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David Hilbert and the Axiomatization of Physics (1894–1905)

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Contents

1. Introduction

In 1900, at a time when his international prominence as a leading mathematician was just becoming firmly established, DAVID HILBERT (1862–1943) delivered one of the central invited lectures at the Second International Congress of Mathematicians, held in Paris. The lecture bore the title "Mathematical Problems". At this very significant opportunity HILBERT attempted to "lift the veil" and peer into the development of mathematics of the century that was about to begin (HILBERT 1902, 438). He chose to present a list of twenty-three problems that in his opinion would and should occupy the efforts of mathematicians in the years to come. This famous list has ever since been an object of mathematical and historical interest. Mathematicians of all specialties and of all countries have taken up its challenges. Solving any item on the list came to be considered a significant mathematical achievement.

The sixth problem of the list deals with the axiomatization of physics. It was suggested by his own recent research on the foundations of geometry; HILBERT proposed "to treat in the same manner [as geometry], by means of axioms, those physical sciences in which mathematics plays an important part (HILBERT 1902, 454)."

This problem differs in an essential way from most others in the list, and its inclusion raises many intriguing questions. In the first place, as formulated by HILBERT, it is more of a general task than a specific mathematical problem. It is far from evident under what conditions this problem may be considered to have been solved. In fact, from reports that have occasionally been written about the current state of research on the twenty-three problems, not only is it hard to decide to what extent this problem has actually been solved, but moreover, one gets the impression that, from among all the problems in the list, this one has received the least attention from mathematicians.[1]

From the point of view of HILBERT's own mathematical work, additional historical questions may be asked. Among them are the following: Why was this problem so central for HILBERT that he included it in the list? What contact, if any, had he himself had with this problem during his mathematical career? What was the actual connection between his work on the foundations of geometry and this problem? What efforts, if any, did HILBERT himself direct after 1900 to its solution?

These questions are particularly pressing because of their bearing on the often accepted identification between HILBERT and the formalist approach to the foundations of mathematics. HILBERT's main achievement concerning the foundations of geometry was — according to a widely-held view — to present this mathematical domain as an axiomatic system devoid of any specific intuitive meaning, in which the central concepts (points, lines, planes) could well be replaced by tables, chairs and beer-mugs, on condition that the latter are postulated to satisfy the relations established by the axioms. The whole system of geometry should remain unaffected by such a change. Therefore, it is often said, HILBERT promoted a view of mathematics as an empty formal game, in which inference rules are prescribed in advance, and deductions are drawn, following those rules, from arbitrarily given systems of postulates.[2] If this was

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[1] See, e.g., WIGHTMAN 1976, GNEDENKO 1979.

[2] Such a view has been put forward by, e.g., the French mathematician JEAN DIEUDONNÉ (1906–1992). In a widely read expository article, DIEUDONNÉ explained the essence of HILBERT's mathematical conceptions by analogy with a game of chess. After explaining that in the latter one does not speak about truths but rather about following correctly a set of stipulated rules, he added (DIEUDONNÉ 1962, 551. Italics in the original) : "Transposons cela en mathématiques, et nous aurons la conception de HILBERT: les mathématiques deviennent un *jeu*, dont les *pièces* sont des signes graphiques se distinguant les uns des autres par leur forme."

indeed HILBERT's view of mathematics, then in what sense could he have intended to apply such a view to physics, as stated in the sixth problem? By asking what HILBERT was aiming at when addressing the question of the axiomatization of physics, we are thus asking what role HILBERT ascribed to axiomatization in mathematics and in science in general (especially physics), and how he conceived the relation between mathematics and physics. Answering this question will help to clarify many aspects of HILBERT's overall conception of mathematics.

The first part of the present article describes the roots of HILBERT's early conception of axiomatics, putting special emphasis on the analogies he drew between geometry and the physical sciences. In this light, HILBERT's axiomatic approach is presented as an endeavor with little connection to the view of mathematical theories as empty formal games, devoid of concrete content — a view that became dominant in wide mathematical circles after the 1930s. Rather, it appears as the opposite: as a method for enhancing our understanding of the mathematical content of theories and for excluding possible contradictions or superfluous assertions that may appear in them. This understanding of HILBERT's axiomatics also explains the place of the sixth problem in his mathematical world. The second part of the article addresses in a more detailed manner the question of how HILBERT conceived the specific application of the axiomatic approach to particular branches of science, and what image of science emerges from that approach. Using the manuscript of a course taught by HILBERT in Göttingen in 1905, I discuss HILBERT's axiomatic treatment of various scientific disciplines and his conception of the conceptual and methodological connections among the latter. This account is also intended to open the way to a broader understanding of HILBERT's later works on physics and, in particular, to a detailed analysis — which I plan to undertake in the near future — of the path that led HILBERT to his research on general relativity.

2. Hilbert as Student and Teacher

Physics was not a side issue that occupied HILBERT's thought only sporadically. At least since the mid-1890s HILBERT had been interested in current progress in physics, and this interest gradually became a constitutive feature of his overall conception of mathematics. In order to describe this properly, one has to consider HILBERT's biography. HILBERT's studies and early mathematical career between 1880 and 1895 took place in his native city of Königsberg, except for a short trip in 1885 — after finishing his dissertation — to FELIX KLEIN (1849–1925) in Leipzig and to CHARLES HERMITE (1822–1901) in Paris. Königsberg had a small university, with a very respectable tradition of research and education in mathematics and physics that had been established during the first half of the nineteenth century by CARL GUSTAV JACOBI (1804–1851) and

FRANZ ERNST NEUMANN (1798–1895).[3] During his first years as a student, HILBERT was able to attend the lectures of the distinguished mathematician HEINRICH WEBER (1842–1913),[4] whose interests covered an astonishing variety of issues ranging from the theory of polynomial equations, to elliptic functions, to mathematical physics. The congenial environment WEBER found in Königsberg for pursuing his manifold mathematical interests was the one within which HILBERT's early mathematical outlook was formed. However, WEBER never developed a circle of students around him, and it is unlikely that — prior to WEBER's departure for Zürich in 1883 — the young HILBERT benefited from direct contact with him or his current research interests.

HILBERT's doctoral adviser was FERDINAND LINDEMANN (1852–1939), a former student of FELIX KLEIN. LINDEMANN's mathematical achievements — he is remembered today mostly for his proof of the transcendence of π — were not outstanding, but he certainly exerted an important influence on HILBERT's mathematical formation. But perhaps the foremost influence on shaping HILBERT's intellectual horizon in Königsberg came from his exceptional relationship with two other young mathematicians: ADOLF HURWITZ (1859–1919), first HILBERT's teacher and later his colleague, and HERMANN MINKOWSKI (1864–1909). Before accepting in 1884 a new chair especially created for him in Königsberg, HURWITZ had studied first with KLEIN in Leipzig and then in Berlin, and had later habilitated in Göttingen in 1882. HURWITZ was thus well aware of the kind of mathematical interests and techniques dominating current research in each of these important centers. HURWITZ taught for eight years in Königsberg before moving to Zürich, and his influence during this time was decisive in shaping HILBERT's very wide spectrum of mathematical interests, both as a student and as a young researcher.

MINKOWSKI's main interests also lay in pure mathematics, but they by no means remained confined to it. As a student, MINKOWSKI spent three semesters in Bonn before receiving his doctorate in Königsberg in 1885. He returned to Bonn as a *Privatdozent* and remained there until 1894, when he moved to Zürich. Not until 1902 did he join HILBERT in Göttingen, following KLEIN's success in persuading the Prussian educational authorities to create a third chair of mathematics especially for him. During all those years the friendship between MINKOWSKI, HURWITZ and HILBERT remained close. MINKOWSKI visited Königsberg each summer, and the three mathematicians would meet daily for mathematical walks. During the Christmas holidays of 1890 MINKOWSKI remained in Bonn, and in a letter to HILBERT he described his current interest in physics. In his obituary of MINKOWSKI, HILBERT reported — in an often-quoted

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[3] On the Königsberg school see KLEIN 1926–7 Vol. 1, 112–115 & 216–221; VOLK 1967. The workings of the Königsberg physics seminar — initiated in 1834 by FRANZ NEUMANN — and its enormous influence on nineteenth-century physics education in Germany are described in great detail in OLESKO 1991.

[4] For more details on WEBER (especially concerning his contributions to algebra) see CORRY 1996, §§1.2 & 2.2.4.

passage — that upon his insistence that MINKOWSKI come to Göttingen to join him and HURWITZ, MINKOWSKI had described himself as being now "contaminated with physics, and in need of a ten-day quarantine" before being able to return to the purely mathematical atmosphere of Königsberg. HILBERT also quoted MINKOWSKI's letter as follows:

> I have devoted myself for the time being completely to magic, that is to say, to physics. I have my practical exercises at the physics institute, and at home I study Thomson, Helmholtz and their accomplices. Starting next weekend, I'll work some days every week in a blue smock in an institute that produces physical instruments; this is a kind of practical training than which you could not even imagine a more shameful one.[5]

MINKOWSKI's interest in physics can certainly be dated even earlier than this; in 1888 he had already published an article on hydrodynamics, submitted to the Berlin Academy by HERMANN VON HELMHOLTZ (MINKOWSKI 1888). Later, during his Zürich years, MINKOWSKI's interest in physics remained alive, and so did his contact with HILBERT. From their correspondence we learn that MINKOWSKI dedicated part of his efforts to mathematical physics, and in particular to thermodynamics.[6] Finally, MINKOWSKI's last years in Göttingen were intensively dedicated to physics. During those years HILBERT's interest in physics became more vigorous than ever before; he and MINKOWSKI, in fact, conducted advanced seminars on physical issues.[7] Attention to current developments in physics was never foreign to HILBERT's and MINKOWSKI's main concerns with pure mathematics.

A balanced understanding of HILBERT's mathematical world cannot be achieved without paying close attention to his teaching, first at Königsberg and especially at Göttingen beginning in 1895. HILBERT directed no less than sixty-eight doctoral dissertations, sixty of them in the relatively short period between 1898 and 1914. As is well-known, at the mathematical institute created in Göttingen by FELIX KLEIN, HILBERT became the leader of a unique scientific center that brought together a gallery of world-class researchers in mathematics and physics.[8] It is hard to exaggerate the influence of HILBERT's thinking and personality on all that came out of the institute under his direction. Fortunately, we can document with great accuracy the contents of HILBERT's Göttingen lectures, which interestingly illuminate the evolution of his ideas on many issues. These lectures were far from being organized presentations of well-known results and established theories. Rather, he used his lectures to explore

[5] For the original letter, from which this passage is translated, see RÜDENBERG & ZASSENHAUS (eds.) 1973, 39–42, on pp. 39–40. For HILBERT's quotation see *GA* Vol. 3, 355. Unless otherwise stated in this article, all translations into English are mine.

[6] See RÜDENBERG & ZASSENHAUS (eds.) 1973, 110–114.

[7] On MINKOWSKI's years in Göttingen, see CORRY 1997a; GALISON 1977; PYENSON 1977, 1979.

[8] Accounts of Göttingen as the world leading center of mathematics, and the roles of KLEIN and HILBERT in fostering this centrality appear in REID 1970; ROWE 1989; PARSHALL & ROWE 1994, 150–154.

new ideas and to think aloud about the issues that currently occupied him. Following a tradition initiated by KLEIN in Göttingen, HILBERT's lecture notes were made available to all students who wished to consult them at the *Lesezimmer*, the heart of the mathematical institute. At least since 1902, in every course he taught, HILBERT chose a student to take notes during the lectures. The student was expected to write up these notes coherently, whereupon HILBERT would go through them, adding his own corrections and remarks.[9] Today the collection of these notes offers an invaluable source for the historian interested in understanding HILBERT's thought.

Late in life HILBERT vividly recalled that these lectures provided important occasions for the free exploration of untried ideas. He thus said:

> The closest conceivable connection between research and teaching became a decisive feature of my mathematical activity. The interchange of scientific ideas, the communication of what one found by himself and the elaboration of what one had heard, was from my early years at Königsberg a pivotal aspect of my scientific work . . . In my lectures, and above all in the seminars, my guiding principle was not to present material in a standard and as smooth as possible way, just to help the student keeping clean and ordered notebooks. Above all, I always tried to illuminate the problems and difficulties and to offer a bridge leading to currently open questions. It often happened that in the course of a semester the program of an advanced lecture was completely changed, because I wanted to discuss issues in which I was currently involved as a researcher and which had not yet by any means attained their definite formulation. (Translated from HILBERT 1971, 79)

Recognizing the centrality of his teaching activities and the extent to which his lectures reflected his current mathematical interests, one is led to reassess long-established assumptions about the periodization of HILBERT's work. In an often-quoted passage, HERMANN WEYL (1944, 619) asserted that HILBERT's work comprised five separate, and clearly discernible main periods: (1) Theory of invariants (1885–1893); (2) Theory of algebraic number fields (1893–1898); (3) Foundations, (a) of geometry (1898–1902), (b) of mathematics in general (1922–1930); (4) Integral equations (1902–1912); (5) Physics (1910–1922). This periodization reflects faithfully the division of HILBERT's published work, and what constituted his central domain of interest at different times. It says much less, however, about the evolution of his thought, and about the efforts he dedicated to other fields simultaneously with his main current interests.[10] As will be seen in what follows, the list of HILBERT's lectures during those years shows a more complex picture than WEYL's periodization suggests. In particular, it will be seen that HILBERT's concern with the physical sciences was a sustained one, which can be documented throughout his career.

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[9] See BORN 1978, 81–85, for a retrospective account of BORN's own experience as HILBERT's student.

[10] In fact, no one was in a better position than WEYL himself to appreciate the impact of HILBERT's docent activities, as he made clear in various opportunities. On WEYL's (sometimes changing) assessments of HILBERT's influence as a teacher, see SIGURDSSON 1994, 356–358.

3. The Background to Hilbert's Axiomatic Approach: Geometry and Physics

HILBERT's first published, comprehensive presentation of an axiomatized discipline appeared in 1899, in the ever since famous *Grundlagen der Geometrie.* The roots of HILBERT's axiomatic conception accordingly and obviously lie in contemporary developments in geometry. In what follows I will briefly describe some of these developments, of which several traditional accounts exist. Only relatively recently, however, has the relevant historical evidence been thoroughly studied.[11] More to the point for my present purposes, I will show that HILBERT's urge to axiomatize physical theories, as well as his conception of how this should be done, arose simultaneously with the consolidation of his axiomatic treatment of geometry. Certainly to a lesser degree than geometry, but still in significant ways, HILBERT's increasing interest in physics plays an important role in understanding the evolution of his thoughts on the axiomatic method.

During the nineteenth century, following the work of JEAN VICTOR PONCELET (1788–1867) in 1822, projective geometry became an active field of research that attracted the attention of many mathematicians, especially in Germany. HILBERT's own interest in foundational questions of geometry arose in connection with long-standing open issues in this domain — mainly having to do with the role of continuity considerations in the subject's foundations. A major contribution here came from the early attempts of FELIX KLEIN to explain the interrelations among the various kinds of geometry and to show that Euclidean and non-Euclidean geometries are in some sense derivative cases of projective geometry. A crucial step in this project was the introduction of a type of distance, or metric, into non-Euclidean structures, without using concepts derived from the Euclidean case. KLEIN introduced one such metric using the concept of the cross-ratio of four points, which is invariant under projective transformations. He relied on ideas originally introduced by ARTHUR CAYLEY (1821–1895) in his work on quadratic invariants,[12] but extended them to cover the non-Euclidean case, which CAYLEY had expressly avoided in his own work. In order to define cross-ratios in purely projective terms, KLEIN appealed to a result of VON STAUDT, according to which one could introduce coordinates into projective geometry, independently of metrical notions and of the parallel postulate. In fact, KLEIN failed to explain in detail how this could be effected, but in any case

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[11] Based on the manuscripts of HILBERT's early lectures, MICHAEL M. TOEPELL (1986) has analyzed in considerable detail the development of HILBERT's ideas previous to the publication of the *Grundlagen*, and his encounters with the foundations of geometry since his Königsberg years. In this section I partly rely on TOEPELL's illuminating account.

[12] For an account of CAYLEY's contributions see KLEIN 1926–7 Vol. 1, 147–151.

his arguments explicitly presupposed the need to add a continuity axiom to VON STAUDT's results.[13]

The uncertainties associated with KLEIN's results, as well as with other contemporary works, indicated to some mathematicians the need to re-examine with greater care the deductive structures of the existing body of knowledge in projective geometry. The first elaborate attempt to do so appeared in 1882, when MORITZ PASCH (1843–1930) published his book *Vorlesungen über neuere Geometrie*, presenting projective geometry in what he saw as an innovative, thoroughly axiomatic fashion.[14] PASCH undertook a revision of EUCLID's basic assumptions and rules of inference, and carefully closed some fundamental logical gaps affecting the latter. In PASCH's reconstruction of projective geometry, once the axioms are determined, all other results of geometry were to be attained by strict logical deduction, and without any appeal to diagrams or to properties of the figures involved. Yet it is important to stress that PASCH always conceived geometry as a "natural science", having as its subject matter the study of the external shape of things, and whose truths can be obtained from a handful of concepts and basic laws (the axioms), that are directly derived from experience. For PASCH, the meaning of the axioms themselves is purely geometrical and cannot be grasped without appeal to the diagrams from which they are derived. PASCH, for instance, considered that the continuity axiom for geometry was not convincingly supported by empirical evidence.[15]

Though PASCH substantially contributed to clarifying many aspects of the logical structure of projective geometry, the true status of continuity assumptions in projective geometry, remained unclear. This is particularly true concerning the possibility of establishing a link between this geometry and a system of real-number coordinates (coordinatization) as well as defining a metrics for it (metrization). The question was open whether continuity should be considered to be given with the very idea of space, or whether it should be reduced to more elementary concepts. KLEIN and WILHELM KILLING (1847–1923) elaborated the first of these alternatives, while HERMANN LUDWIG WIENER (1857–1939) and FRIEDRICH SCHUR (1856–1932) worked out the second. WIENER put forward his point of view in 1891, in a lecture on foundational questions of geometry delivered at the annual meeting of the German Mathematicians' Association (*DMV*) in Halle (WIENER 1891). Wiener claimed that starting solely with the theorems of DESARGUES and PAPPUS (or PASCAL's theorem for two lines, as WIENER, and later also HILBERT called it), it is possible to prove the fundamental theorem of projective geometry, namely, that for two given lines there exists one and only one projective mapping that correlates any three given points of the first to any three given points of the second in a given order. The classical proof of this theorem was based on the projective invariance of the cross-ratio; this

[13] KLEIN 1871 & 1873. For comments on these contributions of KLEIN see ROWE 1994, 194–195; TOEPELL 1986, 4–6; TORRETTI 1978, 110–152, On VON STAUDT's contribution see FREUDENTHAL 1974.

[14] On PASCH's book see, e.g., TORRETTI 1978, 44–53.

[15] See CONTRO 1976, 284–289; NAGEL 1939, 193–199; TORRETTI 1978, 210–218.

invariance implies that the image of a fourth point in the first line is uniquely determined under the given projective mapping, but the existence of the fourth point on the second line typically calls for the introduction of some kind of continuity argument.[16] WIENER's ideas seemed to open the possibility of developing projective geometry from a new perspective without any use of continuity considerations. Later, in 1898, SCHUR further proved PAPPUS's theorem without using any continuity assumptions (SCHUR 1898). This whole issue of the precise role of continuity in the foundations of geometry later became, as we will see, a major stimulus for HILBERT's active involvement in this domain.

PASCH's axiomatic treatment of projective geometry had considerable influence among Italian mathematicians, and in the first place on GIUSEPPE PEANO (1858–1930). PEANO was a competent mathematician, who made significant contributions in analysis and wrote important textbooks in this field.[17] But besides these standard mathematical activities, PEANO invested much of his efforts to advance the cause of international languages — he developed one such language called Interlingua — and to develop an artificial conceptual language that would allow completely formal treatments of mathematical proofs. In 1889 his successful application of such a conceptual language to arithmetic, yielded his famous postulates for the natural numbers. PASCH's systems of axioms for projective geometry posed a challenge to PEANO's artificial language. In addressing this challenge, PEANO was interested in the relationship between the logical and the geometrical terms involved in the deductive structure of geometry, and in the possibility of codifying the latter in his own artificial language. This interest led PEANO to introduce the idea of an independent set of axioms, namely, a set none of whose axioms is a logical consequence of the others. He applied this concept to his own system of axioms for projective geometry, which were a slight modification of PASCH's. PEANO's specific way of dealing with systems of axioms, and the importance he attributed to the search for independent sets of postulates, is similar in many respects to the perspective developed later by HILBERT; yet PEANO never undertook to prove the independence of whole systems of postulates.[18] For all of his insistence on the logical analysis of the deductive structure of mathematical theories, PEANO's overall view of mathematics — like PASCH's before him — was neither formalist nor logicist in the sense later attributed to these terms. PEANO conceived mathematical ideas as being derived from our empirical experience.[19]

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[16] Obviously the theorem can be dually formulated for two pencils of lines. For a more or less contemporary formulation of the theorem see ENRIQUES 1903. Interestingly, ENRIQUES explicitly remarked in the introduction to the German version of his book (p. *vii*) that he was following the classical approach introduced by VON STAUDT, and followed by KLEIN and others, rather than to the more modern one developed recently by PASCH and HILBERT.

[17] A brief account of PEANO's mathematical work appears in KENNEDY 1981. For more elaborate accounts see KENNEDY 1980; SEGRE 1994.

[18] Cf. TORRETTI 1978, 221.

[19] See KENNEDY 1981, 443.

Several Italian mathematicians, influenced by PEANO's ideas, published similar works in which the logical structure of the foundations of geometry was investigated. Among them one should mention MARIO PIERI (1860–1913),[20] who strongly promoted the idea of geometry as a hypothetico-deductive system, and introduced for his systems of postulates a kind of "ordinal independence", somewhat more limited than the one defined by PEANO.[21] Of special interest is the original work of GIUSEPPE VERONESE (1845–1917), who in 1891 published the first systematic study of the possibility of a non-Archimedean geometry,[22] and proved the independence of the Archimedean postulate from the other postulates of geometry.[23] HILBERT too would eventually deal with these issues in his axiomatic study of geometry.[24]

So much for the geometric background against which HILBERT's axiomatic method arose. I will return to it in the next section. But as I have already suggested, we must also look at certain developments in physics in the nineteenth century, in which new axiomatic treatments of old bodies of knowledge were also being pursued, independently of the developments in geometry discussed above. The axiomatic treatment of mechanics put forward by HEINRICH HERTZ (1857–1894) has been much less associated with HILBERT's axiomatics than the above mentioned work of PASCH and the tradition to which it belongs. Yet, as will be seen in what follows, HERTZ's *Principles of Mechanics* made a strong impression on HILBERT, which can be counted among the stimuli for the consolidation of his axiomatic conception.

