Grundlagen der Geometrie* was published, contained a parallel essay by Emil Wiechert (1861–1956) on the foundations of electrodynamics (Wiechert 1899). At any rate, Wiechert's presentation

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41 Quoted in (Gray 2000, 257).

42 This article reproduced a series of lectures delivered by Bohlmann in a *Ferienkurs* in Göttingen (Bohlmann 1900). In his article Bohlmann referred the readers, for more details, to the chapter he had written for the *Encyklopädie* on insurance mathematics.

was by no means axiomatic, in any sense of the term. On the other hand, the topics addressed by him would start attracting Hilbert's attention over the next years, at least since 1905.

Modelling this research on what had already been done for geometry meant that not only theories considered to be 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 later to appear also 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.

3.2 A Context for the Sixth Problem

The sixth problem of the list deals with the axiomatization of physics. It was suggested to Hilbert by his own recent research on the foundations of geometry. He thus proposed "to treat in the same manner, by means of axioms, those physical sciences in which mathematics plays an important part." This sixth problem is not really a problem in the strict sense of the word, but rather a general task for whose complete fulfilment Hilbert set no clear criteria. Thus, Hilbert's detailed account in the opening remarks of his talk as to what a meaningful problem in mathematics is, and his stress on the fact that a solution to a problem should be attained in a finite number of steps, does not apply in any sense to the sixth one. On the other hand, the sixth problem has important connections with three other problems on Hilbert's list: the nineteenth ("Are all the solutions of the Lagrangian equations that arise in the context of certain typical variational problems necessarily analytic?"), the twentieth (dealing with the existence of solutions to partial differential equations with given boundary conditions), closely related to the nineteenth and at the same time to Hilbert's long-standing interest on the Dirichlet Principle,[43] and, finally, the twenty-third (an appeal to extend and refine the existing methods of variational calculus). Like the sixth problem, the latter two are general tasks rather than specific mathematical problems with a clearly identifiable, possible solution.[44] All these three problems are also strongly connected to physics, though unlike the sixth, they are also part of mainstream, tradi-

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43 On 11 October 1899, Hilbert had lectured in Göttingen on the Dirichlet principle, stressing the importance of its application to the theory of surfaces and also to mathematical physics. Cf. *Jahresbericht der Deutschen Mathematiker-Vereinigung* 8 (1900), 22.

44 A similar kind of "general task" problem that Hilbert had perhaps considered adding as the twenty-fourth problem in his list is hinted at in an undated manuscript found in *Nachlass* David Hilbert (Cod. Ms. D. Hilbert, 600). It concerns the definition of criteria for finding simplest proofs in mathematics in general. Cf. a note in (Grattan-Guinness 2001, 167), and a more detailed account in (Thiele 2003).

tional research concerns in mathematics.[45] In fact, their connections to Hilbert's own interests are much more perspicuous and, in this respect, they do not raise the same kind of historical questions that Hilbert's interest in the axiomatization of physics does. Below, I will explain in greater detail how Hilbert conceived the role of variational principles in his program for axiomatizing physics.

Another central issue to be discussed below in some detail is the role the sixth problem played in subsequent developments in mathematics and in physics. At this stage, however, a general point must be stressed about the whole list in this regard. A balanced assessment of the influence of the problems on the development of mathematics throughout the century must take into account not only the intrinsic importance of the problems,[46] but also the privileged institutional role of Göttingen in the mathematical world with the direct and indirect implications of its special status. If Hilbert wished to influence the course of mathematics over the coming century with his list, then his own career was only very partially shaped by it. Part of the topics covered by the list belonged to his previous domains of research, while others belonged to domains where he never became active. On the contrary, domains that he devoted much effort to over the next years, such as the theory of integral equations, were not contemplated in the list. In spite of the enormous influence Hilbert had on his students, the list did not become a necessary point of reference of preferred topics for dissertations. To be sure, some young mathematicians, both in Göttingen and around the world, did address problems on the list and sometimes came up with important mathematical achievements that helped launch their own international careers. But this was far from the only way for talented young mathematicians to reach prominence in or around Göttingen. But, ironically, the sixth problem, although seldom counted among the most influential of the list, will be shown here to count among those that received a greater attention from Hilbert himself and from his collaborators and students over the following years.

For all its differences and similarities with other problems on the list, the important point that emerges from the above account is that the sixth problem was in no sense disconnected from the evolution of Hilbert's early axiomatic conception. Nor was it artificially added in 1900 as an afterthought about the possible extensions of an idea successfully applied in 1899 to the case of geometry. Rather, Hilbert's ideas concerning the axiomatization of physical science arose simultaneously with his increasing enthusiasm for the axiomatic method and they fitted naturally into his overall view of pure mathematics, geometry and physical science—and the relationship among them—by that time. Moreover, as will be seen in the next chapter in some detail, Hilbert's 1905 lectures on axiomatization provide a very clear and comprehensive conception of how the project suggested in the sixth problem should be realized. In fact, it is very likely that this conception was not essentially different from what Hilbert had in mind when formulating his problem in 1900.[47] Interestingly, the devel-

45 For a detailed account of the place of variational principles in Hilbert's work, see (Blum 1994).

46 As treated in (Alexandrov 1979; Browder 1976).

opment of physics from the beginning of the century, and especially *after* 1905, brought many surprises that Hilbert could not have envisaged in 1900 or even when he lectured at Göttingen on the axioms of physics; yet, over the following years Hilbert was indeed able to accommodate these new developments to the larger picture of physics afforded by his program for axiomatization. In fact, some of his later contributions to mathematical physics came by way of realizing the vision embodied in this program, as will be seen in detail in later chapters.

4. FOUNDATIONAL CONCERNS – EMPIRICIST STANDPOINT

Following the publication of *Grundlagen der Geometrie*, Hilbert was occupied for a while with research on the foundations of geometry. Several of his students, such as Max Dehn (1878–1952), Georg Hamel (1877–1954) and Anne Lucy Bosworth (1868–?), worked in this field as well, including on problems relating to Hilbert's 1900 list. Also many meetings of the *Göttinger Mathematische Gesellschaft* during this time were devoted to discussing related topics. On the other hand, questions relating to the foundations of arithmetic and set theory also received attention in the Hilbert circle. Ernst Zermelo (1871–1953) had already arrived in Göttingen in 1897 in order to complete his *Habilitation*, and his own focus of interest changed soon from mathematical physics to set theory and logic. Around 1899–1900 he had already found an important antinomy in set theory, following an idea of Hilbert's.[48] Later on, in the winter semester of 1900–1901, Zermelo was teaching set theory in Göttingen (Peckhaus 1990, 48–49).

Interest in the foundations of arithmetic became a much more pressing issue in 1903, after Bertrand Russell (1872–1970) published his famous paradox arising from Frege's logical system. Although Hilbert hastened to indicate to Frege that similar arguments had been known in Göttingen for several years,[49] it seems that Russell's publication, coupled with the ensuing reaction by Frege,[50] did have an exceptional impact. Probably this had to do with the high esteem that Hilbert professed towards Frege's command of these topics (which Hilbert may have come to appreciate even more following the sharp criticism recently raised by the latter towards his own ideas). The simplicity of the sets involved in Russell's argument was no doubt a further factor that explains its strong impact on the Göttingen mathematicians. If Hilbert had initially expected that the difficulty in completing the full picture of his approach to the foundations of geometry would lie on dealing with more complex assumptions such as the *Vollständigkeitsaxiom*, now it turned out that the problems perhaps started with the arithmetic itself and even with logic. He soon realized that greater attention

47 Cf. (Hochkirchen 1999), especially chap. 1.

48 See (Peckhaus and Kahle 2002).

49 Hilbert to Frege, 7 November 1903. Quoted in (Gabriel et al. 1980, 51–52).

50 As published in (Frege 1903, 253). See (Ferreirós 1999, 308–311).

should be paid to these topics, and in particular to the possible use of the axiomatic method in establishing the consistency of arithmetic (Peckhaus 1990, 56–57).

Hilbert himself gradually reduced his direct involvement with all questions of this kind, and after 1905 he completely abandoned them for many years to come. Two instances of his involvement with foundational issues during this period deserve some attention here. The first is his address to the Third International Congress of Mathematicians, held in 1904 in Heidelberg. In this talk, later published under the title of "On the Foundations of Logic and Arithmetic," Hilbert presented a program for attacking the problem of consistency as currently conceived. The basic idea was to develop simultaneously the laws of logic and arithmetic, rather than reducing one to the other or to set theory. The starting point was the basic notion of thought-object that would be designated by a sign, which offered the possibility of treating mathematical proofs, in principle, as formulae. This could be seen to constitute an interesting anticipation of what later developed as part of Hilbert's proof theory, but here he only outlined the idea in a very sketchy way. Actually, Hilbert did not go much beyond the mere declaration that this approach would help achieve the desired proof. Hilbert cursorily reviewed several prior approaches to the foundations of arithmetic, only to discard them all. Instead, he declared that the solution for this problem would finally be found in the correct application of the axiomatic method (Hilbert 1905c, 131).

Upon returning to Göttingen from Heidelberg, Hilbert devoted some time to working out the ideas outlined at the International Congress of Mathematicians. The next time he presented them was in an introductory course devoted to "The Logical Principles of Mathematical Thinking," which contains the second instance of Hilbert's involvement with the foundation of arithmetic in this period. This course is extremely important for my account here because it contains the first detailed attempt to implement the program for the axiomatization of physics.[51] I will examine it in some detail below. At this point I just want to briefly describe the other parts of the course, containing some further foundational ideas for logic and arithmetic, and some further thoughts on the axiomatization of geometry.

Hilbert discussed in this course the "logical foundations" of mathematics by introducing a formalized calculus for propositional logic. This was a rather rudimentary calculus, which did not even account for quantifiers. As a strategy for proving consistency of axiomatic systems, it could only be applied to very elementary cases.[52] Prior to defining this calculus Hilbert gave an overview of the basic principles of the axiomatic method, including a more detailed account of its application to arithmetic, geometry and the natural sciences. What needs to be stressed concerning this text is that, in spite of his having devoted increased attention over the previous years to foundational questions in arithmetic, Hilbert's fundamentally empiricist

51 There are two extant sets of notes for this course: (Hilbert 1905a and 1905b). Quotations below are taken from (Hilbert 1905a). As these important manuscripts remain unpublished, I transcribe in the footnotes some relevant passages at length. Texts are underlined or crossed-out as in the original. Later additions by Hilbert appear between < > signs.

52 For a discussion of this part of the course, see (Peckhaus 1990, 61–75).

approach to issues in the foundations of geometry was by no means weakened, but rather the opposite. In fact, in his 1905 course, Hilbert actually discussed the role of an axiomatic analysis of the foundations of arithmetic in similar, empiricist terms.

Once again, Hilbert contrasted the axiomatic method with the genetic approach in mathematics, this time making explicit reference to the contributions of Kronecker and Weierstrass to the theory of functions. Yet Hilbert clearly separated the purely logical aspects of the application of the axiomatic method from the "genetic" origin of the axioms themselves: the latter is firmly grounded on empirical experience. Thus, Hilbert asserted, it is not the case that the system of numbers is given to us through the network of concepts (*Fachwerk von Begriffen*) involved in the eighteen axioms. On the contrary, it is our direct intuition of the concept of natural number and of its successive extensions, well known to us by means of the genetic method, which has guided our construction of the axioms:

> The aim of every science is, first of all, to set up a network of concepts based on axioms to whose very conception we are naturally led by *intuition and experience*. Ideally, all the phenomena of the given domain will indeed appear as part of the network and all the theorems that can be derived from the axioms will find their expression there.[53]

What this means for the axiomatization of geometry, then, is that its starting point must be given by the intuitive facts of that discipline,[54] and that the latter must be in agreement with the network of concepts created by means of the axiomatic system. The concepts involved in the network itself, Hilbert nevertheless stressed, are totally detached from experience and intuition.[55] This procedure is rather obvious in the case of arithmetic, and to a certain extent the genetic method has attained similar results for this discipline. In the case of geometry, although the need to apply the pro-

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53 "*Uns war das Zahlensystem schließlich nichts* als ein *Fachwerk von Begriffen*, das durch 18 Axiome definiert war. Bei der Aufstellung dieser leitete uns allerdings die Anschauung, die wir von dem Begriff der Anzahl und seiner genetischen Ausdehnung haben. ... So ist in jeder Wissenschaft die Aufgabe, in den Axiomen zunächst ein Fachwerk von Begriffen zu errichten, bei dessen Aufstellung wir uns natürlich durch die *Anschauung und Erfahrung* leiten lassen; das Ideal ist dann, daß in diesem Fachwerk alle Erscheinungen des betr. Gebietes Platz finden, und daß jeder aus den Axiomen folgende Satz dabei Verwertung findet.
Wollen wir nun für die Geometrie ein Axiomensystem aufstellen, so heißt das, daß wir uns den Anlaß dazu durch die anschaulichen Thatsachen der Geometrie geben lassen, und diesen das aufzurichtende Fachwerk entsprechen lassen; die Begriffe, die wir so erhalten, sind *aber als gänzlich losgelöst von jeder Erfahrung und Anschauung zu betrachten*. Bei der Arithmetik ist diese Forderung verhältnismäßig naheliegend, sie wird in gewissem Umfange auch schon bei der genetischen Methode angestrebt. Bei der Geometrie jedoch wurde die Notwendigkeit dieses Vorgehens viel später erkannt; dann aber wurde eine axiomatische Behandlung eher versucht, als in der Arithmetik, wo noch immer die genetische Betrachtung herrschte. Doch ist die Aufstellung eines vollständigen Axiomensystemes ziemlich schwierig, noch viel schwerer wird sie in der Mechanik, Physik etc. sein, wo das Material an Erscheinungen noch viel größer ist." (Hilbert 1905a, 36–37)

54 "... den Anlaß dazu durch die anschaulischen Thatsachen der Geometrie geben lassen..." (Hilbert 1905a, 37)

55 "... die Begriffe, die wir so erhalten, sind aber als gänzlich losgelöst von jeder Erfahrung und Anschauung zu betrachten." (Hilbert 1905a, 37)

cess truly systematically was recognized much later, the axiomatic presentation has traditionally been the accepted one. And if setting up a full axiomatic system has proven to be a truly difficult task for geometry, then, Hilbert concluded, it will be much more difficult in the case of mechanics or physics, where the range of observed phenomena is even broader.[56]

Hilbert's axioms for geometry in 1905 were based on the system of *Grundlagen der Geometrie*, including all the corrections and additions introduced to it since 1900. Here too he started by choosing three basic kinds of undefined elements: points, lines and planes. This choice, he said, is somewhat "arbitrary" and it is dictated by consideration of simplicity. But the arbitrariness to which Hilbert referred here has little to do with the arbitrary choice of axioms sometimes associated with twentieth-century formalistic conceptions of mathematics; it is not an absolute arbitrariness constrained only by the requirement of consistency. On the contrary, it is limited by the need to remain close to the "intuitive facts of geometry." Thus, Hilbert said, instead of the three chosen, basic kinds of elements, one could likewise start with [no... not with "chairs, tables, and beer-mugs," but rather with] circles and spheres, and formulate the adequate axioms that are still in agreement with the usual, intuitive geometry.[57]

Hilbert plainly declared that Euclidean geometry—as defined by his systems of axioms—is the one and only geometry that fits our spatial experience,[58] though in his opinion, it would not be the role of mathematics or logic to explain why this is so. But if that is the case, then what is the status of the non-Euclidean or non-Archimedean geometries? Is it proper at all to use the term "geometry" in relation to them? Hilbert thought it unnecessary to break with accepted usage and restrict the meaning of the term to cover only the first type. It has been unproblematic, he argued, to extend the meaning of the term "number" to include also the complex numbers, although the latter certainly do not satisfy all the axioms of arithmetic. Moreover, it would be untenable from the logical point of view to apply the restriction: although it is not highly probable, it may nevertheless be the case that some changes would still be introduced in the future to the system of axioms that describes intuitive geometry. In fact, Hilbert knew very well that this "improbable" situation had repeatedly arisen in relation to the original system he had put forward in 1900 in *Grundlagen der Geometrie*. To conclude, he compared once again the respective situations in geometry and in physics: in the theory of electricity, for instance, new theories are continually formulated that transform many of the basic facts of the discipline, but no one thinks that the name of the discipline needs to be changed accordingly.

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56 "... das Material an Erscheinungen noch viel größer ist." (Hilbert 1905a, 37)

57 "Daß wir gerade diese zu Elementardingen des begrifflichen Fachwerkes nehmen, ist willkürlich und geschieht nur wegen ihrer augenscheinlichen Einfachheit; im Princip könnte man die ersten Dinge auch Kreise und Kugeln nennen, und die Festsetzungen über sie so treffen, daß sie diesen Dingen der anschaulichen Geometrie entsprechen." (Hilbert 1905a, 39)

58 "Die Frage, wieso man in der Natur nur gerade die durch alle diese Axiome festgelegte Euklidische Geometrie braucht, bezw. warum unsere Erfahrung gerade in dieses Axiomsystem sich einfügt, gehört nicht in unsere mathematisch-logichen Untersuchungen." (Hilbert 1905a, 67)

Hilbert also referred explicitly to the status of those theories that, like non-Euclidean and non-Archimedean geometries, are created arbitrarily through the purely logical procedure of setting down a system of independent and consistent axioms. These theories, he said, can be applied to any objects that satisfy the axioms. For instance, non-Euclidean geometries are useful to describe the paths of light in the atmosphere under the influence of varying densities and diffraction coefficients. If we assume that the speed of light is proportional to the vertical distance from a horizontal plane, then one obtains light-paths that are circles orthogonal to the planes, and light-times equal to the non-Euclidean distance from them.[59] Thus, the most advantageous way to study the relations prevailing in this situation is to apply the conceptual schemes provided by non-Euclidean geometry.[60]

A further point of interest in Hilbert's discussion of the axioms of geometry in 1905 concerns his remarks about what he called the philosophical implications of the use of the axiomatic method. These implications only reinforced Hilbert's empiricist view of geometry. Geometry, Hilbert said, arises from reality through intuition and observation, but it works with idealizations: for instance, it considers very small bodies as points. The axioms in the first three groups of his system are meant to express idealizations of a series of facts that are easily recognizable as independent from one other; the assertion that a straight line is determined by two points, for instance, never gave rise to the question of whether or not it follows from other, basic axioms of geometry. But establishing the status of the assertion that the sum of the angles in a triangle equals two right angles requires a more elaborate axiomatic analysis. This analysis shows that such an assertion is a separate piece of knowledge, which—we now know for certain—cannot be deduced from earlier facts (or from their idealizations, as embodied in the three first groups of axioms). This knowledge can only be gathered from new, independent empirical observation. This was Gauss's aim, according to Hilbert, when he confirmed the theorem for the first time, by measuring the angles of the large triangle formed by the three mountain peaks.[61] The network of concepts that constitute geometry, Hilbert concluded, has been proved consistent, and therefore it exists mathematically, independently of any observation. Whether or not

59 As in many other places in his lectures, Hilbert gave no direct reference to the specific physical theory he had in mind here, and in this particular case I have not been able to find it.

60 "Ich schließe hier noch die Bemerkung an, daß man jedes solches Begriffschema, das wir so rein logisch aus irgend welchen Axiomen aufbauen, anwenden kann auf beliebige gegenständliche Dinge, wenn sie nur diesen Axiomen genügen. ... Ein solches Beispiel für die Anwendung des Begriffschemas der nichteuklidischen Geometrie bildet das System der Lichtwege in unserer Atmosphäre unter dem Einfluß deren variabler Dichte und Brechungsexponenten; machen wir nämlich die einfachste mögliche Annahme, daß die Lichtgeschwindigkeit proportional ist dem vertikalen Abstande y von einer Horizontalebene, so ergeben sich als Lichtwege gerade die Orthogonalkreise jener Ebene, als Lichtzeit gerade die nichteuklidiche Entfernung auf ihnen. Um die hier obwaltenden Verhältnisse also genauer zu untersuchen, können wir gerade mit Vorteil das Begriffschema der nichteuklidischen Geometrie anwenden." (Hilbert 1905a, 69–70)

61 "In diesem Sinne und zu diesem Zwecke hat zuerst Gauß durch Messung an großen Dreiecken den Satz bestätigt." (Hilbert 1905a, 98)

it corresponds to reality is a question that can be decided only by observation, and our analysis of the independence of the axioms allows determining very precisely the minimal set of observations needed in order to do so.[62] Later on, he added, the same kind of perspective must be adopted concerning physical theories, although there its application will turn out to be much more difficult than in geometry.

In concluding his treatment of geometry, and before proceeding to discuss the specific axiomatization of individual physical theories, Hilbert summarized the role of the axiomatic method in a passage which encapsulates his view of science and of mathematics as living organisms whose development involves both an expansion in scope and an ongoing clarification of the logical structure of their existing parts.[63] The axiomatic treatment of a discipline concerns the latter; it is an important part of this growth but—Hilbert emphasized—only one part of it. The passage, reads as follows:

> The edifice of science is not raised like a dwelling, in which the foundations are first firmly laid and only then one proceeds to construct and to enlarge the rooms. Science prefers to secure as soon as possible comfortable spaces to wander around and only subsequently, when signs appear here and there that the loose foundations are not able to sustain the expansion of the rooms, it sets about supporting and fortifying them. This is not a weakness, but rather the right and healthy path of development.[64]

This metaphor provides the ideal background for understanding what Hilbert went on to realize at this point in his lectures, namely, to present his first detailed account of how the general idea of axiomatization of physical theories would be actually implemented in each case. But before we can really discuss that detailed account, it is necessary to broaden its context by briefly describing some relevant developments in physics just before 1905, and how they were manifest in Göttingen.

5. HILBERT AND PHYSICS IN GÖTTINGEN CIRCA 1905

The previous section described Hilbert's foundational activities in mathematics between 1900 and 1905. Those activities constituted the natural outgrowth of the

62 "Das Begriffsfachwerk der Geometrie selbst ist nach Erweisung seiner Widerspruchslosigkeit natürlich auch unabhängig von jeder Beobachtung mathematisch existent; der Nachweis seiner Übereinstimmung mit der Wirklichkeit kann nur durch Beobachtungen geführt werden, und die kleinste notwendige solche wird durch die Unabhängigkeitsuntersuchungen gegeben." (Hilbert 1905a, 98)

63 Elsewhere Hilbert called these two aspects of mathematics the "progressive" and "regressive" functions of mathematics, respectively (both terms not intended as value judgments, of course). See (Hilbert 1992, 17–18).

64 "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." (Hilbert 1905a, 102.) Other places where Hilbert uses a similar metaphor are (Hilbert 1897, 67; Hilbert 1918, 148).

seeds planted in *Grundlagen der Geometrie* and the developments that immediately followed it. My account is not meant to imply, however, that Hilbert's focus of interest was limited to, or even particularly focused around, this kind of enquiry during those years. On 18 September 1901, for instance, Hilbert's keynote address at the commemoration of the 150th anniversary of the Göttingen Scientific Society (*Gesellschaft der Wissenschaften zu Göttingen*) was devoted to analyzing the conditions of validity of the Dirichlet Principle (Hilbert 1904, 1905d). Although thus far he had published very little in this field, Hilbert's best efforts from then on would be given to analysis, and in particular, the theory of linear integral equations. His first publication in this field appeared in 1902, and others followed, up until 1912. But at the same time, he sustained his interest in physics, which is directly connected with analysis and related fields to begin with, and this interest in physics became only more diverse throughout this period. His increased interest in analysis had a natural affinity with the courses on potential theory (winter semester, 1901–1902; summer semester, 1902) and on continuum mechanics (winter semester, 1902–1903; summer semester, 1903) that he taught at that time. Perhaps worthy of greater attention, however, is Hilbert's systematic involvement around 1905 with the theories of the electron, on which I will elaborate in the present section.

Still, a brief remark on Hilbert's courses on continuum mechanics: The lecture notes of these two semesters (Hilbert 1902–1903, 1903b) are remarkable for the thoroughness with which the subject was surveyed. The presentation was probably the most systematic and detailed among all physical topics taught by Hilbert so far, and it comprised detailed examinations of the various existing approaches (particularly those of Lagrange, Euler and Helmholtz). Back in 1898–1899, in the final part of a course on mechanics, Hilbert had briefly dealt with the mechanics of systems of an infinite number of mass-points while stressing that the detailed analysis of such systems would actually belong to a different part of physics. This was precisely the subject he would consider in 1902. In that earlier course Hilbert had also discussed some variational principles of mechanics, without however presenting the theory in anything like a truly axiomatic perspective. Soon thereafter, in 1900 in Paris, Hilbert publicly presented his call for the axiomatization of physics. But in 1902–1903, in spite of the high level of detail with which he systematically discussed the physical discipline of continuum mechanics, the axiomatic presentation was not yet the guiding principle. Hilbert did state that a main task to be pursued was the axiomatic description of physical theories[65] and throughout the text he specifically accorded the status of axioms to some central statements.[66] Still, the notes convey the distinct impression that Hilbert did not believe that the time was ripe for the fully axiomatic

65 The manuscript shows an interesting hesitation on how this claim was stated: "Das <Als ein wichtiges> Ziel der Vorlesung ist <denke ich mir> die mathematische Beschreibung der Axiome der Physik. Vergl. Archiv der Mathematik und Physik, meine Rede: 'Probleme der Mathematik'." However, it is not clear if this amendment of the text reflects a hesitation on the side of Hilbert, or on the side of Berkowski, who wrote down the notes. (Hilbert 1902–1903, 2)

66 Thus for instance in (Hilbert 1902–1903).

treatment of mechanics, or at least of continuum mechanics, in axiomatic terms similar to those previously deployed in full for geometry.