In 1891 HERTZ began to work for the first time in his career on mechanics. This work, to which all his efforts were directed during the last three years of his life, led to the posthumous publication in 1894 of *The Principles of Mechanics Presented in a New Form.* HERTZ undertook this work motivated by the then widely — though not unanimously — accepted conception that mechanics constitutes the most basic discipline of physics, and at the same time, by his feeling that all accepted presentations of mechanics had serious shortcomings. In particular, HERTZ was deeply dissatisfied with the central role played by the concept of force, a concept which he set out to exclude from his own presentation. This presentation is usually described as 'axiomatic', a term which, however, HERTZ himself never used in describing his own work. In the following paragraphs I will attempt to clarify in what sense this term can usefully be applied to HERTZ's work, in order to trace HERTZ's influence on the emergence of HILBERT's axiomatic approach. This influence, as will be seen, can be found both in the general conception of the role of axiomatization in science and in HILBERT's specific axiomatic treatment of mechanics.

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[20] On PIERI, see KENNEDY 1981a.

[21] Cf. TORRETTI 1978, 225–226.

[22] In VERONESE 1891. See TRICOMI 1981.

[23] On criticism directed at VERONESE's work by German mathematicians see TOEPELL 1986, 56.

[24] For a concise contemporary account of the place of HILBERT's contribution in connection with these developments see SCHUR 1909, *iv–vi.*

HERTZ's preface opened with the assertion, that "all physicists agree that the problem of physics consists in tracing all the phenomena of nature back to the simple laws of mechanics." However, he added, what they disagree about is what these simple laws are and, especially, how they should be presented. Without claiming that his presentation was the only valid one of its kind, HERTZ stressed the need to redefine the very essence of mechanics, in order to be able to decide which assertions about nature are in accordance with it, and which contradict it. Although HERTZ's immediate concern was perhaps with the reduction of the equations of the ether to mechanics, this problem was not directly addressed in his presentation of mechanics.[25] In fact, rather than dealing with the question of the ultimate nature of physical phenomena, the issues discussed by HERTZ in the introduction to his book betrayed a rather general preoccupation with the need to clarify the conceptual content and structure of physical theories. In the particular case of mechanics, such a clarification needed to focus mainly on the problematic concept of force. But this was only a very conspicuous example of what HERTZ saw as a more general kind of deficiency affecting other domains of research. HERTZ's treatment of mechanics implied a more general perspective, from which theories concerning other kind of physical phenomena, not only mechanics, should be reexamined. In the introduction to the *Principles of Mechanics* — a text that has become widely known and has been thoroughly discussed in the literature[26] — HERTZ suggested a perspective that would allow for a systematic assessment of the relative predictive value of various scientific theories, while stressing the need to remove possible contradictions that have gradually accumulated in them. Generalizing from the problems associated with the concept of force, HERTZ wrote:

> Weighty evidence seems to be furnished by the statements which one hears with wearisome frequency, that the nature of force is still a mystery, that one of the chief problems of physics is the investigation of the nature of force, and so on. In the same way electricians are continually attacked as to the nature of electricity. Now, why is it that people never in this way ask what is the nature of gold, or what is the nature of velocity? Is the nature of gold better known to us that of electricity, or the nature of velocity better than that of force? Can we by our conceptions, by our words, completely represent the nature of anything? Certainly not. I fancy the difference must lie in this. With the terms "velocity" and "gold" we connect a large number of relations to other terms; and between all these relations we find no contradictions which offends us. We are therefore satisfied and ask no further questions. But we have accumulated around the terms "force" and "electricity" more relations than can be completely reconciled amongst themselves. We have an obscure feeling of this and want to have things cleared up. Our confused wish finds expression in the confused question as to the nature of force and electricity. But the answer which we want is not really an answer to this question. It is not by finding out more and fresh relations and connections that it can be answered; but by removing the contradictions existing between those already known, and thus perhaps by reducing their

[25] See LÜTZEN 1995, 4–5.

[26] For recent discussions see BAIRD et al. 1997; LÜTZEN 1995.

> number. When these painful contradictions are removed, the question as to the nature of force will not have been answered; but our minds, no longer vexed, will cease to ask illegitimate questions.[27]

HERTZ described theories as "images" (*Bilder*) that we form for ourselves of natural phenomena. He proposed three criteria to evaluate among several possible images of one thing: permissibility, correctness, and appropriateness. An image is permissible, according to HERTZ, if it does not contradict the laws of thought. This requirement appears, even at the most immediate level, as similar to HILBERT's requirement of consistency. But in fact this parallel is even deeper, in the sense that, in speaking about the laws of thought, HERTZ implicitly took logic to be given *a priori*, in KANT's sense, and therefore to be unproblematic in this context. This was also the case in HILBERT's early axiomatic conception although, as will be seen below, his conception later changed in the face of logical paradoxes.

A permissible image is correct for HERTZ if its essential relations do not contradict the relations of external things. In fact, HERTZ actually defined an image by means of the requirement that its "necessary consequents . . . in thought are always the images of the necessary consequents in nature of the things pictured. (p. 1)" One also finds a parallel to this in HILBERT's requirement that all the known facts of a mathematical theory may be derived from its system of postulates.

But given two permissible and correct images of one and the same thing, it is by considering the appropriateness of each that HERTZ proposed to assess their relative value. The appropriateness of an image comprises two elements: distinctness and simplicity. By the former, HERTZ understood the ability to picture the greatest possible amount of "the essential relations of the object." Among various pictures of the same object, the "simpler" one is that which attains this distinctness while including the smaller number of empty relations. HERTZ deemed simpler images more appropriate (p. 2); he used this last criterion directly to argue that his own presentation of mechanics was better than existing ones, since, by renouncing the concept of force, it provided a "simpler" image. In general, however, both distinctness and simplicity are far from being straightforwardly applicable criteria. HILBERT's requirement of independence, although not identical to this, can be seen as a more precise and workable formulation of HERTZ's criterion of appropriateness.

The permissibility and the correctness of an image connect the latter to two different sources of knowledge: the mind and experience respectively. The permissibility of an image, thought HERTZ, can therefore be unambiguously established once and for all. Its correctness is a function of the present state of knowledge, and it may vary as the latter changes. As to the appropriateness of an image, HERTZ conceded that it may be a matter of opinion.

HERTZ also made clear what he understood by "principles" in his work. Although the word had been used with various meanings, he meant by it any

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[27] HERTZ 1956, 7–8. In what follows, all quotations refer to this English translation.

propositions or systems of propositions from which the whole of mechanics can be "developed by purely deductive reasoning without any further appeal to experience (p. 4)." Different choices of principles would yield different images of mechanics.

HERTZ'S own presentation of mechanics, as it is well known, uses only three basic concepts: time, space, mass; HERTZ was trying to eliminate forces from his account of mechanics. He thought that this concept, especially as it concerns forces that act at a distance, was artificial and problematic. He thought, moreover, that many physicists, from Newton on, had expressed their embarrassment when introducing it into mechanical reasoning, though no one had done anything to overcome this situation (pp. 6–7). In his presentation, HERTZ was able to eliminate forces by introducing "concealed masses" and "concealed motions." Based on the criteria discussed in his introduction, HERTZ criticized the two main existing presentations of mechanics: 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. He then explained his own view and — based again on the same criteria — established the superiority of his presentation of mechanics.

This is not the place to give a full account of HERTZ's criticism of the existing presentations of mechanics nor to discuss his own in detail.[28] I will only focus on some of HERTZ's remarks concerning the basic principles of his approach. These will help us in understanding HILBERT's axiomatic conception and will also allow us identify the roots of this conception in HERTZ's work.

In principle, HERTZ's criticism of the traditional approach to mechanics concerned neither its correctness nor its permissibility, but only its appropriateness. Yet he also allowed room for changes in the status of correctness in the future. In criticizing the role played by force in the traditional image of mechanics, HERTZ stressed that the problems raised by the use of this concept are part of our representation of this image, rather than of the essence of the image itself. This representation had simply not attained, in HERTZ's view, scientific completeness; it failed to "distinguish thoroughly and sharply between the elements in the image which arise from the necessity of thought, from experience, and from arbitrary choice (p. 8)." A suitable arrangement of definitions, notations, and basic concepts would certainly lead to an essential improvement in this situation. This improvement in presentation, however, would also allow the correctness of the theory to be evaluated in the face of later changes in the state of knowledge. HERTZ thus wrote:

> Our assurance, of course, is restricted to the range of previous experience: as far as future experience is concerned, there will be yet occasion to return to the question of correctness. To many this will seem to be excessive and absurd caution: to many physicists it appears simply inconceivable that any further experience whatever should find anything to alter in the firm foundations of mechanics. Nevertheless, that which is derived from experience can again be annulled by experience. This overfavorable opinion of the fundamental laws must obviously arise from the fact that

[28] For one such account see LÜTZEN 1995.

> the elements of experience are to a certain extent hidden in them and blended with the unalterable elements which are necessary consequences of our thought. Thus the logical indefiniteness of the representation, which we have just censured, has one advantage. It gives the foundation an appearance of immutability; and perhaps it was wise to introduce it in the beginnings of science and to allow it to remain for a while. The correctness of the image in all cases was carefully provided for by making the reservation that, if need be, facts derived from experience should determine definitions or viceversa. In a perfect science such groping, such an appearance of certainty, is inadmissible. Mature knowledge regards logical clearness as of prime importance: only logically clear images does it test as to correctness; only correct images it compares as to appropriateness. By pressure of circumstances the process is often reversed. Images are found to be suitable for a certain purpose; are next tested in their correctness; and only in the last place purged of implied contradictions. (HERTZ 1956, 10)

It seems natural to assume that by "mature science" HERTZ was referring here to Euclidean geometry. But as HILBERT noticed in 1894 when preparing his Königsberg lectures on the foundations of geometry (discussed below), the situation in this discipline, although perhaps much better than in mechanics, was also begging for further improvement. Then in 1899, HILBERT felt prepared to address those foundational problems of geometry that had remained essentially unanswered since KLEIN's attempts to define a metric for projective geometry. The methodological approach HILBERT adopted for this task resembled very much, as will be seen below, HERTZ's stipulations for mechanics as manifest in the above quoted passage: to attain logical clearness, to test for correctness, to compare as to appropriateness, and to make sure that implied contradictions had been purged. Moreover, like HERTZ before him, HILBERT thought that such a procedure should be applied to all of natural science and not to geometry alone.

In HERTZ's presentation of mechanics, every new statement is deduced only from already established ones. This is what has been called his axiomatic approach. Although this in itself is no guarantee against error, HERTZ conceded, it has the virtue that it allows the logical value of every important statement to be understood, and any mistake to be easily identified and removed. In the second part of the book, HERTZ investigated the logical relation between various principles of mechanics. He was able to specify which statements are equivalent to the fundamental laws of motion, and which statements of the fundamental laws are not implied by a given principle. But to what extent is mechanics thus presented "correct", in HERTZ's sense of the word? Although no known fact of experience was then considered to contradict the results of mechanics, HERTZ admitted that the latter could not be fully confronted with all possible phenomena. Thus, mechanics had been built on some far-reaching assumptions that could conceivably be questioned. For instance: is there a full justification for assuming the centrality of linear differential equations of the first order in describing mechanical processes? Another central, but perhaps not fully justified assumption is that of the continuity of nature. HERTZ described it as "an experience of the most general kind" . . . "an experience which has crystallized into firm conviction in the old proposition — *Natura non facit saltus* (pp. 36–37)." HILBERT, in his treatment of physical theories, would not only accept this assumption, but also attempt to give it a more mathematically consistent formulation.

Finally, in explaining the sense in which his new image of mechanics was simpler than the other existing two, HERTZ stressed that this simplicity (and therefore appropriateness) did not concern the practical side of mechanics, but rather the epistemological one:

> We have only spoken of appropriateness in . . . the sense of a mind which endeavors to embrace objectively the whole of our physical knowledge without considering the accidental position of man in nature . . . The appropriateness of which we have spoken has no reference to the practical application of the needs of mankind. (HERTZ 1956, p. 40)

HERTZ's book was widely praised following its publication in 1894. The interest it aroused concerned his construction of mechanics while avoiding the use of forces acting at a distance, as well as its philosophical aspects and its mathematical elaboration. The actual impact of HERTZ's approach on physical research, however, was far less than the interest it aroused.[29] On the other hand, HERTZ's influence on HILBERT was, as I will show below, more significant than has usually been pointed out. LUDWIG BOLTZMANN (1844–1906) should be mentioned here among those physicists who were strongly impressed by HERTZ's treatment. In 1897 he published his own textbook on mechanics, modeled in many respects after HERTZ's. This book had a lesser impact on HILBERT's general conceptions; yet its treatment of mechanics, as we will see below, was also highly appreciated by HILBERT.

The positive reactions often associated with the publication of HERTZ's *Principles* should not mislead us to believe that the idea of axiomatizing physical disciplines was a widely accepted one, or became so after HERTZ. Although an overall account of the evolution of this idea throughout the nineteenth century and its place in the history of physics seems yet to be unwritten, one should stress here that axiomatization was seldom considered a main task of the discipline. Nevertheless, it is worth discussing here briefly the ideas of two other German professors, CARL NEUMANN and PAUL VOLKMANN, who raised interesting issues concerning the role of axioms in physical science (one of them writing before HERTZ's *Principles*, the second one after). Since their ideas are visibly echoed in HILBERT's work, a brief discussion of NEUMANN's and VOLKMANN's writings will help set up the background against which HILBERT's ideas concerning the axiomatization of physics arose.

CARL NEUMANN (1832–1925) was the son of the Königsberg physicist FRANZ NEUMANN. At variance with the more experimentally-oriented spirit of his father's work, CARL NEUMANN's contributions focused on the mathematical aspects of physics, particularly on potential theory, the domain where he made his most important contributions. His career as professor of mathematics

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[29] See LÜTZEN 1995, 76–83.

evolved in Halle, Basel, Tübingen and Leipzig.[30] NEUMANN's inaugural lecture in Leipzig in 1869 discussed the question of the principles underlying the GALILEO-NEWTON theory of movement. NEUMANN addressed the classical question of absolute vs. relative motion, examining it from a new perspective provided by a philosophical analysis of the basic assumptions behind the law of inertia. The ideas introduced by NEUMANN in this lecture, and the ensuing criticism of them, inaugurated an important trend of critical examination of the basic concepts of dynamics — a trend of which ERNST MACH was also part — which helped to prepare the way for the fundamental changes that affected the physical sciences at the beginning of this century.[31]

NEUMANN opened his inaugural lecture of 1869 by formulating what he considered to be the universally acknowledged goal of the mathematical sciences: "the discovery of the least possible numbers of principles (notably principles that are not further explicable) from which the universal laws of empirically given facts emerge with mathematical necessity, and thus the discovery of principles *equivalent* to those empirical facts."[32] NEUMANN intended to show that the principle of inertia, as usually formulated, could not count as one such basic principle for mechanics. Rather "it must be dissolved into a fairly large number of partly fundamental principles, partly definitions dependent on them. The latter include the definition of rest and motion and also the definition of *equally long time intervals*." NEUMANN's reconsideration of these fundamental ideas of Newtonian mechanics was presented as part of a more general discussion of the aims and methods of theoretical physics.

Echoing some ideas originally formulated as early as the middle ages, and recently revived by physicists like KIRCHHOFF and MACH, NEUMANN claimed that physical theories, rather than explaining phenomena, amounted to a reduction of infinitely many phenomena of like kind to a finite set of unexplained, more basic ones. The best known example of this was the reduction of all phenomena of celestial motion to inertia and gravitational attraction. The latter, while fulfilling their reductionist task properly, remained in themselves unexplained, NEUMANN argued. But NEUMANN went on, and compared this reduction to the one known in geometry, wherein the science of triangles, circles and conic sections "has grown in mathematical rigor out of a few principles, of axioms, that are not further explicable and that are not any further demonstrable." NEUMANN was thus placing mechanics and geometry (like HILBERT did later) on the same side of a comparison, the second side of which was represented by logic and arithmetic; the results attained in these latter domains — as opposed to those of geometry and mechanics — "bear the stamp of *irrevocable certainty*", that provides "the guarantee of an *unassailable truth*." The non-explanatory character of mechanics and geometry, NEUMANN stressed, cannot be

[30] See DISALLE 1993, 345; JUNGNICKEL & MCCORMMACH 1986, Vol. 1, 181–185.

[31] This trend is discussed in BARBOUR 1989, Chp. 12.

[32] NEUMANN 1870, 3. I will refer here to the translation NEUMANN 1993.

considered as a flaw of these sciences. Rather, it is a constraint imposed by human capacities.

The principles to which physical theories are reduced not only remain unexplained, said NEUMANN, but in fact one cannot speak of their being correct or incorrect, or even of their being probable or improbable. The principles of any physical theory — e.g., FRESNEL and YOUNG's theory of light — can only be said to have *temporarily* been confirmed; they are *incomprehensible* (*unbegreiflich*) and *arbitrary* (*willkürlich*). NEUMANN quoted LEIBNIZ, in order to explain his point: nature should indeed be explained from established mathematical and physical principles, but "the *principles themselves* cannot be deduced from the laws of mathematical necessity."[33] Thus, in using the terms *arbitrary* and *incomprehensible*, NEUMANN was referring to the limitations of our power of reasoning. Always relying on basically Kantian conceptions, he contrasted the status of the choice of the principles in the physical sciences to the kind of necessity that guides the choice of mathematical ones. This is what their arbitrariness means. NEUMANN was clearly not implying that physical theories are simply formal deductions of any arbitrarily given, consistent system of axioms devoid of directly intuitive content. Rather they have very concrete empirical origins and interpretations, but, given the limitations of human mental capacities, their status is not as definitive as that of the principles of logic and arithmetic.

NEUMANN concluded the philosophical section of his lecture by reformulating the task of the physicist in the terms discussed before: to reduce physical phenomena

> . . . to the fewest possible arbitrarily chosen principles — in other words, to reduce them to the fewest possible things remaining *incomprehensible*. The greater the number of phenomena encompassed by a physical theory, and the smaller the number of inexplicable items to which the phenomena are reduced, the more perfect is the theory to be judged.

From here he went on to analyze the conceptual difficulties involved in the principle of inertia, usually formulated as follows:

> A material point that was set in motion will move on — if no foreign cause affects it, if it is entirely left to itself — in a *straight line* and it will traverse in equal time equal distances.[34]

The first problem pointed out by NEUMANN concerning this formulation has to do with the concept of straight line. Recognizing a straight line in physical space raises the difficulties traditionally associated with the question of relative vs. absolute space. In addressing this question, NEUMANN introduced the idea of the Body Alpha: a rigid object located somewhere in the universe, to which all motions refer. Thus, the principle of inertia is analyzed, in the first place, into

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[33] NEUMANN 1993, 361. The reference is to LEIBNIZ *Mathematische Schriften*, part 2, Vol. 2 (Halle 1860, p. 135.)

[34] NEUMANN 1870, 14 (1993, 361).

two simpler components: the first asserts the existence of the Body Alpha, the second asserts that every material point left to itself will move in a straight line, i.e., in a path rectilinear in relation to this Body Alpha. This way of analyzing the principle of inertia embodied NEUMANN's prescription of "incomprehensible and arbitrary" principles which helped to make sense of a physical theory. This idea attracted much attention and criticism, and NEUMANN himself reformulated it several times. This is not, however, the place to discuss the idea and its critics in detail.[35]

More directly pertinent to our account, since it will reappear in HILBERT's lectures on physics, was NEUMANN's treatment of the second part of the principle of inertia: the concept of "equal velocities". An appropriate elucidation of this concept is clearly related to the problem of relative vs. absolute time. NEUMANN discussed in his lecture the problem of the measurement of time and of the determination of two equal time-intervals. He proposed to reduce time to movement in order to explain the former. In his view, the correct formulation of the third component of the principle of inertia should read as follows: "Two material points, each left to itself, move in such a way that the equal paths of one of them always correspond to the equal paths of the other." From here one also gets the definition of equal time intervals as those in which a point left to itself covers equal paths.

This part of NEUMANN's analysis also attracted attention and gave rise to criticisms and improvements. Of special interest is the concept of "inertial system", introduced in this context by LUDWIG LANGE in 1886, which became standard and has remained so ever since.[36]

In his closing remarks NEUMANN expressed the hope that his analysis may have shown that "mathematical physical theories in general must be seen as subjective constructions, originating with us, which (starting from arbitrarily chosen principles and developed in a strictly mathematical manner) are intended to supply us with the most faithful pictures possible of the phenomena."[37] Following HELMHOLTZ, NEUMANN claimed that any such theory could only claim objective reality — or at least general necessity — if one could show that its principles "are the *only possible ones*, that no other theory than this one is conceivable which conforms to the phenomena." However, he deemed such a possibility as lying beyond human capabilities. Nevertheless — and this is a point that HILBERT will also stress time and again in his own attempts to axiomatize physical domains — the constant re-examination of principles and of their specific consequences for the theory is vital to the further progress of science. NEUMANN thus concluded:

> High and mighty as a theory may appear, we shall always be forced to render a precise account of its principles. We must always bear in mind that these principles are something *arbitrary*, and therefore something *mutable*. This is necessary in order

[35] See BARBOUR 1989, 646–653; DISALLE 1993, 348–349.
[36] LANGE's ideas are discussed in BARBOUR 1989, 655–662.
[37] NEUMANN 1870, 22 (1993, 367).

to survey wherever possible what effect a change of these principles would have on the entire edifice (*Gestaltung*) of a theory, and to be able to introduce such a change at the right time, and (in a word) that we may be in a position to preserve the theory from a *petrification*, from an *ossification* that can *only* be deleterious and a *hindrance* to the advancement of science.[38]

HILBERT never directly cited NEUMANN's inaugural lecture, or any other of his publications, but it seems fair to assume that HILBERT knew about NEUMANN's ideas from very early on. Together with RUDOLF ALFRED CLEBSCH (1833–1872), NEUMANN founded the *Mathematische Annalen* in 1868 and co-edited it until 1876,[39] and was surely a well-known mathematician. Moreover, in 1885, when HILBERT spent a semester in Leipzig, NEUMANN was one of two professors of mathematics there, and the two must have met, the young HILBERT listening to the older professor. In any case, we will see below how NEUMANN's conceptions described here recurrently appear in HILBERT's discussions about physical theories. This is true of NEUMANN's treatment of mechanics, especially the question of properly defining time and inertia. It is also true of his general conceptions concerning the role of axiomatic treatments of physical theories: the reduction of theories to basic principles, the provisory character of physical theories and the ability to reformulate theories in order to meet new empirical facts, the affinity of geometry and mechanics. NEUMANN had a lifelong concern with the ongoing over-specialization of mathematics and physics, and with their mutual estrangement, which he considered detrimental for both. He believed in the unity of the whole edifice of science and in constant cross-fertilization among its branches.[40] These are also central themes of HILBERT's discourse on mathematics and physics. NEUMANN's concerns as described here illuminate, if not directly the early roots of HILBERT's conceptions, then at the very least, the proper context in which the emergence of HILBERT's axiomatic method should be considered.