On the other hand, it is worth stressing that in many places Hilbert set out to develop a possible unified conception of mechanics, thermodynamics (Hilbert 1903b, 47–91) and electrodynamics (Hilbert 1903b, 91–164) by using formal analogies with the underlying ideas of his presentation of the mechanics of continua. These ideas, which were treated in greater detail from an axiomatic point of view in the 1905 lectures, are described more fully below; therefore, at this point I will not give a complete account of them. Suffice it to say that Hilbert considered the material in these courses to be original and important, and not merely a simple repetition of existing presentations. In fact, the only two talks he delivered in 1903 at the meetings of the *Göttinger Mathematische Gesellschaft* were dedicated to reporting on their contents.[67]

Still in 1903, Hilbert gave a joint seminar with Minkowski on stability theory.[68] He also presented a lecture on the same topic at the yearly meeting of the *Gesellschaft Deutscher Naturforscher und Ärzte* at Kassel,[69] sparking a lively discussion with Boltzmann.[70] In the winter semester of 1904–1905 Hilbert taught an exercise course on mechanics and later gave a seminar on the same topic. The course "Logical Principles of Mathematical Thinking," containing the lectures on axiomatization of physics, was taught in the summer semester of 1905. He then lectured again on mechanics (winter semester, 1905–1906) and two additional semesters on continuum mechanics.

The renewed encounter with Minkowski signified a major source of intellectual stimulation for these two old friends, and it particularly offered a noteworthy impulse to the expansion of Hilbert's horizon in physics. As usual, their walks were an opportunity to discuss a wide variety of mathematical topics, but now physics became a more prominent, common interest than it had been in the past. Teaching in Zürich since 1894, Minkowski had kept alive his interest in mathematical physics, and in particular in analytical mechanics and thermodynamics (Rüdenberg and Zassenhaus 1973, 110–114). Now at Göttingen, he further developed these interests. In 1906 Minkowski published an article on capillarity (Minkowski 1906), commissioned for

67 See the announcements in *Jahresbericht der Deutschen Mathematiker-Vereinigung* 12 (1903), 226 and 445. Earlier volumes of the *Jahresbericht der Deutschen Mathematiker-Vereinigung* do not contain announcements of the activities of the *Göttinger Mathematische Gesellschaft*, and therefore it is not known whether he also discussed his earlier courses there.

68 *Nachlass* David Hilbert, (Cod. Ms. D. Hilbert, 570/1) contains a somewhat random collection of handwritten notes related to many different courses and seminars of Hilbert. Notes of this seminar on stability theory appear on pp. 18–24. Additional, related notes appear in (Cod. Ms. D. Hilbert, 696).

69 *Nachlass* David Hilbert, (Cod. Ms. D. Hilbert, 593) contains what appear to be the handwritten notes of this talk, with the title "Vortrag über Stabilität einer Flüssigkeit in einem Gefässe," and includes some related bibliography.

70 As reported in *Naturwissenschaftliche Rundschau*, vol. 18, (1903), 553–556 (cf. Schirrmacher 2003, 318, note 63). The reporter of this meeting, however, considered that Hilbert was addressing a subtlety, rather than a truly important physical problem.

the physics volume of the Encyklopädie, edited by Sommerfeld. At several meetings of the *Göttinger Mathematische Gesellschaft*, Minkowski lectured on this as well as other physical issues, such as Euler's equations of hydrodynamics and recent work on thermodynamics by Walter Nernst (1864-1941), (Nernst 1906), who by that time had already left Göttingen. Minkowski also taught advanced seminars on physical topics and more basic courses on mechanics, continuum mechanics, and exercises on mechanics and heat radiation.[71] In 1905 Hilbert and Minkowski organized, together with other Göttingen professors, an advanced seminar that studied recent progress in the theories of the electron.[72] In December 1906, Minkowski reported to the *Göttinger Mathematische Gesellschaft* on recent developments in radiation theory, and discussed the works of Hendrik Antoon Lorentz (1853–1928), Max Planck (1858–1947), Wilhelm Wien (1864–1928) and Lord Rayleigh (1842–1919), (Minkowski 1907, 78). Yet again in 1907, the two conducted a joint seminar on the equations of electrodynamics, and that semester Minkowski taught a course on heat radiation, after having studied with Hilbert Planck's recent book on this topic (Planck 1906).[73] Finally, as it is well known, during the last years of his life, 1907 to 1909, Minkowski's efforts were intensively dedicated to electrodynamics and the principle of relativity.

The 1905 electron theory seminar exemplifies the kind of unique scientific event that could be staged only at Göttingen at that time, in which leading mathematicians and physicists would meet on a weekly basis in order to intensively discuss current open issues of the discipline. In fact, over the preceding few years the *Göttinger Mathematische Gesellschaft* had dedicated many of its regular meetings to discussing recent works on electron theory and related topics, so that this seminar was a natural continuation of a more sustained, general interest for the local scientific community.

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71 Cf. *Jahresbericht der Deutschen Mathematikervereinigung* 13 (1904), 492; 16 (1907), 171; 17 (1908), 116. See also the *Vorlesungsverzeichnisse*, Universität Göttingen, winter semester, 1903–1904, 14; summer semester, 1904, 14–16. A relatively large collection of documents and manuscripts from Minkowski's *Nachlass* has recently been made available at the Jewish National Library, at the Hebrew University, Jerusalem. These documents are yet to be thoroughly studied and analyzed. They contain scattered notes of courses taught at Königsberg, Bonn, Zurich and Göttingen. The notes of a Göttingen course on mechanics, winter semester, 1903–1904, are found in Box IX (folder 4) of that collection. One noteworthy aspect of these notes is that Minkowski's recommended reading list is very similar to that of Hilbert's earlier courses and comprises mainly texts then available at the *Lesezimmer*. It included classics such as Lagrange, Kirchhoff, Helmholtz, Mach, and Thomson-Tait, together with more recent, standard items such as the textbooks by Voigt, Appell, Petersen, Budde and Routh. Like Hilbert's list it also included the lesser known (Rausenberg 1888), but it also comprised two items absent from Hilbert's list: (Duhamel 1853–1854) and (Föppl 1901). Further, it recommended Voss's *Encyklopädie* article as a good summary of the field.

72 Pyenson (1979) contains a detailed and painstaking reconstruction of the ideas discussed in this seminar and the contributions of its participants. This reconstruction is based mainly on *Nachlass* David Hilbert, (Cod. Ms. D. Hilbert, 570/9). I strongly relied on this article as a starting point for my account of the seminar in the next several paragraphs. Still, my account departs from Pyenson's views in some respects.

73 The notes of the course appear in (Minkowski 1907).

Besides Minkowski and Hilbert, the seminar was led by Wiechert and Gustav Herglotz (1881–1953). Herglotz had recently joined the Göttingen faculty and received his Habilitation for mathematics and astronomy in 1904. Alongside Wiechert, he contributed important new ideas to the electron theory and the two would later become the leading geophysicists of their time. The list of students who attended the seminar includes, in retrospect, no less impressive names: two future Nobel laureates, Max von Laue (1879–1960) and Max Born (1882–1970), as well as Paul Heinrich Blasius (1883–1970) who would later distinguish himself in fluid mechanics, and Arnold Kohlschütter (1883–1969), a student of Schwarzschild who became a leading astronomer himself. Parallel to this seminar, a second one on electrotechnology was held with the participation of Felix Klein, Carl Runge (1856–1914), Ludwig Prandtl (1875–1953) and Hermann Theodor Simon (1870–1918), then head of the Göttingen Institute for Applied Electricity.[74]

The modern theory of the electron had developed through the 1890s, primarily with the contributions of Lorentz working in Leiden, but also through the efforts of Wiechert at Göttingen and—following a somewhat different outlook—of Joseph Larmor (1857–1942) at Cambridge.[75] Lorentz had attempted to account for the interaction between ether and matter in terms of rigid, negatively charged, particles: the electrons. His article of 1895 dealing with concepts such as stationary ether and local time, while postulating the existence of electrons, became especially influential (Lorentz 1895). The views embodied in Lorentz's and Larmor's theories received further impetus from contemporary experimental work, such as that of Pieter Zeeman (1865–1943) on the effect associated with his name, work by J. J. Thomson (1856–1940) especially concerning the cathode ray phenomena and their interpretation in terms of particles, and also work by Wiechert himself, Wien and Walter Kaufmann (1871–1947). Gradually, the particles postulated by the theories and the particle-laden explanations stemming from the experiments came to be identified with one another.[76]

Lorentz's theory comprised elements from both Newtonian mechanics and Maxwell's electrodynamics. While the properties of matter are governed by Newton's laws, Maxwell's equations describe the electric and magnetic fields, conceived as states of the stationary ether. The electron postulated by the theory provided the connecting link between matter and ether. Electrons moving in the ether generate electric and magnetic fields, and the latter exert forces on material bodies through the electrons themselves. The fact that Newton's laws are invariant under Galilean transformations and Maxwell's are invariant under what came to be known as Lorentz transformations was from the outset a source of potential problems and difficulties for the theory, and in a sense, these and other difficulties would be dispelled only with the formulation of Einstein's special theory of relativity in 1905. In Lorentz's theory

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74 Cf. (Pyenson 1979, 102).
75 Cf. (Warwick 1991).
76 Cf. (Arabatzis 1996).

the conflict with experimental evidence led to the introduction of the famous contraction hypothesis and in fact, of a deformable electron.[77] But in addition it turned out that, in this theory, some of the laws governing the behavior of matter would be Lorentz invariant, rather than Galilean, invariant. The question thus arose whether this formal, common underlying property does not actually indicate a more essential affinity between what seemed to be separate realms, and, in fact, whether it would not be possible to reduce all physical phenomena to electrodynamics.[78]

Initially, Lorentz himself attempted to expand the scope of his theory, as a possible foundational perspective for the whole of physics, and in particular as a way to explain molecular forces in terms of electrical ones. He very soon foresaw a major difficulty in subsuming also gravitation within this explanatory scope. Still, he believed that such a difficulty could be overcome, and in 1900 he actually published a possible account of gravitation in terms of his theory. The main difficulty in this explanation was that, according to existing astronomical data, the velocity of gravitational effects would seem to have to expand much faster than electromagnetic ones, contrary to the requirements of the theory (Lorentz 1900). This and other related difficulties are in the background of Lorentz's gradual abandonment of a more committed foundational stance in connection with electron theory and the electromagnetic worldview. But the approach he had suggested in order to address gravitational phenomena in electromagnetic terms was taken over and further developed that same year by Wilhelm Wien, who had a much wider aim. Wien explicitly declared that his goal was to unify currently "isolated areas of mechanical and electromagnetic phenomena," and in fact, to do so in terms of the theory of the electron while assuming that all mass was electromagnetic in nature, and that Newton's laws of mechanics should be reinterpreted in electromagnetic terms.[79]

One particular event that highlighted the centrality of the study of the connection and interaction between ether and matter in motion among physicists in the German-speaking world was the 1898 meeting of the *Gesellschaft Deutscher Naturforscher und Ärzte*, held at Düsseldorf jointly with the annual meeting of the *Deutsche Mathematiker- Vereinigung*. Most likely both Hilbert and Minkowski had the opportunity to attend Lorentz's talk, which was the focus of interest. Lorentz described the main problem facing current research in electrodynamics in the following terms:

> Ether, ponderable matter, and, we may add, electricity are the building stones from which we compose the material world, and if we could know whether matter, when it moves, carries the ether with it or not, then the way would be opened before us by which we

77 In Larmor's theory the situation was slightly different, and so were the theoretical reasons for adopting the contraction hypothesis, due also to Georg FitzGerald (1851–1901). For details, see (Warwick 2003, 367–376).

78 For a more detailed explanation, cf. (Janssen 2002).

79 See (Wien 1900). This is the article to which Voss referred in his survey of 1901, and that he took to be representative of the new foundationalist trends in physics. Cf. (Jungnickel and McCormmach 1986, 2: 236–240).

> could further penetrate into the nature of these building stones and their mutual relations. (Lorentz 1898, 101)[80]

This formulation was to surface again in Hilbert's and Minkowski's lectures and seminars on electrodynamics after 1905.

The theory of the electron itself was significantly developed in Göttingen after 1900, with contributions to both its experimental and theoretical aspects. The experimental side came up in the work of Walter Kaufmann, who had arrived from Berlin in 1899. Kaufmann experimented with Becquerel rays, which produced high-speed electrons. Lorentz's theory stipulated a dependence of the mass of the electron on its velocity v, in terms of a second order relation on v/c (c being, of course, the speed of light). In order to confirm this relation it was necessary to observe electrons moving at speeds as close as possible to c, and this was precisely what Kaufmann's experiments could afford, by measuring the deflection of electrons in electric and magnetic fields. He was confident of the possibility of a purely electromagnetic physics, including the solution of open issues such as the apparent character of mass, and the gravitation theory of the electron. In 1902 he claimed that his results, combined with the recent developments of the theory, had definitely confirmed that the mass of the electrons is of "purely electromagnetic nature."[81]

The recent developments of the theory referred to by Kaufmann were those of his colleague at Göttingen, the brilliant *Privatdozent* Max Abraham (1875–1922). In a series of publications, Abraham introduced concepts such as "transverse inertia," and "longitudinal mass," on the basis of which he explained where the dynamics of the electron differed from that of macroscopic bodies. He also developed the idea of a rigid electron, as opposed to Lorentz's deformable one. He argued that explaining the deformation of the electron as required in Lorentz's theory would imply the need to introduce inner forces of non-electromagnetic origin, thus contradicting the most fundamental idea of a purely electromagnetic worldview. In Abraham's theory, the kinematic equations of a rigid body become fundamental, and he introduced variational principles to derive them. Thus, for instance, using a Lagrangian equal to the difference between the magnetic and the electrical energy, Abraham described the translational motion of the electron and showed that the principle of least action also holds for what he called "quasi-stationary" translational motion (namely, motion in which the velocity of the electron undergoes a small variation over the time required for light to traverse its diameter). Abraham attributed special epistemological significance to the fact that the dynamics of the electron could be expressed by means of a Lagrangian (Abraham 1903, 168),[82] a point that will surface interestingly in Hilbert's 1905 lectures on axiomatization, as we will see in the next section. Beyond the technical level, Abraham was a staunch promoter of the electromagnetic worldview and his theory of the electron was explicitly conceived to "shake the foundations of the

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80 Translation quoted from (Hirosige 1976, 35).

81 Cf. (Hon 1995; Miller 1997, 44–51, 57–62).

82 On Abraham's electron theory, see (Goldberg 1970; Miller 1997, 51–57).

mechanical view of nature." Still, in 1905 he conceded that "the electromagnetic world picture is so far only a program."[83]

Among the organizers of the 1905 electron theory seminar, it was Wiechert who had been more directly involved in research of closely related issues. Early in his career he became fascinated by the unification of optics and electromagnetism offered by Maxwell's theory, and was convinced of the centrality of the ether for explaining all physical phenomena. In the 1890s, still unaware of Lorentz's work, he published the outlines of his own theory of "atoms of electricity," a theory which he judged to be still rather hypothetical, however. This work contained interesting theoretical and experimental aspects that supported his view that cathode ray particles were indeed the electric atoms of his theory. After his arrival in Göttingen in 1897, Wiechert learnt about Lorentz's theory, and quickly acknowledged the latter's priority in developing an electrodynamics based on the concept of the "electron," the term that he now also adopted. Like Lorentz, Wiechert also adopted a less committed and more skeptical approach towards the possibility of a purely electromagnetic foundation of physics.[84] Obviously Hilbert was in close, continued contact with Wiechert and his ideas, but one rather remarkable opportunity to inspect these ideas more closely came up once again in 1899, when Wiechert published an article on the foundations of electrodynamics as the second half of the *Gauss-Weber Festschrift* (Wiechert 1899).

Not surprisingly, Abraham's works on electron theory were accorded particular attention by his Göttingen colleagues in the 1905 seminar, yet Abraham himself seems not to have attended the meetings in person. He was infamous for his extremely antagonistic and aggressive personality,[85] and this background may partly explain his absence. But one wonders if also his insistence on the foundational implications of electron theory, and a completely different attitude of the seminar leaders to this question may provide an additional, partial explanation for this odd situation. I already mentioned Wiechert's basic skepticism, or at least caution, in this regard. As we will see, also Hilbert and Minkowski were far from wholeheartedly supporting a purely electromagnetic worldview. Kaufmann was closest to Abraham in this point, and he had anyway left Göttingen in 1903. It is interesting to notice, at any rate, that Göttingen physicists and mathematicians held different, and very often conflicting, views on this as well as other basic issues, and it would be misleading to speak of a "Göttingen approach" to any specific topic. The situation around the electron theory seminar sheds interesting light on this fact.

Be that as it may, the organizers relied not on Abraham's, but on other, different works as the seminar's main texts. The texts included, in the first place, Lorentz's 1895 presentation of the theory, and also his more recently published Encyklopädie

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83 Quoted in (Jungnickel and McCormmach 1986, 2: 241). For a recent summary account of the electromagnetic worldview and the fate of its program, see (Kragh 1999, 105–199).

84 Cf. (Darrigol 2000, 344–347).

85 Cf., e.g., (Born 1978, 91 and 134–137).

article (Lorentz 1904a), which was to become the standard reference in the field for many years to come. Like most other surveys published in the Encyklopädie, Lorentz's article presented an exhaustive and systematic examination of the known results and existing literature in the field, including the most recent. The third basic text used in the seminar was Poincaré's treatise on electricity and optics (Poincaré 1901), based on his Sorbonne lectures of 1888, 1890 and 1891. This text discussed the various existing theories of the electrodynamics of moving bodies and criticized certain aspects of Lorentz's theory, and in particular a possible violation of the reaction principle due to its separation of matter and ether.[86]

Alongside the texts of Lorentz, Poincaré and Abraham, additional relevant works by Göttingen scientists were also studied. In fact, the main ideas of Abraham's theory had been recently elaborated by Schwarzschild and by Paul Hertz (1881–1940). The latter wrote a doctoral dissertation under the effective direction of Abraham, and this dissertation was studied at the seminar together with Schwarzschild's paper (Hertz 1904; Schwarzschild 1903). So were several recent papers by Sommerfeld (1904a, 1904b, 1905) who was now at Aachen, but who kept his strong ties to Göttingen always alive. Naturally, the ideas presented in the relevant works of Herglotz and Wiechert were also studied in the seminar (Herglotz 1903; Wiechert 1901).

The participants in this seminar discussed the current state of the theory, the relevant experimental work connected with it, and some of its most pressing open problems. The latter included the nature of the mass of the electron, problems related to rotation, vibration and acceleration in electron motion and their effects on the electromagnetic field, and the problem of faster-than-light motion. More briefly, they also studied the theory of dispersion and the Zeeman effect. From the point of view of the immediate development of the theory of relativity, it is indeed puzzling, as Lewis Pyenson has rightly stressed in his study of the seminar, that the participants were nowhere close to achieving the surprising breakthrough that Albert Einstein (1879–1956) had achieved at roughly the same time, and was about to publish (Pyenson 1979, 129–131).[87] Nevertheless, from the broader point of view of the development of math-

86 Cf. (Darrigol 2000, 351–366).

87 According to Pyenson, whereas Einstein "sought above all to address the most essential properties of nature," the Göttingen seminarists "sought to subdue nature, as it were, by the use of pure mathematics. They were not much interested in calculating with experimentally observable phenomena. They avoided studying electrons in metal conductors or at very low or high temperatures, and they did not spend much time elaborating the role of electrons in atomic spectra, a field of experimental physics then attracting the interest of scores of young physicists in their doctoral dissertations." Pyenson stresses the fact that Ritz's experiment was totally ignored at the seminar and adds: "For the seminar Dozenten it did not matter that accelerating an electron to velocities greater than that of light and even to infinite velocities made little physical sense. They pursued the problem because of its intrinsic, abstract interest." Noteworthy as these points are, it seems to me that by overstressing the question of why the Göttingen group achieved less than Einstein did, the main point is obscured in Pyenson's article, namely, what and why were Hilbert, Minkowski and their friends doing what they were doing, and how is this connected to the broader picture of their individual works and of the whole Göttingen mathematical culture.

ematics and physics at the turn of the century, and particularly of the account pursued here, it is all the more surprising to notice the level of detail and close acquaintance with physical theory and also, to a lesser degree, with experiment, that mathematicians such as Hilbert and Minkowski had attained by that time. All this, of course, while they were simultaneously active and highly productive in their own main fields of current, purely mathematical investigations: analysis, number theory, foundations, etc. Hilbert's involvement in the electron theory seminar clarifies the breadth and depth of the physical background that underlie his lectures on the axiomatization of physics in 1905, and that had considerably expanded in comparison with the one that prompted him to formulate, in the first place, his sixth problem back in 1900.

6. AXIOMS FOR PHYSICAL THEORIES: HILBERT'S 1905 LECTURES

Having described in some detail the relevant background, I now proceed to examine the contents of Hilbert's 1905 lectures on the "Axiomatization of Physical Theories," which devote separate sections to the following topics:

- Mechanics
- Thermodynamics
- Probability Calculus
- Kinetic Theory of Gases
- Insurance Mathematics
- Electrodynamics
- Psychophysics

Here I shall limit myself to discussing the sections on mechanics, the kinetic theory of gases, and electrodynamics.

6.1 Mechanics

Clearly, the main source of inspiration for this section is Aurel Voss's 1901 *Encyklopädie* article (Voss 1901). This is evident in the topics discussed, the authors quoted, the characterization of the possible kinds of axioms for physics, the specific axioms discussed, and sometimes even the order of exposition. Hilbert does not copy Voss, of course, and he introduces many ideas and formulations of his own, and yet the influence is clearly recognizable.

Though at this time Hilbert considered the axiomatization of physics and of natural science in general to be a task whose realization was still very distant,[88] we can mention one particular topic for which the axiomatic treatment had been almost com-

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88 "Von einer durchgeführten axiomatischen Behandlung der Physik und der Naturwissenschaften ist man noch weit entfernt; nur auf einzelnen Teilgebieten finden sich Ansätze dazu, die nur in ganz wenigen Fällen durchgeführt sind. <Die Durchführung ist ein ganzes—grosses—Arbeitsprogramm, Vgl. Dissertation von Schimmack sowie Schur>." (Hilbert 1905a, 121)

pletely attained (and only very recently, for that matter). This is the "law of the parallelogram" or, what amounts to the same thing, the laws of vector-addition. Hilbert based his own axiomatic presentation of this topic on works by Darboux, by Hamel, and by one of his own students, Rudolf Schimmack (1881–1912).[89]

Hilbert defined a force as a three-component vector, and made no additional, explicit assumptions here about the nature of the vectors themselves, but it is implicitly clear that he had in mind the collection of all ordered triples of real numbers. Thus, as in his axiomatization of geometry, Hilbert was not referring to an arbitrary collection of abstract objects, but to a very concrete mathematical entity; in this case, one that had been increasingly adopted after 1890 in the treatment of physical theories, following the work of Oliver Heaviside (1850–1925) and Josiah Willard Gibbs (1839–1903).[90] In fact, in Schimmack's article of 1903—based on his doctoral dissertation—a vector was explicitly defined as a directed, real segment of line in the Euclidean space. Moreover, Schimmack defined two vectors as equal when their lengths as well as their directions coincide (Schimmack 1903, 318).

The axioms presented here were thus meant to define the addition of two such given vectors, as the sums of the components of the given vectors. At first sight, this very formulation could be taken as the single axiom needed to define the sum. But the task of axiomatic analysis is precisely to separate this single idea into a system of several, mutually independent, simpler notions that express the basic intuitions involved in it. Otherwise, it would be like taking the linearity of the equation representing the straight line as the starting point of geometry.[91] Hilbert had shown in his previous discussion on geometry that this latter result could be derived using all his axioms of geometry.

Hilbert thus formulated six axioms to define the addition of vectors: the first three assert the existence of a well-defined sum for any two given vectors (without stating what its value is), and the commutativity and associativity of this operation. The fourth axiom connects the resultant vector with the directions of the summed vectors as follows:

> 4. Let aA denote the vector (aAx, aAy, aAz), having the same direction as A. Then every real number a defines the sum:
>
> $$A + aA = (1 + a)A.$$
>
> i.e., the addition of two vectors having the same direction is defined as the algebraic addition of the extensions along the straight line on which both vectors lie.[92]

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89 The works referred to by Hilbert are (Darboux 1875; Hamel 1905; Schimmack 1903). Schimmak's paper was presented to the *Königliche Gesellschaft der Wissenschaften zu Göttingen* by Hilbert himself. An additional related work, also mentioned by Hilbert in the manuscript, is (Schur 1903).