PAUL VOLKMANN (1856–1938), the second physicist I want to mention here, spent his whole career in Königsberg, where he completed his dissertation in 1880, and was appointed full professor in 1894.[41] In the intimate academic atmosphere of Königsberg, HILBERT certainly met VOLKMANN on a regular basis, perhaps at the weekly mathematical seminar directed by LINDEMANN. Although it is hard to determine with exactitude the nature of his relationship with HILBERT and the extent and direction of their reciprocal influence, looking at VOLKMANN's conception of the role of axiomatic treatments in science can certainly illuminate the atmosphere in which HILBERT was working and within which his own axiomatic conception arose.

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[38] NEUMANN 1870, 23 (1993, 368).

[39] See TOBIES & ROWE (eds.) 1990, 29.

[40] See JUNGNICKEL & MCCORMMACH 1986, Vol. 1, 184–185.

[41] See JUNGNICKEL & MCCORMMACH 1986, Vol. 2, 144–148; OLESKO 1991, 439–448; RAMSER 1974.

VOLKMANN was very fond of discussing epistemological and methodological issues of physics, but his opinions on these issues could be very variable. Concerning the role of axioms or first principles in physical theories, he moved from ignoring them altogether (VOLKMANN 1892), to emphatically denying their very existence (VOLKMANN 1894), to stressing their importance and discussing at length the principles of mechanics in an elementary textbook published in 1900. This book was intended as a thorough defense of the point of view that all of physics can be reduced to mechanics. VOLKMANN acknowledged in his book the influence of HERTZ and of BOLTZMANN, but at the same time he believed that these physicists had paid excessive attention to the mathematics, at the expense of the physical content behind the theories.

In the introduction to his 1900 textbook, VOLKMANN warned his students and readers that his lectures were not a royal road, comfortably leading to an immediate and effortless mastery of the system of science. Rather, he intended to take the reader a full circle around, in which the significance of the foundations and the basic laws would only gradually be fully grasped in the course of the lectures. VOLKMANN adopted this approach since he considered it to mimic the actual doings of science. VOLKMANN illustrated what he meant by comparing the development of science to the construction of an arch. He wrote:

> The conceptual system of physics should not be conceived as one which is produced bottom-up like a building. Rather it is like a thorough system of cross-references, which is built like a vault or the arch of a bridge, and which demands that the most diverse references must be made in advance from the outset, and reciprocally, that as later constructions are performed the most divers retrospections to earlier dispositions and determinations must hold. Physics, briefly said, is a conceptual system which is consolidated retroactively. (VOLKMANN 1900, 3–4)

This retroactive consolidation is the one provided by the first principles of a theory. That is, the foundational analysis of a scientific discipline is not a starting point, but rather a relatively late stage in its development. This latter idea is also central to understanding HILBERT's axiomatic conception. In fact, the building metaphor itself was one that HILBERT was to adopt wholeheartedly and to refer to repeatedly throughout his career when explaining his conception. In his Paris 1900 address (see below), HILBERT already alluded to this metaphor, but only later did he use it in the more articulate way put forward here by VOLKMANN. More importantly, the role assigned by VOLKMANN to the axiomatic analysis of a theory was similar to HILBERT's, not only for physical theories, but also for geometry.

VOLKMANN's epistemological discussion stressed a further point that is also found at the focus of HILBERT's own view: science as a product of the dialectical interaction between the empirical world and the world of thought. Given the inherent limitations of man's intellect one can attain only a subjective comprehension of experience, which is of necessity flawed by errors. The aim of science is to eliminate these errors and to lead to the creation of an objective experience. This aim is achieved with the help of first principles, which open the way to the use of mathematics to solving physical problems. Once the mathematical foundations of a discipline are laid, a dialectical process of interaction between subjective perception and objective reality begins. A constant reformulation and adaptation of ideas will help to close the unavoidable gap between these two poles (VOLKMANN 1900, 10). VOLKMANN's account here, as will be seen below, also matches to a large extent HILBERT's own views. But of greater interest is the fact that according to VOLKMANN, the principles involved in this process are of three kinds: axioms (or postulates), hypotheses, and natural laws.

VOLKMANN's treatment of these three categories is not very clear or concise, yet it seems to have tacitly conveyed a very significant classification that also HILBERT would allude to when putting forward specific systems of axioms for physical theories. Its essence may be grasped through the examples that VOLKMANN gave of the three kinds of principles. As examples of postulates or axioms, he mentioned the principle of conservation of energy and the GALILEO-NEWTON inertia law. Among hypotheses, the undulatory nature of light (whether elastic or electromagnetic), and an atomistic theory of the constitution of matter. Among natural laws: NEWTON's gravitation laws and COULOMB's law.

Very roughly, these three kinds of propositions differ from one another in the generality of their intended range of validity, in the degree of their universal acceptance, and in the greater or lesser role played in them by intuitive, as opposed to conceptual, factors. Thus, the axioms or postulates concern science as a whole, or at least a considerable portion of it, they are universally or very generally accepted, and they can predominantly be described as direct expressions of our intuition (*Anschauung*). Natural laws stand at the other extreme of the spectrum, and they are predominantly conceptual. Physical hypotheses stand in between. They express very suggestive images that help us to overcome the limitations of the senses, leading to the formulation of more precise relations. VOLKMANN's axioms cannot be directly proved or disproved through measurement. Only when these postulates are applied to special fields of physics and transformed into laws, can this be done. The more an axiom is successfully applied to particular domains of physics, without leading to internal contradiction, the more strongly it is retrospectively secured as a scientific principle.[42]

It is not our concern here to evaluate the originality or fruitfulness of these ideas of VOLKMANN. Nor, I think, is it possible to establish their influence on HILBERT's own thought. Rather, I have described them in some detail in order to fill out the picture of the kind of debate around the use of axioms in physics that HILBERT witnessed or was part of. Still, in analyzing in some detail HILBERT's axiomatization of particular domains of physics, we will find clear echoes of VOLKMANN's ideas. It should also be stressed, that in his 1900 book, Volkmann cited HILBERT's *Grundlagen* as a recent example of a successful treatment of the ancient problem of the axioms of geometry (p. 363).

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[42] For more details, see VOLKMANN 1900, 12–20. On pp. 78–79, he discusses in greater detail NEWTON's laws of motion and the universal law of gravitation as examples of principles and laws of nature respectively.

4. Axiomatics, Geometry and Physics in Hilbert's Early Lectures

During his Königsberg years, geometry was certainly not HILBERT's main area of interest. However, he taught several courses on it, and the issues on which he lectured in the mathematical seminar of the university bear witness to his continued interest in geometry and the question of its foundations.[43] HILBERT taught projective geometry for the first time in 1891. His course was modeled mainly after two existing texts on projective geometry (*Geometrie der Lage*) — by VON STAUDT and by THEODOR REYE (1839–1919)[44] — whose approach was thoroughly constructive and synthetic, and not in anyway axiomatic. In his introductory remarks, however, HILBERT discussed a more general picture of the discipline and the various ways to approach it. He mentioned three different, complementary branches of geometry: intuitive (or *Geometrie der Anschauung* — including school geometry, projective geometry and *analysis situs*), axiomatic and analytic. Whereas for HILBERT the value of the first branch was mainly aesthetic and pedagogical, and the last one was the most important for mathematical and scientific purposes, he deemed the axiomatic treatment of geometry to be mainly of epistemological importance. His definition of what an axiomatic treatment implies, however, was here rather loose and certainly far from putting forward actual guidelines for teaching or research. In any case, this was an approach HILBERT did not follow in these lectures; he was interested in the latest developments of projective geometry and the foundational issues associated with them, independently of any axiomatic consideration. Moreover, in the bibliographical list quoted in the introduction to the course, HILBERT did not mention PASCH's book — published back in 1882 — nor discuss the virtues or limitations of his account.[45]

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 played a decisive role. This position, which we have already seen espoused by CARL NEUMANN, is explicitly manifest in the following, significant passage taken from the introduction to the course:

> 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

[43] This is documented in TOEPELL 1986, 11–12.

[44] REYE 1886 and VON STAUDT 1847, respectively. See TOEPELL 1986, 26–38, for a detailed account of this course.

[45] TOEPELL, 1986, 38, quotes a remark added by HILBERT on the back of the titlepage of the manuscript, mentioning PASCH's book as a source for studying the axioms and the foundations of geometry. There are reasons to believe, however, that this remark was added only much later, and not during the time of the course itself.

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.[46]

In 1891 HILBERT also attended the lecture mentioned above in which HERMANN WIENER discussed the foundational role of the theorems of DESARGUES and PASCAL for projective geometry.[47] He may also have attended in 1893 a second lecture in which WIENER explained the implications of these ideas for affine and Euclidean geometry.[48] While becoming gradually interested in these kinds of foundational problems and gradually aware of possible ways to address them, HILBERT also began pondering the use of the axiomatic approach as the most convenient perspective from which to do so. In preparing his next course on geometry, to be given in 1893, HILBERT already adopted the axiomatic point of view that two years earlier he had only mentioned in passing, as a possible alternative. As the original manuscript of the course clearly reveals, HILBERT decided to follow now the model put forward by PASCH. Like PASCH, HILBERT saw the application of this axiomatic approach as a direct expression of a naturalistic approach to geometry, rather than as opposed to it: 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.[49] From the outset, however, HILBERT realized some of the shortcomings in PASCH's treatment, and in particular, certain redundancies that affected it. HILBERT had understood the convenience of pursuing the study of the foundations of geometry on the lines advanced by PASCH, but at the same time he perceived that the task of establishing the minimal set of presuppositions from which the whole of geometry could be deduced had not yet been fully accomplished. In particular, HILBERT pointed out that PASCH's Archimedean axiom could be derived from his others.[50]

Sometime in 1894 HILBERT became acquainted with HERTZ's ideas on the role of first principles in physical theories. This seems to have provided a final, significant catalyst towards the wholehearted adoption of the axiomatic perspective for geometry, while simultaneously establishing, 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

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[46] 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.

[47] See TOEPELL 1986, 40.

[48] WIENER's second talk was published as WIENER 1893. See ROWE 1996a.

[49] HILBERT 1893/94, 10: "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."

[50] TOEPELL 1986, 45, quotes a letter to KLEIN, dated May 23, 1893, where HILBERT expresses these opinions.

that HILBERT came to hear about HERTZ is in itself not at all surprising; he would most probably have read HERTZ's book sooner or later. But the fact that he read it so early was undoubtedly an expression of MINKOWSKI's influence. In the obituary already mentioned, HILBERT stressed that during his Bonn years, MINKOWSKI felt closer to HERTZ and to his work than to anything else. HILBERT also reported MINKOWSKI's explicit declaration that, had it not been for HERTZ's untimely death, he would have dedicated himself exclusively to physics.[51]

No details are known about the actual relationship between MINKOWSKI and HERTZ, and in particular about the extent of their intellectual contact at the time of the writing of the *Principles*. But all the circumstances would seem to indicate that from very early on, HILBERT had in MINKOWSKI a reliable, and very sympathetic, first-hand source of information — in spirit, if not in detail — concerning the kind of ideas being developed by HERTZ while working on his *Principles*. 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. We do possess clear evidence that as early as 1894, even if HILBERT had not actually read the whole book, then at least he thought that the ideas developed in its introduction were highly relevant to his own treatment of geometry and that they further endorsed the axiomatic perspective as a convenient choice. As only one student registered for HILBERT's course in 1893, it was not given until the next year.[52] When revising the manuscript for teaching the course in 1894 HILBERT added the following comment:

> Nevertheless the origin [of geometrical knowledge] is in experience. The axioms are, as HERTZ would say, images or symbols in our mind, such that consequents of the images are again images of the consequences, i.e., what we can logically deduce from the images is itself valid in nature.[53]

In these same lectures HILBERT also pointed out the need to establish the independence of the axioms of geometry. In doing so, however, he stressed the objective and factual character of the 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

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[51] HILBERT *GA* Vol. 3, 355. Unfortunately, there seems to be no independent confirmation of MINKOWSKI's own statement to this effect.

[52] See TOEPELL 1986, 51.

[53] HILBERT 1893/94, 10: "Dennoch der Ursprung aus der Erfahrung. Die Axiome sind, wie Herz [sic] sagen würde, Bilde[r] oder Symbole in unserem Geiste, so dass Folgen der Bilder wieder Bilder der Folgen sind d.h. was wir aus den Bildern logisch ableiten, stimmt wieder in der Natur."

It is worth noticing 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.

> order that a complete description and arrangement of all the geometrical facts be possible by means of this system of things.[54]

Of central importance in this respect was the axiom of continuity, whose actual role in allowing for a coordinatization of projective geometry, as has been already pointed out, had been widely discussed over the years and still remained an open question to which HILBERT directed much effort. VERONESE's book appeared in German translation only in 1894, and it is likely that HILBERT had not read it before then. He had initially believed that the axiom of continuity could be derived from the other axioms. Eventually he added the axiom to the manuscript of the lecture.[55]

Concerning the validity of the parallel axiom, HILBERT adopted an interestingly empiricist approach: he referred to GAUSS's experimental measurement of the sum of angles of a triangle between three high mountain peaks. Although GAUSS's result had convinced him of the correctness of Euclidean geometry as a true description of physical space,[56] HILBERT said, the possibility was still open that future measurements would show otherwise. In subsequent lectures on physics, HILBERT would return to this example very often to illustrate the use of axiomatics in physics. In the case of geometry only this particular axiom must 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 that:

> 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.[57]

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

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[54] Quoted from the original in TOEPELL 1986, 58–59.

[55] See TOEPELL 1986, 74–76.

[56] The view that GAUSS considered his measurement as related to the question of the parallel axiom has recently been questioned (BREITENBERGER 1984, MILLER 1972), arguing that it was strictly part of his geodetic investigations. For a reply to this argument see SCHOLZ 1993, 642–644. It is agreed however, that by 1860 the view expressed here by HILBERT was the accepted one, wrongly or rightly so. HILBERT, at any rate, did believe that this had been GAUSS's actual intention, and he repeated this opinion on many occasions.

[57] Quoted in TOEPELL 1986, 58.

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."[58]

By 1894, then, HILBERT's interest in foundational issues of geometry had increased considerably. WIENER's suggestions concerning the possibility of proving central results of projective geometry without recourse to continuity considerations had a great appeal for him. He had also begun to move towards the axiomatic approach as a convenient way of addressing these issues. His acquaintance with HERTZ's ideas then helped him to conceive the axiomatic treatment of geometry as part of a larger enterprise, relevant also for other physical theories, and also offered methodological guidelines how to realize this analysis. Finally, it is possible that HILBERT was also aware, to some extent, of the achievements of the Italian school, although it is hard to say specifically which of their works he read, and how they influenced his thought.[59]

In 1899 HILBERT lectured in Göttingen on the elements of Euclidean geometry. In the opening lecture of his course, he restated the main result 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 mentioned the precise source from where he had taken this formulation: the introduction to HERTZ's *Principles of Mechanics*.[60] This kind of task, however, was not limited in his view to geometry. While writing his *Grundlagen*, HILBERT lectured on mechanics in Göttingen (WS 1898/99) for the first time. In the introduction to this course, HILBERT stressed once gain the affinity between geometry and the natural sciences, and the role of axiomatization in the mathematization of the latter. He compared the two domains with the following words:

> 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 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 the 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

[58] Quoted in TOEPELL 1986, 94.
[59] See TOEPELL 1986, 55–57.
[60] See TOEPELL 1986, 204.

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.[61]

We thus find in this lecture the first explicit presentation of HILBERT's program for axiomatizing natural science in general. The 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. In the manuscript of his 1899 course on Euclidean geometry we also find HILBERT's explicit and succinct characterization of geometry as part of natural science, in the following words: "Geometry is the most perfect of (*vollkommenste*) the natural sciences".[62]

5. *Grundlagen der Geometrie*

The turn of the century is often associated in the history of mathematics with two landmarks in HILBERT's career: the publication of the *Grundlagen der Geometrie* and the 1900 lecture held in Paris at the International Congress of Mathematicians. Both events are relevant to the present account and we will discuss them briefly now.

The *Grundlagen der Geometrie* appeared in June 1899 as part of a *Festschrift* issued in Göttingen in honor of the unveiling of the GAUSS–WEBER monument. It consisted of an elaboration of the first course taught by HILBERT in Göttingen on the foundations of Euclidean geometry, in the winter semester of 1898–99. The very announcement of this course had come as a surprise to many in Göttingen,[63] since HILBERT's interest in this mathematical domain signified, on

[61] HILBERT 1898/9, 1–3 (Emphasis in the original) : "Auch die Geometrie ist aus der Betrachtung der Natur, aus der Erfahrung hervorgegangen und insofern eine Experimentalwissenschaft. . . . Aber diese experimentellen Grundlagen sind so unumstösslich und so allgemein anerkannt, haben sich so überall bewährt, dass es einer 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 Maasse 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."

[62] Quoted in TOEPELL 1986, vii: "Geometrie ist die vollkommende Naturwissenschaft."

[63] Cf. BLUMENTHAL 1935, 402: "Das erregte bei den Studenten Verwunderung, denn auch wir älteren Teilnehmer an den 'Zahlkörperspaziergängen' hatten nie gemerkt, daß Hilbert sich mit geometrischen Fragen beschäftigte: er sprach uns nur von Zahlköpren."

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.[64] As we have already seen, the issue had occupied HILBERT's thoughts at least since 1891, when he first taught projective geometry in Königsberg; but it was SCHUR's 1898 proof of the PAPPUS theorem without recourse to continuity that made HILBERT concentrate all his efforts on the study of the foundations of geometry.[65] It was then that he embarked 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. A main concern of this whole effort was the unsettled issue of the coordinatization of projective geometry — the connecting link between synthetic and analytic geometry — and of the specific role of continuity assumptions in the proof of the fundamental theorems. HILBERT's axiomatic method appeared as a powerful and effective tool for addressing these important issues properly.

The 1899 *Festschrift* was the first full-fledged version of HILBERT's axiomatic treatment of geometry, but by no means the last. In spite of all the rigor claimed for this axiomatic analysis, 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 to attain all the goals of this demanding project. Still it must be stressed that all 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 the *Grundlagen*. It would be well beyond the scope of the present article to discuss all the details of the *Grundlagen*, and how it addressed the main foundational questions of geometry.[66] But since this is HILBERT's first articulate, thorough presentation of a particular theory in axiomatic terms, it is clearly relevant to comment on some features of this work, and in particular on the kind of questions systematically addressed here by HILBERT and thus established for any future axiomatic study.

In line with his earlier pronouncements concerning the role of axiomatization in geometry as well as in other physical theories, HILBERT described the aim of his *Festschrift* as an attempt to lay down a "simple" and "complete"

[64] For instance, in his obituary lecture on HILBERT, HERMANN WEYL wrote (1994, 635): "[T]here could not have been a more complete break than the one dividing Hilbert's last paper on the theory of number fields from his classical book Grundlagen der Geometrie."

[65] For the events around the publication of Schur's proof and its effect on Hilbert, see TOEPELL 1986, 114–122.

[66] This is precisely a main contribution of TOEPELL 1986. See especially, pp. 143–236.

system of "mutually independent" axioms,[67] from which all known theorems of geometry might be deduced. HILBERT's axioms for geometry — formulated for three systems of undefined objects (and named "points", "lines" and planes") — establish mutual relations to be satisfied by these objects. These axioms are divided into five groups (axioms of incidence, of order, of congruence, of parallels and of continuity), but the groups have no pure logical significance in themselves. Rather they reflect HILBERT's actual conception of the axioms as an expression of our spatial intuition: each group expresses a particular way in which these intuitions manifest themselves.

HILBERT's requirement for independence of the axioms is the direct manifestation of the foundational concerns that directed his research. When analysing 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 geometry are logically interrelated. But as we have seen, this requirement had already appeared — more vaguely formulated — in HILBERT's early lectures on geometry, as a direct echo of Hertz's demand for appropriateness. In the *Grundlagen*, independence of axioms not only appeared as a more clearly formulated requirement, but HILBERT also provided the tools to prove systematically the mutual independence among the individual axioms within the groups and among the various groups of axioms in the system. He did so by introducing the method that has since become standard: he constructed models of geometries which fail to satisfy a given axiom of the system but satisfy all the others. It is important to stress that HILBERT's study of mutual independence focused on geometry itself rather than on the abstract relations embodied in the axioms; the *Grundlagen* was by no means a general study of the abstract relations between systems of axioms and their possible models. It is for this reason that HILBERT's original system of axioms was not — from the logical point of view — the most economical possible one. In fact, several mathematicians noticed quite soon that HILBERT's system of axioms, seen as a single collection rather than as five groups, contained a certain degree of redundancy.[68] HILBERT's own aim was to establish the interrelations among the groups of axioms rather than among individual axioms belonging to different groups.

The requirement of simplicity had also been explicitly put forward by HERTZ; it complements that of independence. It means, roughly, that an axiom should contain 'no more than a single idea'. This requirement is mentioned in

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[67] See HILBERT 1899, 1 (Emphasis in the original): " . . . ein *einfaches* und *vollständiges* System von einander unabhängiger Axiome aufzustellen . . ."

[68] 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 the *Grundlagen* 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 may be derived from the others. See also CORRY 1996, § 3.5; TORRETTI 1978, 239 ff.