90 Cf. (Crowe 1967, 150 ff.; Yavetz 1995).

91 "... das andere wäre genau dasselbe, wie wenn man in der Geometrie die Linearität der Geraden als einziges Axiom an die Spitze stellen wollte (vgl. S. 118)." (Hilbert 1905a, 123)

92 "Addition zweier Vektoren derselben Richtung geschieht durch algebraische Addition der Strecken auf der gemeinsamen Geraden." (Hilbert 1905a, 123)

The fifth one connects addition with rotation:

> 5. If D denotes a rotation of space around the common origin of two forces A and B, then the rotation of the sum of the vectors equals the sum of the two rotated vectors:
>
> $$D(A+B) = DA + DB$$
>
> i.e., the relative position of sum and components is invariant with respect to rotation.[93]

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 the endpoints of A and B respectively, such that the endpoint of the sum of any two vectors belonging to each of these domains will always fall inside G.[94]

These are all simple axioms—Hilbert continued, without having really explained what a "simple" axiom is—and if we think of the vectors as representing forces, they also seem rather plausible. The axioms thus correspond to the basic known facts of experience, i.e., that the action of two forces on a point may always be replaced by a single one; that the order and the way in which they are added do not change the result; that two forces having one and the same direction can be replaced by a single force having the same direction; and, finally, that the relative position of the components and the resultant is independent of rotations of the coordinates. Finally, the demand for continuity in this system is similar and is formulated similarly to that of geometry.

That these six axioms are in fact necessary to define the law of the parallelogram was first claimed by Darboux, and later proven by Hamel. The main difficulties for this proof arose from the sixth axiom. Schimmack proved in 1903 the independence of the six axioms (in a somewhat different formulation), using the usual technique of models that satisfy all but one of the axioms. Hilbert also mentioned some possible modifications of this system. Thus, Darboux himself had showed that the continuity axiom may be abandoned, and in its place, it may be postulated that the resultant lies on the same plane as, and within the internal angle between, the two added vectors. Hamel, on the other hand, following a conjecture of Friedrich Schur, proved that the fifth axiom is superfluous if we assume that the locations of the endpoints of the resultants, seen as functions of the two added vectors, have a continuous derivative. In fact—Hilbert concluded—if we assume that all functions appearing in the natural sciences have at least one continuous derivative, and take this assumption as an even

93 "Nimmt man eine Drehung D des Zahlenraumes um den gemeinsamen Anfangspunkt vor, so entsteht aus $A+B$ die Summe der aus A und B durch D entstehenden Vektoren: $D(A+B) = DA+DB$; d.h. die relative Lage von Summe und Komponenten ist gegenüber allen Drehungen invariant." (Hilbert 1905a, 124)

94 "Zu einem genügend kleinen Gebiete G um den Endpunkt von $A+B$ kann man stets um die Endpunkte von A und B solche Gebiete G_1, G_2 abgrenzen, daß der Endpunkt der Summe jedes in G_1 u. G_2 endigenden Vectorpaares nach G fällt." (Hilbert 1905a, 124)

more basic axiom, then vector addition is defined by reference to only the four first axioms in the system.[95]

The sixth axiom, the 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—Hilbert said in his lecture—that 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, as we saw, who described continuity as "an experience of the most general kind," and who saw it as a very basic assumption of all physical science. Boltzmann, in his 1897 textbook, had also pointed out the continuity of motion as the first basic assumption of mechanics, which in turn should provide the basis for all of physical science.[96] Hilbert advanced in his lectures the following general formulation of the principle of continuity:

> 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.[97]

Experiment—Hilbert continued—compels us to place this axiom at the top of every natural science, since it allows us to assert the validity of our assumptions and claims.[98] In every special case, this general axiom must be given the appropriate version, as Hilbert had shown for geometry in an earlier part of the lectures and here for vector addition. Of course there are many important differences between the Archimedean axiom, and the one formulated here for physical theories, but Hilbert seems to have preferred stressing the similarity rather than sharpening these differences. In fact, he suggested that from a strictly mathematical point of view, it would be possible to conceive interesting systems of physical axioms that do without continuity, that is, axioms that define a kind of "non-Archimedean physics." He did not consider such systems here, however, since the task was to see how the ideas and methods of axiomatics could be fruitfully applied to physics.[99] Nevertheless, this is an extremely important topic in Hilbert's axiomatic treatment of physical theories. When speaking of applying axiomatic ideas and methods to these theories, Hilbert

95 "Nimmt man nun von vornherein als Grundaxiom aller Naturwissenschaft an, daß alle auftretenden Funktionen einmal stetig differenzierbar sind, so kommt man hier mit den ersten 4 Axiomen aus." (Hilbert 1905a, 127)

96 Quoted in (Boltzmann 1974, 228–229).

97 "Schreibt man für die Erfüllung der Behauptung einen gewissen genügend kleinen Genaugikeitsgrad vor, so läßt sich ein Bereich angeben, innerhalb dessen man die Voraussetzungen frei wählen kann, ohne daß die Abweichung der Behauptung jenen vorgeschriebenen Grad überschreitet." (Hilbert 1905a, 125)

98 "Das Experiment zwingt uns geradezu dazu, ein solches Axiom an die Spitze aller Naturwissenschaft zu setzen, denn wir können bei ihm stets nur das ~~Ein~~<Zu>treffen von Voraussetzung und Behauptung mit einer gewissen beschränkten Genauigkeit feststellen." (Hilbert 1905a, 125–126)

meant in this case existing physical theories. But the possibility suggested here, of examining models of theories that preserve the basic logical structure of classical physics, except for a particular feature, opens the way to the introduction and systematic analysis of alternative theories, close enough to the existing ones in relevant respects. Hilbert's future works on physics, and in particular his work on general relativity, would rely on the actualization of this possibility.

An additional point that should be stressed in relation to Hilbert's treatment of vector addition has to do with his disciplinary conceptions. The idea of a vector space, and the operations with vectors as part of it, has been considered an integral part of algebra at least since the 1920s.[100] This was not the case for Hilbert, who did not bother here to make any connection between his axioms for vector addition and, say, the already well-known axiomatic definition of an abstract group. For Hilbert, as for the other mathematicians he cites in this section, this topic was part of physics rather than of algebra.[101] In fact, the articles by Hamel and by Schur were published in the *Zeitschrift für Mathematik und Physik*—a journal that bore the explicit subtitle: *Organ für angewandte Mathematik*. It had been established by Oscar Xavier Schlömilch (1823–1901) and by the turn of the century its editor was Carl Runge, the leading applied mathematician at Göttingen.

After the addition of vectors, Hilbert went on to discuss a second domain related to mechanics: statics. Specifically, he considered the axioms that describe the equilibrium conditions of a rigid body. The main concept here is that of a force, which can be described as a vector with an application point. The state of equilibrium is defined by the following axioms:

> I. Forces with a common application point are equivalent to their sum.
>
> II. Given two forces, K, L, with different application points, P, Q, if they have the same direction, and the latter coincides with the straight line connecting P and Q, then these forces are equivalent.
>
> III. A rigid body is in a state of equilibrium if all the forces applied to it taken together are equivalent to 0.[102]

99 "Rein mathematisch werden natürlich auch <physikalische> Axiomensysteme, die auf <diese> Stetigkeit Verzicht leisten, also eine 'nicht-Archimedische Physik' in erweitertem Sinne definieren, von hohem Interesse sein können; wir werden jedoch zunächst noch von ihrer Betrachtung absehen können, da es sich vorerst überhaupt nur darum handelt, die fruchtbaren Ideen und Methoden der Axiomatik in die Physik einzuführen." (Hilbert 1905a, 126)

100 Cf. (Dorier 1995; Moore 1995).

101 This point, which helps understanding Hilbert's conception of algebra, is discussed in detail in (Corry 2003, § 3.4).

102 "1., Kräfte mit demselben Angriffspunkt sind ihrer Summe (im obigen Sinne) '*aequivalent*.' 2., 2 Kräfte K, L mit verschiedenen Angriffspunkten P, Q und dem gleichen (auch gleichgerichteten) Vektor, deren Richtung in die Verbindung P, Q fällt, heißen gleichfalls *aequivalent*. … Ein starrer Körper befindet sich im *Gleichgewicht*, wenn die an ihm angreifenden Kräfte zusammengenommen der Null aequivalent sind." (Hilbert 1905a, 127–128)

From these axioms, Hilbert asserted, the known formulae of equilibrium of forces lying on the same plane (e.g., for the case of a lever and or an inclined plane) can be deduced. As in the case of vector addition, Hilbert's main aim in formulating the axioms was to uncover the basic, empirical facts that underlie our perception of the phenomenon of equilibrium.

In the following lectures Hilbert analyzed in more detail the principles of mechanics and, in particular, the laws of motion. In order to study motion, one starts by assuming space and adds time to it. Since geometry provides the axiomatic study of space, the axiomatic study of motion will call for a similar analysis of time.

According to Hilbert, two basic properties define time: (1) its uniform passage and (2) its unidimensionality.[103] A consistent application of Hilbert's axiomatic approach to this characterization immediately leads to the question: Are these two independent facts given by intuition,[104] or are they derivable the one from the other? Since this question had very seldom been pursued, he said, one could only give a brief sketch of tentative answers to it. The unidimensionality of time is manifest in the fact, that, whereas to determine a point in space one needs three parameters, for time one needs only the single parameter t. This parameter t could obviously be transformed, by changing the marks that appear on our clocks,[105] which is perhaps impractical but certainly makes no logical difference. One can even take a discontinuous function for t, provided it is invertible and one-to-one,[106] though in general one does not want to deviate from the continuity principle, desirable for all the natural sciences. Hilbert's brief characterization of time would seem to allude to Carl Neumann's (Neumann 1870), in his attempt to reduce the principle of inertia into simpler ones.

Whereas time and space are alike in that, for both, arbitrarily large values of the parameters are materially inaccessible, a further basic difference between them is that time can be experimentally investigated in only one direction, namely, that of its increase.[107] While this limitation is closely connected to the unidimensionality of time,[108] the issue of the uniform passage of time is an experimental fact, which has to be deduced, according to Hilbert, from mechanics alone.[109] This claim was elaborated into a rather obscure discussion of the uniform passage for which, as usual, Hilbert gave no direct references, but which clearly harks back to Hertz's and Larmor's

103 "... ihr *gleichmäßiger Verlauf* und ihre *Eindimensionalität*." (Hilbert 1905a, 128)

104 "... anschauliche unabhängige Tatsachen." (Hilbert 1905a, 129)

105 "Es ist ohne weiteres klar, daß dieser Parameter t durch eine beliebige Funktion von sich ersetzt werden kann; das würde etwa nur auf eine andere Benennung der Ziffern der Uhr oder einen unregelmäßigen Gang des Zeigers hinauskommen." (Hilbert 1905a, 129)

106 One is reminded here of a similar explanation, though in a more general context, found in Hilbert's letter to Frege, on 29 December 1899. See (Gabriel et al. 1980, 41).

107 "~~Der~~ <Ein> wesentlicher Unterschied von Zeit und Raum ist nur der, daß wir in der Zeit nur in einem Sinne, dem des *wachsenden Parameters* experimentieren können, während Raum und Zeit darin übereinstimmen, daß uns *beliebig große Parameterwerte* unzugänglich sind." (Hilbert 1905a, 129)

108 Here Hilbert adds with his own handwriting (p. 130): <Astronomie! Wie wichtig wäre Beobachtungen in ferner Vergangenheit u. Zukunft!>."

109 "... eine experimentelle nur aus der Mechanik zu entnehmende Tatsache." (Hilbert 1905a, 130)

discussions and referred to by Voss as well, as mentioned earlier. I try to reproduce Hilbert's account here without really claiming to understand it. In short, Hilbert argued that if the flow of time were non-uniform then an essential difference between organic and inorganic matter would be reflected in the laws mechanics, which is not actually the case. He drew attention to the fact that the differential expression $m \cdot d^2x/dt^2$ characterizes a specific physical situation only when it vanishes, namely, in the case of inertial motion. From a logical point of view, however, there is no apparent reason why the same situation might not be represented in terms of a more complicated expression, e.g., an expression of the form

$$m_1 \frac{d^2x}{dt^2} + m_2 \frac{dx}{dt}.$$

The magnitudes m_1 and m_2 may depend not only on time, but also on the kind of matter involved,[110] e.g., on whether organic or inorganic matter is involved. By means of a suitable change of variables, $t = t(\tau)$, this latter expression could in turn be transformed into $\mu \cdot d^2x/d\tau^2$, which would also depend on the kind of matter involved. Thus different kinds of substances would yield, under a suitable change of variables, different values of "time," values that nevertheless still satisfy the standard equations of mechanics. One could then use the most common kind of matter in order to measure time,[111] and when small variations of organic matter occurred along large changes in inorganic matter, clearly distinguishable non-uniformities in the passage of time would arise.[112] However, it is an intuitive (*anschauliche*) fact, indeed a mechanical axiom, Hilbert said, that the expression $m \cdot d^2x/dt^2$, always appears in the equations with *one and the same* parameter t, independently of the kind of substance involved, contrary to what the above discussion would seem to imply. This latter fact, to which Hilbert wanted to accord the status of axiom, is then the one that establishes the uniform character of the passage of time. Whatever the meaning and the validity of this strange argument, one source where Hilbert was likely to have found it is Aurel Voss's 1901 *Encyklopädie* article, which quotes, in this regard, similar passages of Larmor and Hertz.[113]

Following this analysis of the basic ideas behind the concept of time, Hilbert repeated the same kind of reasoning he had used in an earlier lecture concerning the role of continuity in physics. He suggested the possibility of elaborating a non-Galilean mechanics, i.e., a mechanics in which the measurement of time would depend on the kind of matter involved, in contrast to the characterization presented earlier in his lecture. This mechanics would, in most respects, be in accordance with

110 "... die m_1, m_2 von der Zeit, vor allem aber von dem Stoffe abhängig sein können." (Hilbert 1905a, 130)

111 "... der häufigste Stoff etwa kann dann zu Zeitmessungen verwandt werden." (Hilbert 1905a, 130–131)

112 "... für uns leicht große scheinbare Unstetigkeiten der Zeit auftreten." (Hilbert 1905a, 131)

113 See (Voss 1901, 14). Voss quoted (Larmor 1900, 288) and (Hertz 1894, 165).

the usual one, and thus one would be able to recognize which parts of mechanics depend essentially on the peculiar properties of time, and which parts do not. It is only in this way that the essence of the uniform passage of time can be elucidated, he thought, and one may thus at last understand the exact scope of the connection between this property and the other axioms of mechanics.

So much for the properties of space and time. Hilbert went on to discuss the properties of motion, while concentrating on a single material point. This is clearly the simplest case and therefore it is convenient for Hilbert's axiomatic analysis. However, it must be stressed that Hilbert was thereby distancing himself from Hertz's presentation of mechanics, in which the dynamics of single points is not contemplated. One of the axioms of statics formulated earlier in the course stated that a point is in equilibrium when the forces acting on it are equivalent to the null force. From this axiom, Hilbert derived the Newtonian law of motion:

$$m.\frac{d^2x}{dt^2} = X; m.\frac{d^2y}{dt^2} = Y; m.\frac{d^2z}{dt^2} = Z.$$

Newton himself, said Hilbert, had attempted to formulate a system of axioms for his mechanics, but his system was not very sharply elaborated and several objections could be raised against it. A detailed criticism, said Hilbert, was advanced by Mach in his *Mechanik*.[114]

The above axiom of motion holds for a free particle. If there are constraints, e.g., that the point be on a plane $f(x, y, z) = 0$ then one must introduce an additional axiom, namely, Gauss's principle of minimal constraint. Gauss's principle establishes that a particle in nature moves along the path that minimizes the following magnitude:

$$\frac{1}{m}\{(mx'' - X)^2 + (my'' - Y)^2 + (mz'' - Z)^2\} = \text{Minim.}$$

Here x'', y'', and z'' denote the components of the acceleration of the particle, and X, Y, Z the components of the moving force. Clearly, although Hilbert did not say it in his manuscript, if the particle is free from constraints, the above magnitude can actually become zero and we simply obtain the Newtonian law of motion. If there are constraints, however, the magnitude can still be minimized, thus yielding the motion of the particle.[115]

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114 A detailed account of the kind of criticism advanced by Mach, and before him by Carl Neumann and Ludwig Lange, appears in (Barbour 1989, chap. 12).

115 For more detail on Gauss's principle, see (Lanczos 1962, 106–110). Interestingly, Lanczos points out that "Gauss was much attached to this principle because it represents a perfect physical analogy to the 'method of least squares' (discovered by him and independently by Legendre) in the adjustment of errors." Hilbert also discussed this latter method in subsequent lectures, but did not explicitly make any connection between Gauss's two contributions.

In his lectures, Hilbert explained in some detail how the Lagrangian equations of motion could be derived from this principle. But he also stressed that the Lagrangian equations could themselves be taken as axioms and set at the top of the whole of mechanics. In this case, the Newtonian and Galilean principles would no longer be considered as necessary assumptions of mechanics. Rather, they would be logical consequences of a distinct principle. Although this is a convenient approach that is often adopted by physicists, Hilbert remarked, it has the same kinds of disadvantages as deriving the whole of geometry from the demand of linearity for the equations of the straight line: many results can be derived from it, but it does not indicate what the *simplest* assumptions underlying the considered discipline may be. All the discussion up to this point, said Hilbert, concerns the simplest and oldest systems of axioms for the mechanics of systems of points. Beside them there is a long list of other possible systems of axioms for mechanics. The first of these is connected to the principle of conservation of energy, which Hilbert associated with the law of the impossibility of a *perpetuum mobil*e and formulated as follows: "If a system is at rest and no forces are applied, then the system will remain at rest."[116]

Now the interesting question arises, how far can we develop the whole of mechanics by putting this law at the top? One should follow a process similar to the one applied in earlier lectures: to take a certain result that can be logically derived from the axioms and try to find out if, and to what extent, it can simply replace the basic axioms. In this case, it turns out that the law of conservation alone, as formulated above, is sufficient, though not necessary, for the derivation of the conditions of equilibrium in mechanics.[117] In order to account for the necessary conditions as well, the following axiom must be added: "A mechanical system can *only* be in equilibrium if, in accordance with the axiom of the impossibility of a *perpetuum mobile*, it is at rest."[118] The basic idea of deriving all of mechanics from this law, said Hilbert, was first introduced by Simon Stevin, in his law of equilibrium for objects in a slanted plane, but it was not clear to Stevin that what was actually involved was the reduction of the law to simpler axioms. The axiom was so absolutely obvious to Stevin, claimed Hilbert, that he had thought that a proof of it could be found without starting from any simpler assumptions.

From Hilbert's principle of conservation of energy, one can also derive the virtual velocities of the system, by adding a new axiom, namely, the principle of d'Alembert. This is done by placing in the equilibrium conditions, instead of the components

116 "Ist ein System in Ruhe und die Kräftefunction konstant (wirken keine Kräfte), so bleibt es in Ruhe." (Hilbert 1905a, 137)

117 "Es lässt sich zeigen, daß unter allen den Bedingungen, die die Gleichgewichtssätze der Mechanik liefern, wirklich Gleichgewicht eintritt." (Hilbert 1905a, 138)

118 "Es folgt jedoch nicht, daß diese Bedingungen auch *notwendig* für das Gleichgewicht sind, daß nicht etwa auch unter andern Umständen ein mechanisches System im Gleichgewicht sein kann. Es muß also noch ein Axiom hinzugenommen werden, des Inhaltes etwa: Ein mechanisches System kann *nur* dann im Gleichgewicht sein, wenn es dem Axiom von <der Unmöglichkeit des> Perpetuum mobile gemäß in Ruhe ist." (Hilbert 1905a, 138)

X, Y, Z of a given force-field acting on every mass point, the expressions $X - mx,'' Y - my'', Z - mz''$.. In other words, the principle establishes that motion takes place in such a way that at every instant of time, equilibrium obtains between the force and the acceleration. In this case we obtain a very systematic and simple derivation of the Lagrangian equations, and therefore of the whole of mechanics, from three axioms: the two connected with the principle of conservation of energy (as sufficient and necessary conditions) and d'Alembert's principle, added now.

A third way to derive mechanics is based on the concept of impulse. Instead of seeing the force field K as a continuous function of t, we consider K as first null, or of a very small value; then, suddenly, as increasing considerably in a very short interval, from t to $t + \tau$, and finally decreasing again suddenly. If one considers this kind of process at the limit, namely, when $\tau = 0$ one then obtains an impulse, which does not directly influence the acceleration, like a force, but rather creates a sudden velocity-change. The impulse is a time-independent vector, which however acts at a given point in time: at different points in time, different impulses may take place. The law that determines the action of an impulse is expressed by Bertrand's principle. This principle imposes certain conditions on the kinetic energy, so that it directly yields the velocity. It states that:

> The kinetic energy of a system set in motion as a consequence of an impulse must be maximal, as compared to the energies produced by all motions admissible under the principle of conservation of energy.[119]

The law of conservation is invoked here in order to establish that the total energy of the system is the same before and after the action of the impulse.

Bertrand's principle, like the others, could also be deduced from the elaborated body of mechanics by applying a limiting process. To illustrate this idea, Hilbert resorted to an analogy with optics: the impulse corresponds to the discontinuous change of the refraction coefficients affecting the velocity of light when it passes through the surface of contact between two media. But, again, as with the other alternative principles of mechanics, we could also begin with the concept of impulse as the basic one, in order to derive the whole of mechanics from it. This alternative assumes the possibility of constructing mechanics without having to start from the concept of force. Such a construction is based on considering a sequence of successive small impulses in arbitrarily small time-intervals, and in recovering, by a limiting process, the continuous action of a force. This process, however, necessitates the introduction of the continuity axiom discussed above. In this way, finally, the whole of mechanics is reconstructed using only two axioms: Bertrand's principle and the said axiom of continuity. In fact, this assertion of Hilbert is somewhat misleading, since his very formulation of Bertrand's principle presupposes the acceptance of the law of conservation of energy. In any case, Hilbert believed that also in this case, a

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119 "Nach einem Impuls muß die kinetische Energie des Systems bei der <wirklich> eintretenden Bewegung ein Maximum sein gegenüber allen mit dem Satze von der Erhaltung der Energie verträglichen Bewegungen." (Hilbert 1905a, 141)

completely analogous process could be found in the construction of geometric optics: first one considers the process of sudden change of optical density that takes place in the surface that separates two media; then, one goes in the opposite direction, and considers, by means of a limiting process, the passage of a light ray through a medium with continuously varying optical density, seeing it as a succession of many infinitely small, sudden changes of density.

Another standard approach to the foundations of mechanics that Hilbert discussed is the one based on the use of the Hamiltonian principle as the only axiom. Consider a force field K and a potential scalar function U such that K is the gradient of U. If T is the kinetic energy of the system, then Hamilton's principle requires that the motion of the system from a given starting point, at time t_1, and an endpoint, at time t_2, takes place along the path that makes the integral

$$\int_{t_1}^{t_2} (T - U) dt$$

an *extremum* among all possible paths between those two points. The Lagrangian equations can be derived from this principle, and the principle is valid for continuous as well as for discrete masses. The principle is also valid for the case of additional constraints, insofar as these constraints do not contain differential quotients that depend on the velocity or on the direction of motion (non-holonomic conditions). Hilbert added that Gauss's principle was valid for this exception.

Hilbert's presentation of mechanics so far focused on approaches that had specifically been criticized by Hertz: the traditional one, based on the concepts of time, space, mass and force, and the energetic one, based on the use of Hamilton's principle. To conclude this section, Hilbert proceeded to discuss the approaches to the foundations of mechanics introduced in the textbooks of Hertz and Boltzmann respectively. Hilbert claimed that both intended to simplify mechanics, but each from an opposite perspective.

Expressing once again his admiration for the perfect Euclidean structure of Hertz's construction of mechanics,[120] Hilbert explained that for Hertz, all the effects of forces were to be explained by means of rigid connections between bodies; but he added that this explanation did not make clear whether one should take into account the atomistic structure of matter or not. Hertz's only axiom, as described by Hilbert, was the principle of the straightest path (*Das Prinzip von der geradesten Bahn*), which is a special case of the Gaussian principle of minimal constraint, for the force-free case. According to Hilbert, Hertz's principle is obtained from Gauss's by substituting in the place of the parameter t, the arc lengths s of the curve. The curvature

120 "Er liefert jedenfalls von dieser Grundlage aus in abstrakter und präcisester Weise einen wunderbaren Aufbau der Mechanik, indem er ganz nach Euklidischem Ideale ein vollständiges System von Axiomen und Definitionen aufstellt." (Hilbert 1905a, 146)

$$m.\left\{\left(\frac{d^2x}{ds^2}\right)^2+\left(\frac{d^2y}{ds^2}\right)^2+\left(\frac{d^2z}{ds^2}\right)^2\right\}$$

of the path is to be minimized, in each of its points, when compared with all the other possible paths in the same direction that satisfy the constraint. On this path, the body moves uniformly if one also assumes Newton's first law.[121] In fact, this requirement had been pointed out by Hertz himself in the introduction to the Principles. As one of the advantages of his mathematical formulation, Hertz mentioned the fact that he does not need to assume, with Gauss, that nature intentionally keeps a certain quantity (the constraint) as small as possible. Hertz felt uncomfortable with such assumptions.

Boltzmann, contrary to Hertz, intended to explain the constraints and the rigid connections through the effects of forces, and in particular, of central forces between any two mass points. Boltzmann's presentation of mechanics, according to Hilbert, was less perfect and less fully elaborated than that of Hertz.

In discussing the principles of mechanics in 1905, Hilbert did not explicitly separate differential and integral principles. Nor did he comment on the fundamental differences between the two kinds. He did so, however, in the next winter semester, in a course devoted exclusively to mechanics (Hilbert 1905–6, § 3.1.2).[122]

Hilbert closed his discussion on the axiomatics of mechanics with a very interesting, though rather speculative, discussion involving Newtonian astronomy and continuum mechanics, in which methodological and formal considerations led him to ponder the possibility of unifying mechanics and electrodynamics. It should be remarked that neither Einstein's nor Poincaré's 1905 articles on the electrodynamics of moving bodies is mentioned in any of Hilbert's 1905 lectures; most likely, Hilbert was not aware of these works at the time.[123] Hilbert's brief remarks here, on the other hand, strongly bring to mind the kind of argument, and even the notation, used by Minkowski in his first public lectures on these topics in 1907 in Göttingen.

Earlier presentations of mechanics, Hilbert said, considered the force—expressed in terms of a vector field—as given, and then investigated its effect on motion. In

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121 "Die Bewegung eines jeden Systemes erfolgt gleichförmig in einer '*geradesten Bahn*', d.h. für einen Punkt: die Krümmung

$$m.\left\{\left(\frac{d^2x}{ds^2}\right)^2+\left(\frac{d^2y}{ds^2}\right)^2+\left(\frac{d^2z}{ds^2}\right)^2\right\}$$

der Bahnkurve soll ein Minimum sein, in jedem Orte, verglichen mit allen andern den Zwangsbedingungen gehorchenden Bahnen derselben Richtung, und auf dieser Bahn bewegt sich der Punkt gleichförmig." (Hilbert 1905a, 146–147)

122 The contents of this course are analyzed in some detail in (Blum 1994).

123 This particular lecture of Hilbert is dated in the manuscript 26 July 1905, whereas Poincaré's article was submitted for publication on 23 July 1905, and Einstein's paper three weeks later. Poincaré had published a short announcement on 5 June 1905, in the *Comptes rendus* of the Paris Academy of Sciences.

Boltzmann's and Hertz's presentations, for the first time, force and motion were considered not as separate, but rather as closely interconnected and mutually interacting, concepts. Astronomy is the best domain in which to understand this interaction, since Newtonian gravitation is the only force acting on the system of celestial bodies. In this system, however, the force acting on a mass point depends not only on its own position but also on the positions and on the motions of the other points. Thus, the motions of the points and the acting forces can only be determined simultaneously. The potential energy in a Newtonian system composed of two points $(a|b|c)$ and $(x|y|z)$ equals, as it is well-known, $-\frac{1}{r_{a,b,c \atop x,y,z}}$, the denominator of this fraction being the distance between the two points. This is a symmetric function of the two points, and thus it conforms to Newton's law of the equality of action and reaction. Starting from these general remarks, Hilbert went on to discuss some ideas that, he said, came from an earlier work of Boltzmann and which might lead to interesting results. Which of Boltzmann's works Hilbert was referring to here is not stated in the manuscript. However, from the ensuing discussion it is evident that Hilbert had in mind a short article by Boltzmann concerning the application of Hertz's perspective to continuum mechanics (Boltzmann 1900).

Hertz himself had already anticipated the possibility of extending his point of view from particles to continua. In 1900 Richard Reiff (1855–1908) published an article that developed this direction (Reiff 1900), and soon Boltzmann published a reply pointing out an error. Boltzmann indicated, however, that Hertz's point of view could be correctly extended to include continua, the possibility seemed to arise of constructing a detailed account of the whole world of observable phenomena.[124] Boltzmann meant by this that one could conceivably follow an idea developed by Lord Kelvin, J.J. Thomson and others, that considered atoms as vortices or other similar stationary motion phenomena in incompressible fluids; this would offer a concrete representation of Hertz's concealed motions and could provide the basis for explaining all natural phenomena. Such a perspective, however, would require the addition of many new hypotheses which would be no less artificial than the hypothesis of action at a distance between atoms, and therefore—at least given the current state of physical knowledge—little would be gained by pursuing it.

Boltzmann's article also contained a more positive suggestion, related to the study of the mechanics of continua in the spirit of Hertz. Following a suggestion of Brill, Boltzmann proposed to modify the accepted Eulerian approach to this issue. The latter consisted in taking a fixed point in space and deriving the equations of motion of the fluid by studying the behavior of the latter at the given point. Instead of this Boltzmann suggested a Lagrangian approach, deducing the equations by looking at an element of the fluid as it moves through space. This approach seemed to Boltzmann

124 "... ein detailliertes Bild der gesamten Erscheinungswelt zu erhalten." (Boltzmann 1900, 668)

to be the natural way to extend Hertz's point of view from particles to continua, and he was confident that it would lead to the equations of motion of an incompressible fluid as well as to those of a rigid body submerged in such a fluid.[125] In 1903 Boltzmann repeated these ideas in a seminar taught in Vienna, and one of his students decided to take the problem as the topic of his doctoral dissertation of 1904: this was Paul Ehrenfest (1880–1993). Starting from Boltzmann's suggestion, Ehrenfest studied various aspects of the mechanics of continua using a Lagrangian approach. In fact, Ehrenfest in his dissertation used the terms Eulerian and Lagrangian with the meaning intended here, as Boltzmann in his 1900 article had not (Ehrenfest 1904, 4–5). The results obtained in the dissertation helped to clarify the relations between the differential and the integral variational principles for non-holonomic systems, but they offered no real contribution to an understanding of all physical phenomena in terms of concealed motions and masses, as Boltzmann and Ehrenfest may have hoped.[126]

Ehrenfest studied in Göttingen between 1901 and 1903, and returned there in 1906 for one year, before moving with his mathematician wife Tatyana to St. Petersburg. We don't know the details of Ehrenfest's attendance at Hilbert's lectures during his first stay in Göttingen. Hilbert taught courses on the mechanics of continua in the winter semester of 1902–1903 and in the following summer semester of 1903, which Ehrenfest may well have attended. Nor do we know whether Hilbert knew anything about Ehrenfest's dissertation when he taught his course in 1905. But be that as it may, at this point in his lectures, Hilbert connected his consideration of Newtonian astronomy to the equations of continuum mechanics, while referring to the dichotomy between the Lagrangian and the Eulerian approach, and using precisely those terms. Interestingly enough, the idea that Hilbert pursued in response to Boltzmann's article was not that the Lagrangian approach would be the natural one for studying mechanics of continua, but rather the opposite, namely, that a study of the continua following the Eulerian approach, and assuming an atomistic world view, could lead to a unified explanation of all natural phenomena.

Consider a free system subject only to central forces acting between its mass-points —and in particular only forces that satisfy Newton's law, as described above. An axiomatic description of this system would include the usual axioms of mechanics, together with the Newtonian law as an additional one. We want to express this system, said Hilbert, as concisely as possible by means of differential equations. In the most general case we assume the existence of a continuous mass distribution in space, $\rho = (x, y, z, t)$. In special cases we have $\rho = 0$ within a well-delimited region; the case of astronomy, in which the planets are considered mass-points, can be derived from this special case by a process of passage to the limit. Hilbert explained what the Lagrangian approach to this problem would entail. That approach, he added, is the most appropriate one for discrete systems, but often it is also conveniently used

125 For more details, cf. (Klein 1970, 64–66).
126 For details on Ehrenfest's dissertation, see (Klein 1970, 66–74).

in the mechanics of continua. Here, however, he would follow the Eulerian approach to derive equations of the motion of a unit mass-particle in a continuum. The ideas discussed in this section, as well as in many other parts of this course, hark back to those he developed in somewhat greater technical detail in his 1902–1903 course on continuum mechanics, but here a greater conceptual clarity and a better understanding of the possible, underlying connections across disciplines is attained, thanks to the systematic use of an axiomatic approach in the discussion.

Let V denote the velocity of the particle at time t and at coordinates (x, y, z) in the continuum. V has three components $u = u(x, y, z, t)$, v and w. The acceleration vector for the unit particle is given by dV/dt, which Hilbert wrote as follows:[127]

$$\frac{dV}{dt} = \frac{\partial V}{\partial t} + u\frac{\partial V}{\partial x} + v\frac{\partial V}{\partial y} + w\frac{\partial V}{\partial z} = \frac{\partial V}{\partial t} + V \times \operatorname{curl} V - \frac{1}{2}\operatorname{grad}(V.V).$$

Since the only force acting on the system is Newtonian attraction, the potential energy at a point $(x|y|z)$ is given by

$$P = -\iiint -\frac{\rho'}{r_{\substack{x',y',z'\\x,y,z}}} dx'dy'dz'$$

where ρ' is the mass density at the point $(x'|y'|z')$. The gradient of this potential equals the force acting on the particle, and therefore we obtain three equations of motion that can succinctly be expressed as follows:

$$\frac{\partial V}{\partial t} + V \times \operatorname{curl} V - \frac{1}{2}\operatorname{grad}(V.V) = \operatorname{grad} P$$

One can add two additional equations to these three. First, the Poisson equation, which Hilbert calls "potential equation of Laplace":

$$\Delta P = 4\pi\rho$$

where Δ denotes the Laplacian operator (currently written as ∇^2). Second, the constancy of the mass in the system is established by means of the continuity equation:[128]

$$\frac{\partial \rho}{\partial t} = -\operatorname{div}(\rho \cdot V)$$

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127 In the manuscript the formula in the leftmost side of the equation appears twice, having a "-" sign in front of $V \times \operatorname{curl} V$. This is obviously a misprint, as a straightforward calculation readily shows.

128 In his article mentioned above, Reiff had tried to derive the pressure forces in a fluid starting only from the conservation of mass (Reiff 1900). Boltzmann pointed out that Reiff had obtained a correct result because of a compensation error in his mathematics. See (Klein 1970, 65).

We have thus obtained five differential equations involving five functions (the components u, v, w of V, P and ρ of the four variables x, y, z, t. The equations are completely determined when we know their initial values and other boundary conditions, such as the values of the functions at infinity. Hilbert called the five equations so obtained the "Newtonian world-functions," since they account in the most general way and in an axiomatic fashion for the motion of the system in question: a system that satisfies the laws of mechanics and the Newtonian gravitational law. It is interesting that Hilbert used the term "world-function" in this context, since the similar ones "world-point" and "world-postulate," were introduced in 1908 by Minkowski in the context of his work on electrodynamics and the postulate of relativity. Unlike most of the mathematical tools and terms introduced by Minkowski, this particular aspect of his work was not favorably received, and is hardly found in later sources (with the exception of "world-line"). Hilbert, however, used the term "world-function" not only in his 1905 lectures, but also again in his 1915 work on general relativity, where he again referred to the Lagrangian function used in the variational derivation of the gravitational field equations as a "world-function."

Besides the more purely physical background to the issues raised here, it is easy to detect that Hilbert was excited about the advantages and the insights afforded by the vectorial formulation of the Eulerian equations. Vectorial analysis as a systematic way of dealing with physical phenomena was a fairly recent development that had crystallized towards the turn of the century, mainly through its application by Heaviside in the context of electromagnetism and through the more mathematical discussion of the alternative systems by Gibbs.[129] The possibility of extending its use to disciplines like hydrodynamics had arisen even more recently, especially in the context of the German-speaking world. Thus, for instance, the *Encyklopädie* article on hydrodynamics, written in 1901, still used the pre-vectorial notation (Love 1901, 62–63).[130] Only one year before Hilbert's course, speaking at the International Congress of Mathematicians in Heidelberg, the Göttingen applied mathematician Ludwig Prandtl still had to explain to his audience how to write the basic equations of hydrodynamics "following Gibbs's notation" (Prandtl 1904, 489). Among German textbooks on vectorial analysis of the turn of the century,[131] formulations of the Eulerian equations like that quoted above appear in Alfred Heinrich Bucherer's textbook of 1903 (Bucherer 1903, 77–84) and in Richard Gans's book of 1905 (Gans 1905, 66–67). Whether he learnt about the usefulness of the vectorial notation in this context from his colleague Prandtl or from one of these textbooks, Hilbert was certainly impressed by the unified perspective it afforded from the formal point of view. Moreover, he seems also to have wanted to deduce far-reaching physical conclusions from this formal similarity. Hilbert pointed out in his lectures the strong analogy between this formulation of the

129 Cf. (Crowe 1967, 182–224).

130 The same is the case for (Lamb 1895, 7). This classical textbook, however, saw many later editions in which the vectorial formulation was indeed adopted.

131 Cf. (Crowe 1967, 226–233).

equations and Maxwell's equations of electrodynamics, though in the latter we have two vectors E, and B, the electric and the magnetic fields, against only one here, V. . He also raised the following question: can one obtain the whole of mechanics starting from these five partial equations as a single axiom, or, if that is not the case, how far can its derivation in fact be carried? In other words: if we want to derive the whole of mechanics, to what extent can we limit ourselves to assuming only Newtonian attraction or the corresponding field equations?[132] It would also be interesting, he said, to address the question of how far the analogy of gravitation with electrodynamics can be extended. Perhaps, he said, one can expect to find a formula that simultaneously encompasses these five equations and the Maxwellian ones together. This discussion of a possible unification of mechanics and electrodynamics also echoed, of course, the current foundational discussion that I have described in the preceding sections. It also anticipates what will turn out to be one of the pillars of Hilbert's involvement with general relativity in 1915.

Hilbert's reference to Hertz and Boltzmann in this context, and his silence concerning recent works of Lorentz, Wien, and others, is the only hint he gave in his 1905 lectures as to his own position on the foundational questions of physics. In fact, throughout these lectures Hilbert showed little inclination to take a stand on physical issues of this kind. Thus, his suggestion of unifying the equations of gravitation and electrodynamics was advanced here mainly on methodological grounds, rather than expressing, at this stage at least, any specific commitment to an underlying unified vision of nature. At the same time, however, his suggestion is quite characteristic of the kind of mathematical reasoning that would allow him in later years to entertain the possibility of unification and to develop the mathematical and physical consequences that could be derived from it.

6.2 Kinetic Theory of Gases

A main application of the calculus of probabilities that Hilbert considered is in the kinetic theory of gases. He opened this section by expressing his admiration for the remarkable way this theory combined the postulation of far-reaching assumptions about the structure of matter with the use of probability calculus, a combination that had been applied in a very illuminating way, leading to new physical results. Several works that appeared by end of the nineteenth century had changed the whole field of the study of gases, thus leading to a more widespread appreciation of the value of the statistical approach. The work of Planck, Gibbs and Einstein attracted a greater interest in and contributed to an understanding of Boltzmann's statistical interpretation of entropy.[133]

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132 "Es wäre nun die Frage, ob man mit ~~einem~~ diesen 5 partiellen Gleichungen als einzigem Axiom nicht auch überhaupt in der Mechanik auskommt, oder wie weit das geht, d.h. wie weit man sich auf Newtonsche Attraktion bezw. auf die entsprechenden Feldgleichungen beschränken kann." (Hilbert 1905a, 154)

It is easy to see, then, why Hilbert would have wished to undertake an axiomatic treatment of the kinetic theory of gases: not only because it combined physical hypotheses with probabilistic reasoning in a scientifically fruitful way, as Hilbert said in these lectures, but also because the kinetic theory was a good example of a physical theory where, historically speaking, additional assumptions had been gradually added to existing knowledge without properly checking the possible logical difficulties that would arise from this addition. The question of the role of probability arguments in physics was not settled in this context. In Hilbert's view, the axiomatic treatment was the proper way to restore order to this whole system of knowledge, so crucial to the contemporary conception of physical science.

In stating the aim of the theory as the description of the macroscopic states of a gas, based on statistical considerations about the molecules that compose it, Hilbert assumed without any further comment the atomistic conception of matter. From this picture, he said, one obtains, for instance, the pressure of the gas as the number of impacts of the gas molecules against the walls of its container, and the temperature as the square of the sum of the mean velocities. In the same way, entropy becomes a magnitude with a more concrete physical meaning than is the case outside the theory. Using Maxwell's velocity distribution function, Boltzmann's logarithmic definition of entropy, and the calculus of probabilities, one obtains the law of constant increase in entropy. Hilbert immediately pointed out the difficulty of combining this latter result with the reversibility of the laws of mechanics. He characterized this difficulty as a paradox, or at least as a result not yet completely well established.[134] In fact, he stressed that the theory had not yet provided a solid justification for its assumptions, and ever new ideas and stimuli were constantly still being added.

Even if we knew the exact position and velocities of the particles of a gas— Hilbert explained—it is impossible in practice to integrate all the differential equations describing the motions of these particles and their interactions. We know nothing of the motion of individual particles, but rather consider only the average magnitudes that are dealt with by the probabilistic kinetic theory of gases. In an oblique reference to Boltzmann's replies, Hilbert stated that the combined use of probabilities and infinitesimal calculus in this context is a very original mathematical contribution, which may lead to deep and interesting consequences, but which at this stage has in no sense been fully justified. Take, for instance, one of the well-known results of the theory, namely, the equations of vis viva. In the probabilistic version of the theory, Hilbert said, the solution of the corresponding differential equation does not emerge solely from the differential calculus, and yet it is correctly determined. It might conceivably be the case, however, that the probability calculus could have contradicted well-known results of the theory, in which case, using that calculus would clearly

133 Kuhn (1978, 21) quotes in this respect the well-known textbook, (Gibbs 1902), and an "almost forgotten" work, (Einstein 1902).

134 "Hier können wir aber bereits ein paradoxes, zum mindesten nicht recht befriedigendes Resultat feststellen." (Hilbert 1905a, 176)

yield what would be considered unacceptable conclusions. Hilbert explained this warning by showing how a fallacious probabilistic argument could lead to contradiction in the theory of numbers.

Take the five classes of congruence module 5 in the natural numbers, and consider how the prime numbers are distributed among these classes. For any integer x, let $A(x)$ be the number of prime numbers which are less than x, and let $A_0(x),\ldots,A_4(x)$, be the corresponding values of the same function, when only the numbers in each of the five classes are considered. Using the calculus of probabilities in a similar way to that used in the integration of the equations of motion of gas particles, one could reason as follows: The distribution of prime numbers is very irregular, but according to the laws of probability, this irregularity is compensated if we just take a large enough quantity of events. In particular, the limits at infinity of the quotients $A_i(x)/A(x)$ are all equal for $i = 0,\ldots,4$, and therefore equal to $1/5$. But it is clear, on the other hand, that in the class of numbers of the form $5m$, there are no prime numbers, and therefore $A_0(x)/A(x) = 0$. One could perhaps correct the argument by limiting its validity to the other four classes, and thus conclude that:

$$\underset{x=\infty}{L} \frac{A_i(x)}{A(x)} = \frac{1}{4}, \text{ for } i = 1,2,3,4.$$

Although this latter result is actually correct, Hilbert said, one cannot speak here of a real proof. The latter could only be obtained through deep research in the theory of numbers. Had we not used here the obvious number-theoretical fact that $5m$ can never be a prime number, we might have been misled by the probabilistic proof. Something similar happens in the kinetic theory of gases, concerning the integration of the vis viva. One assumes that Maxwell's distribution of velocities obeys a certain differential equation of mechanics, and in this way a contradiction with the known value of the integral of the vis viva is avoided. Moreover, according to the theory, because additional properties of the motion of the gas particles, which are prescribed by the differential equations, lie very deep and are only subtly distinguishable, they do not affect relatively larger values, such as the averages used in the Maxwell laws.[135] As in the case of the prime numbers, however, Hilbert did not consider this kind of reasoning to be a real proof.

All this discussion, which Hilbert elaborated in further detail, led him to formulate his view concerning the role of probabilistic arguments in mathematical and

135 "Genau so ist es nun hier in der kinetischen Gastheorie. Indem wir behaupten, daß die Maxwellsche Geschwindigkeitsverteilung den mechanischen Differentialgleichnungen genügt, vermeiden wir wohl einen Verstoß gegen das sofort bekannte Integral der lebendigen Kraft; weiterhin aber wird die Annahme gemacht, ~~daß~~ die durch die Differentialgleichungen geforderten weiteren Eigenschaften der Gaspartikelbewegung liegen soviel tiefer und sind so feine Unterscheidungen, daß sie so grobe Aussagen über mittlere Werte, wie die des Maxwellschen Gesetzes, nicht berühren." (Hilbert 1905a, 180–181)

physical theories. In this view, surprisingly empiricist and straightforwardly formulated, the calculus of probability is not an exact mathematical theory, but one that may appropriately be used as a first approximation, provided we are dealing with immediately apparent mathematical facts. Otherwise it may lead to significant contradictions. The use of the calculus of probabilities is justified—Hilbert concluded—insofar as it leads to results that are correct and in accordance with the facts of experience or with the accepted mathematical theories.[136]

Beginning in 1910 Hilbert taught courses on the kinetic theory of gases and on related issues, and also published original contributions to this domain. In particular, as part of his research on the theory of integral equations, which began around 1902, he solved in 1912 the so-called Boltzmann equation.[137].

6.3 Electrodynamics

The manuscript of the lecturer indicates that Hilbert did not discuss electrodynamics before 14 July 1905. By that time Hilbert must have been deeply involved with the issues studied in the electron-theory seminar. These issues must surely have appeared in the lectures as well, although the rather elementary level of discussion in the lectures differed enormously from the very advanced mathematical sophistication characteristic of the seminar. As mentioned above, at the end of his lectures on mechanics Hilbert had addressed the question of a possible unification of the equations of gravitation and electrodynamics, mainly based on methodological considerations. Now he stressed once more the similarities underlying the treatment of different physical domains. In order to provide an axiomatic treatment of electrodynamics similar to those of the domains discussed above—Hilbert opened this part of his lectures—one needs to account for the motion of an electron by describing it as a small electrified sphere and by applying a process of passage to the limit.

One starts therefore by considering a material point m in the classical presentation of mechanics. The kinetic energy of a mass-point is expressed as

$$L(v)= \frac{1}{2}mv^2.$$

The derivatives of this expression with respect to the components v_s of the velocity v define the respective components of the momentum

136 "… sie ist keine exakte mathematische Theorie, aber zu einer ersten Orientierung, wenn man nur alle unmittelbar leicht ersichtlichen mathematischen Tatsachen benutzt, häufig sehr geeignet; sonst führt sie sofort zu groben Verstößen. Am besten kann man wohl immer nachträglich sagen, daß die Anwendung der Wahrscheinlichkeitsrechnung immer dann berechtigt und erlaubt ist, wo sie zu richtigen, mit der Erfahrung bezw. der sonstigen mathematischen Theorie übereinstimmenden Resultaten führt." (Hilbert 1905a, 182–183)

137 In (Hilbert 1912a, chap. XXII).

$$\frac{\partial L(v)}{\partial v_s} = m.v_s.$$

If one equates the derivative of the latter with respect to time to the components of the forces—seen as the negative of the partial derivatives of the potential energy—one gets the equations of motion:

$$\frac{d\frac{\partial}{\partial v_s}}{dt} + \frac{\partial U}{\partial s} = 0 \qquad s = (x, y, z).$$

As was seen earlier in the lectures on mechanics, an alternative way to attain these equations is to use the functions L, U and the variational equation characteristic of the Hamiltonian principle:

$$\int_{t_1}^{t_2} (L - U) dt = \text{Minim.}$$

This principle can be applied, as Laplace did in his *Celestial Mechanics*, even without knowing anything about L, except that it is a function of the velocity. In order to determine the actual form of L, one must then introduce additional axioms. Hilbert explained that in the context of classical mechanics, Laplace had done this simply by asserting what for him was an obvious, intuitive notion concerning relative motion, namely, that we are not able to perceive any uniform motion of the whole universe.[138] From this assumption Laplace was able to derive the actual value $L(v) = \frac{1}{2}mv^2$. This was for Hilbert a classical instance of the main task of the axiomatization of a physical science, as he himself had been doing throughout his lectures for the cases of the addition of vectors, thermodynamics, insurance mathematics, etc.: namely, to formulate the specific axiom or axioms underlying a particular physical theory, from which the specific form of its central, defining function may be derived. In this case, Laplace's axiom is nothing but the expression of the Galilean invariance, of the Newtonian laws of motion, although Hilbert did not use this terminology here.

In the case of the electron, as Hilbert had perhaps recently learnt in the electron-theory seminar, this axiom of Galilean invariance, is no longer valid, nor is the specific form of the Lagrangian function. Yet—and this is what Hilbert stressed as a remarkable fact—the equation of motion of the electron can nevertheless be derived

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138 "Zur Festlegung von L muß man nun natürlich noch Axiome hinzunehmen, und Laplace kommt da mit einer allgemeinen, ihm unmittelbar anschaulichen Vorstellung über Relativbewegung aus, daß wir nämlich eine gleichförmige Bewegung des ganzen Weltalls nicht merken würden. Alsdann läßt sich die Form $mv^2/2$ von $L(v)$ bestimmen, und das ist wieder die ganz analoge Aufgabe zu denen, die das Fundament der Vektoraddition, der Thermodynamik, der Lebensversicherungsmathematik u.a. bildeten." (Hilbert 1905a, 187)

following considerations similar to those applied in Laplace's case. One need only find the appropriate axiom to effect the derivation. Without further explanation, Hilbert wrote down the Lagrangian that describes the motion of the electron. This may be expressed as

$$L(v) = \mu\frac{1-v^2}{v}\cdot\log\frac{1+v}{1-v}$$

where v denotes the ratio between the velocity of the electron and the speed of light, and μ is a constant, characteristic of the electron and dependent on its charge. This Lagrangian appears, for instance, in Abraham's first article on the dynamics of the electron, and a similar one appears in the article on Lorentz's *Encyklopädie* article.[139] If not earlier than that, Hilbert had studied these articles in detail in the seminar, where Lorentz's article was used as a main text.