HILBERT's introduction, but it was neither explicitly formulated nor otherwise realized in any clearly identifiable way in the *Grundlagen*. It was present, however, in an implicit way and remained here — as well as in other, later works — as an aesthetic desideratum for axiomatic systems, which was not transformed into a mathematically controllable feature.[69]

The "completeness" that HILBERT demanded for his system of axioms runs parallel to HERTZ's demand for correctness.[70] Very much like HERTZ's stipulation for correct images, HILBERT required 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 the *Grundlagen*, the author claimed, would allow all the known results of Euclidean, as well as of certain non-Euclidean, geometries to be elaborated from scratch, depending on which groups of axioms were admitted.[71] 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 theorems of Desargues and the theorem of PASCAL. HILBERT's analysis allowed a clear understanding of the actual premises necessary for coordinatizing projective geometry, which, as already stressed, was a key step in building the bridge between the latter and other kinds of geometry and a main concern of HILBERT. HILBERT's results implied, for instance, that these two fundamental theorems are valid in Euclidean geometry, as well as in a non-Archimedean geometry, such as the one introduced earlier by VERONESE.[72]

Unlike independence, 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 proving. In the case of Euclidean geometry, it seemed to HILBERT that it

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[69] As will be seen below, in his 1905 lectures on the axiomatization of physics, HILBERT explicitly demanded the simplicity of the axioms for physical theories. It should also be remarked that in a series of investigations conducted in the USA in the first decade of the present century under the influence of the *Grundlagen*, a workable criterion for simplicity of axioms was still sought after. For instance, EDWARD HUNTINGTON (1904, p. 290) included simplicity among his requirements for axiomatic systems, yet he warned that "the idea of a simple statement is a very elusive one which has not been satisfactorily defined, much less attained."

[70] And, importantly, it should not be confused with the later, model-theoretical notion of completeness, which is totally foreign to HILBERT's early axiomatic approach.

[71] Several important changes concerning the derivability of certain theorems appeared in the successive editions of the *Grundlagen*. I do not mention them here, as they are not directly relevant to the main concerns of this article.

[72] However, there were many subsequent corrections and additions, by HILBERT as well as by others, that sharpened still further the picture put forward by HILBERT in the first edition of the *Grundlagen*. A full account of the *Grundlagen* would require a detailed discussion of the differences between the successive editions. TOEPELL 1986, 252, presents a table summarizing the interconnections between theorems and groups of axioms as known by 1907. See also FREUDENTHAL 1957 for later developments.

was enough to show that the specific synthetic geometry derivable from his axioms could be translated into the standard Cartesian geometry (with the whole field of real numbers as axes).

The question of the consistency of the various kinds of geometries was an additional concern of HILBERT's analysis, but it is not explicitly mentioned in the introduction to the *Grundlagen.* He addressed this issue in the *Festschrift* immediately after introducing all the groups of axioms and after discussing their immediate consequences. Seen from the point of view of HILBERT's later meta-mathematical research and the developments that followed it, the question of consistency appears as the most important one undertaken in the *Grundlagen*; but in the historical context of the evolution of his ideas it certainly was not. In fact, the consistency of the axioms is discussed in barely two pages, and it is not immediately obvious why HILBERT addressed it at all. It doesn't seem likely that in 1899 HILBERT would envisage the possibility that the body of theorems traditionally associated with Euclidean geometry might contain contradictions. Euclidean geometry, after all, was for HILBERT a natural science whose subject matter is the properties of physical space. HILBERT seems rather to have been echoing here HERTZ's requirements for scientific theories, in particular his demand for the permissibility of images. As seen above, HILBERT had stressed in his lectures — following an idea of HERTZ — that the axiomatic analysis of physical theories was meant to clear away any possible contradictions brought about over time by the gradual addition of new hypotheses to a specific theory. Although this was not likely to be the case for the well-established discipline of geometry, it might still happen that the particular way in which the *axioms* had been formulated in order to account for the theorems of this science led to statements that contradict each other. The recent development of non-Euclidean geometries made this possibility only more patent. Thus, HILBERT believed that in the framework of his system of axioms for geometry he could also easily show that no such contradictory statements would appear.

As is well-known, HILBERT established through the *Grundlagen* the relative consistency of geometry vis-à-vis arithmetic, i.e., he proved that any contradiction existing in Euclidean geometry must manifest itself in the arithmetic system of real numbers. He did this by defining a hierarchy of fields of algebraic numbers. It is significant that in the first edition of the *Grundlagen,* HILBERT contented himself with constructing a model that satisfied all the axioms, using only a proper sub-field, rather than the whole field of real numbers (HILBERT 1899, 21). It was only in the second edition of the *Grundlagen,* published in 1903, that he added an additional axiom, the so-called "axiom of completeness" (*Vollständigkeitsaxiom*); the latter was meant to ensure that, although infinitely many incomplete models satisfy all the other axioms, there is only one complete model that satisfies this last axiom as well, namely, the usual Cartesian geometry, obtained when the whole field of real numbers is used in the model (HILBERT 1903, 22–24). Moreover, as HILBERT stressed, this axiom cannot be derived from the Archimedean axiom, which was the only one included in the continuity group in the first edition. 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.[73]

The question of the consistency of geometry was thus reduced to that of the consistency of arithmetic. The further necessary step of proving the latter was not even mentioned in the *Festschrift*, and presumably at the time of its publication HILBERT did not yet consider that such a proof could involve a difficulty of principle. Soon, however, he was to assign a high priority to it as an important open problem of mathematics. Thus, among the 1900 list of twenty-three problems, upon which I will comment in a moment, the second one concerns the proof of the "compatibility of arithmetical axioms."[74] In fact, as early as October 1899 HILBERT delivered a lecture in Munich to the *DMV* — later published as "*Über den Zahlbergriff*" — in which he spoke explicitly for the first time about the need to prove the consistency of arithmetic, and proposed a system of axioms for this domain. This system essentially reproduced the properties of the "systems of complex numbers" that HILBERT had used in constructing his various models in the *Grundlagen*. Beyond his book on the foundations of geometry, this was his only other early publication connected with the application of the axiomatic method and, interestingly enough, in spite of the central role he accorded to this method, HILBERT emphasized here that he did not see it as the only possible one. He discussed two different ways of dealing with concepts in mathematics: the genetic approach and the axiomatic approach. The classical example of the possibility of defining a mathematical entity genetically is provided by the system of real numbers. On the other hand, there is the axiomatic method, typically used in geometry. HILBERT claimed that both tendencies usually complement each other in mathematics, but he raised the question as to their relative value. Finally he stated his opinion:

> In spite of the high pedagogic value of the genetic method, the axiomatic method has the advantage of providing a conclusive exposition and full logical confidence to the contents of our knowledge. (HILBERT 1900, 184)[75]

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[73] The axiom is formulated in HILBERT 1903, 16. TOEPELL 1986, 254–256, briefly describes the relationship between HILBERT's *Vollständigkeit* axiom 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.

[74] HILBERT 1901, 299–300. As is well-known, KURT GÖDEL (1906–1978) proved in 1931 that such a proof is impossible in the framework of arithmetic itself.

[75] It is worth pointing out that in one of his letters (January 6, 1900), FREGE expressed his agreement with the view expressed by HILBERT in this talk. See GABRIEL et al. (eds.) 1980, 44.

With this article HILBERT set forth the guidelines for applying to arithmetic the kind of axiomatic analysis he had formerly applied to geometry. At the same time, he suggested that some of the problems raised by the introduction of transfinite cardinals might be solved by applying the same kind of axiomatic analysis to the concept of set. On the contrary, he did not connect such questions and procedures in any way with concerns of methodology or logic.

These are, then, HILBERT's main requirements concerning the axiomatic systems that define geometry: completeness, consistency, independence, and simplicity. *In principle*, there should be no reason why a similar analysis could not apply for any given system of postulates that establishes mutual abstract relations among undefined elements arbitrarily chosen in advance and having no concrete mathematical meaning. But *in fact*, HILBERT's own conception of axiomatics did not convey or encourage the formulation of abstract axiomatic systems as such: his work was instead directly motivated by the need for better understanding of mathematical and scientific theories. In HILBERT's view, the definition of systems of abstract axioms and the kind of axiomatic analysis described above was meant to be carried out, retrospectively, for 'concrete', *well-established and elaborated* mathematical entities. In this context, one should notice that in the years immediately following the publication of the *Grundlagen*, several mathematicians, especially in the USA, undertook an analysis of the systems of abstract postulates for algebraic concepts such as groups, fields, Boolean algebras, etc., based on the application of techniques and conceptions similar to those developed by HILBERT in his study of the foundations of geometry.[76] There is no evidence that HILBERT showed any interest in this kind of work, and in fact there are reasons to believe that they implied a direction of research that HILBERT did not contemplate when putting forward his axiomatic program. It seems safe to assert that HILBERT even thought of this direction of research as mathematically ill-conceived.[77]

A commonly accepted image of twentieth-century mathematics depicts it as a collection of theories actually constructed on systems of postulates that establish arbitrary abstract relations among undefined elements, and that frequently lack a direct, concrete intuitive meaning. In fact, according to this image, the profusion of theories of this kind in contemporary mathematical research should be seen as evidence of the success and influence of HILBERT's own point of view and as one of his main contributions to shaping contemporary mathematical thinking. The impact of HILBERT's axiomatic research, coupled with the "formalism" associated with his name in the framework of the so-called "foundational crisis" of the 1920s, has occasionally been seen as promoting the

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[76] For instance MOORE 1902a, HUNTINGTON 1902.

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

view of mathematics as an empty, formal game.[78] HILBERT's own axiomatic research, however, was never guided by such a view — certainly not in the early stages of its development — and in fact he often opposed it explicitly. Thus, for instance, in a course taught as late as 1919, and aware of existing misconceptions concerning the nature of mathematical science, HILBERT explained to a general audience his views on this issue and on the role played by axiomatic definitions:

> [Mathematics] has nothing to do with arbitrariness. Mathematics is in no sense like a game, in which certain tasks are determined by arbitrarily established rules. Rather, it is a conceptual system guided by internal necessity, that can only be so, and never otherwise.[79]

6. The Frege–Hilbert Correspondence

An additional, important early source for understanding HILBERT's axiomatic conception is found in an oft-cited exchange of letters with GOTTLOB FREGE (1846–1925), immediately following the publication of the *Grundlagen.*[80] Historians and philosophers have devoted considerable attention to this correspondence, especially for the debate it contains between HILBERT and FREGE concerning the nature of mathematical truth. HILBERT expressed here the view that the axiomatic research of mathematical theories not only confers a greater degree of certainty on existing knowledge, but also provides mathematical concepts with justification, and indeed with their very existence. This view, which equates mathematical truth with logical consistency, provided *a-posteriori* legitimacy to proofs of existence by contradiction, like the one advanced in 1893 by HILBERT himself for the finite basis theorem of algebraic invariants.[81] But this frequently-emphasized issue is only one side of a more complex picture advanced by HILBERT in his letters. In the first place, HILBERT explicitly stated that his motivations were different from FREGE's. Axiomatic research, HILBERT stated, was not for him an end in itself with inherent justification, but rather a tool to achieve a clearer understanding of mathematical theories. The need to

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[78] A typical instance of such a view appears in RESNIK 1974, 389: " [HILBERT's conception] removed the stigma of investigating axioms which do not describe any known 'reality' and opened the way to the creation of new mathematical theories by simply laying down new axioms." See also REID 1970, 60–64.

[79] HILBERT 1992, 14. For HILBERT's views on the role of *Anschauung*, as opposed to formal manipulation of empty concepts, in his system of geometry see also TOEPELL 1986, 258–261.

[80] The relevant letters between HILBERT and FREGE appear in GABRIEL et al. (eds.) 1980, esp. pp. 34–51. For comments on this interchange see BOOS 1985; MEHRTENS 1990, 117 ff.; PECKHAUS 1990, 40–46; RESNIK 1974.

[81] See CORRY 1996, § 3.1.

undertake axiomatic analysis was forced upon him, as it were, by problems HILBERT had found in his day-to-day mathematical research. Thus in a letter dated December 29, 1899, HILBERT wrote to FREGE:

> If we want to understand each other, we must not forget that the intentions that guide the two of us differ in kind. It was of necessity that I had to set up my axiomatic system: I wanted to make it possible to understand those geometrical propositions that I regard as the most important results of geometrical enquiries: that the parallel axiom is not a consequence of the other axioms, and similarly Archimedes' axiom, etc. ... I wanted to make it possible to understand and answer such questions as why the sum of the angles in a triangle is equal to two right angles and how this fact is connected with the parallel axiom.[82]

In this same letter HILBERT explained his well-known view concerning the relationship between axioms and truth. Expressing his disagreement with what FREGE had written in an earlier letter, HILBERT claimed that "if the arbitrarily given axioms do not contradict one another with all their consequences, then they are true and the things defined by the axioms exist. This is for me the criterion of existence and truth."[83] Clear and concise as it is, this statement in no way implies that HILBERT's own axioms of geometry were *actually* arbitrary!

In answering this letter, FREGE summarized HILBERT's position as follows: "It seems to me that you want to detach geometry from spatial intuition and to turn it into a purely logical science like arithmetic."[84] As HILBERTS's reply contained just a few lines and no substantial content (on account, he said, of overburden with work),[85] we know of no direct response from HILBERT to FREGE's characterization of HILBERT's aims. HILBERT had indeed stated that a thorough axiomatization of geometry would allow all its theorems to be derived without direct reliance on intuition. But it is essential to recall that for HILBERT, as for PASCH before him, *the axioms themselves* are not detached from spatial intuition, but rather are meant to fully capture it and account for it. Thus, contrary to FREGE's characterization, HILBERT's aim was to detach the *deduction* (but only the deduction) of geometrical theorems from spatial intuition, i.e., to avoid the need to rely on intuition when deriving the theorems from the axioms. But at the same time, by choosing correct axioms that reflect spatial intuition, HILBERT was aiming, above all, at strengthening the effectiveness of geometry as the science — the *natural* science, one should say — of space.

In the same letter, FREGE also commented upon HILBERT's proofs of independence. He thought HILBERT's technique adequate and valuable, but he warned that it would be far less interesting if applied to arbitrary systems of axioms. He thus wrote:

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[82] Quoted in GABRIEL et al. (eds.) 1980, 38.

[83] Quoted in GABRIEL et al. (eds.) 1980, 39.

[84] In a letter from Jena, dated January 6, 1900. Quoted in GABRIEL et al. (eds.) 1980, 43.

[85] HILBERT to FREGE, January 15, 1900. Quoted in GABRIEL et al. (eds.) 1980, 48.

> The main point seems to me to be that you want to place Euclidean geometry under a higher point of view. And indeed, the mutual independence of the axioms, if it can be proved at all, can only be proved in this way. Such an undertaking seems to me to be of the greatest scientific interest if it refers to the axioms in the old traditional sense of the elementary Euclidean geometry. If such an undertaking extends to a system of propositions which are arbitrarily set up, it should *in general* be of far less scientific importance.[86]

Again, we are lacking HILBERT's reply to this particular qualm of FREGE. But from all that we do know, there is no reason to believe that he would have disagreed with him on this point. As already said, HILBERT expressed no direct interest in postulational research that considered the analysis of abstract systems of axioms as such as a domain of inquiry with inherent mathematical value. In fact, in this discussion the insistence on arbitrary, rather than on concrete, axiomatic systems seems to have come here from FREGE rather than from HILBERT. One may wonder, then, to what extent FREGE's reading of HILBERT's enterprise has helped to spread a different image of HILBERT's conceptions from that revealed by HILBERT's own writing.

A second, frequently overlooked, trait of this correspondence — one that is of particular interest for the present account — concerns the kind of difficulties reported by HILBERT as having motivated the development of his axiomatic outlook. These difficulties were found by HILBERT mainly in *physical*, rather than mathematical theories. HILBERT's explanations here show a clear connection to similar concerns expressed by HERTZ in stressing the need to analyze carefully the addition of ever new assumptions to physical theories, so as to avoid possible contradictions. They also help us to understand many of HILBERT's later endeavours in physics. In the same letter of December 29, he wrote:

> After a concept has been fixed completely and unequivocally, it is on my view completely illicit and illogical to add an axiom — a mistake made very frequently, especially by physicists. By setting up one new axiom after another in the course of their investigations, without confronting them with the assumptions they made earlier, and without showing that they do not contradict a fact that follows from the axioms they set up earlier, physicists often allow sheer nonsense to appear in their investigations. One of the main sources of mistakes and misunderstandings in modern physical investigations is precisely the procedure of setting up an axiom, appealing to its truth (?), and inferring from this that it is compatible with the defined concepts. One of the main purposes of my *Festschrift* was to avoid this mistake.[87]

In a different passage of the same letter, HILBERT commented on the possibility of replacing the basic objects of an axiomatically formulated theory by a different system of objects, provided the latter can be put in a one-to-one, invertible

[86] Quoted in GABRIEL et al. (eds.) 1980, 44. Italics in the original.

[87] Quoted in GABRIEL et al. (eds.) 1980, 40. The question mark "(?) " appears in the German original (after the word "Wahrheit").

relation with the former. In this case, the known theorems of the theory are equally valid for the second system of objects. Concerning physical theories, HILBERT wrote:

> All the statements of the theory of electricity are of course valid for any other system of things which is substituted for the concepts magnetism, electricity, etc., provided only that the requisite axioms are satisfied. But the circumstance I mentioned can never be a defect in a theory [footnote: it is rather a tremendous advantage], and it is in any case unavoidable. However, to my mind, the application of a theory to the world of appearances always requires a certain measure of good will and tactfulness: e.g., that we substitute the smallest possible bodies for points and the longest possible ones, e.g., light-rays, for lines. At the same time, the further a theory has been developed and the more finely articulated its structure, the more obvious the kind of application it has to the world of appearances, and it takes a very large amount of ill will to want to apply the more subtle propositions of [the theory of surfaces] or of Maxwell's theory of electricity to other appearances than the ones for which they were meant . . .[88]

HILBERT's letters to FREGE show very clearly, then, the direct motivation of his axiomatic point of view. That point of view in no sense involved either an empty game with arbitrary systems of postulates nor a conceptual break with the classical entities and problems of mathematics and empirical science. Rather it sought an improvement in the mathematician's understanding of the latter.

7. The 1900 List of Problems

The next occasion in which HILBERT explained his views concerning the centrality of axiomatics as a vehicle for defining mathematical concepts and as the source of mathematical truth was a very special one. In fact, it was an opportunity to explain to a selected audience many of his ideas about mathematics in general; it came in 1900, at the occasion of the Second International Congress of Mathematicians held in Paris. By the time of the congress, HILBERT's mathematical reputation was so well established that he was invited to deliver one of the main talks. Following a suggestion of MINKOWSKI, HILBERT decided to provide a glimpse into what — in his view — the new century would bring for mathematics. This he did by presenting a list of problems which he considered to pose significant challenges that would lead mathematicians trying to solve them to fruitful research and to new and illuminating ideas.

In presenting the problems, HILBERT was trying to establish, as it were, a research program for the entire mathematical community for years to come. At the same time he was making a clear statement: a wealth of significant open problems is a necessary condition for the healthy development of any mathematical branch and, more generally, of that living organism that he took

[88] Quoted in GABRIEL et al. (eds.) 1980, 41. I have substituted here "theory of surfaces" for "Plane geometry", which was the English translator's original choice. In the German original the term used is "Flächentheorie".

mathematics to be.[89] From HILBERT's remarks on this issue one can also learn much about the central place he accorded to empirical motivations as a main source of nourishment for that organism. In fact, HILBERT made clear once more the close interrelation that, in his mind, underlies mathematics and the physical sciences (HILBERT 1902, 440). In particular, the quest for rigor in analysis and arithmetic should be extended to geometry and the physical sciences, not only because it would perfect our understanding, but also because its results would provide mathematics with ever new and fruitful ideas. Commenting on the opinion that geometry, mechanics and other physical sciences are beyond the possibility of a rigorous treatment, he wrote:

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

HILBERT described the development of mathematical ideas — using terms very similar to those of VOLKMANN's 1900 book — 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.[90] Moreover, using the "building metaphor", he stressed 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:

> . . . the study of the foundations of a science is always particularly attractive, and the testing of the foundations will always be among the foremost problems of the investigator . . . [But] a thorough understanding of its special theories is necessary to 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. (HILBERT 1902, 455)

Speaking more specifically about the importance of problems for the healthy growth of mathematics, HILBERT characterized an interesting problem as one which is "difficult in order to entice us, yet not completely inaccessible, lest it mock at our efforts (p. 438)." 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

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[89] See especially the opening remarks in HILBERT 1902, 438. See also his remarks on p. 480.

[90] 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.

a finite number of hypotheses which are implied in the statement of the problem and which must always be exactly formulated (p. 441)." On this occasion HILBERT also expressed his celebrated opinion that every mathematical problem can indeed be solved: "In mathematics there is no *ignorabimus* (p. 445)."

This is not the place to discuss in detail the list of problems and its historical context.[91] Our main concern here is with the sixth problem on the list. But before coming to it, one must stress that HILBERT's concern with axiomatization, as part of the much more general tasks he envisaged for mathematics in the future, was expressed succinctly as part of the second problem on the list. In formulating this problem — which called for the proof of the consistency of arithmetic — HILBERT described once again his views concerning the relation between logical consistency and mathematical truth. HILBERT 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 finite number of logical steps. (HILBERT 1902, 447)

The sixth problem on the list is directly connected to the general view expressed here by HILBERT. The problem called for the axiomatization of physical science. HILBERT wrote as follows:

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

HILBERT mentioned several existing works as examples of what he had in mind here: the fourth edition of MACH's *Die Mechanik in ihrer Entwicklung*, HERTZ's *Principles*, BOLTZMANN's 1897 *Vorlesungen über die Principien der Mechanik*, and also VOLKMANN's 1900 *Einführung*. BOLTZMANN's work offered a good example of what axiomatization would offer. BOLTZMANN 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 which HILBERT considered of the highest theoretical importance.[92]

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[91] For one such discussion see ROWE 1996.

[92] More on HILBERT's appreciation of BOLTZMANN's work, below.

Together with these well-known works on mechanics, HILBERT also mentioned a recent work by the Göttingen actuarial mathematician GEORG BOHLMANN (1869–1928) on the foundations of the calculus of probabilities.[93] 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. This point will be explained in some detail below, when studying the contents of HILBERT's 1905 lectures.

Modeling 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 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 for the axioms (p. 445).

At the beginning of this article, I claimed that this sixth problem is different from the others in the list. Now the differences can be more clearly described. In the first place, it is not really a problem in the strict sense of the word, but rather a general task for whose complete fulfillment HILBERT set no clear criteria. This is the more striking given HILBERT's detailed account, in the opening remarks to 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. Clearly, this particular problem does not fit his criteria. Second, on the evidence of HILBERT's published work alone, it would be difficult to understand the place of this project as part of HILBERT's general conception of mathematics and of his work up to that time. Although beginning in 1912 HILBERT was to publish important work related to mathematical physics, before 1900 his published works show no clue to this kind of interest. Moreover, unlike most of the other items in the list, this is not the kind of issue that mainstream mathematical research had been pointing to in past years.

But at the same time, and in spite of its peculiar character, the sixth problem has also 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

[93] BOHLMANN 1900. This article reproduced a series of lectures delivered by BOHLMANN in a *Ferienkurs* in Göttingen. In his article BOHLMANN referred the readers, for more details, to the chapter he had written for the *Encyclopädie der mathematischen Wissenschaften* on insurance mathematics. BOHLMANN's axioms will be further discussed below.

analytic?"), the twentieth, closely related to the former and at the same time to HILBERT's long-standing interest in the domain of validity of the Dirichlet principle (dealing with the existence of solutions to partial differential equations with given boundary conditions) and 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 solution. All these three problems are also strongly connected to physics, though unlike the sixth, they are also part of mainstream, traditional research concerns in mathematics.[94] 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 illustrate how HILBERT conceived the role of variational principles in his program for axiomatizing physics.

For all its differences and similarities with other problems in 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 for 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, a detailed examination of HILBERT's 1905 lectures shows a very clear and comprehensive conception of how that project 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. Interestingly, the development of physics from the beginning of the century, and especially *after* 1905, brought about many surprises that HILBERT could not have envisaged in 1900 or even when lecturing 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. With this picture in mind, it is now time to turn to the examination of HILBERT's 1905 lectures on the axiomatic method.

8. Hilbert's 1905 Lectures on the Axiomatic Method

As we have seen in the preceding sections, the axiomatization of mathematical and scientific disciplines posed for HILBERT a meaningful mathematical challenge that attracted his attention in the same way as many other open

[94] For a detailed account of the place of variational principles in HILBERT's work, see BLUM 1994 (unpublished).

mathematical problems did. This was, in particular, his attitude to the proof of the consistency of arithmetic, as well as to the relation between the axioms of set theory and the continuum hypothesis. Until 1903, the main focus of HILBERT's axiomatic interest continued to be the foundations of geometry. HILBERT used the terms "logic" and "logical" in a rather loose manner throughout his writings, and his attention was not specifically directed towards the more methodological and philosophical issues raised by the application of the axiomatic method. In 1903, however, an important change of direction occurred, following RUSSELL's publication of the paradox arising in FREGE's logical system. Although contradictory arguments of the kind discovered by RUSSELL had been made known in Göttingen a couple of years earlier by ZERMELO,[95] it seems that RUSSELL's publication led HILBERT to attribute to the axiomatic analysis of logic and of the foundations of set theory a much more central role in establishing the consistency of arithmetic than he had earlier. Beginning in 1903, intense activity developed in Göttingen in this direction: it was at this time, that the systematic study of logic and set theory as a central issue in the foundations of mathematics was initiated in HILBERT's mathematical circle.[96]

The first published evidence of this change of orientation and emphasis appeared in a lecture delivered by HILBERT at 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" (1905a), HILBERT called for a "partly simultaneous development of the laws of logic and arithmetic." He presented his ideas in a very sketchy formulation, which he only developed later in greater detail in a course delivered in the summer semester of 1905 in Göttingen, under the name of "The Logical Principles of Mathematical Thinking."[97] There HILBERT attempted to develop a formalized calculus for prepositional logic, one that would provide the basis on which to reconstruct the logical foundations of mathematics — the project that was then gradually beginning to draw his attention. HILBERT's logical calculus was rather rudimentary, and it did not even account for quantifiers. As a strategy for proving consistency of axiomatic systems, it could only be applied to very elementary cases. At that same time, ZERMELO was working by himself on the proof of the consistency of arithmetic and on the axiomatization of set theory, following the guidelines established by HILBERT. HILBERT was confident of ZERMELO's ability to tackle the whole problem of foundations as he now conceived it, and in fact, it was for this purpose that he made efforts to bring him to Göttingen and keep

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[95] PECKHAUS 1990, 48–49.

[96] PECKHAUS 1990, 56–57.

[97] In what follows, I transcribe in the footnotes some relevant passages of this unpublished manuscript (HILBERT 1905). The reference to the original pagination in the manuscript is given here in square brackets. Texts are underlined or crossed-out as in the original. Later additions by HILBERT appear between ⟨ ⟩ signs. There is a second manuscript of these lectures in HILBERT's *Nachlass* in the Niedersächsischen Staats- und Universitätsbibliothek Göttingen (Cod Ms D. HILBERT 558a), annotated by MAX BORN.

him there. After his 1905 course, HILBERT dedicated no further effort to such foundational studies, and ZERMELO was left alone to pursue the project. In 1908 ZERMELO published his well-known paper on the foundations of set-theory,[98] and he also gave a course in Göttingen in which he elaborated a new logical calculus of his own.[99]

HILBERT's 1905 course is of special interest for our present concerns, because in a lengthy section he presented axiomatic treatments of several physical disciplines. Thus, the manuscript of the lectures provides the first clear evidence of what HILBERT envisaged as the solution, or at least the way to the solution, of the sixth of his 1900 list of problems. The course was divided into two separate parts, of which the second developed the "logical foundations" of mathematics, including the logical calculus mentioned above.[100] The way to the issues discussed in the second part of the course was prepared in its first part, where 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. HILBERT summarized in the opening lectures the aims and basic tools of the axiomatic method, repeating what he had already said in former works: when analyzing an axiomatic system we are interested in studying the logical independence of its axioms, and their completeness, namely, that all the known theorems of the theory may be derived from the proposed system of axioms. This time he also mentioned explicitly the study of the consistency of the system as a main task of the axiomatic analysis. Our main focus will be on HILBERT's axioms for physical disciplines, but I will consider first some of the points he raised in his discussion of arithmetic and geometry. These points make very clear the empiricist underpinnings of HILBERT's conception of the axiomatic method, and the central role he accorded to intuition and experience in the construction of mathematical theories.

Arithmetic and Geometry

HILBERT's axioms for arithmetic, eighteen in number, repeated more or less what had appeared in "*Über den Zahlbegriff*". He discussed the Archimedean axiom this time at some length, stressing its importance for the application of mathematics to any measurement of physical quantities. In fact, HILBERT said, a most basic assumption of every science involving measurements is that all the physical magnitudes of a kind be mutually comparable, in the sense stipulated by the axiom. The whole science of astronomy, for instance, is based on the idea that celestial dimensions can be expressed in terms of terrestrial ones, by

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[98] ZERMELO 1908. A comprehensive account of the background, development and influence of ZERMELO's axioms see MOORE 1982. For an account of the years preceding the publication, see esp. pp. 155 ff.

[99] For an account of HILBERT's and ZERMELO's logical calculi, see PECKHAUS 1994.

[100] For a detailed account of this part of the course, see PECKHAUS 1990, 61–75.

straightforward, if somewhat lengthy, successive addition. HILBERT saw this commonplace assumption as far from trivial. It was precisely by means of axiomatic analysis that one could understand both its pervasiveness and the need to state it explicitly.[101] In fact, HILBERT continued to stress the importance of this axiom at every opportunity. For instance, in his 1918 article " *Axiomatisches Denken*", he returned to the analysis of the independence of this axiom, describing it as a very central one for both mathematical and physical theories. He stressed the lack of attention that its explicit formulation had received from physicists. Significantly, his remarks on the role of this axiom in physics underscore once again his empiricist conception of geometry. HILBERT wrote:

> In the theory of real numbers it is proven that the axiom of magnitude (*Messens*), the so-called Archimedean axiom, is independent of all other arithmetical axioms. This result is acknowledged as being of the utmost significance for geometry, but it seems to me that it is also so for physics, since it leads us to the following result: that the fact that by continually adding terrestrial distances, we are able to reach the distances of bodies in the outer spaces, i.e., that celestial distances can be measured by terrestrial measure, and likewise the fact that the distances in the internal parts of atoms are expressible in terms of meters, are in no way plain logical consequences of the theorem on the congruence of triangles or of geometric configurations, but rather results obtained from empirical research. The validity of the Archimedean axiom in nature must be confirmed by experiment in the same way that the theorem of the sum of angles in a triangle has been confirmed in a well-knwon manner. (HILBERT 1918, 149)

Before discussing the axioms of arithmetic, HILBERT mentioned the genetic method as the traditional way of creating the system of numbers, starting from the basic intuition of natural number (*Anzahl*). This method, he said, had usefully been applied by KRONECKER and WEIERSTRASS, for instance, in laying the foundations of the theory of functions. However, it raises some problems because, being based on a specific process of creation, it cannot always account for all the properties of the objects created. For example: the irrational numbers are defined as infinite sequences of integers [0, 14132 = (1, 4, 1, 3, 2, . . .)]. What properties can one expect these numbers to satisfy? If the sequence is defined by throwing a die, is the resulting sequence still an irrational number? The genetic method, concluded HILBERT, may find it difficult to answer questions of this kind.

Yet HILBERT clearly separated the purely logical aspects of the application of the axiomatic method from the "genetic" origin of the axioms themselves: the

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[101] [34] In jeder Wissenschaft, in der man die Zahlen anwenden will, muß sich so erst die Erkenntnis Bahn brechen, daß die Dinge, mit denen man es zu thun hat, gleichartig endlich und im Sinne von Ax. 17 durcheinander meßbar sind. So ist z.B. der Ausgangpunkt der Astronomie die Erkenntnis, daß man durch Aneinanderfügen irdischer Entfernungen die der Körper im Weltraume erreichen und übertreffen kann, d.h. daß man die himmelischen Entfernungen durch die irdischen messen kann.

latter is firmly grounded on 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, that has guided our construction of the axioms. He concluded this brief discussion by claiming that:

> 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.[102]

What this means for the axiomatization of geometry, then, is that its starting point must be given by the intuitive facts of that discipline,[103] 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.[104] 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 process systematically was recognized much later, the axiomatic presentation has been the traditional one. And if setting up a full axiomatic system has proven to be a truly difficult task for

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[102] [36] 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 zunächts ein Fachwerk von Begriffen zu errichten, bei dessen Aufsetllung 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.

[37] 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 aufzurichende 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 Notwedigkeit dieses Vorgehens viel später erkannt; dann aber wurde eine axiomatische Behandlung eher versucht, als ein 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.

[103] [37] . . . den Anlaß dazu durch die anschaulischen Thatsachen der Geometries geben lassen . . .;

[104] [37] . . . die Begriffe, die wir so erhalten, sind aber als gänzlich losgelöst von jeder Erfahrung und Anschauung zu betrachten.

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 wider.[105]

HILBERT's axioms for geometry in 1905 were based on the system of the *Grundlagen*, 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 certain 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.[106]

Although in his opinion, it is not for logic or mathematics to explain the reasons for this state of affairs, HILBERT plainly declared Euclidean geometry — as defined by his systems of axioms — to be the one and only geometry that fits our spatial experience.[107] But if that is the case, what is then 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 will still be introduced to the system of axioms that describes the 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 the *Grundlagen*. To conclude, he once more compared the situations in geometry and in physics: in the theory of electricity, for instance, new theories

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105 [37] . . . das Material an Erscheinungen noch viel größer ist.

106 [39] Daß wir gerade diese zu Elementardingen des begrifflichen Fachwerkes nechmen, ist willkürlich und geschieht nur wegen ihrer augenscheinlichen Einfachkeit; 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.

107 [67] Die Frage, wieso man in der Natur nur gerade die durch alle diese Axiome festgelegte Euklidische Geometrie braucht, bzw. warum unsere Erfahrung gerade in dieses Axiomsystem sich einfügt, gehört nicht in unsere mathematisch-logichen Untersuchungen.

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.

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.[108] Thus, the most advantageous way to study the relations prevailing in this situation is to apply the conceptual schemes provided by non-Euclidean geometry.[109]

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 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 ones 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,

[108] 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.

[109] [69] 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 Begriffschema der nichteuklidischen Geometrie bildet das System der Lichtwege in unserer Atmosphäre unter dem Einfluß deren variabler Dichte und Brechungsexponenten; machen wir [70] 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.

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.[110] 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 it corresponds to reality is a question that can be decided only by observation, and our analysis of the independence of the axioms allows to determine very precisely the minimal set of observations that need to be made in order to do so.[111] Later on, he added, the same kind of perspective must be adopted concerning physical theories, though its application will turn out to be much more difficult there than in geometry.

In concluding his treatment of geometry, and before his first specific treatment of a physical theory, HILBERT summarized the role of the axiomatic method in a passage which encapsulates his view of science and of mathematics as a living organism, whose development involves both an expansion in scope and an ongoing clarification of the logical structure of its existing parts.[112] 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, which strongly echoes an idea of VOLKMANN's already quoted above, 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.[113]

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[110] [98] In diesem Sinne und zu diesem Zwecke hat zuerst GAUß durch Messung an großen Dreiecken den Satz bestätigt.

[111] [98] Das Begriffsfachwerk der Geometrie selbst ist nach Erweisung seiner Widerspruchslosigkeit natürlich auch unabhängig von jeder Beobachtung matematisch existent; der Nachweis seiner Übereinstimung mit der Wirklichkeit kann nur durch Beobachtungen geführt werden, und die kleinste notwendige solcher wird durch die Unabhängigkeitsuntersuchungen gegeben.

[112] Elsewhere HILBERT called these two aspects of mathematics the "progressive" and "regressive" functions of mathematics, respectively (both terms not intended as value judgements, of course). See HILBERT 1992, 17–18.

[113] [102] Das Gebäude der Wissenschaft wird nicht aufgerichtet wie ein Wohnhaus, wo zuerst die Grundmauern fest fundiert werden und man dann erst zum Auf- und Ausbau der Wohnräume schreitet; die Wissenschaft zieht es vor, sich möglichst schnell wohnliche Räume zu verschaffen, in denen sie schalten kann, und erst nachträglich, wenn es sich zeigt, dass hier und da die locker gefügten Fundamente den Ausbau der Wohnräume nicht zu tragen vermögen, geht sie daran, dieselben zu stützen und zu befestigen. Das ist kein Mangel, sondern die richtige und gesunde Entwicklung.

Other places where HILBERT uses the building metaphor are HILBERT 1897, 67; HILBERT 1917, 148.

Mechanics

Mechanics is the first physical discipline whose axiomatization HILBERT discussed in 1905. The axiomatization of physics and of natural science, said HILBERT in opening this section of his lectures, is a task whose realization is still very far away.[114] Yet one particular issue for which the axiomatic treatment has been almost completely attained (and only very recently, for that matter) is the "law of the parallelogram" or, what amounts to the same thing, the laws of vector-addition. In the lectures, HILBERT based his own axiomatic presentation of this topic on works by GASTON DARBOUX (1842–1917), by GEORG HAMEL (1877–1954), and by one of his own students, RUDOLF SCHIMMACK.[115]

HILBERT's axiomatic treatment starts by defining a force as a three-component vector. HILBERT made no explicit additional 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, like 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 over the past decades in the treatment of physical theories.[116] In fact, in SCHIMMACK's article of 1903 — based on his doctoral dissertation — a vector was explictly 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. This addition — said HILBERT — is usually defined as the vector whose components are 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

114 [121] Von einem 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 wenige Fällen durchgeführt sind. ⟨Die Durchführung ist ein ganzes grosses Arbeitsprogramm, Vgl. Dissertation von SHIMMACK sowie SCHUR⟩.

115 The works referred to by HILBERT are DARBOUX 1875, HAMEL 1905, SCHIMMACK 1903. An additional related work, also mentioned by HILBERT in the manuscript, is SCHUR 1903.

116 The contributions of OLIVER HEAVISIDE (1850–1925), JOSIAH WILLARD GIBBS (1839–1903), and their successors, to the development of the concept of a vector space, in close connection with physical theories from 1890 on, are described in CROWE 1967, 150 ff.

geometry.[117] This result, as HILBERT had shown in his previous discussion on geometry, could be derived using all his axioms of geometry.

Having said that, HILBERT 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 (aA_x, aA_y, aA_z), 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.[118]

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.[119]

The sixth axiom concerns continuity:

6. Addition is a continuous operation, i.e., given a sufficiently small domain G around the endpoint 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.[120]

These are all simple axioms — continued HILBERT, 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

[117] [123] ... 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).

[118] [123] Addition zweier Vektoren derselben Richtung geschieht durch algebraische Addition der Strecken auf der gemeinsame Geraden.

[119] [124] Nimmt man eine Drehung D des Zahlenraumes um den gemeinsamen Anfangspunkt vor, so entsteht aus $A + B$ die Summe der aus A und aus B einzeln durch D entstehenden Vektoren:

$$D(A + B) = DA + DB.$$

d.h. die relative Lage von Summe und Komponenten ist gegenüber allen Drehungen invariant.

[120] [124] Zu einem genügend kleiner Gebiete G um den Endpunkt von $A + B$ kann man stets um die Endpunkte von A und B solche Gebiete G_1 and G_2 abgrenzen, daß der Endpunkt der Summe jedes im G_1 u. G_2 endigenden Vectorpaares nach G fällt.

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 to that of geometry, and is formulated as it is done in geometry.[121]

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. In his 1903 article, SCHIMMACK proved 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 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 — concluded HILBERT — if we assume that all functions appearing in the natural sciences have at least one continuous derivative, and take this assumption as an even more basic axiom, then vector addition is defined by reference to only the four first axioms in the system.[122]

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 — said HILBERT 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

121 [125]: . . . endlich kommt noch die Stetigkeitsforderung 6) hinzu (neben der schon durch annahmendes reellen Zahlensystems hinein gebrachten Stetigkeit), die noch ein besonderes Wort verdient. Das Axiom ist ganz analog formuliert und spielt dieselbe Rolle, wie das dritte Axiom in der zweiten Begründungsart der Geometrie . . . , das von der 'Abgeschlossenheit des Systemes der Bewegungen.'

122 [127] Nimmt man 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.

physical science (BOLTZMANN 1974, 228–229). 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.[123]

Experiment — continued HILBERT — compels us to place this axiom on top of every natural science, since it allows us to assert the validity of our assumptions and claims.[124] 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.

As we have already seen, elucidating the role of continuity in foundational issues had been among the main motivations behind HILBERT's interest in geometry. In physics, HILBERT also assigned a fundamental role to continuity, but one has to bear in mind the difference between the principle of continuity formulated above for physical theories, on the one hand, and the equivalent of the principle of continuity in HILBERT's geometry, i.e., the Archimedean axiom, on the other hand HILBERT himself was not very careful in drawing this distinction in his lectures. As a point of interest, 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 can be fruitfully applied to physics.[125] 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 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

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[123] [125] 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 den vorgeschriebenen Grad überschreitet.

[124] [125] Das Experiment zwingt uns geradezu dazu, ein solches Axiom an die Spitze aller Wissenschaft zu setzen, denn wir können bei ihm stets nur das ~~Ein~~ ⟨Zu⟩treffen von Voraussetzung und Behauptung mit einer gewissen beschränkten Genauigkeit feststellen.

[125] [126] Rein mathematisch werden natürlich auch physikalische Axiomensysteme, die auf diese Stetigkeit Verzicht leisten, also eine 'nicht-Archimedische Physik' in erweiterten Sinne definieren, von hohen 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 in die Physik einzuführen.

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.[126] 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.[127] In fact, the articles by HAMEL and by SCHUR were published in the *Zeitschrift für Mathematik und Physik* – a journal that bore the explicit sub-title: "*Organ für angewandte Mathematik.*" This journal had been founded by OSCAR XAVIER SCHLÖMLICH (1823–1901) and by the turn of the century its editor was CARL RUNGE (1856–1927), a leading Göttingen applied mathematician.

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.[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 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

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[126] See, for instance, DORIER 1995, MOORE 1995.

[127] This point, which helps understanding HILBERT's conception of algebra, is discussed in detail in CORRY 1996, § 3.4. See also CORRY 1996a.

[128] [127] I. Kräfte mit denselben Angriffspunkten sind ihrer Summe (im obigen Sinne) aequivalent.
II. 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. ...
III. Ein starrer Körper befindet sich im Gleichgewicht, wenn die an ihn angreifenden Kräfte zusammengenommen der Null aequivalent sind.

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.[129] Following his usual methodology, HILBERT asked: Are these two independent facts given by intuition,[130] 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 earlier 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.[131] This is perhaps impractical, HILBERT said, but it certainly makes no logical difference. One can even take a discontinuous function for t, provided it is invertible and one-to-one,[132] though in general one does not want to deviate from the continuity principle, desirable for all the natural sciences.

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.[133] While this limitation is closely connected to the unidimensionality of time,[134] the issue of the uniform passage of time is an experimental fact, which has to be deduced, according to HILBERT, from mechanics alone.[135] The ensuing discussion of the uniform passage of time is somewhat obscure and, as usual, HILBERT gave no direct references for his sources. In the next paragraph, for the reader's information, I will render it as succinctly and faithfully as possible without claiming to explain HILBERT's meaning fully.

The obscurity of this discussion is connected to HILBERT's use of an argument according to which, if time flowed in a non-uniform manner then an essential difference between organic and inorganic matter would be reflected in the laws mechanics, which is not actually the case. HILBERT suggested that the

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129 [129] . . ihr gleichmäßiger Verlauf und ihre Eindimensionalität.

130 . . . anschauliche unabhängige Tatsachen.

131 [129] 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 unregelmäßiger gang des Zeiger hinauskommen.

132 One is reminded here of a similar explanation, though in a more general context, found in HILBERT's letter to FREGE, on December 29, 1899. See GABRIEL et al. 1980, 41.

133 [129] ~~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.

134 Here HILBERT adds with his own handwriting: [130] ⟨Astronomie! Wie wichtig wäre Beobachtungen in ferner Vergangenheit u. Zukunft!⟩

135 [130] . . . eine experimentelle nur aus der Mechanik zu entnehmende Tatsache.

essence of the uniform passage of time may be explained by focusing on the differential expression $m.\frac{d^2x}{dt^2}$. This product characterizes a specific physical situation, HILBERT said, 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[136] — 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 $\mu.\frac{d^2x}{d\tau^2}$, which would also depend on the kind of matter involved. What this means, HILBERT explained, is that each kind of substance would yield, under a suitable change of variables, different values of the "time", values that nevertheless still satisfy the standard equations of mechanics. Now, HILBERT continued, one could use the most common kind of matter in order to measure time;[137] then, when small variations of organic matter occurred along large changes in inorganic matter, clearly distinguishable non-uniformities in the passage of time would arise [?!].[138] But it is an intuitive (*anschauliche*) fact, indeed a mechanical axiom, that the expression $m.\frac{d^2x}{dt^2}$ always appears in the equations with *one and the same* parameter t, independently of the kind of substance involved. Thus, HILBERT concluded his argument, it is this fact which determines the uniform character of the passage of time.

Following this analysis of the basic ideas behind the concept of time, HILBERT repeated the 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 matter involved, in contrast to the characterization presented earlier in his lecture. This mechanics would, in most respects, be in accordance with 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.