If, as in the case of classical mechanics, one again chooses to consider the differential equation or the corresponding variational equation as the single, central axiom of electron theory, taking L as an undetermined function of v whose exact expression one seeks to derive, then—Hilbert said—in order to do so, one must introduce a specific axiom, characteristic of the theory and as simple and plausible as possible. Clearly—he said concluding this section—this theory will require more, or more complicated, axioms than the one introduced by Laplace in the case of classical mechanics.[140] The electron-theory seminar had been discussing many recent contributions, by people such as Poincaré, Lorentz, Abraham and Schwarzschild, who held conflicting views on many important issues. It was thus clear to Hilbert that, at that point in time at least, it would be too early to advance any definite opinion as to the specific axiom or axioms that should be placed at the basis of the theory. This fact, however, should not affect in principle his argument as to how the axiomatic approach should be applied to the theory.

It is noteworthy that in 1905 Hilbert did not mention the Lorentz transformations, which were to receive very much attention in his later lectures on physics. Lorentz published the transformations in an article of 1904 (Lorentz 1904b), but this article was not listed in the bibliography of the electron theory seminar,[141] and it is likely that Hilbert was not aware of it by the time of his lectures. In subsequent chapters we will see how he became aware of the centrality of the transformations mainly through the work of Minkowski.

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139 Respectively, (Abraham 1902, 37; Lorentz 1904a, 184). Lorentz's Lagrangian is somewhat different, since it contains two additional terms, involving the inverse of v^3.

140 "Nimmt man nun wieder die Differentialgleichungen bzw. das zugehörige Variationsproblem als Axiom und läßt L zunächst als noch unbestimmte Funktion von v stehen, so handelt es sich darum, dafür möglichst einfache und plausible Axiome so zu konstruiren, daß sie gerade jene Form von $L(v)$ bestimmen. Natürlich werden wir mehr oder kompliciertere Axiome brauchen, als in dem einfachen Falle der Mechanik bei Laplace." (Hilbert 1905a, 188)

141 Cf. (Pyenson 1979, 103).

6.4 A post-1909 addendum

To conclude this account of the 1905 lectures, it is interesting to notice that several years after having taught the course, Hilbert returned to the manuscript and added some remarks on the front page in his own handwriting. He mentioned two more recent works he thought relevant to understanding the use of the axiomatic method in physics. First, he referred to a new article by Hamel on the principles of mechanics. Hamel's article, published in 1909, contained philosophical and critical remarks concerning the issues discussed in his own earlier article of 1905 (the one mentioned by Hilbert with reference to the axiomatization of vector addition). In particular, it discussed the concepts of absolute space, absolute time and force, as a priori concepts of mechanics. The contents of this article are beyond the scope of our discussion here. Hilbert's interest in it may have stemmed from a brief passage where Hamel discussed the significance of Hilbert's axiomatic method (Hamel 1909, 358). More importantly perhaps, it also contained an account of a new system of axioms for mechanics.[142]

Second, in a formulation that condenses in a very few sentences his understanding of the principles and goals of axiomatization, as they apply to geometry and to various domains of physics, Hilbert also directed attention to what he saw as Planck's application of the axiomatic method in the latter's recent research on quantum theory. Hilbert thus wrote:

> It is of special interest to notice how the axiomatic method is put to use by Planck—in a more or less consistent and in a more or less conscious manner—even in modern quantum theory, where the basic concepts have been so scantily clarified. In doing this, he sets aside electrodynamics in order to avoid contradiction, much as, in geometry, continuity is set aside in order to remove the contradiction in non-Pascalian geometry, or like, in the theory of gases, mechanics is set aside in favor of the axiom of probability (maximal entropy), thus applying only the Stossformel or the Liouville theorem, in order to avoid the objections involved in the reversibility and recurrence paradoxes.[143]

From this remark we learn not only that Hilbert was aware of the latest advances in quantum theory (though, most probably, not in great detail) but also that he had a good knowledge of recent writings of Paul and Tatyana Ehrenfest. Beginning in 1906

142 According to Clifford Truesdell (1968, 336), this article of Hamel, together with the much later (Noll 1959), are the "only two significant attempts to solve the part of Hilbert's sixth problem that concern mechanics [that] have been published." One should add to this list at least another long article (Hamel 1927) that appeared in vol. 5 of the *Handbuch der Physik*.

143 Hilbert (1905a), added "<Besonders interessant ist es zu sehen, wie die axiomatische Methode von Planck sogar bei der modernen Quantentheorie, wo die Grundbegriffe noch so wenig geklärt sind, in mehr oder weniger konsequenter und in mehr oder weniger bewusster Weise zur Anwendung gebracht werden: dabei Ausschaltung der Elektrodynamik, um Widerspruch zu vermeiden—gerade wie in der Geometrie Ausschaltung der Stetigkeit, um den Widerspruch gegen die Nichtpaskalsche Geometrie zu beseitigen, oder in der Gastheorie Ausschaltung der Mechanik (Benutzung allein der Stossformel oder des Liouvilleschen Satzes) dafür Axiom der Wahrscheinlichkeit—(Entropie Maximum), um den Widerspruch gegen den Umkehr- oder Wiederkehreinwand zu beseitigen.>"

the Ehrenfests had made important contributions to clarifying Boltzmann's ideas in a series of publications on the conceptual foundations of statistical mechanics. The two last terms used by Hilbert in his hand-written remark (*Umkehr- oder Wiederkehreinwand)* were introduced only in 1907 by them, and were made widely known only through their *Encyklopädie* article that appeared in 1912. Hilbert may have known the term earlier from their personal contact with them, or through some other colleague.[144] Also, the *Stossformel* that Hilbert mentioned here referred probably to the *Stossanzahlansatz*, whose specific role in the kinetic theory, together with that of the Liouville theorem (that is the physicists' Liouville theorem), the Ehrenfests' article definitely contributed to clarify.[145] Moreover, the clarification of the conceptual interrelation between Planck's quantum theory and electrodynamics—alluded to by Hilbert in his added remark—was also one of Paul Ehrenfest's central contributions to contemporary physics.[146]

7. THE AXIOMATIZATION PROGRAM BY 1905 – PARTIAL SUMMARY

Hilbert's 1905 cycle of lectures on the axiomatization of physics represents the culmination of a very central thread in Hilbert's early scientific career. This thread comprises a highly visible part of his published work, namely that associated with *Grundlagen der Geometrie*, but also additional elements that, though perhaps much less evident, were nevertheless prominent within his general view of mathematics, as we have seen. Hilbert's call in 1900 for the axiomatization of physical theories was a natural outgrowth of the background from which his axiomatic approach to geometry first developed. Although in elaborating the point of view put forward in the *Grundlagen der Geometrie* Hilbert was mainly driven by the need to solve certain, open foundational questions of geometry, his attention was also attracted in this context by recent debates on the role of axioms, or first principles in physics. Hertz's textbook on mechanics provided an elaborate example of a physical theory presented in strict axiomatic terms, and—perhaps more important for Hilbert—it also discussed in detail the kind of requirements that a satisfactory system of axioms for a physical theory must fulfill. Carl Neumann's analysis of the "Galilean principle of inertia"—echoes of which we find in Hilbert's own treatment of mechanics— provided a further example of the kind of conceptual clarity that one could expect to gain from this kind of treatment. The writings of Hilbert's senior colleague at Königsberg, Paul Volkmann, show that towards the end of the century questions of this kind were also discussed in the circles he moved in. Also the works of both Boltzmann and Voss provided Hilbert with important sources of information and inspiration. From his ear-

144 Hilbert was most likely present when, on 13 November 1906, Paul Ehrenfest gave a lecture at the *Göttinger Mathematische Gesellschaft* on Boltzmann's H-theorem and some of the objections (*Einwände*) commonly raised against it. This lecture is reported in *Jahresbericht der Deutschen Mathematiker-Vereinigung*, Vol. 15 (1906), 593.

145 Cf. (Klein 1970, 119–140).

146 Cf. (Klein 1970, 230–257).

liest attempts to treat geometry in an axiomatic fashion in order to solve the foundational questions he wanted to address in this field, Hilbert already had in mind the axiomatization of other physical disciplines as a task that could and should be pursued in similar terms.

Between 1900 and 1905 Hilbert had the opportunity to learn much new physics. The lecture notes of his course provide the earliest encompassing evidence of Hilbert's own picture of physical science in general and, in particular, of how he thought the axiomatic analysis of individual theories should be carried out. Hilbert's physical interests now covered a broad range of issues, and he seems to have been well aware of the main open questions being investigated in most of the domains addressed. His unusual mathematical abilities allowed him to gain a quick grasp of existing knowledge, and at the same time to consider the various disciplines from his own idiosyncratic perspective, suggesting new interpretations and improved mathematical treatments. However, one must exercise great care when interpreting the contents of these notes. It is difficult to determine with exactitude the extent to which he had studied thoroughly and comprehensively all the existing literature on a topic he was pursuing. The relatively long bibliographical lists that we find in the introductions to many of his early courses do not necessarily mean that he studied all the works mentioned there. Even from his repeated, enthusiastic reference to Hertz's textbook we cannot safely infer to what extent he had read that book thoroughly. Very often throughout his career he was content when some colleague or student communicated to him the main ideas of a recent book or a new piece of research. In fact, the official assignment of many of his assistants—especially in the years to come—was precisely that: to keep him abreast of recent advances by studying in detail the research literature of a specific field. Hilbert would then, if he were actually interested, study the topic more thoroughly and develop his own ideas.

It is also important to qualify properly the extent to which Hilbert carried out a full axiomatic analysis of the physical theories he discussed. As we saw in the preceding sections, there is a considerable difference between what he did for geometry and what he did for other physical theories. In these lectures, Hilbert never actually proved 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 (significantly, works of his students or collaborators). In other cases there were no such works to mention, and—as in the case of thermodynamics—Hilbert simply *stated* that his axioms are indeed independent. In still other cases, he barely mentioned anything about independence or other properties of his axioms. Also, his derivations of the basic laws of the various disciplines from the axioms are rather sketchy, when they appear at all. Often, Hilbert simply declared that such a derivation was possible. What is clear is that Hilbert considered that an axiomatization along the lines he suggested was plausible and could eventually be fully performed following the standards established in *Grundlagen der Geometrie*.

Yet for all these qualifications, the lecture notes of 1905 present an intriguing picture of Hilbert's knowledge of physics, notable both for its breadth and its incisiveness. They afford a glimpse into a much less known side of his Göttingen teaching activity, which must certainly be taken into account in trying to understand the atmosphere that dominated this world center of science, as well as its widespread influence. More specifically, these notes illustrate in detail how Hilbert envisaged that axiomatic analysis of physical theories could not only contribute to conceptual clarification but also prepare the way for the improvement of theories, in the eventuality of future experimental evidence that conflicted with current predictions. If one knew in detail the logical structure of a given theory and the specific role of each of its basic assumptions, one could clear away possible contradictions and superfluous additional premises that may have accumulated in the building of the theory. At the same time, one would be prepared to implement, in an efficient and scientifically appropriate way, the local changes necessary to readapt the theory to meet the implications of newly discovered empirical data, in the eventuality of such discoveries. Indeed, Hilbert's own future research in physics, and in particular his incursion into general relativity, will be increasingly guided by this conception. The details of his various efforts in this direction will be discussed in the next chapters.

The nature and use of axioms in physical theories was discussed by many of Hilbert's contemporaries, as we have seen. Each had his own way of classifying the various kinds of axioms that are actually used or should be used. Hilbert himself did not discuss any possible such classification in detail but in his lectures we do find three different kinds of axioms actually implemented. This *de facto* classification is reminiscent, above all, of the one previously found in the writings of Volkmann. In the first place, every theory is assumed to be governed by specific axioms that characterize it and only it. These axioms usually express mathematical properties establishing relations among the basic magnitudes involved in the theory. Secondly, there are certain general mathematical principles that Hilbert saw as being valid for all physical theories. In the lectures he 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 was a central underlying assumption of all of Hilbert's work on physics, and that kind of reasoning appears 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 idea 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 that Hilbert, however, avoided addressing in his 1905 lectures. That type comprises claims about the ultimate nature of physical phenomena, an issue that was particularly controversial during the years preceding these lectures. Although Hilbert's sympathy for the mechanical worldview 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 is thus intended to be fully understood independently of any particular position in this debate. Hilbert himself would later adopt a different stance. His work on general relativity will be based directly on his adoption of the electromagnetic worldview and, beginning in 1913, a quite specific version of it, namely, Gustav Mie's electromagnetic theory of matter. On the other hand, Hermann Minkowski's work on electrodynamics, with its seminal reinterpretation of Einstein's special theory of relativity in terms of space-time geometry, should be understood as an instance of the kind of axiomatic analysis that Hilbert advanced in his 1905 lectures in which, at the same time, the debate between the mechanical and the electromagnetic world views is avoided.

When reading the manuscript of these lectures, one cannot help speculating about the reaction of the students who attended them. This was, after all, a regular course offered in Göttingen, rather than an advanced seminar. Before the astonished students stood the great Hilbert, rapidly surveying so 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 possess a unified view of all these matters. The impression must have been thrilling, but perhaps the understanding he imparted to the students did not run very deep. Hermann Weyl's account of his experience as a young student attending Hilbert's course upon his arrival in Göttingen offers direct evidence to support this impression. Thus, in his obituary of Hilbert, Weyl wrote:

> In the fullness of my innocence and ignorance I made bold to take the course Hilbert had announced for that term, on the notion of number and the quadrature of the circle. Most of it went straight over my head. But the doors of a new world swung open for me, and I had not sat long at Hilbert's feet before the resolution formed itself in my young heart that I must by all means read and study what this man had written. (Weyl 1944, 614)

But the influence of the ideas discussed in Hilbert's course went certainly beyond the kind of general inspiration described here so vividly by Weyl; they had an actual influence on later contributions to physics. Besides the works of Born and Carathéodory on thermodynamics, and of Minkowski on electrodynamics, there were many dissertations written under Hilbert, as well as the articles written under the influence of his lectures and seminars. Ehrenfest's style of conceptual clarification of existing theories, especially as manifest in the famous *Encyklopädi*e on statistical mechanics, also bears the imprint of Hilbert's approach. Still, one can safely say that little work on physical theories was actually published along the specific lines of axiomatic analysis suggested by Hilbert in *Grundlagen der Geometrie*. It seems, in fact, that such techniques were never fully applied by Hilbert or by his students and collaborators to yield detailed analyses of axiomatic systems defining physical theories. Thus, for instance, in 1927 Georg Hamel wrote a long article on the axiomatization of mechanics for the *Handbuch der Physik* (Hamel 1927). Hamel did mention Hilbert's work on geometry as the model on which any modern axiomatic analysis should be based. However, his own detailed account of the axioms needed for defining mechanics as known at that time was not followed by an analysis of the independence of the axioms, based on the construction of partial models, such as Hilbert had carried out for geometry. Similarly, the question of consistency was discussed only summarily. Nevertheless, as Hamel said, his analysis allowed for a clearer comprehension of the logical structure of all the assumptions and their interdependence.

If the 1905 lectures represent the culmination of a thread in Hilbert's early career, they likewise constitute the beginning of the next stage of his association with physics. In the next years, Hilbert himself became increasingly involved in actual research in mathematical physics and he taught many courses on various topics thus far not included within his scientific horizons.

8. LECTURES ON MECHANICS AND CONTINUUM MECHANICS

In his early courses on mechanics or continuum mechanics, Hilbert's support for the atomistic hypothesis, as the possible basis for a reductionistic, mechanical foundation of the whole of physics, was often qualified by referring to the fact that the actual attempts to provide a detailed account of how such a reduction would work in specific cases for the various physical disciplines had not been fully and successfully realized by then. Thus for instance, in his 1906 course on continuum mechanics, Hilbert described the theory of elasticity as a discipline whose subject-matter is the deformation produced on solid bodies by interaction and displacement of molecules. On first sight this would seem to be a classical case in which one might expect a direct explanation based on atomistic considerations. Nevertheless Hilbert suggested that, for lack of detailed knowledge, a different approach should be followed in this case:

> We will have to give up going here into a detailed description of these molecular processes. Rather, we will only look for those parameters on which the measurable deformation state of the body depends at each location. The form of the dependence of the Lagrangian function on these parameters will then be determined, which is actually composed by the kinetic and potential energy of the individual molecules. Similarly, in thermodynamics we will not go into the vibrations of the molecules, but we will rather introduce temperature itself as a general parameter and we will investigate the dependence of energy on it.[147]

147 "Wir werden hier auf eine eingehende Beschreibung dieser molekularen Vorgänge zu verzichten haben und dafür nur die Parameter aufsuchen, von denen der meßbare Verzerrungszustand der Körper an jeder Stelle abhängt. Alsdann wird festzustellen sein, wie die Form der Abhängigkeit der Lagranschen Funktion von diesen Parametern ist, die sich ja eigentlich aus kinetischer und potentieller Energie der einzelnen Molekel zusammensetzen wird. Ähnlich wird man in der Thermodynamik nicht auf die Schwingungen der Molekel eingehen, sondern die Temperatur selbst als allgemeinen Parameter einführen, und die Abhängigkeit der Energie von ihr untersuchen." (Hilbert 1906, 8–9)

The task of deducing the exact form of the Lagrangian under specific requirements postulated as part of the theory was the approach followed in the many examples already discussed above. This tension between reductionistic and phenomenological explanations in physics is found in Hilbert's physical ideas throughout the years and it eventually led to his abandonment of mechanical reductionism. The process becomes gradually manifest after 1910, though Hilbert still stuck to his original conceptions until around 1913.

The course on mechanics in the winter semester of 1910–1911 opened with an unambiguous statement about the essential role of mechanics as the foundation of natural science in general (Hilbert 1910–1911, 6). Hilbert praised the textbooks of Hertz and Boltzmann for their successful attempts to present in similar methodological terms, albeit starting from somewhat different premises, a fully axiomatic derivation of mechanics. This kind of presentation, Hilbert added, was currently being disputed. The course itself covered the standard topics of classical mechanics. Towards the end, however, Hilbert spoke about the "new mechanics." In this context he neither used the word "relativity" nor mentioned Einstein. Rather, he mentioned only Lorentz and spoke of invariance under the Lorentz transformations of all differential equations that describe natural phenomena as the main feature of this new mechanics. Hilbert stressed that the Newtonian equations of the "old" mechanics do not satisfy this basic principle, which, like Minkowski, he called the *Weltpostulate*. These equations must therefore be transformed, he said, so that they become Lorentz-invariant.[148] Hilbert showed that if the Lorentz transformations are used instead of the "Newton transformations," then the velocity of light is the same for every non-accelerated, moving system of reference.

Hilbert also mentioned the unsettled question of the status of gravitation in the framework of this new mechanics. He connected his presentation directly to Minkowski's sketchy treatment of this topic in 1909, and, like his friend, Hilbert does not seem to have been really bothered by the difficulties related with it. One should attempt to modify the Newtonian law in order to make it comply to the world-postulate, Hilbert said, but we must exercise special care when doing this since the Newtonian law has proved to be in the closest accordance with experience. As Hilbert knew from Minkowski's work, an adaptation of gravitation to the new mechanics would imply that its effects must propagate at the speed of light. This latter conclusion contradicts the "old theory," while in the framework of the "new mechanics," on the contrary, it finds a natural place. In order to adapt the Newtonian equations to the new mechanics, concluded Hilbert, we proceed, "as Minkowski did, via electromagnetism."[149]

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148 "Alle grundlegenden Naturgesetzen entsprechenden Systeme von Differentialgleichungen sollen gegenüber der Lorentz-Transformation kovariant sein. ... Wir können durch Beobachtung von irgend welcher Naturvorgängen niemals entscheiden, ob wir ruhen, oder uns gleichförmig bewegen. Diesen Weltpostulate genügen die Newtonschen Gleichungen der älteren Mechanik nicht, wenn wir die Lorentz Transformation zugrunde legen: wir stehen daher vor die Aufgabe, sich dementsprechend umgestalten." (Hilbert 1910–1911, 292)

The manuscript of the course does not record whether in the classroom Hilbert showed how, by proceeding "as Minkowski did, via electromagnetism," the adaptation of Newton's law should actually be realized. Perhaps at that time he still believed that Minkowski's early sketch could be further elaborated. Be that as it may, the concerns expressed here by Hilbert are not unlike those of other, contemporary physicists involved in investigating the actual place of the postulate of relativity in the general picture of physics. It is relevant to recall at this stage, however, that Einstein himself published nothing on this topic between 1907 and June 1911.

9. KINETIC THEORY

After another standard course on continuum mechanics in the summer of 1911, Hilbert taught a course specifically devoted to kinetic theory of gases for the first time in the winter of 1911–1912. This course marked the starting point of Hilbert's definitive involvement with a broader range of physical theories. Hilbert opened the course by referring once again to three possible, alternative treatments of any physical theory. First, is the "phenomenological perspective,"[150] often applied to study the mechanics of continua. Under this perspective, the whole of physics is divided into various chapters, each of which can be approached using different, specific assumptions, from which different mathematical consequences can be derived. The main mathematical tool used in this approach is the theory of partial differential equations. In fact, much of what Hilbert had done in his 1905 lectures on the axiomatization of physics, and then in 1906 on mechanics of continua, could be said to fall within this approach.

The second approach that Hilbert mentioned assumes the validity of the "theory of atoms." In this case a "much deeper understanding is reached. ... We attempt to put forward a system of axioms which is valid for the whole of physics, and which enables all physical phenomena to be explained from a unified point of view."[151] The mathematical methods used here are obviously quite different from those of the phe-

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149 "Wir können nun an die Umgestaltung des Newtonsches Gesetzes gehen, dabei müssen wir aber Vorsicht verfahren, denn das Newtonsche Gesetz ist das desjenige Naturgesetz, das durch die Erfahrung in Einklang bleiben wollen. Dieses wird uns gelingen, ja noch mehr, wir können verlangen, dass die Gravitation sich mit Lichtgeschwindigkeit fortpflanzt. Die alte Theorie kann das nicht, eine Fortpflanzung der Gravitation mit Lichtgeschwindigkeit widerspricht hier der Erfahrung: Die neue Theorie kann es, und man ist berechtigt, das als eine Vorzug derselben anzusehen, den eine momentane Fortpflanzung der Gravitation passt sehr wenig zu der modernen Physik. Um die Newtonschen Gleichungen für die neue Mechanik zu erhalten, gehen wir ähnlich vor wie Minkowski in der Elektromagnetik." (Hilbert 1910–1911, 295)

150 Boltzmann had used the term in this context in his 1899 Munich talk that Hilbert had attended. Cf. (Boltzmann 1899, 92–96).

151 "Hier ist das Bestreben, ein Axiomensystem zu schaffen, welches für die ganze Physik gilt, und aus diesem einheitlichen Gesichtspunkt alle Erscheinungen zu erklären. ... Jedenfalls gibt sie unvergleichlich tieferen Laufschuhes über Wesen und Zusammenhang der physikalischen Begriffe, ausserdem auch neue Aufklärung über physikalische Tatsachen, welche weit über die bei A) erhaltene hinausgeht." (Hilbert 1911–1912, 2)

nomenological approach: they can be subsumed, generally speaking, under the methods of the theory of probabilities. The most salient examples of this approach are found in the theory of gases and in radiation theory. From the point of view of this approach, the phenomenological one is a palliative, indispensable as a primitive stage on the way to knowledge, which must however be abandoned "as soon as possible, in order to penetrate the real sanctuary of theoretical physics."[152] Unfortunately, Hilbert said, mathematical analysis is not yet developed sufficiently to provide for all the demands of the second approach. One must therefore do without rigorous logical deductions and be temporarily satisfied with rather vague mathematical formulae.[153] Hilbert considered it remarkable that by using this method one nevertheless obtains ever new results that are in accordance with experience. He thus declared that the "main task of physics," embodied in the third possible approach, would be "the molecular theory of matter" itself, standing above the kinetic theory, as far as its degree of mathematical sophistication and exactitude is concerned. In the present course, Hilbert intended to concentrate on kinetic theory, yet he promised to consider the molecular theory of matter in the following semester. He did so, indeed, a year later.

Many of the important innovations implied by Hilbert's solution of the Boltzmann equation are already contained in this course of 1911–1912.[154] It was Maxwell in 1860 who first formulated an equation describing the distribution of the number of molecules of a gas, with given energy at a given point in time. Maxwell, however, was able to find only a partial solution which was valid only for a very special case.[155] In 1872 Boltzmann reformulated Maxwell's equation in terms of a single, rather complex, integro-differential equation, that has remained associated with his name ever since. The only exact solution Boltzmann had been able to find, however, was still valid for the same particular case that Maxwell had treated in his own model (Boltzmann 1872). By 1911, some progress had been made on the solution of the Boltzmann equation. The laws obtained from the partial knowledge concerning those solutions, which described the macroscopic movement and thermal processes in gases, seemed to be qualitatively correct. However, the mathematical methods used in the derivations seemed inconclusive and sometimes arbitrary. It was quite usual to rely on average magnitudes and thus the calculated values of the coefficients of heat conduction and friction appeared to be dubious. A more accurate estimation of these values remained a main concern of the theory, and the techniques developed by Hilbert apparently offered the means to deal with it.[156]

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152 "Wenn man auf diesem Standpunkt steht, so wird man den früheren nur als einer Notbehelf bezeichnen, der nötig ist als eine erste Stufe der Erkenntnis, über die man aber eilig hinwegschreiten muss, um in die eigentlichen Heiligtümer der theoretischen Physik einzudringen." (Hilbert 1911–1912, 2)

153 "... sich mit etwas verschwommenen mathematischen Formulierungen zufrieden geben muss." (Hilbert 1911–1912, 2)

154 In fact, in December 1911 Hilbert presented to the *Göttinger Mathematische Gesellschaft* an overview of his recent investigations on the theory, stating that he intended to publish them soon. Cf. *Jahresbericht der Deutschen Mathematiker-Vereinigung* 21 (1912), 58.