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136 [130] ... die m_1, m_2 von der Zeit, vor allem aber von dem Stoffe abhängig sein können.

137 [130] ... der häufigste Stoff etwa kann dann zu Zeitmessungen verwandt werden.

138 [131] ... für uns leicht große scheinbare Unstetigkeiten der Zeit auftreten.

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 very 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:

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

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.*[139]

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.[140]

In his lectures, HILBERT explained in some detail how the Lagrangian equations of motion can be derived from this principle. But he also stressed that the Lagrangian equations could themselves be taken as axioms and set on 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,

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[139] A detailed account of the kind of criticism advanced by MACH, and before him by CARL NEUMANN and LUDWIG LANGE, appears in BARBOUR 1989, Chp. 12.

[140] For more detail on GAUSS's principle see LANCZOS 1986, 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". As will be seen below, HILBERT also discussed this latter method in subsequent lectures, but did not explicitly make any connection between GAUSS's two contributions.

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 form it, but it does not indicate what the *simplest* assumptions underlying the discipline considered may be.

All the discussion up to this point, said HILBERT, concerns the simplest and oldest systems of axioms of mechanics of point systems. 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 mobile and formulated as follows: "If a system is at rest and no forces are applied, then the system will remain at rest."[141]

Now the interesting question arises, HILBERT continued, how far we can develop the whole of mechanics by putting this law on top of it. 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.[142] 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."[143] 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 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,

[141] [137] Ist ein System in Ruhe und die Kräftefunction konstant (wirken keine Kräfte), so bleibt es in Ruhe.

[142] [138] Es läßt sich zeigen, daß unter allen den Bedingungen, die die Gleichgewichtsbedingungen liefern, wirklich Gleichgewicht eintritt.

[143] [138] Es folgt jedoch nicht, daß diese Bedingungen auch notwendig für das Gleichgewicht sind, daß nicht etwa auch unter andern Umständen ein mechanisches System im Gliechgewicht 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 der Unmöglichkeit des Perpetuum mobile gemäß in Ruhe ist.

equilibrium obtains between the force and the acceleration. In this case we obtain a very systematic and simple derivation of the Lagrange 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, which specifies certain conditions on the kinetic energy, thus directly yielding the velocity. The principle 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.[144]

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 completely analogous process could be found in the construction of geometric

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[144] [141] 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.

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 infinitely many 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 Lagrange 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.

Finally, HILBERT discussed two additional approaches to the foundations of mechanics, introduced in the textbooks of HERTZ and BOLTZMANN respectively. HILBERT described them as both intended to simplify mechanics, but as doing so from opposed perspectives. Expressing once again his admiration for the perfect Euclidean structure of HERTZ's construction of mechanics,[145] 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

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

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

[145] [146] Er liefert jedenfalls von dieser Grundlage aus in abstrakter und präcisestes Weise einen wunderbaren Aufbau der Mechanik, indem er ganz nach Euklidischen Ideale ein vollständiges system von Axiomen und Definitionen aufstellt.

path, the body moves uniformly if one also assumes NEWTON's first law.[146] 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.[147]

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).[148]

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; it seems that HILBERT was not aware of these works at the time.[149] In fact, simultaneous with the course, an advanced seminar was co-directed by HILBERT in Göttingen, dealing with the latest advances in the theory of the electron; although many of POINCARÉ's related works were among the main texts of the seminar, his paper on the electrodynamics of moving bodies was not discussed there.[150] HILBERT's brief remarks here, on the other

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[146] [146] Die Bewegung eines jeden Systemes erfolgt gleichförmig in einer 'geradesten Bahn', d.h. für einen Punkt: die Krümmung

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

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.

[147] See HERTZ 1956, 31. This point is discussed in LÜTZEN 1995, 35–36.

[148] The contents of this course is analyzed in some detail in BLUM 1994 (unpublished).

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

[150] This seminar and the sources studied in it have been discussed in detail in PYENSON 1979.

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.[151] Although MINKOWSKI's lectures are beyond the scope of the present discussion, this particular detail of HILBERT's course, as well as related remarks appearing in his later courses, makes it quite clear that MINKOWSKI's early contributions to the study of special relativity must be properly discussed by referring to HILBERT's program for the axiomatization of physical science. I will discuss this significant issue in a forthcoming article.[152]

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 BOLTZMANN's and HERTZ's presentations, for the first time, force and motion were considered not as separate concepts, but rather as closely interconnected and mutually interacting. 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 $(a \mid b \mid c)$ and $(x \mid y \mid z)$ equals, as it is well-known, $-\dfrac{1}{r_{\substack{a,b,c\\x,y,z}}}$, 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 R. REIFF published an article in 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.[153] BOLTZMANN meant by this that one could conceivably follow an idea developed by Lord KELVIN, J. J. THOMSON and others, and to consider 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

[151] Published, not in their actual order, as MINKOWSKI 1915 and MINKOWSKI 1908.

[152] See CORRY 1997.

[153] BOLTZMANN 1900, 668: "... ein detailliertes Bild der gesamten Erscheinungswelt zu erhalten."

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 ALEXANDER 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 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.[154] 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: this was PAUL EHRENFEST (1880–1933), whose dissertation was completed in 1904. 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.[155]

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 1902–03 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 Lagrangain approach would be the natural one for studying mechanics of continua, but rather the opposite, namely, that a study of the

[154] For more details on this see KLEIN 1970, 64–66.
[155] For details on EHRENFEST's dissertation see KLEIN 1970, 66–74.

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, $\rho = \rho(x, y, z, t)$. 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 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.

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 $\frac{dV}{dt}$, which Hilbert wrote as follows:[156]

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

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":

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

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[156] 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.

where Δ denotes what the Laplacian operator (currently written as ∇^2). Second, the constancy of the mass in the system is established by means of the continuity equations:[157]

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

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" (HILBERT 1916, 396).

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.[158] 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 *Encyclopädie* article on hydrodynamics, written in 1901, still used the pre-vectorial notation (LOVE 1901, 62–63).[159] Only one year before HILBERT's course, speaking at the International Congress of Mathematicians in Heidelberg, the Göttingen applied mathematician LUDWIG PRANDTL

[157] In his article mentioned above, REIFF had tried to derive the pressure forces in a fluid starting only from the conservation of mass. BOLTZMANN pointed out that REIFF had obtained a correct result because of a compensation error in his mathematics. See KLEIN 1970, 65.

[158] See CROWE 1967, 182–224.

[159] 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.

(1875–1953) 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,[160] 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 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?[161] It would also be interesting, said HILBERT, 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.

In discussing a possible unification of mechanics and electrodynamics HILBERT was echoing a major concern of contemporary physicists. On the one hand there was the tradition of the supporters of the mechanical world view, going back to MAXWELL, HERTZ, and BOLTZMANN.[162] Their point of view sought to derive the laws of electrodynamics from mechanical foundations. More recently, a trend had been developing in the opposite direction, giving rise to the so-called electromagnetic view of nature. This trend vigorously developed in connection with current research on electron theory, and among its main proponents one can mention HENDRIK A. LORENTZ, HENRI POINCARÉ, WILHELM WIEN, MAX ABRAHAM, and WALTER KAUFMANN.[163] The forces exerted by moving electrons upon one another depended only upon the distance between the attracting bodies. This difference is noticeable given that HILBERT chose to begin

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[160] On early textbooks on vectorial analysis see CROWE 1967, 226–233.

[161] [154] Es wäre nun die Frage, ob man mit 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 bzw. auf die entsprechenden Feldgleichungen beschränken kann.

[162] Below, in the section dealing with HILBERT's lectures on the kinetic theory of gases, this tradition and HILBERT's direct reaction to it are discussed in greater detail.

[163] For the development of the electromagnetic view of nature see MCCORMMACH 1970, especially pp. 471–485.

his closing discussion of mechanics with a remarks concerning the possible dependence of attraction upon motion.

HILBERT's reference to HERTZ and BOLTZMANN in this context, and his silence concerning recent works on LORENTZ, WIEN, and others, is the only hint he gave in his 1905 lectures as to his own position on this basic physical issue. In fact, throughout these lectures HILBERT showed little inclination to take stands 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.

Thermodynamics

After mechanics, HILBERT went on to examine two other domains of science in which "an axiomatic treatment is especially suggestive."[164] The first is thermodynamics.[165] The central concern of this discipline is the elucidation of the two main theorems of the theroy of heat. Until now, said HILBERT, there were two usual ways to provide foundations for thermodynamics. The first, advanced by CLAUSIUS and PLANCK,[166] was based on the second theorem, which had been formulated as the "Law of the impossibility of a perpetuum mobile of the second kind" as follows:

> In a state of thermal equilibrium, given an arbitrary quantity of heat contained in a heat source, it is impossible to increase the total amount of work by means of purely cyclical processes (i.e., processes in which the bodies involved return finally to their initial positions).[167]

HILBERT did not mention the concept of entropy in this context, nor the irreversibility connotations that PLANCK had attached to it in his initial formulation.

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[164] [154] Ich . . . will nun noch auf zwei besondere Gebiete der Naturwissenschaft übergehen, wo eine axiomatische Behandlung besonders nahe liegt.

[165] At the beginning of the section on thermodynamics, HILBERT added on the margin: ⟨Axiome der elementaren Strahlungstheorie einschieben⟩. HILBERT dealt with the theory of radiation beginning around 1912. This remark may have been added after that time.

[166] On the relationship between CLAUSIUS's and PLANCK's formulations of the principle see HIEBERT 1968, 10–16; KUHN 1978, 14–16.

[167] [155] Es ist unmöglich, bei thermischem Gleichgewicht aus einer beliebige Wärmemengen enthaltenden Wärmequelle, durch reine Kreisprocesse Arbeit zu gewinnen (d.h. durch solche Processe, bei denen alle Körper schließlich wieder in der Anfangszustand zurückkehren).

The second kind of foundation, continued HILBERT, was advanced by HELMHOLTZ. It uses far-reaching mechanical analogies and describes thermodynamical processes by means of cyclical systems and virtual masses. It is similar to HERTZ's mechanics, and in fact HERTZ was motivated in his book, as HILBERT pointed out, by this work of HELMHOLTZ.[168]

HILBERT declared his intention to set forth a new foundation of thermodynamics, which would resemble closely the kind of axiomatic treatment used earlier in his discussion of mechanics. His stress on the mathematical elegance of the presentation led him to introduce the concepts in an unusual sequence, in which the immediate physical motivations are not directly manifest. For simplicity he considered only homogeneous bodies (a gas, a metal), denoting by v the reciprocal of the density. If H denotes the entropy of the body, then these two magnitudes are meant to fully characterize the elastic and the thermodynamical state of the body. HILBERT introduced the energy function $\varepsilon = \varepsilon(v, H)$, meant to describe the state of matter. The various possible states of a certain amount of matter are represented by the combinations of values of v and H, and they determine the corresponding values of the function ε. This function then makes it possible to provide a foundation for thermodynamics by means of five axioms, as follows:

I. Two states 1, 2 of a certain amount of matter are in elastic equilibrium with one another if

$$\left[\frac{\partial \varepsilon(v, H)}{\partial v}\right]_{\substack{v=v_1\\H=H_1}} = \left[\frac{\partial \varepsilon(v, H)}{\partial v}\right]_{\substack{v=v_2\\H=H_2}}$$

i.e., when they have the same pressure. By pressure we understand here the negative partial derivative of the energy with respect to v

$$p = -\frac{\partial \varepsilon(v, H)}{\partial v} = p(v, H).$$

II. Two states 1, 2 of matter are in thermal equilibrium when

$$\left[\frac{\partial \varepsilon(v, H)}{\partial H}\right]_{\substack{v=v_1\\H=H_1}} = \left[\frac{\partial \varepsilon(v, H)}{\partial H}\right]_{\substack{v=v_2\\H=H_1}},$$

i.e., when they have the same temperature θ. By temperature we understand here the derivative of the energy with respect to entropy:

$$\theta = \frac{\partial \varepsilon(v, H)}{\partial H} = \theta(v, H).$$

The purely mathematical definitions of pressure and temperature exemplify HILBERT's subordinating the physical meaning of concepts to considerations of mathematical convenience. Assume that v and H are functions of time t, and

[168] For a recent account of HELMHOLTZ's treatment of thermodynamics, see BIERHALTER 1993.

call the set of points in the v, H plane between any two states a path. He then introduces two new functions of the parameter t: $Q(t)$ (heat) and $A(t)$ (work). Given two states and a path between them, the total heat acquired between the two states is $\int_1^2 dQ = \int_1^2 (dQ/dt)dt$, and similarly for work. HILBERT added the following axiom involving these functions:

III. The sum of acquired work and heat on a given path between 1 and 2 equals the difference of the energy-functions at the endpoints:

$$\int_{t_2}^{t_1} dQ + \int_{t_2}^{t_1} dA = [\varepsilon]_1^2 = \varepsilon(v_2, H_2) - \varepsilon(v_1, H_1).$$

This the law of conservation of energy, or of the mechanical equivalent.

The remaining axioms are:

IV. On a path with $H = \text{const.}$, the total heat acquired equals zero. A path of this kind (parallel to the v-axis) is called adiabatic.
V. On a path with $v = \text{const.}$ the total work introduced equals zero.

To these five HILBERT added — as he had done before for geometry, for vector addition, and for mechanics — the continuity axiom. For thermodynamics it is formulated as follows:

VI. Given two paths connecting the points 1, 2, the quantities of heat added when moving along those two paths may be made to diverge from one another less than any arbitrarily given quantity, if the two paths are sufficiently close to one another in a uniform way (i.e., the two lie in a sufficiently narrow strip).

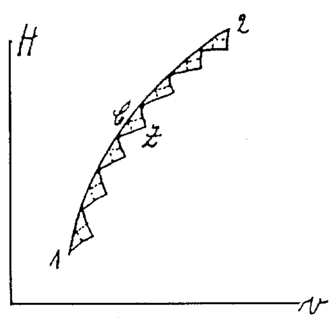

Fig. 40.

HILBERT stressed an important feature he saw in this system of six axioms, namely, that it treats work and heat in a completely symmetrical way. Moreover, he said, the system exhibits a remarkable analogy with systems previously introduced in other sciences.[169] Thus, the symmetrical treatment of heat and work appears as a very convenient one from the perspective of HILBERT's mathematical account of the theory, which fits his overall image of physics, but it does rather obscure the physical differences between reversible and irreversible processes. HILBERT also discussed briefly the logical interdependence of the axioms. From axioms VI. and III., for instance, one can deduce a continuity condition similar to VI., but valid for work rather than for heat.

HILBERT proceeded to show how some of the basic results of thermodynamics can actually be derived from this system. An important example is the derivation of the entropy formula, which is also sometimes used as a definition of this concept. Consider the curves of constant temperature (isothermals) $\theta(v, H) = \text{const.}$ In order to move along one of these curves from the point $\theta = 0$, to the point θ, one uses a certain amount of heat, which depends only on the temperature θ and on H:

$$\left[\int_{H=0}^{H} dQ\right]_{\theta(v,H)=\theta} = f(\theta, H).$$

The quantity of heat involved in moving along an isothermal line is given by the function $f(\theta, H)$. But what is the exact form of this function? Its determination, HILBERT said in this lecture, is typical of the axiomatic method. It is the same problem as, in the case of geometry, the determination of the function that represents the straight line; or, in the addition of vectors, the proof that the components of the vector that represents the addition are equal to the sums of the components of the factors. In all these cases, the idea is to decompose the properties of a certain function into small, directly evident axioms, and from them to obtain its precise, analytical representation. In this way — he concluded — we obtain the basic laws of the discipline directly from the axioms.[170] And in fact, in all the domains that HILBERT considered in his 1905 lectures, the determination of a particular function of the kind prescribed here, starting from the particular axioms defining that domain, lies at the focus of his presentation. We saw it above in his presentation of geometry and vector addition, and we will see it below in the discussion of other domains. In this way HILBERT's application of the axiomatic approach results in a remarkable unity of presentation. A detailed description of HILBERT's determination of this function in the

169 [161] Damit haben wir nun ein vollständiges und notwendiges Axiomensystem der Thermodynamik, der sehr übersichtlich und klar ist und insbesondere auch den Vorzug hat, die Wärme Q und Arbeit A völlig symmetrisch einzuführen, obendrein has es in seinem Aufbau noch eine große Analogie mit früheren Axiomensystemen anderer Wissenschaften.

170 [163] Allemal handelt es sich darum, die Eigenschaften einer gewissen Funktion in kleine unmittelbarer evidente Axiome zu zerlegen, und aus ihnen dann die anlitysch Darstellung der Funktion herzuleiten; diese läßt dann die wesentlichen Eigenschaften der Sätze der vorliegenden Disziplin unmitelbar zu erkennen.

case of thermodynamics will help us to grasp directly the manner in which he linked the axiomatic approach to specific physical theories.

It is clear, in the first place, that $f(0, H) = 0$. Consider now a parallel C to the H-axis ($v = \text{const}$), between the points $1(v\,|\,0)$ and $2(v\,|\,H)$. This line may be divided by arbitrarily close points $H_1 = 0, H_2, H_3, \ldots$. Through these points draw the isothermal lines $\theta(v, H) = \theta_1, \theta_2, \theta_3, \ldots$ as well as the horizontal lines $H = H_2, H_3, \ldots$ and form a zigzag line Z, whose triangles can be made as small as desired, by increasing the number of points in the partition of the line.

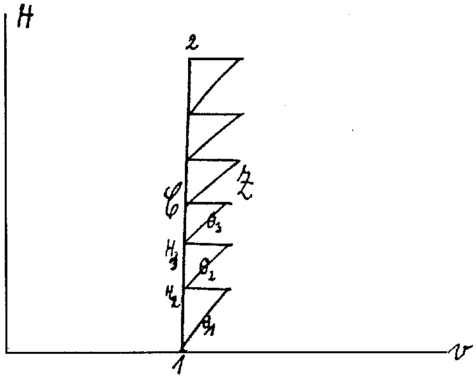

Fig. 42.

Using now axiom VI, and noticing that the limit of Z is C when we take an infinite number of points in the partition, the heat added when moving through C is

$$\int_{1(C)}^{2} dQ = \lim \int_{1(Z)}^{2} dQ.$$

But by axiom IV, all the contributions to the left hand side integral by the horizontal segments ($H = \text{const}$) are zero. As for the segments that correspond to isothermal lines, say $\theta = \theta_1$, the addition of heat corresponding to it equals, by definition of $f(\theta, H)$, to

$$f(\theta_1, H_2) - f(\theta_1, H_1) = \left(\frac{\partial f}{\partial H}\right)_{\theta_1} (H_2 - H_1)$$

where the derivative is taken for an average value of H in the isothermal (H_1, H_2). Hence

$$\int_{1(C)}^{2} dQ = \lim \left\{\left(\frac{\partial f}{dH}\right)_{\theta_1} (H_2 - H_1) + \left(\frac{\partial f}{dH}\right)_{\theta_2} (H_3 - H_2) + \ldots\right\}.$$

Finally, the passage to the limit yields:

$$\int_1^2 dQ = \int_1^2 \frac{\partial f}{\partial H}\, dH. \tag{a}$$

But now by axiom V, since in the curve $C, v = \text{const}$, the parallel integral for work is zero. Applying now Axiom III, one obtains

$$\int_1^2 dQ = [\varepsilon]_1^2.$$

But again, since on $C, v = \text{const.}$, the difference of energies over C can be expressed as follows

$$\int_1^2 dQ = [\varepsilon]_1^2 = \int_1^2 \frac{\partial \varepsilon}{\partial H}\, dH = \int_1^2 \theta\, dH. \tag{b}$$

Finally, from (a) and (b)

$$\int_1^2 \frac{\partial f}{\partial H}\, dH = \int_1^2 \theta\; dH.$$

This identity holds for all values of 2 over the line C, and therefore the integrands are equivalent. That is,

$$\frac{\partial f}{\partial H} = \theta$$

and therefore

$$f = \theta . H + W(\theta).$$

But the function $W(\theta)$ must be identically zero, since $f(\theta, 0) = 0$. Therefore we obtain

$$f(\theta, H) = \theta.\mathrm{H}.$$

This result could be extended now to paths C more general than in the former case, by an adequate use of the continuity axiom.

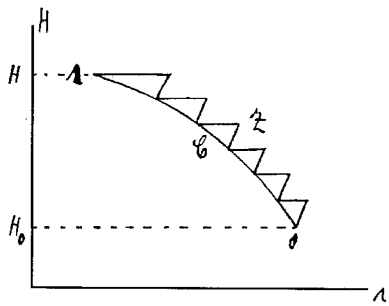

Fig. 43.

A similar value is thus found for any value of H; $f(\theta, H) = \theta.H$. In this case as well

$$\frac{\partial f}{\partial H} = \theta$$

and hence

$$Q = \int_{H_0}^{H_1} \theta \, dH.$$

This is the formula for the heat absorbed as the system moves along an arbitrary path C. By differentiation with respect to H, one gets

$$\frac{dQ}{dH} = \theta.$$

If all these magnitudes are seen as functions of v over the path C, then

$$dH = \frac{dQ}{\theta}.$$

Integrating between 0 and 1, one gets

$$[H]_0^1 = \int_0^1 \frac{dQ}{\theta}$$

which is the known formula for the change of entropy, in terms of change of heat and temperature. In the usual presentation of the theory, which considers the increase of temperature as the primary process, this formula is used as the definition of entropy.[171]

One well-known published work on the foundations of thermodynamics was directly influenced by these lectures of HILBERT, and perhaps even more by the scientific atmosphere in Göttingen within which HILBERT developed his ideas: this is an article of 1909 by CONSTANTIN CARATHÉODORY (1873–1950). CARATHÉODORY received his doctorate in Göttingen in 1904, and habilitated there in 1905. He taught as Privatdozent until 1908, when he moved to Bonn, and later returned to lecture in Göttingen from 1913 to 1918. His early stay at Göttingen had a lasting influence on his mathematical thinking and he always remained associated with the HILBERT circle. MAX BORN, who had been a close friend of CARATHÉODORY since their student days recounted in his autobiography how he had suggested to CARATHÉODORY the main idea behind the latter's study of thermodynamics. In 1907 BORN spent a semester in Cambridge, England doing mainly experimental research. At that time he also read GIBB's book on thermodynamics, which strongly attracted his attention. He later wrote:

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171 At the end of the section, HILBERT added in his handwriting [167]: ⟨Nernst's dritte Wärmersatz!⟩

From [my reading of Gibbs] sprang an essential piece of progress in thermodynamics — not by myself, but by my friend Carathéodory. I tried hard to understand the classical foundation of the two theorems, as given by Clausius and Kelvin; they seemed to me wonderful, like a miracle produced by a magician's wand, but I could not find the logical and mathematical root of these marvelous results. A month later I visited Carathéodory in Brussels where he was staying with his father, the Turkish ambassador, and told him about my worries. I expressed the conviction that a theorem expressible in mathematical terms, namely the existence of a function of state like entropy, with definite properties, must have a proof using mathematical arguments which for their part are based on physical assumptions or experiences but clearly distinguished from these. (BORN 1978, 119)

Whether or not BORN's reminiscences faithfully reflect the actual course of events, we know for certain that HILBERT had precisely put forward, in considerable detail, a similar idea in the lectures that BORN himself annotated for him in 1905. It is likely that BORN's reading of GIBBS rekindled the line of thought he had earlier heard in those lectures. In any case, it is clear that both CARATHÉODORY and BORN were acting here, if not in the details then certainly in the general spirit, under the spell of the kind of axiomatic analysis of physical theories promoted by HILBERT.