155 Cf. (Brush 1976, 432–446).

Very much as he had done with other theories in the past, Hilbert wanted to show how the whole kinetic theory could be developed starting from one basic formula, which in this case would be precisely the Boltzmann equation. His presentation would depart from the phenomenological approach by making some specific assumptions about the molecules, namely that they are spheres identical to one another in size. In addition he would focus, not on the velocity of any individual such molecule, but rather on their velocity distribution φ over a small element of volume.

In the opening lectures of the course, a rather straightforward discussion of the elementary physical properties of a gas led Hilbert to formulate a quite complicated equation involving φ. Hilbert asserted that a general solution of this equation was impossible, and it was thus necessary to limit the discussion to certain specific cases (Hilbert 1911–1912, 21). In the following lectures he added some specific, physical assumptions concerning the initial and boundary conditions for the velocity distribution in order to be able to derive more directly solvable equations. These assumptions, which he formulated as axioms of the theory, restricted the generality of the problem to a certain extent, but allowed for representing the distribution function as a series of powers of a certain parameter. In a first approximation, the relations between the velocity distributions yielded the Boltzmann distribution. In a second approximation, they yielded the propagation of the average velocities in space and time. Under this representation the equation appeared as a linear symmetric equation of the second type, where the velocity distribution φ is the unknown function, thus allowing the application of Hilbert's newly developed techniques. Still, he did not prove in detail the convergence of the power series so defined, nor did he complete the evaluation of the transport coefficient appearing in the distribution formula.

Hilbert was evidently satisfied with his achievement in kinetic theory. He was very explicit in claiming that without a direct application of the techniques he had developed in the theory of integral equations, and without having formulated the physical theory in terms of such integral equations, it would be impossible to provide a solid and systematic foundation for the theory of gases as currently known (Hilbert 1912a, 268; 1912b, 562). And very much as with his more purely mathematical works, also here Hilbert was after a larger picture, searching for the underlying connections among apparently distant fields. Particularly interesting for him were the multiple connections with radiation theory, which he explicitly mentioned at the end of his 1912 article, thus opening the way for his forthcoming courses and publications. In his first publication on radiation theory he explained in greater detail and with unconcealed effusiveness the nature of this underlying connection. He thus said:

> In my treatise on the "Foundations of the kinetic theory of gases," I have shown, using the theory of linear integral equations, that starting alone from the Maxwell-Boltzmann
>
> fundamental formula —the so-called collision formula— it is possible to construct systematically the kinetic theory of gases. This construction is such, that it requires only a consistent implementation of the methods of certain mathematical operations prescribed

156 Cf. (Born 1922, 587–589).

> in advance, in order to obtain the proof of the second law of thermodynamics, of Boltzmann's expression for the entropy of a gas, of the equations of motion that take into account both the internal friction and the heat conduction, and of the theory of diffusion of several gases. Likewise, by further developing the theory, we obtain the precise conditions under which the law of equipartition of energies over the intermolecular parameter is valid. Concerning the motion of compound molecules, a new law is also obtained according to which the continuity equation of hydrodynamics has a much more general meaning than the usual one. ...
>
> Meanwhile, there is a second physical domain whose principles have not yet been investigated at all from the mathematical point of view, and for the establishment of whose foundations—as I have recently discovered—the same mathematical tools provided by the integral equations are absolutely necessary. I mean by this the elementary theory of radiation, understanding by it the phenomenological aspect of the theory, which at the most immediate level concerns the phenomena of emission and absorption, and on top of which stand Kirchhoff's laws concerning the relations between emission and absorption. (Hilbert 1912b, 217–218)

Hilbert could boast now two powerful mathematical tools that allowed him to address the study of a broad spectrum of physical theories. On the one hand, the axiomatic method would help dispel conceptual difficulties affecting established theories—thus fostering their continued development—and also open the way for a healthy establishment of new ones. In his earlier courses he had already explored examples of the value of the method for a wide variety of disciplines, but Minkowski's contributions to electrodynamics and his analysis of the role of the principle of relativity offered perhaps, from Hilbert's point of view, the most significant example so far of the actual realization of its potential contribution. On the other hand, the theory of linear integral equations had just proven its value in the solution of such a central, open problem of physics. As far as he could see from his own, idiosyncratic perspective, the program for closing the gap between physical theories and mathematics had been more successful so far than he may have actually conceived when posing his sixth problem back in 1900. Hilbert was now prepared to attack yet another central field of physics and he would do so by combining once again the two mathematical components of his approach. The actual realization of this plan, however, was less smooth than one could guess from the above-quoted, somewhat pompous, declaration. As will be seen in the next section, although Hilbert's next incursion into the physicists' camp led to some local successes, as a whole they were less impressive in their overall significance than Hilbert would have hoped.

But even though Hilbert was satisfied with what his mastery of integral equations had allowed him to do thus far, and with what his usual optimism promised to achieve in other physical domains in the near future, there was an underlying fundamental uneasiness that he was not able to conceal behind the complex integral formulas and he preferred to explicitly share this uneasiness with his students. It concerned the possible justification of using probabilistic methods in physics in general and in kinetic theory in particular. Hilbert's qualms are worth quoting in some detail:

> If Boltzmann proves … that the Maxwell distribution … is the most probable one from among all distributions for a given amount of energy, this theorem possesses in itself a

> certain degree of interest, but it does not allow even a minimal inference concerning the velocity distribution that actually occurs in any given gas. In order to lay bare the core of this question, I want to recount the following example: in a raffle with one winner out of 1000 tickets, we distribute 998 tickets among 998 persons and the remaining two we give to a single person. This person thus has the greatest chance to win, compared to all other participants. His probability of winning is the greatest, and yet it is highly improbable that he will win. The probability of this is close to zero. In the same fashion, the probability of occurrence of the Maxwell velocity distribution is greater than that of any other distribution, but equally close to zero, and it is therefore almost absolutely certain that the Maxwell distribution will not occur.
>
> What is needed for the theory of gases is much more than that. We would like to prove that for a specified distribution, there is a probability very close to 1 that distribution is asymptotically approached as the number of molecules becomes infinitely large. And in order to achieve that, it is necessary to modify the concept of "velocity distribution" in order to obtain some margin for looseness. We should formulate the question in terms such as these: What is the probability for the occurrence of a velocity distribution that deviates from Maxwell's by no more than a given amount? And moreover: what allowed deviation must we choose in order to obtain the probability 1 in the limit?[157]

Hilbert discussed in some detail additional difficulties that arise in applying probabilistic reasoning within kinetic theory. He also gave a rough sketch of the kind of mathematical considerations that could in principle provide a way out to the dilemmas indicated. Yet he made clear that he could not give final answers in this regard.[158] This problem would continue to bother him in the near future. In any case, after this brief excursus, Hilbert continued with the discussion he had started in the

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157 "Wenn z.B. Boltzmann beweist—übrigens auch mit einigen Vernachlässigungen—dass die Maxwellsche Verteilung (die nach dem Exponentialgesetz) unter allen Verteilungen von gegebener Gesamtenergie die wahrscheinlichste ist, so besitzt dieser Satz ja an und für sich ein gewisses Interesse, aber er gestattet auch nicht der geringsten Schluss auf die Geschwindigkeitsverteilung, welche in einem bestimmten Gase wirklich eintritt. Um den Kernpunkt der Frage klar zu legen, will ich an folgendes Beispiel erinnern: In einer Lotterie mit einem Gewinn und von 1000 Losen seien 998 Losen auf 998 Personen verteilt, die zwei übrigen Lose möge eine andere Person erhalten. Dann hat diese Person im Vergleich zu jeder einzelnen andern die grössten Gewinnchancen. Die Wahrscheinlichkeit des Gewinnen ist für sie am grössten, aber es ist immer noch höchst unwahrscheinlich, dass sie gewinnt. Denn die Wahrscheinlichkeit ist so gut wie Null.
Ganz ebenso ist die Wahrscheinlichkeit für den Eintritt der Maxwellschen Geschwindigkeitsverteilung zwar grösser als die für das Eintreten einer jeden bestimmten andern, aber doch noch so gut wie Null, und es ist daher fast mit absoluter Gewissheit sicher, dass die Maxwellsche Verteilung nicht eintritt.
Was wir für die Gastheorie brauchen, ist sehr viel mehr. Wir wünschen zu beweisen, dass für eine gewisse ausgezeichnete Verteilung eine Wahrscheinlichkeit sehr nahe an 1 besteht, derart, dass sie sich mit Unendliche wachsende Molekülzahl der 1 asymptotisch annähert. Und um das zu erreichen, müssen wir den Begriff der „Geschwindigkeitsverteilung" etwas modifizieren, indem wir einen gewissen Spielraum zulassen. Wir hätten die Frage etwa so zu formulieren: Wie gross ist die Wahrscheinlichkeit dafür, dass eine Geschwindigkeitsverteilung eintritt, welche von der Maxwellschen nur um höchstens einen bestimmten Betrag abweicht—und weiter: wie gross müssen wir die zugelassenen Abweichungen wählen, damit wir im limes die Wahrscheinlichkeit eins erhalten?" (Hilbert 1911–1912, 75–76)

first part of his lectures and went on to generalize the solutions already obtained to the cases of mixtures of gases or of polyatomic gases.

In spite of its very high level of technical sophistication of his approach to kinetic theory, it is clear that Hilbert did not want his contribution to be seen as a purely mathematical, if major, addition to the solution of just one central, open problem of this theory. Rather, his aim was to be directly in touch with the physical core of this and other, related domains. The actual scope of his physical interests at the time becomes more clearly evident in a seminar that he organized in collaboration with Erich Hecke (1887–1947), shortly after the publication of his article on kinetic theory.[159] The seminar was also attended by the Göttingen docents Max Born, Paul Hertz, Theodor von Kármán (1881–1963), and Erwin Madelung (1881–1972), and the issues discussed included the following:[160]

- the ergodic hypothesis and its consequences;
- on Brownian motion and its theories;
- electron theory of metals in analogy to Hilbert's theory of gases;
- report on Hilbert's theory of gases;
- on dilute gases;
- theory of dilute gases using Hilbert's theory;
- on the theory of chemical equilibrium, including a reference to the
- related work of Sackur;
- dilute solutions.

The names of the participants and younger colleagues indicate that these deep physical issues, related indeed with kinetic theory but mostly not with its purely mathematical aspects, could not have been discussed only superficially. Especially indicative of Hilbert's surprisingly broad spectrum of interests is the reference to the work of Otto Sackur (1880–1914). Sackur was a physical chemist from Breslau whose work dealt mainly with the laws of chemical equilibrium in ideal gases and on Nernst law of heat. He also wrote a widely used textbook on thermochemistry and thermodynamics (Sackur 1912). His experimental work was also of considerable significance and, more generally, his work was far from the typical kind of purely technical, formal mathematical physics that is sometimes associated with Hilbert and the Göttingen school.[161]

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158 "Ich will Ihnen nun auseinandersetzen, wie ich mir etwa die Behandlung dieser Frage denke. Es sind da sicher noch grosse Schwierigkeiten zu überwinden, aber die Idee nach wird man wohl in folgender Weise vorgehen müssen: ... " (Hilbert 1911–1912, 77)

159 Hecke had also taken the notes of the 1911–1912 course.

160 References to this seminar appear in (Lorey 1916, 129). Lorey took this information from the German student's journal *Semesterberichte des Mathematischen Vereins*. The exact date of the seminar, however, is not explicitly stated.

10. RADIATION THEORY

Already in his 1911–1912 lectures on kinetic theory, Hilbert had made clear his interest in investigating, together with this domain and following a similar approach, the theory of radiation.[162] Kirchhoff's laws of emission and absorption had traditionally stood as the focus of interest of this theory. These laws, originally formulated in late 1859, describe the energetic relations of radiation in a state of thermal equilibrium.[163] They assert that in the case of purely thermal radiation (i.e., radiation produced by thermal excitation of the molecules) the ratio between the emission and absorption capacities of matter, η and α respectively, is a universal function of the temperature T and the wavelength λ,

$$\frac{\eta}{\alpha} = K(T, \lambda)$$

and is therefore independent of the substance and of any other characteristics of the body in question. One special case that Kirchhoff considered in his investigations is the case $\alpha = 1$, which defines a "black body," namely, a hypothetical entity that completely absorbs all wavelengths of thermal radiation incident in it.[164]

In the original conception of Kirchhoff's theory the study of black-body radiation may not have appeared as its most important open problem, but in retrospect it turned out to have the farthest-reaching implications for the development of physics at large. In its initial phases, several physicists attempted to determine over the last decades of the century the exact form of the spectral distribution of the radiation $K(T, \lambda)$ for a black body. Prominent among them was Wilhelm Wien, who approached the problem by treating this kind of radiation as loosely analogous to gas molecules. In 1896 he formulated a law of radiation that predicted very accurately recent existing measurements. Planck, however, was dissatisfied with the lack of a theoretical justification for what seemed to be an empirically correct law. In searching for such a justification within classical electromagnetism and thermodynamics, he modeled the atoms at the inside walls of a black-body cavity as a collection of electrical oscillators which absorbed and emitted energy at all frequencies. In 1899 he came forward with an expression for the entropy of an ideal oscillator, built on an analogy with Boltzmann's kinetic theory of gases, that provided the desired theoretical justification of

161 See Sackur's obituary in *Physikalische Zeitschrift* 16 (1915), 113–115. According to Reid's account (1970, 129), Ewald succinctly described Hilbert's scientific program at the time of his arrival in Göttingen with the following, alleged quotation of the latter: "We have reformed mathematics, the next thing to reform is physics, and then we'll go on to chemistry." Interest in Sackur's work, as instantiated in this seminar would be an example of an intended, prospective attack on this field. There are not, however, many documented, further instances of this kind.

162 Minkowski and Hilbert even had planned to have a seminar on the theory of heat radiation as early as 1907 (Minkowski 1907).

163 Cf., e.g., (Kirchhoff 1860).

164 Cf. (Kuhn 1978, 3–10).

Wien's law (Planck 1899). Later on, however, additional experiments produced values for the spectrum at very low temperatures and at long wavelengths that were not anymore in agreement with this law.

Another classical attempt was advanced by John William Strutt, Lord Rayleigh (1842–1919), and James Jeans (1877–1946), also at the beginning of the century.[165] Considering the radiation within the black-body cavity to be made up of a series of standing waves, they derived a law that, contrary to Wien's, approximated experimental data very well at long wavelengths but failed at short ones. In the latter case, it predicted that the spectrum would rise to infinity as the wavelength decreased to zero.[166]

In a seminal paper of 1900, Planck formulated an improved law that approximated Wien's formula in the case of short wavelengths and the Rayleigh-Jeans law in the case of long wavelengths. The law assumed that the resonator entropy is calculated by counting the number of distributions of a given number of finite, equal "energy elements" over a set of resonators, according to the formula:

$$E = nh\nu$$

where n is an integer, ν is the oscillators' frequency, and h is the now famous Planck constant, h = 6.55 x 10 -27 erg-sec. (Planck 1900). Based on this introduction of energy elements, assuming thermal equilibrium and applying statistical methods of kinetic theory, Planck derived the law that he had previously obtained empirically and that described the radiant energy distribution of the oscillators:

$$U_\nu = \frac{h\nu}{e^{h\nu/kT} - 1}.$$

Planck saw his assumption of energy elements as a convenient mathematical hypothesis, and not as a truly physical claim about the way in which matter and radiation actually interchange energy. In particular, he did not stress the significance of the finite energy elements that entered his calculation and he continued to think about the resonators in terms of a continuous dynamics. He considered his assumption to be very important since it led with high accuracy to a law that had been repeatedly confirmed at the experimental level, but at the same time he considered it to be a provisional one that would be removed in future formulations of the theory. In spite of its eventual revolutionary implications on the developments of physics, Planck did not realize before 1908 that his assumptions entailed any significant departure from the fundamental conceptions embodied in classical physics. As a matter of fact, he did not publish any further research on black-body radiation between 1901 and 1906.[167]

The fundamental idea of the quantum discontinuity was only slowly absorbed into physics, first through the works of younger physicists such as Einstein, Laue and

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165 Cf. (Kuhn 1978, 144–152).

166 Much later Ehrenfest (1911) dubbed this phenomenon "ultraviolet catastrophe."

167 This is the main claim developed in detail in the now classic (Kuhn 1978). For a more recent, summary account of the rise of quantum theory, see (Kragh 1999, chap. 5).

Ehrenfest, then by leading ones such as Planck, Wien and Lorentz, and finally by their readers and followers. The details pertaining to this complex process are well beyond the scope of my account here. Nonetheless, it is worth mentioning that a very significant factor influencing Planck's own views in this regard was his correspondence with Lorentz in 1908. Lorentz had followed with interest since 1901 the debates around black-body radiation, and he made some effort to connect them with his own theory of the electron. At the International Congress of Mathematicians held in Rome in 1908, Lorentz was invited to deliver one of the plenary talks, which he devoted to this topic. This lecture was widely circulated and read thereafter and it represented one of the last attempts at interpreting cavity radiation in terms of a classical approach (Lorentz 1909). But then, following critical remarks by several colleagues, Lorentz added a note to the printed version of his talk where he acknowledged that his attempt to derive the old Rayleigh-Jeans radiation law from electron theory was impracticable unless the foundations of the latter would be deeply modified. A letter to Lorentz sent by Planck in the aftermath of the publication contains what may be the latter's first acknowledgment of the need to introduce discontinuity as a fundamental assumption. Lorentz himself, at any rate, now unambiguously adopted the idea of energy quanta and he stressed it explicitly in his lectures of early 1909 in Utrecht.[168] Later, in his 1910 Wolfskehl cycle in Göttingen, Lorentz devoted one of the lectures to explaining why the classical Hamilton principle would not work for radiation theory. An "entirely new hypothesis," he said, needed to be introduced. The new hypothesis he had in mind was "the introduction of the energy elements invented by Planck" (Lorentz 1910, 1248). Hilbert was of course in the audience and he must have attentively listened to his guest explaining the innovation implied by this fundamental assertion.

Starting in 1911 research on black-body radiation became less and less prominent and at the same time the quantum discontinuity hypothesis became a central issue in other domains such as thermodynamics, specific heats, x-rays, and atomic models. The apparent conflicts between classical physics and the consequences of the hypothesis stood at the focus of discussions in the first Solvay conference organized in Brussels in 1911.[169] These discussion prompted Poincaré, who until then was reticent to adopt the discontinuity hypothesis, to elaborate a mathematical proof that Planck's radiation law necessarily required the introduction of quanta (Poincaré 1912). His proof also succeeded in convincing Jeans in 1913, who thus became one of the latest prominent physicists to abandon the classical conception in favor of discontinuity (Jeans 1914).[170]

The notes of Hilbert's course on radiation theory in the summer semester of 1912, starting in late April, evince a clear understanding and a very broad knowledge of all the main issues of the discipline. In his previous course on kinetic theory, Hilbert had

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168 Cf. (Kuhn 1978, 189–197).
169 Cf. (Barkan 1993).
170 Cf. (Kuhn 1978, 206–232).

promised to address "the main task of physics," namely, the molecular theory of matter itself, a theory he described as having a greater degree of mathematical sophistication and exactitude than kinetic theory. To a certain extent, teaching this course meant fulfilling that promise. Hilbert declared that he intended to address now the "domain of physics properly said," which is based on the point of view of the atomic theory. Hilbert was clearly very much impressed by recent developments in quantum theory. "Never has there been a more proper and challenging time than now," he said, "to undertake the research of the foundations of physics." What seems to have impressed Hilbert more than anything else were the deep interconnections recently discovered in physics, "of which formerly no one could have even dreamed, namely, that optics is nothing but a chapter within 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."[171] And above all, the "atomic theory," the "principle of discontinuity," which was not a hypothesis anymore, but rather, "like Copernicus's theory, a fact confirmed by experiment."[172]

Hilbert opened with a summary account of four-vector analysis[173] and of Special Theory of Relativity. Taking the relativity postulate to stand "on top" of physics as a whole, he then formulated the basics of electrodynamics as currently conceived, including Born's concept of a rigid body. This is perhaps Hilbert's first systematic discussion of Special Theory of Relativity in his lecture courses. As in the case of kinetic theory, Hilbert already raised here some of the ideas that he would later develop in his related, published works. But again, the course was far from being just an exercise in applying integral equations techniques to a particularly interesting, physical case. Rather, Hilbert covered most of the core, directly relevant, physical questions. Thus, among the topics discussed in the course we find the energy distribution of black-body radiation (including a discussion of Wien's and Rayleigh's laws) and Planck's theory of resonators under the effect of radiation. Hilbert particularly stressed the significance of recent works by Ehrenfest and Poincaré, as having shown the necessity of a discontinuous form of energy distribution (Hilbert 1912c, 94).[174] Hilbert also made special efforts to have Sommerfeld invited to give the last two lectures in the course, in which important, recent topics in the theory were discussed.[175]

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171 "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 träumen 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." (Hilbert 1912c, 2)

172 "... wie die Lehre des Kopernikus, eine durch das Experimente bewiesene Tatsache." (Hilbert 1912c, 2)

173 A hand-written addition to the typescript (Hilbert 1912c, 4) gives here a cross-reference to Hilbert's later course, (Hilbert 1916, 45–56), where the same topic is discussed in greater detail.

174 He referred to (Ehrenfest 1911) and (Poincaré 1912). Hilbert had recently asked Poincaré for a reprint of his article. See Hilbert to Poincaré, 6 May 1912. (Hilbert 1932–1935, 546)

However, as with all other physical theories, what Hilbert considered to be the main issue of the theory of radiation as a whole was the determination of the precise form of a specific law that stood at its core. In this case the law in question was Kirchhoff's law of emission and absorption, to which Hilbert devoted several lectures. Of particular interest for him was the possibility of using the techniques of the theory of integral equations for studying the foundations of the law and providing a complete mathematical justification for it. This would also become the main task pursued in his published articles on the topic, which I discuss in detail in the next four sections. In fact, just as his summer semester course was coming to a conclusion, Hilbert submitted for publication his first paper on the "Foundations of the Elementary Theory of Radiation."

11. STRUCTURE OF MATTER AND RELATIVITY: 1912–1914

After this account of Hilbert's involvement with kinetic theory and radiation theory, I return to 1912 in order to examine his courses in physics during the next two years.[176] The structure of matter was the focus of attention here, and Hilbert now finally came to adopt electromagnetism as the fundamental kind of phenomena to which all others should be reduced. The atomistic hypothesis was a main physical assumption underlying all of Hilbert's work from very early on, and also in the period that started in 1910. This hypothesis, however, was for him secondary to more basic, mathematical considerations of simplicity and precision. A main justification for the belief in the validity of the hypothesis was the prospect that it would provide a more accurate and detailed explanation of natural phenomena once the tools were developed for a comprehensive mathematical treatment of theories based on it. Already in his 1905 lectures on the axiomatization of physics, Hilbert had stressed the problems implied by the combined application of analysis and the calculus of probabilities as the basis for the kinetic theory, an application that is not fully justified on mathematical grounds. In his physical courses after 1910, as we have seen, he again expressed similar concerns. The more Hilbert became involved with the study of kinetic theory itself, and at the same time with the deep mathematical intricacies of the theory of linear integral equations, the more these concerns increased. This situation, together with his growing mastery of specific physical issues from diverse disciplines, helps to explain Hilbert's mounting interest in questions related to the structure of matter that occupied him in the period I discuss now. The courses described below cover a wide range of interesting physical questions. In this account, for reasons of space, I will

175 Cf. Hilbert to Sommerfeld, 5 April 1912 (*Nachlass* Arnold Sommerfeld, Deutsches Museum, Munich. HS1977–28/A, 141).

176 The printed version of the *Verzeichnis der Vorlesungen an der Georg-August-Universität zu Göttingen* registers several courses for which no notes or similar documents are extant, and about which I can say nothing here: summer semester, 1912 - Mathematical Foundations of Physics; winter semester, 1912–1913 - Mathematical Foundations of Physics.

comment only on those aspects that are more directly connected with the questions of axiomatization, reductionism and the structure of matter.

11.1 Molecular Theory of Matter - 1912–1913

Hilbert's physics course in the winter semester of 1912–1913 was devoted to describing the current state of development of the molecular theory of matter (Hilbert 1912–1913),[177] and particularly the behavior of systems of huge quantities of particles moving in space, and affecting each other through collisions and other kinds of interacting forces.[178] The first of the course's three parts deals with the equation of state, including a section on the principles of statistical mechanics. The second part is characterized as "phenomenological" and the third part as "kinetic," in which entropy and the quantum hypothesis are discussed. This third part also includes a list of axioms for the molecular theory of matter. Hilbert was thus closing a circle initiated with the course on kinetic theory taught one year earlier.

Hilbert suggested that the correct way to come to terms with the increasingly deep mathematical difficulties implied by the atomistic hypothesis would be to adopt a "physical point of view." This means that one should make clear, through the use of the axiomatic method, those places in which physics intervenes into mathematical deduction. This would allow separating three different components in any specific physical domain considered: first, what is arbitrarily adopted as definition or taken as an assumption of experience; second, what a-priori expectations follow from these assumptions, which the current state of mathematics does not yet allow us to conclude with certainty; and third, what is truly proven from a mathematical point of view.[179] This separation interestingly brings to mind Minkowski's earlier discussion on the status of the principle of relativity. It also reflects to a large extent the various levels of discussion evident in Hilbert's articles on radiation theory, and it will resurface in his reconsideration of the view of mechanics as the ultimate explanation of physical phenomena.

In the first part of the course, Hilbert deduced the relations between pressure, volume and temperature for a completely homogenous body. He considered the body as

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177 A second copy of the typed notes in found in *Nachlass* Max Born, Staatsbibliothek Berlin, Preussischer Kulturbesitz #1817.

178 "Das Ziel der Vorlesung ist es, die Molekulartheorie der Materie nach dem heutigen Stande unseres Wissens zu entwickeln. Diese Theorie betrachtet die physikalischen Körper und ihre Veränderungen unter dem Scheinbilde eines Systems ungeheuer vieler im Raum bewegter Massen, die durch die Stösse oder durch andere zwischen ihnen wirkenden Kräfte einander beeinflussen." (Hilbert 1912–1913, 1)

179 "Dabei werden wir aber streng axiomatisch die Stellen, in denen die Physik in die mathematische Deduktion eingreift, deutlich hervorheben, und das voneinander trennen, was erstens als logisch willkürliche Definition oder Annahme der Erfahrung entnommen wird, zweitens das, was a priori sich aus diesen Annahmen folgern liesse, aber wegen mathematischer Schwierigkeiten zur Zeit noch nicht sicher gefolgert werden kann, und dritten, das, was bewiesene mathematische Folgerung ist." (Hilbert 1912–1913, 1)

a mechanical system composed of molecules, and applied to it the standard laws of mechanics. This is a relatively simple case, he said, that can be easily and thoroughly elucidated. However, deriving the state equation and explaining the phenomenon of condensation covers only a very reduced portion of the empirically manifest properties of matter. Thus the second part of the lectures was devoted to presenting certain, more complex physical and chemical phenomena, the kinetic significance of which would then be explained in the third part of the course.[180] The underlying approach was to express the basic facts of experience in mathematical language, taking them as axioms in need of no further justification. Starting from these axioms one would then deduce as many results as possible, and the logical interdependence of these axioms would also be investigated. In this way, Hilbert declared, the axiomatic method, long applied in mathematics with great success, can also be introduced into physics.[181]

A main task that Hilbert had pursued in his 1905 lectures on axiomatization was to derive, from general physical and mathematical principles in conjunction with the specific axioms of the domain in question, an equation that stands at the center of each discipline and that accounts for the special properties of the particular system under study. Hilbert explicitly stated this as a main task for his system of axioms also in the present case.[182] A first, general axiom he introduced was the "principle of equilibrium," which reads as follows:

> In a state of equilibrium, the masses of the independent components are so distributed with respect to the individual interactions and with respect to the phases, that the characteristic function that expresses the properties of the system attains a minimum value.[183]

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180 "Wir haben bisher das Problem behandelt, die Beziehung zwischen $p, v,$ und ϑ an einem chemisch völlig homogenen Körper zu ermitteln. Unser Ziel war dabei, diese Beziehung nach den Gesetzen der Mechanik aus der Vorstellung abzuleiten, dass der Körper ein mechanisches System seiner Molekele ist. In dem bisher behandelten, besonders einfache Falle, in dem wir es mit einer einzigen Molekel zu tun hatten, liess sich dies Ziel mit einer gewissen Vollständigkeit erreichen. Eine in einem bestimmten Temperaturintervall mit der Erfahrung übereinstimmende Zustandsgleichung geht nämlich aus der Kinetischen Betrachtung hervor. Mit der Kenntnis der Zustandsgleichung und der Kondensationserscheinungen ist aber nur ein sehr kleiner Teil, der sich empirisch darbietenden Eigenschaften der Stoffe erledigt. *Wir werden daher in diesem zweiten Teile diejenigen Ergebnisse der Physik und Chemie zusammenstellen, deren kinetische Deutung wir uns später zur Aufgabe machen wollen*." (Hilbert 1912–1913, 50)

181 "Die reinen Erfahrungstatsachen werden dabei in mathematischer Sprache erscheinen und als Axiome auftreten, die hier keiner weiteren Begründung bedürfen. Aus diesen Axiomen werden wir soviel als möglich, rein mathematische Folgerungen ziehen, und dabei untersuchen, welche unter den Axiomen voneinander unabhängig sind und welche zum Teil auseinander abgeleitet werden können. Wir werden also den axiomatischen Standpunkt, der in der modernen Mathematik schon zur Geltung gebracht ist, auf die Physik anwenden." (Hilbert 1912–1913, 50)

182 "Um im einzelnen Falle die charakteristische Funktion in ihrer Abhängigkeit von der eigentlichen Veränderlichen und den Massen der unabhängigen Bestandteile zu ermitteln, müssen verschiedenen neue Axiome hinzugezogen werden." (Hilbert 1912–1913, 60)

183 "Im Gleichgewicht verteilen sich die Massen der unabhängigen Bestandteile so auf die einzelnen Verbindungen und Phasen, dass die charakteristische Funktion, die den Bedingungen des Systems entspricht, ein Minimum wird." (Hilbert 1912–1913, 60)

Hilbert declared that such an axiom had not been explicitly formulated before and claimed that its derivation from mechanical principles should be done in terms of purely kinetic considerations, such as would be addressed in the third part of the course.[184] At the same time he stated that, in principle, this axiom is equivalent to the second law of thermodynamics, which Hilbert had usually formulated in the past as the impossibility of the existence of a "*perpetuum mobile*."

The topics for which Hilbert carried out an axiomatic analysis included the equation of state and the third law of thermodynamics. Hilbert's three axioms for the former allowed him a derivation of the expression for the thermodynamical potential of a mixture of gases that was followed by a discussion of the specific role of each of the axioms involved.[185] Concerning the third law of thermodynamics, Hilbert introduced five axioms meant to account for the relationship between the absolute zero temperature, specific heats and entropy. Also in this case he devoted some time to discussing the logical and physical interdependence of these axioms. Hilbert explained that the axiomatic reduction of the most important theorems into independent components (the axioms) is nevertheless not yet complete. The relevant literature, he said, is also full of mistakes, and the real reason for this lies at a much deeper layer. The basic concepts seem to be defined unclearly even in the best of books. The problematic use of the basic concepts of thermodynamics went back in some cases even as far as Helmholtz.[186]

The third part of the course contained, as promised, a "kinetic" section especially focusing on a discussion of rigid bodies. Hilbert explained that the results obtained in the previous sections had been derived from experience and then generalized by means of mathematical formulae. In order to derive them a-priori from purely mechanical considerations, however, one should have recourse to the "fundamental principle of statistical mechanics,"[187] presumably referring to the assumption that all accessible states of a system are equally probable. Hilbert thought that the task of the

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184 "Es muss kinetischen Betrachtung überlassen bleiben, es aus den Prinzipien der Mechanik abzuleiten und wir werden im dritten Teil der Vorlesung die erste Ansätze an solchen kinetischen Theorie kennen lernen." (Hilbert 1912–1913, 61)

185 "Die drei gegebenen Axiome reichen also hin, um das thermodynamische Potential der Mischung zu berechnen. Aber sind nicht in vollem Umfange dazu Notwendig. Nimmt man z.B. das dritte Axiom für eine bestimmte Temperatur gültig an, so folgt es für jede beliebige Temperatur aus den beiden ersten Axiomen. Ebensowenig ist das erste und zweite Axiom vollständig voneinander unabhängig." (Hilbert 1912–1913, 66)

186 "Die axiomatische Reduktion der vorstehenden Sätze auf ihre unabhängigen Bestandteile ist demnach noch nicht vollständig durchgeführt, und es finden sich auch in der Literatur hierüber verschiedene Ungenauigkeiten. Was den eigentlichen Kern solcher Missverständnisse anlangt, so glaube ich, dass er tiefer liegt. Die Grundbegriffe scheinen mir selbst in den besten Lehrbüchern nicht genügend klar dargestellt zu sein, ja, in einem gleich zu erörternden Punkte geht die nicht ganz einwandfreie Anwendung der thermodynamischen Grundbegriffe sogar auch Helmholtz zurück." (Hilbert 1912–1913, 80)

187 "Um die empirisch gegebenen und zu mathematischen Formeln verallgemeinerten Ergebnisse des vorigen Teiles a priori und zwar auf rein mechanischem Wege abzuleiten, greifen wir wieder auf des Grundprinzip des statistischen Mechanik zurück, von der wir bereits im ersten Teil ausgegangen waren." (Hilbert 1912–1913, 88)

course would be satisfactorily achieved if those results that he had set out to derive were indeed reduced to the theorems of mechanics together with this principle.[188] At any rate, the issues he discussed in this section included entropy, thermodynamics laws and the quantum hypothesis.

It is noteworthy that, although in December 1912, Born himself lectured on Mie's theory of matter at the *Göttinger Mathematische Gesellschaft*,[189] and that this theory touched upon many of the issues taught by Hilbert in this course, neither Mie's name nor his theory are mentioned in the notes. Nor was the theory of relativity theory mentioned in any way.

11.2 Electron Theory: 1913

In April of 1913 Hilbert organized a new series of Wolfskehl lectures on the current state of research in kinetic theory, to which he invited the leading physicists of the time. Planck lectured on the significance of the quantum hypothesis for kinetic theory. Peter Debye (1884–1966), who would become professor of physics in Göttingen the next year, dealt with the equation of state, the quantum hypothesis and heat conduction. Nernst, whose work on thermodynamics Hilbert had been following with interest,[190] spoke about the kinetic theory of rigid bodies. Von Smoluchowski came from Krakow and lectured on the limits of validity of the second law of thermodynamics, a topic he had already addressed at the Münster meeting of the *Gesellschaft Deutscher Naturforscher und Ärzte*. Sommerfeld came from Munich to talk about problems of free trajectories. Lorentz was invited from Leiden; he spoke on the applications of kinetic theory to the study of the motion of the electron. Einstein was also invited, but he could not attend.[191] Evidently this was for Hilbert a major event and he took pains to announce it very prominently on the pages of the *Physikalische Zeitschrift*, including rather lengthy and detailed abstracts of the expected lectures for the convenience of those who intended to attend.[192] After the meeting Hilbert also wrote a detailed report on the lectures in the *Jahresbericht der Deutschen Mathematiker-Vereiningung*[193]as well as the introduction to the published collection (Planck et al. 1914). Hilbert expressed the hope that the meeting would stimulate further inter-

188 "Auf die Kritik dieses Grundprinzipes und die Grenzen, die seiner Anwendbarkeit gesteckt sind, können wir hier nicht eingehen. Wir betrachten vielmehr unser Ziel als erreicht, wenn die Ergebnisse, die abzuleiten wir uns zur Aufgabe stellen, auf die Sätze der Mechanik und auf jenes Prinzip zurückgeführt sind." (Hilbert 1912–1913, 88)

189 *Jahresbericht der Deutschen Mathematikervereinigung* 22 (1913), 27.

190 In January 1913, Hilbert had lectured on Nernst's law of heat at the Göttingen Physical Society (*Nachlass* David Hilbert, (Cod. Ms. D. Hilbert, 590). See also a remark added in Hilbert's handwriting in (Hilbert 1905a, 167).

191 Cf. Einstein to Hilbert, 4 October 1912 (CPAE 5, Doc. 417).

192 *Physikalische Zeitschrift* 14 (1913), 258–264. Cf. also (Born 1913).

193 *Jahresbericht der Deutschen Mathematiker-Vereinigung* 22 (1913), 53–68, which includes abstract of all the lectures. Cf. also *Jahresbericht der Deutschen Mathematiker-Vereinigung* 23 (1914), 41.

est, especially among mathematicians, and lead to additional involvement with the exciting world of ideas created by the new physics of matter.

That semester Hilbert also taught two courses on physical issues, one on the theory of the electron and another on the principles of mathematics, quite similar to his 1905 course on the axiomatic method and including a long section on the axiomatization of physics as well. Hilbert's lectures on electron theory emphasized throughout the importance of the Lorentz transformations and of Lorentz covariance, and continually referred back to the works of Minkowski and Born. Hilbert stressed the need to formulate unified theories in physics, and to explain all physical processes in terms of motion of points in space and time.[194] From this reductionistic point of view, the theory of the electron would appear as the most appropriate foundation of all of physics.[195] However, given the difficulty of explicitly describing the motion of, and the interactions among, several electrons, Hilbert indicated that the model provided by kinetic theory had to be brought to bear here. He thus underscored the formal similarities between mechanics, electrodynamics and the kinetic theory of gases. In order to describe the conduction of electricity in metals, he developed a mechanical picture derived from the theory of gases, which he then later wanted to substitute by an electrodynamical one.[196] Hilbert stressed the methodological motivation behind his quest after a unified view of nature, and the centrality of the demand for universal validity of the Lorentz covariance, in the following words:

> But if the relativity principle [i.e., invariance under Lorentz transformations] is valid, then it is so not only for electrodynamics, but for the whole of physics. We would like to consider the possibility of reconstructing the whole of physics in terms of as few basic concepts as possible. The most important concepts are the concept of force and of rigidity. From this point of view the electrodynamics would appear as the foundation of all of physics. But the attempt to develop this idea systematically must be postponed for a later opportunity. In fact, it has to start from the motion of one, of two, etc. electrons, and there are serious difficulties on the way to such an undertaking. The corresponding problem for Newtonian physics is still unsolved for more than two bodies.[197]

When looking at the kind of issues raised by Hilbert in this course, one can hardly be surprised to discover that somewhat later Gustav Mie's theory of matter eventually

194 "Alle physikalischen Vorgänge, die wir einer axiomatischen Behandlung zugängig machen wollen, suchen wir auf Bewegungsvorgänge an Punktsystem in Zeit und Raum zu reduzieren." (Hilbert 1913b, 1)

195 "Die Elektronentheorie würde daher von diesem Gesichtpunkt aus das Fundament der gesamten Physik sein." (Hilbert 1913b, 13)

196 "Unser nächstes Ziel ist, eine Erklärung der Elektrizitätsleitung in Metallen zu gewinnen. Zu diesem Zwecke machen wir uns von der Elektronen zunächst folgendes *der Gastheorie entnommene mechanische Bild*, das wir später durch ein elektrodynamisches ersetzen werden." (Hilbert 1913b, 14)

197 "Die wichtigsten Begriffe sind die der *Kraft* und der *Starrheit*. Die Elektronentheorie würde daher von diesem Gesichtspunkt aus das Fundament der gesamten Physik sein. Den Versuch ihres systematischen Aufbaues verschieben wir jedoch auf später; er hätte von der Bewegung eines, zweier Elektronen u.s.w. auszugehen, und ihm stellen sich bedeutende Schwierigkeiten in der Weg, da schon die entsprechenden Probleme der Newtonschen Mechanik für mehr als zwei Körper ungelöst sind." (Hilbert 1913a, 1913c)

attracted his attention. Thus, for instance, Hilbert explained that in the existing theory of electrical conductivity in metals, only the conduction of electricity—which itself depends on the motion of electrons—has been considered, while assuming that the electron satisfies both Newton's second law, $F = ma$, and the law of collision as a perfectly elastic spherical body (as in the theory of gases).[198] This approach assumes that the magnetic and electric interactions among electrons are described correctly enough in these mechanical terms as a first approximation.[199] However, if we wish to investigate with greater exactitude the motion of the electron, while at the same time preserving the basic conception of the kinetic theory based on colliding spheres, then we should also take into account the field surrounding the electron and the radiation that is produced with each collision. We are thus led to investigate the influence of the motion of the electron on the distribution of energy in the free ether, or in other words, to study the theory of radiation from the point of view of the mechanism of the motion of the electron. In his 1912 lectures on the theory of radiation, Hilbert had already considered this issue, but only from a "phenomenological" point of view. This time he referred to Lorentz's work as the most relevant one.[200] From Lorentz's theory, he said, we can obtain the electrical force induced on the ether by an electron moving on the x-axis of a given coordinate system.

Later on, Hilbert returned once again to the mathematical difficulties implied by the basic assumptions of the kinetic model. When speaking of clouds of electrons, he said, one assumes the axioms of the theory of gases and of the theory of radiation. The n-electron problem, he said, is even more difficult than that of the n-bodies, and in any case, we can only speak here of averages. Hilbert thus found it more convenient to open his course by describing the motion of a single electron, and, only later on, to deal with the problem of two electrons.

In discussing the behavior of the single electron, Hilbert referred to the possibility of an electromagnetic reduction of all physical phenomena, freely associating ideas developed earlier in works by Mie and by Max Abraham. The Maxwell equations and

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198 "In der bisherigen Theoire der Elektricitätsleitung in Metallen haben wir nur den Elektrizitätstransport, der durch die Bewegung der Elektronen selbst bedingt wird, in Betracht gezogen; unter der Annahme, dass d*ie Elektronen erstens dem Kraftgesetz $K = mb$ gehorchen und zweitens dasselbe Stossgesetz wie vollkommen harten elastischen Kugeln befolgen* (wie in der Gastheorie)." (Hilbert 1913b, 14)

199 "Auf die elektrischen und magnetische Wirkung der Elektronen aufeinander und auf die Atome sind wir dabei nicht genauer eingegangen, vielmehr haben wir angenommen, dass die gegenseitige Beeinflussung durch das Stossgesetz in erster Annäherung hinreichend genau dargestellt würde." (Hilbert 1913b, 14)

200 "Wollte man die Wirkung der Elektronenbewegung genauer verfolgen—jedoch immer noch unter Beibehaltung des der Gastheorie entlehnten Bildes stossender Kugeln—so müsste man *das umgebende Feld der Elektronen und die Strahlung* in Rechnung setzen, die sie bei jedem Zusammenstoß aussenden. Man wird daher naturgemäß darauf geführt, den Einfluss der Elektronenbewegung auf die Energieverteilung im freien aether zu untersuchen. Ich gehe daher dazu über, die *Strahlungstheorie,* die wir früher vom phänomenologischen Standpunkt aus kennen gelernt haben (summer semester, 1912), aus dem Mechanismus der Elektronenbewegung verständlich zu machen. Eine diesbezügliche Theorie hat *H. A. Lorentz* aufgestellt." (Hilbert 1913b, 14)

the concept of energy, Hilbert said, do not suffice to provide a foundation of electrodynamics; the concept of rigidity has to be added to them. Electricity has to be attached to a steady scaffold, and this scaffold is what we denote as an electron. The electron, he explained to his students, embodies the concept of a rigid connection of Hertz's mechanics. In principle at least it should be possible to derive all the forces of physics, and in particular the molecular forces, from these three ideas: Maxwell's equations, the concept of energy, and rigidity. However, he stressed, one phenomenon has so far evaded every attempt at an electrodynamic explanation: the phenomenon of gravitation.[201] Still, in spite of the mathematical and physical difficulties that he considered to be associated with a conception of nature based on the model underlying kinetic theory, Hilbert did not fully abandon at this stage the mechanistic approach as a possible one, and in fact he asserted that the latter is a necessary consequence of the principle of relativity.[202]

11.3 Axiomatization of Physics: 1913

In 1913 Hilbert gave a course very similar to the one taught back in 1905, and bearing the same name: "Elements and Principles of Mathematics."[203] The opening page of the manuscript mentions three main parts that the lectures intended to cover:

A. Axiomatic Method.
B. The Problem of the Quadrature of the Circle.
C. Mathematical Logic.

In the actual manuscript, however, one finds only two pages dealing with the problem of the quadrature of the circle. Hilbert explained that, for lack of time, this section would be omitted in the course. Only a short sketch appears, indicating the stages involved in dealing with the problem. The third part of the course, "*Das mathematisch Denken und die Logik*," discussed various issues such as the paradoxes of set theory, false and deceptive reasoning, propositional calculus (*Logikkalkül*), the concept of number and its axioms, and impossibility proofs. The details of the contents

201 "Auf die Maxwellschen Gleichungen und den Energiebegriff allein kann man die Elektrodynamik nicht gründen. Es muss noch der Begriff der *Starrhei*t hinzukommen; die Elektrizität muss an ein festes Gerüst angeheftet sein. Dies Gerüst bezeichnen wir als Elektron. In ihm ist der Begriff der starrer Verbindung der Hertzschen Mechanik verwirklicht. Aus den Maxwellschen Gleichungen, dem Energiebegriff und dem Starrheitsbegriff lassen sich, im Prinzip wenigstens, die vollständigen Sätze der Mechanik entnehmen, auf sie lassen sich die gesamten Kräfte der Physik, im Besonderen die Molekularkräfte zurückzuführen. Nur die Gravitation hat sich bisher dem Versuch einer elektrodynamischen Erklärung widersetzt." (Hilbert 1913b, 61–62)

202 "Es sind somit die zum Aufbau der Physik unentbehrlichen starren Körper nur in den kleinsten Teilen möglich; man könnte sagen: das Relativitätsprinzip ergibt also als notwendige Folge die Atomistik." (Hilbert 1913b, 65)

203 The lecture notes of this course, (Hilbert 1913c), are not found in the Göttingen collections. Peter Damerow kindly allowed me to consult the copy of the handwritten notes in his possession. The notes do not specify who wrote them. In *Nachlass* David Hilbert, (Cod. Ms. D. Hilbert, 520, 5), Hilbert wrote that notes of the course were taken by Bernhard Baule.

of this last part, though interesting, are beyond our present concern here. In the first part Hilbert discussed in detail, like in 1905, the axiomatization of several physical theories.

Like in 1905, Hilbert divided his discussion of the axiomatic method into three parts: the axioms of algebra, the axioms of geometry, and the axioms of physics. In his first lecture Hilbert repeated the definition of the axiomatic method:

> The axiomatic method consists in choosing a domain and putting certain facts on top of it; the proof of these facts does not occupy us anymore. The classical example of this is provided by geometry.[204]

Hilbert also repeated the major questions that should be addressed when studying a given system of axioms for a determined domain: Are the axioms consistent? Are they mutually independent? Are they complete?[205] The axiomatic method, Hilbert declared, is not a new one; rather it is deeply ingrained in the human way of thinking.[206]

Hilbert's treatment of the axioms of physical theories repeats much of what he presented in 1905 (the axioms of mechanics, the principle of conservation of energy, thermodynamics, calculus of probabilities, and psychophysics), but at the same time it contains some new sections: one on the axioms of radiation theory, containing Hilbert's recently published ideas on this domain, and one on space and time, containing an exposition of relativity. I comment first on one point of special interest appearing in the section on mechanics.

In his 1905 course Hilbert had considered the possibility of introducing alternative systems of mechanics defined by alternative sets of axioms. As already said, one of the intended aims of Hilbert's axiomatic analysis of physical theories was to allow for changes in the existing body of certain theories in the eventuality of new empirical discoveries that contradict the former. But if back in 1905, Hilbert saw the possibility of alternative systems of mechanics more as a mathematical exercise than as a physically interesting task, obviously the situation was considerably different in 1913. This time Hilbert seriously discussed this possibility in the framework of his presentation of the axioms of Newtonian mechanics. As in geometry, Hilbert said, one could imagine for mechanics a set of premises different from the usual ones and, from a logical point of view, one could think of developing a "non-Newtonian Mechanics."[207] More specifically, he used this point of view to stress the similarities between mechanics and electrodynamics. He had already done something similar in 1905, but now his remarks had a much more immediate significance. I quote them here in some extent:

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204 "Die axiomatische Methode besteht darin, daß man ein Gebiet herausgreift und bestimmte Tatsachen an die Spitze stellt u. nun den Beweis dieser Tatsachen sich nicht weiter besorgt. Das Musterbeispiel hierfür ist die Geometrie." (Hilbert 1913c, 1)

205 Again, Hilbert is not referring here to the model-theoretical notion of completeness. See § 2.1.

206 "Die axiomatische Methode ist nicht neu, sondern in der menschlichen Denkweise tief begründet." (Hilbert 1913c, 5)

One can now drop or partially modify particular axioms; one would then be practicing a non-Newtonian, non-Galileian, or non-Lagrangian mechanics.

This has a very special significance: electrodynamics has compelled us to adopt the view that our mechanics is only a limiting-case of a more general one. Should anyone in the past have thought by chance of defining the kinetic energy as:

$$T = \mu \frac{1 - v^2}{v} \log \frac{1 + v}{1 - v},$$

he would have then obtained the [equation of] motion of the electron, where μ is constant and depends on the electron's mass. If one ascribes to all of them [i.e., to the electrons] kinetic energy, then one obtains the theory of the electron, i.e., an essential part of electrodynamics. One can then formulate the Newtonian formula:

$$ma = F$$

But now the mass depends essentially on the velocity and it is therefore no more a physical constant. In the limit case, when the velocity is very small, we return to the classical physics....

Lagrange's equations show how a point moves when the conditions and the forces are known. How these forces are created and what is their nature, however, this is a question which is not addressed.

Boltzmann attempted to build the whole of physics starting from the forces; he investigated them, and formulated axioms. His idea was to reduce everything to the mere existence of central forces of repulsion or of attraction. According to Boltzmann there are only mass-points, mutually acting on each other, either attracting or repelling, over the straight line connecting them. Hertz was of precisely of the opposite opinion. For him there exist no forces at all; rigid bonds exist among the individual mass-points. Neither of these two conceptions has taken root, and this is for the simple reason that electrodynamics dominates all.