In opening his 1909 article, CARATHÉODORY claimed that there were no hypotheses in thermodynamics that could not now be experimentally verified. In a formulation that recalls HILBERT's own, he explained that the axioms he put forward for this domain were "generalizations of the facts of experience, which have been observed in especially simple circumstances" (CARATHÉODORY 1909, 139). He also claimed, though he gave no proof, that his axioms were mutually independent. For purposes of comparison, it is useful to quote here CARATHÉODORY's axioms for thermodynamics. The basic concepts of his presentation of the theory are: phase, volume, pressure, adiabatic processes, equivalent systems, equilibrium. He formulated only two axioms:

I. In a state of equilibrium, to every phase ϕ_i of a system S there corresponds a certain function ε_i of the magnitudes

$$V_i, p_i, m_i$$

called the internal energy of the phase, which is proportional to its total volume V_i. The sum

$$\varepsilon = \varepsilon_1 + \ldots + \varepsilon_\alpha$$

over all the phases, is called the internal energy of the system.
In adiabatic state transformations the change of energy due to external work is zero. In symbols, if $\varepsilon, \bar{\varepsilon}$ represent the initial and final values of the energy, then

$$\bar{\varepsilon} - \varepsilon + A = 0.$$

II. In the surroundings of any arbitrarily given initial conditions there are certain conditions that cannot be approximated as much as desired.

After formulating the axioms CARATHÉODORY went on the develop the declared aim of his paper, namely, to explain how, with the help of the two main axioms, it is possible to determine by experiment the internal energy of a system and to establish the general properties of the energy-function ε (pp. 139–140).

Many years later, CARATHÉODORY presented a second axiomatic treatment of thermodynamics. Elaborating on a suggestion of PLANCK, he discussed in 1925 the place of irreversible processes in thermodynamics. He referred again to his earlier paper and explained what he had tried to do in it. His explanation makes clear the extent of HILBERT's influence on him. He wrote:

> If one believes that geometry should be seen as the first chapter of mathematical physics, it seems judicious to treat other portions of this discipline in the same manner as geometry. In order to do so, we are in possession since ancient times of a method that leaves nothing to be desired in terms of clarity, and that is so perfect that it has been impossible ever since to improve essentially on it. Newton felt this already when trying to present his mechanics also in an external form that would fit the classical model of geometry. It is quite remarkable that with even less effort than in mechanics, classical thermodynamics can be treated by the same methods as geometry.
> This method consists in the following:
> 1. Create thought experiments, as in the case of geometry, constructing figures or moving around spaces figures already constructed.
> 2. Apply to these thought experiments the axioms that the objects considered are supposed in general to satisfy.
> 3. Extract the logical conclusion that follows from the given premises. (CARATHÉODORY 1925, 176–177)

CARATHÉODORY explained that in his 1909 article he had proceeded exactly in this way, but, in his opinion, the parallel application of the axiomatic method to thermodynamics and geometry was more clearly manifest only in this paper.

That CARATHÉODORY's work had itself little impact among contemporary physicists is manifested in a paper published in 1921 by MAX BORN in the *Physikalische Zeitschrift*, aimed precisely at making CARATHÉODORY's point of view more widely known than it was. BORN's article, in turn, interestingly displays the influence of HILBERT on his own conception of the link between physics and mathematics. In the introduction BORN asserted that the logical elaboration of a physical theory can be considered as concluded only when the theory has been transformed into a "normal" chapter of mathematics. BORN stressed the relatively reduced kinds of differential equations that appear time and again in the various domains of physics. Thus, for instance, every domain dealing with continuous processes is equations provide the basic building blocks from which the physicist always starts his investigation. He then works out the empirical data, refashions and remolds the laws obtained from this data until these fit one of the already existing equational forms.

To this account of the way differential equations are used in physics, however, BORN saw an important exception in the case of thermodynamics. No other field in physics, he wrote, is based on equations similar to those representing Carnot processes, or related ones. Since the kinds of mathematical equations used in thermodynamics are so typical and specific to this domain, it seems that if one takes away the physical content intrinsic to it, one is left with no independent mathematical structure. In its traditional presentation, then, thermodynamics had not attained the logical separation — so desirable, and in fact necessary, in the eyes of this disciple of the Göttingen school — between the physical content and the mathematical representation of the theory. BORN's characterization of the litmus test for recognizing when this separation is achieved brings us back directly to HILBERT's 1905 lecture: a clear specification of the way to determine the form of the entropy function (BORN 1921, 218).

BORN mentioned CARATHÉODORY's article of 1909 as an important and successful attempt to attain for thermodynamics the desired separation between physical content and mathematical form. BORN thought, moreover, that CARATHÉODORY's presentations had important pedagogical advantages and could be used with profit in the classroom. This attempt, however, was barely known among physicists and BORN saw two main reasons for that. The first concerned the generality and abstract character of the article. The second reason, BORN suggested, was its publication in a journal which few physicists read: the *Mathematische Annalen.* BORN's own article was intended to bring CARATHÉODORY's point of view to his colleagues. Revealing once again his Hilbertian influences, BORN emphasized that his presentation would start with the simplest facts of experience and would lead up to the final form of the main mathematical theorems of the theory. The relationship between this and the traditional way of formulating the theory BORN described as follows:

> This presentation of the theory should also be seen as putting forward a certain criticism of the classical one. Nevertheless, it should in no way be seen as belittling the huge achievements of the masters who were guided by their intuition. Rather, the intention is only to clear away some ruins that pious tradition has not hitherto ventured to remove. (BORN 1921, 219)

In the article, BORN reworked CARATHÉODORY's presentation of thermodynamics, in a way he thought more accessible to physicists. His article seems to have had as little noticeable influence as the one that inspired it.[172] But for the purposes of the present account it helps us to understand the way HILBERT wanted to go about axiomatizing physical theories: starting from the basic facts of experience, one strives to formulate an elaborate mathematical theory in which the physical theorems are derived from simple axioms. This theory may itself be different from the classical, more physically intuitive one, but the mathematical presentation contributes to a more unified view of physics as a whole.

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[172] In BORN's autobiography one can read the following, relevant passage (1978, 119): "I tried to popularize [CARATHÉODORY's ideas] in a series of articles which appeared in the *Physikalische Zeitschrift.* But only a few of my colleagues accepted this method, amongst them R.H. Fowler, one of the foremost experts in this field. Fowler and I intended, a few years ago, to write a little book on this subject in order to make it better known in the English-speaking world, when he suddenly died. That will, I suppose, be the end of it, until somebody re-discovers and improves the method."

Probability Calculus

The next discipline discussed by HILBERT in his 1905 lectures, is, after thermodynamics, the second one for which he considered the axiomatic treatment to be especially appropriate, namely, the calculus of probabilities. This domain of study is utterly different from the preceding ones, he said, yet it can be treated in a completely analogous way.

The axioms for the calculus of probabilities that HILBERT presented in his lectures were taken from an article on insurance mathematics that GEORG BOHLMANN published in the *Encyclopädie der mathematischen Wissenschaften* (1901). As already mentioned, in formulating his sixth problem HILBERT had also cited among the texts representative of the task of axiomatizing physics a lecture of BOHLMANN published in 1900. But (as BOHLMANN himself stated in a footnote to his 1900 article) the *Encyclopädie* article contained a much more precise mathematical formulation of the axioms underlying the mathematical treatment of life insurance, which in the earlier article appear as very general, somewhat loosely formulated assumptions. BOHLMANN's axioms in the *Encyclopädie* article are presented in two separate groups: general axioms of probability and special axioms of insurance mathematics (*Sterbenswahrscheinlichkeit*). The first group he credited to an article on probability by the Austrian mathematician EMANUEL CZUBER (1851–1925), appearing in the same volume (CZUBER 1900, 735–740). The second group he credited to a second article in the volume, on the applications of probability to statistics, written by the St. Petersburg statistician LADISLAUS VON BORTKIEWICZ (1868–1931) (VON BORTKIEWICZ 1900, 837–846). BOHLMANN also referred to POINCARÉ's textbook on probability as a main source of ideas for his axiomatization.[173] However, although the ideas embodied in some of BOHLMANN's axioms can indeed be retrospectively recognized in the texts he cites (and also in his own 1900 article), none of these sources contains the kind of systematic and concise treatment that BOHLMANN himself adopted in the *Encyclopädie* article. Under the manifest influence of HILBERT's *Grundlagen*, BOHLMANN was probably the first to provide this kind of axiomatization for the calculus of probabilities, although, on the other hand, he did not analyze, or mention, the properties of independence, completeness or simplicity as related to his system.

In HILBERT's 1905 lectures, probability was defined, following BOHLMANN, by means of a function $p(E)$, where E is any event, and $0 \leqq p(E) \leqq 1$. HILBERT explained that this is considered a definition in the theory, although, at its present state of development, the "axioms" and the "definitions" somewhat overlap with each other.[174] He was obviously referring to BOHLMANN's

[173] The reference is to POINCARÉ 1896, 12. In order to make the context of ideas more precise, it is worth mentioning that the subtitle of POINCARÉ's book is "Cours de physique mathematique."

[174] [168] Wir fassen das einfach als Definitionen auf, wiewohl im gegenwärtigen Zustande der Entwicklung besonders die Bezeichnungen 'Axiom' und Definition noch etwas durcheinandergehen.

treatment, in which definitions and axioms indeed appear intermingled, in a way that HILBERT himself would have avoided if he had systematically followed the model of the *Grundlagen*. HILBERT's remark here is interesting in view of the interchange mentioned above between HILBERT and FREGE, in which the interrelation between axioms and definitions in a mathematical theory was discussed. HILBERT in his lectures did not bother to separate axioms and definitions more completely than BOHLMANN had done before him, and thus — in appearance, at least — he presents a more flexible position on this issue than the one he expressed in his letters to FREGE. One of the main points put forward by HILBERT in that correspondence was the impossibility to define concepts in mathematics without connecting them to axioms. It is only the system of axioms taken as a whole, he had written to FREGE, that yields a complete definition of the concepts involved.[175] In treating the axioms of probability and speaking of the need to separate — rather than to combine — axioms and definitions, HILBERT was perhaps stressing the early state in which the theory was then found. And as a matter of fact, BOHLMANN's system of axioms was far from satisfying HILBERT's standards, a fact not mentioned in the manuscript of the lectures.

HILBERT adopted the notation used by BOHLMANN in his article. The simultaneous occurrence of two events E_1, E_2 is denoted by $E_1 + E_2$, whereas $E_1 \cdot E_2$ denotes their disjunction. Two events are mutually exclusive if $p(E_1 + E_2) = 0$, while $p(E_1|E_2)$, denotes conditional probability.[176] HILBERT did not mention an additional definition appearing in BOHLMANN's article, namely, that two events E_1, E_2 are independent if the probability of their simultaneous occurrence equals $p(E_1) \cdot p(E_2)$. Following BOHLMANN's presentation, HILBERT introduced the following two axioms as defining the theory:

$$\text{I. } p(E_1 \cdot E_2) = p(E_1) + p(E_2), \quad \text{if } p(E_1 + E_2) = 0.$$

$$\text{II. } p(E_1 + E_2) = p(E_1) \cdot p(E_1 \mid E_2).$$

In order to clarify the import of BOHLMANN's contribution, it should be stressed that these two axioms appear in POINCARÉ's book *as theorems* (*théorèmes des probabilités composées et totales* — respectively), and they are proved with reference to the relative frequencies of the events involved (POINCARÉ 1896, 12).

Like BOHLMANN in his article, beyond stating the axioms as such HILBERT went no further. He did not comment on the independence, consistency or "completeness" of these axioms. In fact, this system was a rather crude one by HILBERT's own criteria; more elaborate ones had already been attempted since BOHLMANN. In 1904 RUDOLF LAEMMEL, in a dissertation written in Zurich, had addressed the issue of the axioms of probability. He mentioned there CZUBER's article, but, strangely enough, not BOHLMANN's axioms. LAEMMEL proposed two

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[175] See GABRIEL et al. (eds.) 1980, 40.

[176] [170] 'Wenn E_1 ist, so ist stets auch E_2' oder 'E_2 folgt aus E_1' schreiben wir $E_1 \mid E_2$.

axioms and three definitions as a "minimal system" for the theory, formulating them in terms of "set-theoretical" notions (like those used by DEDEKIND and CANTOR in their works). He then asserted that his axioms were independent and sufficient to develop the whole theory, but he did not mention the problem of consistency.[177] It is not clear how far LAEMMEL was acquainted with HILBERT's *Grundlagen* nor whether he intended, through his axiomatization, to arrive for this domain at the goals HILBERT had reached in his book. It is likely that HILBERT in turn was not aware of the existence of this dissertation by 1905. In 1907, however, one of HILBERT's doctoral students, UGO BROGGI, took up once more the issue of the axiomatization of the calculus of probability, attempting to perfect — following the guidelines established in the *Grundlagen* — the earlier proposals of BOHLMANN and LAEMMEL.[178] Based on LEBESGUE's theory of measure, BROGGI not only formulated a system of axioms for probability, but also showed that his axioms were complete (in HILBERT's sense), independent and consistent, thus demonstrating the shortcomings of BOHLMANN's earlier system.[179] In 1908, addressing the Fourth International Congress of Mathematicians in Rome, BOHLMANN himself referred to BROGGI's dissertation and conceded that the latter had shown the need to provide a more thorough logical analysis of the concept of event (*Ereignissbegriff*) in the theory of probabilities (BOHLMANN 1909).

HILBERT in 1905, however, was much less interested in the calculus of probabilities as such, than in its applications. The first important application concerns what HILBERT referred to as the theory of compensations of errors (*Ausgleichungsrechnung*), which deals with the methods for eliminating, as far as possible, the influence of observational errors that may arise when repeatedly measuring physical magnitudes. The systematic study of measurement errors had originated at the beginning of the nineteenth century, especially in connection with observational errors in astronomy. Later, it had been expanded to cover measurement in other physical domains as well. One of the central slogans of the physical seminar of Königsberg, led since 1834 by FRANZ NEUMANN, had been its insistence on the value of exactness in measurement as a leading principle of physical research. NEUMANN not only took pains to impart this principle directly to his seminar students but also developed mathematical techniques to determine the theoretical limitations of the instruments used in his laboratory exercises; also more generally, he dedicated much effort to the study of elaborate methods of error analysis.[180] Of course, HILBERT did

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[177] LAEMMEL's dissertation is reproduced in SCHNEIDER (ed.) 1988, 359–366.

[178] Reproduced in SCHNEIDER (ed.) 1988, 367–377.

[179] For a review of later attempts to axiomatize the calculus of probabilities until 1933, see SCHNEIDER (ed.) 1988, 353–358. A more detailed account appears in VON PLATO 1994; see especially pp. 179–278, for the foundational works of VON MISES, KOLMOGOROV, and DE FINETTI.

[180] The centrality of this principle for NEUMANN's Königsberg seminar for physics, especially at the pedagogic level, is thoroughly discussed throughout the chapters of OLESKO 1991.

not himself attend NEUMANN's seminar, but it is likely that the influence of the latter was felt in Königsberg long after the latter's retirement in 1876. It should not come as a surprise, therefore, that in HILBERT's general overview of the axiomatization of physics this subject was also considered.

In his lectures HILBERT claimed that the theory of compensation of errors is based on a single axiom, from which the whole theory could be derived:

> If various values have been obtained from measuring a certain magnitude, the most probable actual value of the magnitude is given by the arithmetical average of the various measurements.[181]

Two theorems appear here as particularly interesting. The first one is GAUSS's error theorem, according to which the frequency of error in measuring a given magnitude is given by the integral

$$\int_0^{\tau} e^{-t^2}\,dt.$$

The second theorem is the so-called principle of least squares addition: the most probable value of the variables measured is obtained by minimizing the squares of the errors involved in each observation.

As in the case of mechanics, any of these three equivalent statements — the axiom and the two theorems — could be taken as basis for the whole theory. But from HILBERT's point of view, the main contribution of his analysis was in clarifying the need to assume at least one of the three statements. Earlier, he said, attempts had been made to prove one of the three without assuming the others, but now it was clear that this is impossible.[182] On the other hand, however, it could still be of great interest to attempt a reduction of them to other axioms with a more limited content and greater intuitive plausibility, as was done for the theories considered earlier in the lectures. Since there are so many possibilities of providing foundations for a discipline, he concluded, our actual choices are always arbitrary, and depend on personal inclinations and on the particular state of science in general at a certain time.[183]

What HILBERT really considered important and certain to remain as the real contribution of this kind of work were "the interdependencies that this research makes manifest."[184] This remark — essential for understanding HILBERT's whole

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[181] [171] Liegen für eine Größe mehrere Werte aus Beobachtungen vor, so ist ihr wahrscheinlichster Wert das arithmetische Mittel aller beobachteten Werte.

[182] [171] Es ist also gleichgültig, welches dieser 3 vollkommen aequivalenten Axiome man zu grunde liegt. Eines von ihnen zu 'beweisen', wie man [172] früher wohl versuchte, ist natürlich unmöglich.

[183] [172] Was man daß wirklich gerade als Grundlage aussprechen will, wenn sich so verschiedene Möglichkeiten ergeben haben, is wie stets willkürlich und hängt von pesönlichen Momenten und dem allgemeinen Stande der Wissenschaft ab.

[184] [172] [D] as dauernd bleibende und wichtige sind die Abhängigkeiten, die bei diesen Untersuchungen zu Tage treten.

conception of the axiomatization of physics — would reappear in a very similar formulation in 1924, when HILBERT published an up-to-date, corrected version of his 1915 paper containing the field equations of general relativity.[185] HILBERT also suggested in this lecture the possible interest of finding and analyzing other kinds of error-laws, less well-established than those mentioned above. For instance: what happens if one takes the absolute values of the deviations instead of their squares, as in GAUSS's law? These questions, according to HILBERT, had recently been investigated. A relevant source that HILBERT may have been thinking of here was the *Encyclopädie* article on this issue. In order to understand properly the context in which the theory of compensation of errors was presented in the *Encyclopädie* — and in which HILBERT himself considered the question — it should be noticed that this article was commissioned from an astronomer, since astronomy is the domain in which the theory was traditionally considered. The article, however, written by the Berlin astronomer JULIUS BAUSCHINGER (1860–1934), does not itself contain anything like an axiomatic analysis (BAUSCHINGER 1900).[186] HILBERT concluded this part of his lectures by pointing out that additional, deeper, work was to be expected in this domain, as in all others that have been treated axiomatically.

Kinetic Theory of Gases

A second main application of the calculus of probabilities is to the kinetic theory of gases. HILBERT expressed 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. This combination was applied in a very illuminating way, leading to new physical results. In order to understand HILBERT's presentation of the theory, it seems necessary to give a brief account of some of the main issues in kinetic theory of gases during the last decades of the nineteenth century.[187]

JAMES CLERK MAXWELL (1831–1879) was the first to develop a theory of the behavior of gases, based on the idea that the velocities of the molecules of a gas are not uniform and do not tend to uniformity, but rather produce a range of velocities. In a paper published in 1860 he claimed that in order to calculate most of the observable properties of a gas it is not necessary to know the

[185] See HILBERT 1924, 2: "Ich glaube sicher, daß die hier von mir entwickelte Theorie einen bleibenden Kern enthält und einen Rahmen schafft, innerhalb dessen für den künftigen Aufbau der Physik im Sinne eines feldtheoretischen Einheitsideals genügender Spielraum da ist."

[186] For an account of BAUSCHINGER's contributions to astronomy see HOPMANN 1934.

[187] Two classical, detailed accounts of the development of the kinetic theory of gases (particularly during the late nineteenth century) can be consulted: BRUSH 1976 and KLEIN 1970 (esp. 95–140). In the following paragraphs I have drawn heavily on them.

positions and velocities of all particles at a given time: it suffices to know the average number of molecules having various positions and velocities. Assuming that the number of molecules in a given volume of gas is uniformly distributed, MAXWELL addressed the problem of determining the velocity distribution function $f(v)$, where $f(v)dv$ expresses the average number of molecules with velocities between v and $v + dv$. Assuming, moreover, that the velocity components along the three orthogonal directions are statistically independent, he deduced the specific form of the distribution function as follows:

$$f(x) = \frac{1}{\alpha\sqrt{\pi}} e^{-(x^2/\alpha^2)}.$$

Here x is one of the orthogonal components of the velocity, and α^2 is a constant that MAXWELL showed to be equal to 2/3 of the mean-square velocity of the particles. If N is the total number of particles contained in the gas, then the number of particles having velocity between v and $v + dv$ is given by:

$$N \frac{4}{\alpha^3 \sqrt{\pi}} v^2 \ e^{-(x^2/\alpha^2)} \ dv.$$

Based on this probability function, MAXWELL was able to calculate, among others, the average potential energy, the average kinetic energy, and the mean free path of a molecule.

The assumptions made by MAXWELL in his 1860 paper were not altogether unproblematic. In 1867 he rederived the same function, assuming this time only that the velocities of any two colliding particles, rather than the components of the velocity of a single particle, were statistically independent. MAXWELL also relied on the principle of conservation of energy. This line of reasoning was adopted and developed by LUDWIG BOLTZMANN, beginning in 1868. BOLTZMANN continued to work intensively (though not exclusively on it) over the rest of his career, and his name came to be identified with the theory, and more particularly with the atomistic view of matter associated with it: the behavior of macroscopic matter was to be explained in terms of statistical laws describing the motion of the atoms, which themselves behave according to Newtonian laws of motion.