The foundations of mechanics, and especially its goal, are not yet well established. Therefore it has no definitive value to construct and develop these foundations in detail, as has been done for the foundations of geometry. Nevertheless, this kind of foundational research has its value, if only because it is mathematically very interesting and of an inestimably high value.[208]

This passage illuminates Hilbert's conceptions by 1913. At the basis of his approach to physics stands, as always, the axiomatic method as the most appropriate way to examine the logical structure of a theory and to decide what are the individual assumptions from which all the main laws of the theory can be deduced. This deduction, however, as in the case of Lagrange's equation, is independent of questions concerning the ultimate nature of physical phenomena. Hilbert mentions again the

207 "Logisch wäre es natürlich auch möglich andere Def. zu Grunde zu liegen und so eine 'Nicht-Newtonsche Mechanik' zu begründen." An elaborate formulation of a non-Newtonian mechanics had been advanced in 1909 by Gilbert N. Lewis (1875–1946) and Richard C. Tolman (1881–1948), in the framework of an attempt to develop relativistic mechanics independently of electromagnetic theory (Lewis and Tolman 1909). Hilbert did not give here a direct reference to that work but it is likely that he was aware of it, perhaps through the mediation of one of his younger colleagues. (Hilbert 1913c, 91)

mechanistic approach promoted by Hertz and Boltzmann, yet he admits explicitly, perhaps for the first time, that it is electromagnetism that pervades all physical phenomena. Finally, the introduction of Lagrangian functions from which laws of motion may be derived that are more general than the usual ones of classical mechanics was an idea that in the past might have been considered only as a pure mathematical exercise; now—Hilbert cared to stress—it has become a central issue in mechanics, given the latest advances in electrodynamics.

The last section of Hilbert's discussion of the axiomatization of physics addressed the issue of space and time, and in fact it was a discussion of the principle of relativity.[209] What Hilbert did in this section provides the most detailed evidence of his conceptions concerning the principle of relativity, mechanics and electrodynamics before his 1915 paper on the foundations of physics. His presentation did not really incorporate any major innovations, yet Hilbert attempted to make the "new mechanics" appear as organically integrated into the general picture of physics that he was so eager to put forward at every occasion, and in which all physical theories appear as in

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208 "Man kann nun gewisse Teile der Axiome fallen lassen oder modifizieren; dann würde man also "*Nicht-Newtonsche*," od. "*Nicht-Galileische*", od. "*Nicht-Lagrangesche*" *Mechanik* treiben.

Das hat ganz besondere Bedeutung: Durch die Elektrodynamik sind wir zu der Auffassung gezwungen worden, daß unsere Mechanik nur eine Grenzfall einer viel allgemeineren Mechanik ist. Wäre jemand früher zufällig darauf gekommen die kinetisch Energie zu definieren als:

$$T = \mu \frac{1-v^2}{v} \log \frac{1+v}{1-v},$$

so hatte er die Bewegung eines Elektrons, wo μ eine Constante der elektr. Masse ist. Spricht man ihnen allen kinetisch Energie zu, dann hat man die Elektronentheorie d.h. einen wesentlichen Teil der Elektrodynamik. Dann kann man die Newtonschen Gleichungen aufstellen:

$$mb = K$$

Nun hängt aber die Masse ganz wesentlich von der Geschwindigkeit ab und ist keine physikalische Constante mehr. Im Grenzfall, daß die Geschwindigkeit sehr klein ist, kommt man zu der alten Mechanik zurück. (Cf. H. Stark "Experimentelle Elektrizitätslehre," S. 630).

Die Lagrangesche Gleichungen geben die Antwort wie sich ein Punkt bewegt, wenn man die Bedingungen kennt und die Kräfte. Wie diese Kräfte aber beschaffen sind und auf die Natur die Kräfte selbst gehen sie nicht ein.

Boltzmann hat versucht die Physik aufzubauen indem er von der Kräften ausging; er untersuchte diese, stellte Axiome auf u. seine Idee war, alles auf das bloße Vorhandensein von Kräften, die zentral abstoßend oder anziehend wirken sollten, zurückzuführen. Nach Boltzman gibt es nur Massenpunkte die zentral gradlinig auf einander anzieh. od. abstoßend wirkend.

Hertz hat gerade den entgegengesetzten Standpunkt. Für ihn gibt es überhaupt keine Kräfte; starre Verbindungen sind zwischen den einzelnen Massenpunkten.

Beide Auffassungen haben sich nicht eingebürgert, schon aus dem einfachen Grunde, weil die Elektrodynamik alles beherrscht.

Die Grundlagen der Mechanik und besonders die Ziele stehen noch nicht fest, so daß es auch noch nicht definitiven Wert hat die Grundlagen in den einzelnen Details so auf- und ausbauen wie die *Grundlagen der Geometrie*. Dennoch behalten die axiomatischen Untersuchungen ihren Wert, schon deshalb, weil sie mathematisch sehr interessant und von unschätzbar hohen Werte sind." (Hilbert 1913c, 105–108) principle axiomatized (or at least axiomatizable). Back in 1905, Hilbert had suggested, among the possible ways to axiomatize classical dynamics, defining space axiomatically by means of the already established axioms of geometry, and then expanding this definition with some additional axioms that define time. He suggested that something similar should be done now for the new conception of space and time, but that the axioms defining time would clearly have to change. He thus assumed the axioms of Euclidean geometry and proceeded to redefine the concept of time using a "light pendulum." Hilbert then connected the axiomatically constructed theory with the additional empirical consideration it was meant to account for, namely, the outcome of the Michelson-Morley experiment when the values $\vartheta = 0, \pi/2, \pi,$ are measured in the formula describing the velocity of the ray-light γ_ϑ in the pendulum:

$$\gamma_\vartheta = \sqrt{\frac{\xi^2 + \eta^2}{t^2}} = \sqrt{\cos^2\vartheta + \sin^2\vartheta - 2v\cos\vartheta + v^2} = [1 - 2v\cos\vartheta + v^2]^{\frac{1}{2}}.$$

Hilbert stressed the similarities between the situation in this case, and in the case in geometry, when one invokes Gauss's measurement of angles in the mountain triangle for determining the validity of Euclidean geometry in reality. In his earlier lectures, Hilbert had repeatedly mentioned this experiment as embodying the empirical side of geometry. The early development of relativity theory had brought about a deep change in the conception of time, but Hilbert of course could not imagine that the really significant change was still ahead. To the empirical discovery that triggered the reformulation of the concept of time, Hilbert opposed the unchanged conception of space instantiated in Gauss's experiment. He thus said:

> Michelson set out to test the correctness of these relations, which were obtained working within the old conception of time and space. The [outcome of his] great experiment is that these formulas do not work, whereas Gauss had experimentally confirmed (i.e., by measuring the Hoher Hagen, the Brocken, and the Inselsberg) that in Euclidean geometry, the sum of the angles of a triangle equals two right angles.[210]

Although he spoke here of an old conception of space and time, Hilbert was referring to a change that actually affected only time. From the negative result of Michelson's experiment, one could conclude that the assumption implied by the old conception—according to which, the velocity of light measured in a moving system has different values in different directions—leads to contradiction. The opposite

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209 The following bibliographical list appears in the first page of this section (Hilbert 1913c, 119):
M. Laue Das Relativitätsprinzip 205 S.
M. Planck 8 Vorlesungen über theoretische Physik 8. Vorlesung S. 110–127
A. Brill Das Relativitätsprinzip: ein Einführung in die Theorie 28 S.
H. Minkowski Raum und Zeit XIV Seiten
Beyond this list, together with the manuscript of the course, in the same binding, we find some additions, namely, (1) a manuscript version of Minkowski's famous work (83 pages in the same handwriting as the course itself), (2) the usual preface of A. Gutzmer, appearing as an appendix, and (3) two pages containing a passage copied form Planck's *Vorlesungen*.

assumption was thus adopted, namely that the velocity of light behaves with respect to moving systems as it had been already postulated for stationary ones. Hilbert expresses this as a further axiom:

> Also in a moving system, the velocity of light is identical in all directions, and in fact, identical to that in a stationary system. The moving system has no priority over the first one.[211]

Now the question naturally arises: what is then the true relation between time as measured in the stationary system and in the moving one, t and τ respectively? Hilbert answered this question by introducing the Lorentz transformations, which he discussed in some detail, including the limiting properties of the velocity of light,[212] and the relations with a third system, moving with yet a different uniform velocity.

11.4 Electromagnetic Oscillations: 1913–1914

In the winter semester of 1913–1914, Hilbert lectured on electromagnetic oscillations. As he had done many times in the past, Hilbert opened by referring to the example of geometry as a model of an experimental science that has been transformed into a purely mathematical, and therefore a "theoretical science," thanks to our thorough knowledge of it. Foreshadowing the wording he would use later in his axiomatic formulation of the general theory of relativity, 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 the relativistic mechanics, or four-dimensional electrodynamics, which belong to mathematics, as I have been already convinced for a long time.[213]

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210 "Diese aus der alten Auffassung von Raum und Zeit entspringende Beziehung hat Michelson auf ihre Richtigkeit geprüft. Das große Experiment ist nun das, daß diese Formel nicht stimmt, während bei der Euklidischen Geometrie Gauss durch die bestimmte Messung Hoher Hagen, Brocken, Inselsberg bestätigte, daß die Winkelsumme im Dreieck 2 Rechte ist."
On p. 128 Hilbert explained the details of Michelson's calculations, namely, the comparison of velocities at different angles via the formula:

$$\frac{1}{\gamma_{\vartheta}}+\frac{1}{\gamma_{\vartheta+\pi}} = (1-2v\cos\vartheta+\vartheta^2)^{-\frac{1}{2}}+(1+2v\cos\vartheta+\vartheta^2)^{-\frac{1}{2}} = 2+v^2(3\cos^2\vartheta-1)+\ldots$$

where the remaining terms are of higher orders. (Hilbert 1913c, 124)

211 "Es zeigt sich also, daß unsere Folgerung der alten Auffassung, daß die Lichtgeschwindigkeit im bewegtem System nach verschiedenen Richtungen verschieden ist, auf Widerspruch führt. Wir nehmen deshalb an: *Auch im bewegtem System ist die Lichtgeschwindigkeit nach allem Seiten gleich groß, und zwar gleich der im ruhenden. Das bewegte System hat vor dem alten nicht voraus*." (Hilbert 1913c, 128–129)

212 "Eine größen Geschwindigkeit als die Lichtgeschwindigkeit kann nicht vorkommen." (Hilbert 1913c, 132)

Hilbert's intensive involvement with various physical disciplines over the last years had only helped to strengthen an empirical approach to geometry rather than promoting some kind of formalist views. But as for his conceptions about physics itself, by the end of 1913 his new understanding of the foundational role of electrodynamics was becoming only more strongly established in his mind, at the expense of his old mechanistic conceptions. The manuscript of this course contains the first documented instance where Hilbert seems to allude to Mie's ideas and, indeed, it is among the earliest explicit instances of a more decided adoption of electrodynamics, rather than mechanics, as the possible foundation for all physical theories. At the same time, the whole picture of mathematics was becoming ever more hierarchical and organized into an organic, comprehensive edifice, of which theoretical physics is also an essential part. Hilbert thus stated:

> In the meantime it looks as if, finally, theoretical physics completely arises from electrodynamics, to the extent that every individual question must be solved, in the last instance, by appealing to electrodynamics. According to what method each mathematical discipline more predominantly uses, one could divide mathematics (concerning contents rather than form) into one-dimensional mathematics, i.e., arithmetic; then function theory, which essentially limits itself to two dimensions; then geometry, and finally four-dimensional mechanics.[214]

In the course itself, however, Hilbert did not actually address in any concrete way the kind of electromagnetic reduction suggested in its introduction, but rather, it continued, to a certain extent, his previous course on electron theory. In the first part Hilbert dealt with the theory of dispersion of electrons, seen as a means to address the n-electron problem. Hilbert explained that the role of this problem in the theory of relativity is similar to that of the *n*-body problem in mechanics. In the previous course he had shown that the search for the equations of motion for a system of electrons leads to a very complicated system of integro-differential equations. A possibly fruitful way to address this complicated problem would be to integrate a certain simplified version of these equations and then work on generalizing the solutions thus obtained. In classical mechanics the parallel simplification of the n-body problem is embodied

213 "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 ist, von der Mathematik assimiliert zu werden, und dass auch die theoretische Physik jetzt im Begriff steht, sich diesen Ruhm zu erwerben. In erster Linie gilt dies von der Relativitätsmechanik oder vierdimensionalen Elektrodynamik, von deren Zuhörigkeit zur Mathematik ich seit langem überzeugt bin." (Hilbert 1913–1914, 1)

214 "Es scheint indessen, als ob die theoretische Physik schliesslich ganz und gar in der Elektrodynamik aufgeht, insofern jede einzelne 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 entgeltlich 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." (Hilbert 1913–1914, 1)

in the theory of small oscillations, based on the idea that bodies cannot really attain a state of complete rest. This idea offers a good example of a possible way forward in electrodynamics, and Hilbert explained that, indeed, the elementary theory of dispersion was meant as the implementation of that idea in this field. Thus, this first part of the course would deal with it.[215]

In the second part of the course Hilbert dealt with the magnetized electron. He did not fail to notice the difficulties currently affecting his reductionist program. At the same time he stressed the value of an axiomatic way of thinking in dealing with such difficulties. He thus said:

> We are really still very distant from a full realization of our leading idea of reducing all physical phenomena to the *n*-electron problem. Instead of a mathematical foundation based on the equations of motion of the electrons, we still need to adopt partly arbitrary assumptions, partly temporary hypothesis, that perhaps one day in the future might be confirmed. We also must adopt, however, certain very fundamental assumptions that we later need to modify. This inconvenience will remain insurmountable for a long time. What sets our presentation apart from that of others, however, is the insistence in making truly explicit all its assumptions and never mixing the latter with the conclusions that follow from them.[216]

Hilbert did not specify what assumptions he meant to include under each of the three kinds mentioned above.Yet it would seem quite plausible to infer that the "very fundamental assumptions," that must be later modified, referred in some way or another to physical, rather than purely mathematical, assumptions, and more specifically, to the atomistic hypothesis, on which much of his own physical conceptions had hitherto been based. An axiomatic analysis of the kind he deemed necessary for physical theories could indeed compel him to modify even his most fundamental assumptions if necessary. The leading principle should remain, in any case, to separate as clearly as possible the assumptions of any particular theory from the theorems that can be derived in it. Thus, the above quotation suggests that if by this time Hil-

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215 "So wenig man schon mit dem n-Körperproblem arbeiten kann, so wäre es noch fruchtloser, auf die Behandlung des *n*-Elektronenproblemes einzugehen. Es handelt sich vielmehr für uns darum, das n-Elektronenproblem zu verstümmeln, die vereinfachte Gleichungen zu integrieren und von ihren Lösungen durch Korrekturen zu allgemeineren Lösungen aufzusteigen. Die gewöhnliche Mechanik liefert uns hierfür ein ausgezeichnetes Vorbild in der Theorie der kleinen Schwingungen; die Vereinfachung des n-Körperproblems besteht dabei darin, dass die Körper sich nur wenig aus festen Ruhelagen entfernen dürfen. In der Elektrodynamik gibt es ein entsprechendes Problem, und zwar würde ich die Theorie der Dispersion als das dem Problem der kleinen Schwingungen analoge Problem ansprechen." (Hilbert 1913–1914, 2)

216 "Von der Verwirklichung unseres leitenden Gedankens, alle physikalischen Vorgänge auf das n-Elektronenproblem zurückzuführen, sind wir freilich noch sehr weit entfernt. An Stelle einer mathematischen Begründung aus den Bewegungsgleichungen der Elektronen müssen vielmehr noch teils willkürliche Annahmen treten, teils vorläufige Hypothesen, die später einmal begründet werden dürften, teils aber auch Annahmen ganz prinzipieller Natur, die sicher später modifiziert werden müssen. Dieser Übelstand wird noch auf lange Zeit hinaus unvermeidlich sein. Unsere Darstellung soll sich aber gerade dadurch auszeichnen, dass die wirklich nötigen Annahmen alle ausdrücklich aufgeführt und nicht mit ihren Folgerungen vermischt werden." (Hilbert 1913–1914, 87–88)

bert had not yet decided to abandon his commitment to the mechanistic reductionism and its concomitant atomistic view, he was certainly preparing the way for that possibility, should the axiomatic analysis convince him of its necessity.

In the subsequent lectures in this course, Hilbert referred more clearly to ideas of the kind developed in Mie's theory, without however explicitly mentioning his name (at least according to the record of the manuscript). Outside ponderable bodies, which are composed of molecules, Hilbert explained, the Maxwell equations are valid. He formulated them as follows:

$$\mathrm{curl}M - \frac{\partial e}{\partial t} = \rho\varpi;\ \mathrm{div}e = \rho$$
$$\mathrm{curl}e + \frac{\partial M}{\partial t} = 0;\ \mathrm{div}\ M = 0$$

This is also how the equations are formulated in Born's article of 1910, the text on which Hilbert was basing this presentation. But Hilbert asserted here for the first time that the equations are valid also inside the body. And he added:

> Inside the body, however, the vectors e and M are very different, since the energy density is always different from zero inside the sphere of the electron, and these spheres undergo swift oscillations. It would not help us to know the exact value of the vector fields inside the bodies, since we can only observe mean values.[217]

Hilbert thus simply stated that the Maxwell equations inside the body should be rewritten as:

$$\mathrm{curl}\overline{M}\left(-\frac{\partial \bar{e}}{\partial t}\right) = \bar{\rho}\bar{\varpi};\ \mathrm{div}e = \rho$$
$$\mathrm{curl}\bar{e} + \frac{\partial \overline{M}}{\partial t} = 0;\ \mathrm{div}\ M = 0$$

where overstrike variables indicate an average value over a space region.

Hilbert went on to discuss separately and in detail specific properties of the conduction-, polarization- and magnetization-electrons. He mentioned Lorentz as the source for the assumption that these three kinds of electrons exist. This assumption, he said, is an "assumption of principle" that should rather be substituted by a less arbitrary one.[218] By saying this, he was thus not only abiding by his self-imposed rules that every particular assumption must be explicitly formulated, but he was also implicitly stressing once again that physical assumptions about the structure of matter

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217 "Diese Gleichungen gelten sowohl innerhalb wie *ausserhalb des Körpers*. Im innern des Körpers werden aber die Vektoren E und M sich räumlich und zeitlich sehr stark ändern, da die Dichte der Elektrizität immer nur innerhalb der Elektronenkugeln von Null verschieden ist und diese Kugeln rasche Schwingungen ausführen. Es würde uns auch nicht helfen, wenn wir innerhalb des Körpers die genauen Werte der Feldvektoren kennen würden; denn zur Beobachtung gelangen doch nur Mittelwerte." (Hilbert 1913–1914, 89)

218 "Wir machen nur eine reihe von Annahmen, die zu den prinzipiellen gehören und später wohl durch weniger willkürlich scheinende ersetzt werden können." (Hilbert 1913–1914, 90)

are of a different kind than merely mathematical axioms, that they should be avoided whenever possible, and that they should eventually be suppressed altogether.

In a later section of his lecture, dealing with diffuse radiation and molecular forces, Hilbert addressed the problem of gravitation from an interesting point of view that, once again, would seem to allude to the themes discussed by Mie, without however explicitly mentioning his name. Hilbert explained that the problem that had originally motivated the consideration of what he called “diffuse electron oscillations” (a term he did not explain) was the attempt to account for gravitation. In fact, he added, it would be highly desirable—from the point of view pursued in the course—to explain gravitation based on the assumption of the electromagnetic field and the Maxwell equations, together with some auxiliary hypotheses, such as the existence of rigid bodies. The idea of explaining gravitation in terms of “diffuse radiation of a given wavelength” was, according to Hilbert, closely related to an older idea first raised by Georges-Louis Le Sage (1724–1803). The latter was based on the assumption that a great number of particles move in space with a very high speed, and that their impact with ponderable bodies produces the phenomenon of weight.[219] However, Hilbert explained, more recent research has shown that an explanation of gravitation along these lines is impossible.[220] Hilbert was referring to an article published by Lorentz in 1900, showing that no force of the form $1/r^2$ is created by “diffuse radiation” between two electrical charges, if the distance between them is large enough when compared to the wavelength of the radiation in question (Lorentz 1900).[221]

And yet in 1912, Erwin Madelung had readopted Lorentz’s ideas in order to calculate the force produced by radiation over short distances and, eventually, to account for the molecular forces in terms of radiation phenomena (Madelung 1912). Madelung taught physics at that time in Göttingen and, as we saw, he had attended Hilbert’s 1912 advanced seminar on kinetic theory. Hilbert considered that the mathematical results obtained by him were very interesting, even though their consequences could not be completely confirmed empirically. Starting from the Maxwell equations and some simple, additional hypotheses, Madelung determined the value of

219 LeSage’s corpuscular theory of gravitation, originally formulated in 1784, was reconsidered in the late nineteenth century by J.J. Thomson. On the Le Sage-Thomson theory see (North 1965, 38–40; Roseaveare 1982, 108–112). For more recent discussions, cf. also (Edwards 2002).

220 “Das Problem, das zunächst die Betrachtung diffuser Elektronenschwingungen anregte, war die Erklärung der Gravitation. In der Tat muss es ja nach unserem leitenden Gesichtspunkte höchst wünschenswert erscheinen, die Gravitation allein aus der Annahme eines elektromagnetischen Feldes sowie er Maxwellschen Gleichungen und gewisser einfacher Zusatzhypothesen, wie z.B. die Existenz starrer Körper eine ist, zu erklären. Der Gedanke, den Grund für die Erscheinung der Gravitation in einer diffusen Strahlung von gewisser Wellelänge zu suchen, ähnelt entfernt einer Theorie von Le Sage, nach der unzählige kleine Partikel sich mit grosser Geschwindigkeit im Raume bewegen sollen und durch ihren Anprall gegen die ponderablen Körper die Schwere hervorbringen. Wie in dieser theorie ein Druck durch bewegte Partikel auf die Körper ausgeübt wird, hat man jetzt den modernen Versuch unternommen, den Strahlungsdruck für die Erklärung der Gravitation dienstbar zu machen.” (Hilbert 1913–1914, 107–108)

221 On this theory, see (McCormmach 1970, 476–477).

an attraction force that alternatively attains positive and negative values as a function of the distance.[222]

As a second application of diffuse radiation, Hilbert mentioned the possibility of deriving Planck's radiation formula without recourse to quantum theory. Such a derivation, he indicated, could be found in two recent articles of Einstein, one of them (1910) with Ludwig Hopf (1884–1939) and the second one (1913) with Otto Stern (1888–1969).

Hilbert's last two courses on physics, before he began developing his unified theory and became involved with general relativity, were taught in the summer semester of 1914 (statistical mechanics) and the following winter semester, 1914–1915 (lectures on the structure of matter).[223]

12. BROADENING PHYSICAL HORIZONS - CONCLUDING REMARKS

The present chapter has described Hilbert's intense and wide-ranging involvement with physical issues between 1910 and 1914. His activities comprised both published work and courses and seminars. In the published works, particular stress was laid on considerably detailed axiomatic analysis of theories, together with the application of the techniques developed by Hilbert himself in the theory of linear integral equations. The courses and seminars, however, show very clearly that Hilbert was not just looking for visible venues in which to display the applicability of these mathematical tools. Rather, they render evident the breadth and depth of his understanding of, and interest in, the actual physical problems involved.

Understanding the mixture of these two components—the mathematical and the physical—helps us to understand how the passage from mechanical to electromagnetic reductionism was also the basis of Hilbert's overall approach to physics, and particularly of his fundamental interest in the question of the structure of matter. In spite of the technical possibilities offered by the theory of integral equations in the way to solving specific, open problems in particular theories, Hilbert continued to be concerned about the possible justification of introducing probabilistic methods in physical theories at large. If the phenomenological treatment of theories was only a preliminary stage on the way to a full understanding of physical processes, it turned out that also those treatments based on the atomistic hypothesis, even where they helped reach solutions to individual problems, raised serious foundational questions that required further investigation into the theory of matter as such. Such consider-

222 "Die mathematischen Ergebnisse dieser Arbeit sind von grossem Interesse, auch wenn sich die Folgerungen nicht sämtlich bewähren sollten. Es ergibt sich nämlich allein aus den Maxwellschen Gleichungen und einfachen Zusatzhypothese eine ganz bestimmte Attraktionskraft, die als Funktion der Entfernung periodisch positiv und negativ wird." (Hilbert 1913–1914, 108)

223 The winter semester, 1914–1915 course is registered in the printed version of the *Verzeichnis der Vorlesungen an der Georg-August-Universität zu Göttingen* (1914–1915, on p. 17) but no notes seem to be extant.

ations were no doubt a main cause behind Hilbert's gradual abandonment of mechanical reductionism as a basic foundational assumption.

This background should suffice to show the extent to which his unified theory of 1915 and the concomitant incursion into general theory of relativity were organically connected to the life-long evolution of his scientific horizon, and were thus anything but isolated events. In addition to this background, there are two main domains of ideas that constitute the main pillars of Hilbert's theory and the immediate catalysts for its formulation. These are the electromagnetic theory of matter developed by Gustav Mie starting in 1912, on the one hand, and the efforts of Albert Einstein to generalize the principle of relativity, starting roughly at the same time.

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