One of BOLTZMANN's main achievements was to work out in detail the connection between the thermodynamic concept of entropy and the kinetic theory of gases.[188] A mechanical interpretation of the second law of thermodynamics had been a principal motivation behind BOLTZMANN's work from the outset, and most of his subsequent work evolved as a process of constant reformulation and improvement of his results in response to harsh criticisms directed against them. Central among the latter was the apparent contradiction between the irreversible character of the statistically described state of the gas

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[188] Although it must be stressed that, until MAX PLANCK's treatment of the issue in 1900, this connection was largely ignored by other physicists involved in the study of the macroscopic behavior of gases. See KUHN 1978, 20–21.

and the reversible, Newtonian behavior of its individual molecules. This basic tension raised by the kinetic theory of gases came later to be known as the reversibility paradox (or objection): *Umkehreinwand*. BOLTZMANN's first attempt to deal with this particular argument, elaborated in detail by his Vienna colleague JOSEF LOSCHMIDT in 1876, dates from 1877.[189] BOLTZMANN claimed that his proof of the second law was based not on mechanics alone, but on combining the laws of mechanics and of probability: the probability of initial states that would produce an increase in entropy was enormously larger than that of states leading to decreases. HILBERT, as we will see presently, would specifically address this point in his 1905 lectures.

Another conceptual difficulty inherent in the kinetic theory of gases is the so-called recurrence paradox (or objection): *Widerkehreinwand*.[190] In 1890 POINCARÉ published a theorem of mechanics, according to which any mechanical system constrained to move in a finite volume with fixed total energy must eventually return to the neighborhood of any specified initial configuration. As a consequence of this theorem, the kinetic model, which is a mechanical one, appears to be incompatible with the constant increase in entropy stipulated by the second law of thermodynamics. Anyone who considered the latter as an irrefutable fact of experience, would have to conclude that the kinetic theory of gases — and more generally, the atomistic interpretation of nature — should be abandoned. Among the scientists who held such a view one can mention POINCARÉ himself, ERNST MACH, WILHELM OSTWALD, PIERRE DUHEM, MAX PLANCK (especially at an early stage)[191] and — the one who actually published his objections on these grounds against BOLTZMANN's theory — ERNST ZERMELO.

ZERMELO's earliest scientific interest was in applied mathematics and theoretical physics. In 1894 he completed a dissertation on the calculus of variations, working with HERMANN ARMANDUS SCHWARZ in Berlin. From 1894 to 1897 he was MAX PLANCK's assistant at the institute for theoretical physics in Berlin, before going to Göttingen, where he habilitated in 1899 with a work on hydrodynamics and with a lecture on the application of the calculus of probabilities to the study of dynamical systems. Only at the turn of the century did his interests begin to shift to set theory, the field with which his name came to be associated.[192] In 1896, ZERMELO became involved in an intense and long-lasting discussion with BOLTZMANN concerning the interrelation between the second law and the kinetic theory. On the basic status of the atomistic approach, ZERMELO's position was even more extreme than that of his former

[189] BOLTZMANN 1877. See BRUSH 1976, 605–627.

[190] The terms *Umkehreinwand* and *Widerkehreinwand* were introduced only in 1907 by TATYANA and PAUL EHRENFEST. See KLEIN 1970, 115.

[191] For the subtleties of PLANCK's position on this issue see HIEBERT 1971, 72–79; KUHN 1978, 22–29.

[192] Although several detailed studies of ZERMELO's contribution to set-theory and logic are available (e.g., MOORE 1982, PECKHAUS 1990, 76–122), his complete biography is yet to be written.

teacher PLANCK, who believed that by considering a continuous, rather than a molecular model of matter, the mechanic and the thermodynamic views could be reconciled: according to ZERMELO, either one or the other had to be abandoned.[193]

ZERMELO also raised some additional objections about technical details of BOLTZMANN's argument. Specifically, he claimed that the properties attributed by BOLTZMANN to the so-called H-curve, which provided the core of his mathematical argument, were not only unproved, but actually incompatible with the laws of mechanics. One particular detail of ZERMELO's argument concerned the fact that the probability of occurrence of a certain value of H should be measured by the volume in phase space of all states having this value. A theorem known to physicists as the LIOUVILLE theorem[194] states that the equations of motion imply that this volume is independent of time, and from this ZERMELO concluded that the H-curve would have no clear tendency to increase or decrease.

BOLTZMANN's reply to the "reversibility paradox" was to identify the reason for the increase of entropy in the physical world with the relatively enormous probability of attaining a state of disorder, starting from either one of order or of disorder, as compared to that of attaining one of order. This had the virtue of providing a new, statistical interpretation of the formerly mysterious concept of entropy: *it identified the latter with greater disorder in a system.* In fact, BOLTZMANN defined the entropy of a system in terms of the relative probability of a certain macroscopic state actually to happen. Irreversibility is then nothing but a tendency to go from less probable to more probable states.

To the recurrence argument BOLTZMANN replied that according to the statistical point of view a particular initial state of a system was likely to reappear provided one waited long enough. This, however, was unlikely to be confirmed by experience, since the time needed to observe the recurrence would be immensely long. BOLTZMANN suggested that the universe as a whole is a system in a state of equilibrium, and that experience of a "direction of time", due to the increase of entropy, was only a subjective phenomenon observable within relatively small regions, such as for example a galaxy. He thus reconciled locally irreversible phenomena (like entropy), the validity of mechanical laws, and cosmic reversibility and recurrence. As for ZERMELO's objection to the properties of the H-curve, BOLTZMANN wrote several articles in which he refined his own treatment of the curve, though many issues connected to it remained quite unclear. Beginning in 1906 TATYANA and PAUL EHRENFEST (the latter a student of BOLTZMANN's) contributed to clarifying BOLTZMANN's ideas still further in a series of publications of the conceptual foundations of statistical mechanics.

A third controversy around the kinetic theory of gases concerned the so-called equipartition theorem, an important consequence of the MAXWELL-BOLTZMANN distribution formula, according to which the energy of a gas is evenly

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[193] See KUHN 1978, 26–27.

[194] This is different from LIOUVILLE's theorem on analytic functions.

distributed on average throughout all the volume. During the early years of BOLTZMANN's elaboration of the theory, the consequences of this theorem were contradicted by several new experimental results concerning the heat capacity of certain gases. As with the other two kinds of objection, this too led BOLTZMANN to clarify his formulations, but he was not able to dispel all doubts related to this particular point. In fact, the difficulties raised by the equipartition theorem were not thoroughly settled until the development of quantum theory.

At the turn of the century several works appeared that changed the whole field of the study of gases, leading to more widespread appreciation of the value of the statistical approach. The work of PLANCK, GIBBS and EINSTEIN and contributed to focus much more interest on BOLTZMANN's statistical interpretation of entropy.[195]

One can thus see 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 his lectures, but also because the kinetic theory was a good example of a physical theory in which, 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 a settled one 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.[196] In fact, he stressed, the theory has not yet provided a solid justification for its assumptions, and ever new ideas and stimuli are still being constantly added.

Even if we knew the exact position and velocities of the particles of gas — HILBERT explained — it is impossible in practice to integrate all the differential equations describing the motions of these particles and their interactions.

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[195] EINSTEIN 1902, GIBBS 1902. See KUHN 1978, 21.

[196] [176] Hier können wir aber bereits ein paradoxes, zum mindesten nicht recht befriedigendes Resultat feststellen.

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 the use of that calculus would clearly be considered to yield 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:

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

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.[197] As in the case of the prime numbers, however, Hilbert did not consider this kind of reasoning to 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 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.[198]

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.[199] Moreover, he directed the work of three doctoral students, who in 1913–14 completed dissertations dealing with problems connected with the theory (Hans Bolza, Bernhard Baule and Kurt Schellenberg),[200] and inspired additional publications by younger Göttingen scientists.[201]

In his published works, Hilbert did not even come close to expressing any opinion concerning crucial physical questions related to the theory, such as the

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[197] [180] Genau so ist es nun hier in der kinetischen Gastheorie. Indem wir behaupten, daß die Maxwellsche Geschwindigkeitsverteilung den mechanischen Differentialgleichungen [181] 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 große Aussagen über mittlere Werte, wie die des Maxwellschen Gesetzes, nicht berühren.

[198] [182] 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 großen Verstößen. Am besten kann man immer nachträglich sagen, daß die Anwendung der Wahrscheinlichkeit immer dann berechtigt und erlaubt ist, wo sie zu richtigen, mit der Erfahrung [183] bzw. der sonstigen mathematischen Theorie übereinstimmenden Resultaten führt.

[199] In Hilbert 1912, Chpt. XXII.

[200] See Hilbert GA Vol. 3, 433. Two of the dissertations were published as Baule 1914 and Schellenberg 1915.

[201] Cf. for instance: Bolza, Born & van Kármán 1913; Hecke 1913; Hecke 1922. The all-important article of Paul and Tatyana Ehrenfest on the conceptual foundations of statistical mechanics (Ehrenfest 1912), published in 1912 in the *Encyclopädie der mathematischen Wissenschaften*, also makes Hilbert's influence manifest in several respects. It would be far beyond the scope of the present article, however, to study this influence in greater detail.

status of the atomistic conception.[202] In his lectures, although he was still quite cautious when it came to such questions, occasionally he did — sometimes explicitly and sometimes implicitly — express opinions, and the latter often changed over time. In his 1905 course, as was already said, HILBERT praised the fruitfulness of combining "far-reaching physical assumptions" with the theory of probabilities and thus implicitly endorsed BOLTZMANN's atomistic view of physics. On the other hand, he avoided explicitly taking sides on any unsolved question of the theory, or in any discussion concerning its foundations. In the winter semester of 1911–12, HILBERT lectured specifically on the kinetic theory. In the introduction to these lectures he discussed different ways in which physical domains can be rigorously formulated in mathematical terms. First one has the "phenomenological perspective." In this case, the whole of physics is divided into various chapters: thermodynamics, electrodynamics, optics, etc. Each of these domains can be approached using different assumptions, peculiar to each, and different mathematical consequences are thus derived from these assumptions. The main mathematical tool used under this approach is the theory of partial differential equations. The second possible way is to assume 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."[203] The mathematical methods used here, continued HILBERT, are obviously quite different from the former: 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 in the way to knowledge, which must however be abandoned "as soon as possible, in order to penetrate to the real sanctuary of theoretical physics."[204] Unfortunately, said HILBERT, mathematical analysis is not developed enough to be able to satisfy all the demands of the second approach. We must therefore do without rigorous

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[202] In his account of the development of the kinetic theory, STEPHEN BRUSH (1976, p. 448) claims that, in dealing with the BOLTZMANN equation, HILBERT had no direct interest in the theory, but rather "he was simply looking for another possible application of his mathematical theories." The present account is meant to allow a broader look at the motivations behind HILBERT's contribution, than the one implied by BRUSH's assertion.

[203] HILBERT 1911–12, 2: "Hier ist das Bestreben, ein Axiomensystem zu schaffen, welches für die ganze Physik gilt, und aus diesem einheitichen Gesichtspunkt alle Erscheinungen zu erklären. . . . Jedenfalls gibt sie unvergleichlich tieferen Aufschluss über Wesen und Zusammenhang der physikalischen Begriffe, ausserdem auch neue Aufklärung über physikalische Tatsachen, welche weit über die bei A) erhaltene hinausgeht."

[204] HILBERT 1911–12, 2: "Wenn man auf diesem Standpunkt steht, so wird man den früheren nur als einer Notbehelf bezeichnen, der nötig ist als eine erste Stufe der Erkentnnis, über die man aber eilig hinwegschreiten muss, um in die eigentlichen Heiligtümer der theoretischen Physik einzudringen."

logical deductions and be temporarily satisfied with rather vague mathematical formulae.[205] It is amazing, HILBERT thought, that using this method we nevertheless obtain ever new results that are in accordance with experience. What can be considered the "main task of physics", he said in concluding the introduction to his 1911–12 lectures, is "the molecular theory of matter."

The molecular theory of matter was the subject of HILBERT's course in the following winter. This theory, he said in the introductory lecture, studies physical bodies and the changes affecting them, by considering systems composed of large numbers of masses moving in space, and acting on each other through collisions and other kinds of interacting forces. Such a study, he said — repeating a view he had already stated on different occasions — meets with enormous difficulties, which force us to adopt a "physical" point of view. This point of view is attained by clearly emphasizing, through the use of the axiomatic method, those places where physics intervenes in mathematical deduction. In this way, he proposed to separate – echoing a distinction formerly drawn by both HERTZ and VOLKMANN – three different components of the specific domain considered: first, what is arbitrarily adopted as definition or taken as assumptions of experience; second, what we expect *a priori* should follow from these assumptions, but 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.[206]

But then, in his next series of lectures, in the summer semester of 1913, HILBERT was already adopting a view quite different from the molecular one and he now embraced with full commitment the unified, electromagnetic view of nature that was to underlie his general relativistic theory of gravitation in 1915.[207] This change, together with the opinions expressed in his 1911–12 and 1912–13 courses, seems to suggest that a main reason for HILBERT's willingness to abandon the atomic theory of matter which he had espoused until then (though perhaps never zealously), came from the enormous difficulty he recognized in developing a thorough mathematical treatment of the theory that was the foremost expression of the atomistic view. Given HILBERT's overarching mathematical knowledge — and more specifically, given his recent work on the theory of integral equations, with its all-important applications in kinetic theory — it seems that no one was in a better position than he to judge those

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[205] HILBERT 1911–12, 2: "... sich mit etwas verschwommenen mathematischen Formulierungen zufrieden geben muss."

[206] HILBERT 1912–13, 1: "Dabei werden wir aber streng axiomatisch die Stellen, in denen die Physik in die mathematische Deduction eingreift, deutlich hervorheben, und das voneinander trennen, was erstens als logisch willkürliche Definition oder Annahme der Erfahrung entnomen 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 drittens, das, was bewiesene mathematische Folgerung ist."

[207] For more details on this important issue, see below the section on electrodynamics.

difficulties. The "physical point of view" he was "forced" to adopt in view of the mathematical difficulties encountered when starting from the atomistic conception was then in decline among physicists, but it proved indeed fruitful in leading HILBERT to develop what he saw as his contribution to the foundations of the whole of physics.

Insurance Mathematics

The third application of the calculus of probabilities considered by HILBERT was the insurance calculus: this domain is treated — again following BOHLMANN — by taking the axioms of probability introduced above, and adding more specific definitions and axioms. In discussing thermodynamics, the state of matter has been expressed in terms of a function $\varepsilon = (v, H)$. A similar move was made here: for the purposes of insurance, an individual person is characterized by means of a function $p(x, y)$, defined for $y > x$. This function expresses the probability that a person of age x will reach age y, and it is required to satisfy the following axiom:

> The probabilities $p(x, y)$, $p'(x', y')$ associated with two different individuals are independent for all pairs x, y x', y' of positive numbers.

Now, a collection of individuals, such for that any two of them $p(x, y) = p'(x, y)$, is called an equal-risk group. From the point of view of insurance, the individuals of any of these collections are identical, since the function p wholly characterizes their relevant behavior.

HILBERT attempted to develop the analogy between thermodynamics and the insurance calculus even further. In the former discipline, the main result achieved in the lectures was the explicit derivation of the form of the function $f(\theta, H)$, using only the particular axioms postulated. Something similar should be pursued for all other disciplines, and in this particular case, the aim would be the determination of a certain function of one variable.[208] The axiomatic system on which HILBERT proposed to base the insurance calculus was thus postulated as follows: Every equal-risk group associated with a function of probability $p(x, y)$ defines a "virtual mortality-order" (*fingierte Absterbeordnung*). This means that one can associate to every such group a function $l(x)$ of the continuous variable x, called the "number of living people of age x" or "life function", satisfying the following properties:[209]

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[208] [184] Wie wir nun in der Thermodynamik zunächst als wichtigstes Resultat aus den Axiomen die Gestalt einer gewissen Funktion $f(\theta, H)$ herleiten mußten, und ähnliches auch mehrfach in andern Disciplinen halten, so ist auch hier die fundamentalste Tatsache die Existenz einer gewissen Funktion einer Variablen und ihre Darstellung.

[209] [185] Jede Gesammtheit von gleichartigen Risiken, zu denen die Wahrscheinlichkeit $p(x, y)$ gehört, besitzt eine (fingierte) Absterbeordnung; d.h. zu ihr gehört eine Funktion $l(x)$ der kontinuerlichen Variablen x, gennant die Zahl der Lebenden des Altes x oder Lebensfunktion mit folgenden Eigenschaften: ...

1. l is well-determined up to a constant factor.
2. l is non-negative and decreases with x,

$$l(x) \geqq 0, \quad \frac{dl(x)}{dx} \leqq 0$$

3. It is possible to establish the relation

$$p(x, y) = \frac{l(y)}{l(x)}.$$

HILBERT did not prove any of the results pertaining to this theory and to the functions p and l. He stated only that such proofs would involve a kind of deduction similar to those used in the other domains. He added, however, that in these deductions also, an unspecified axiom of continuity of the kind assumed in the former domains — the particular version of which he would not formulate explicitly in this case — plays a central role.

Electrodynamics

In subsequent lectures, HILBERT discussed several questions concerning electrodynamics. The manuscript of the lecture indicates that this particular domain had not been discussed by HILBERT before July 14, 1905. By that time HILBERT must have been deeply involved with the issues studied in the advanced seminar on electron-theory that was being run in Göttingen parallel to his lecture course. 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

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

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

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

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:

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

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.[210] 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 following considerations similar to those applied

[210] [187] Zur Festlegung von L muß man nun natürlich noch Axiome hinzunehmen, und Laplace kommt da mit einer allgemeinen, ihm unmitelbar anschaulichen Vorstellung über Relativbewegung aus, daß wir nämlich eine gleichförmige Bewegung des ganzes 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.

in LAPLACE's case. One need only find the appropriate axiom to effect the derivation. Without further explanation, HILBERT wrote down the Lagrangian describing 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 MAX ABRAHAM's article on the dynamics of the electron (ABRAHAM 1902, 37), and a similar one appears in the article on electron theory written by HENDRIK A. LORENTZ in 1903 and published in 1904 in the volume on mechanics of the *Encyclopädie der mathematischen Wissenschaften* (LORENTZ 1904, 184).[211] If not earlier than that, HILBERT had studied these articles in detail in the advanced seminar on electron theory, where LORENTZ's article was used as a main text.[212] An important work reviewed in that article, which also received some attention in the seminar, was ABRAHAM's second article on the dynamics of the electron. In its central section, ABRAHAM described translational motion by means of still another Lagrangian (equal to the difference between magnetic and electrical energy) and showed that the principle of least action also holds for what he called "quasi-stationary" translational motion.[213] That the dynamics of the electron could be expressed by means of a Lagrangian was for ABRAHAM a result of special epistemological significance (ABRAHAM 1903, 168).[214] ABRAHAM, it must be stressed here, had been *Privatdozent* in Göttingen since 1900, and while certainly HILBERT may have learned much from him about the specific, physical results of the theory, it must also have been the case that ABRAHAM's basic ideas about what is of importance — and in particular, of epistemological importance — in the mathematical treatment of physical theories were in turn influenced by HILBERT's ideas.

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

[211] LORENTZ's Lagrangian is somewhat different, since it contains two additional terms, involving the inverse of v^3.

[212] See PYENSON 1979, 103.

[213] Namely, motion in which the variation in the velocity of the electron in the time required for light to traverse its diameter is small.

[214] On ABRAHAM's electron theory see GOLDBERG 1970.

introduced by LAPLACE in the case of classical mechanics.[215] The electron-theory seminar in which HILBERT was participating had been discussing many recent contributions, by people such as POINCARÉ, LORENTZ, ABRAHAM and KARL SCHWARZSCHILD, who on many important issues held contradicting views.[216] It was thus clear to HILBERT that, at that 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 HILBERT in 1905 did not mention the LORENTZ transformations, which were to recieve very much attention in his later lectures on physics. LORENTZ published the transformations in an article of 1904, but this article was not listed in the bibliography of the electron theory seminar, and it is likely that HILBERT was not aware of it by the time of his lectures (LORENTZ 1904a).[217] The next time HILBERT lectured on electron theory was in the summer semester of 1913. This time the demand of invariance under LORENTZ transformations was the first topic discussed in the lectures, and it appeared as a general principle that should be taken as valid for the whole of physics. Moreover, at a time when recent developments in physics — above all, the development of quantum theory — had raised significant difficulties for the electromagnetic view of nature,[218] HILBERT's initial inclinations towards a mechanical view had cleared the way for an explicit preference for the unified, electromagnetic conception that in the next two years was to provide the physical basis for his relativistic theory of gravitation.[219] In his 1913 lectures, stressing again the methodological motivation behind the quest for a unified view of nature, HILBERT said:

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[215] [188] Nimmt man nun wieder die Differentialgleichungen bzw. das zugehörige Variationsproblem als Axiom und läßt L zunächst als noch unbestimmte Funktion von v stehe, 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.

[216] For a detailed discussion of the various positions, as manifest at the 1905 electron-theory seminar in Göttingen, see PYENSON 1979, 110–128. On the differences between ABRAHAM and LORENTZ, as seen by ABRAHAM, see GOLDBERG 1970, 19–22.

[217] See PYENSON 1979, 103.

[218] MCCORMMACH 1970, 485–491.

[219] On December 17, 1912, MAX BORN lectured at the Göttingen Mathematical Society on MIE's theory of matter (see the announcement in the *Jahresbericht der Deutschen Mathematiker-Vereinigung* Vol. 22 (1913), 50). This is the first recorded evidence of the theory being discussed in Göttingen. On October 22, 1913, that is, during the semester following HILBERT's above-mentioned lectures, MIE wrote a letter to HILBERT expressing his satisfaction for the interest that the latter had manifested (in an earlier letter which is not preserved) on MIE's recent work. MIE's letter is in HILBERT's *Nachlass*, NSUB Göttingen — Cod Ms David Hilbert 254/1.

> 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 electrodynamics would appear as the foundations of all of physics. But the attempt to develop this idea systematically must be postponed for a later occasion. In fact, it has to start from the movement 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.[220]

Since the very first endeavors of LORENTZ and WILHELM WIEN to implement their unifying program for an electromagnetic view of nature, the task of subsuming gravitation under it had been unsuccessfully attempted.[221] Of particular interest for this account is the fact that in MINKOWSKI's 1907 detailed derivation of the equations of electrodynamics, he discussed in a final appendix a sketch of how this possible reduction could be actually worked out, outlining a Lorentz-invariant theory of gravitation (MINKOWSKI 1908, 401–404). In fact, the possibility of extending to all of physics the validity of invariance under Lorentz transformations was a main theme of MINKOWSKI's article, which he formulated in terms very similar to those used by HILBERT here. MINKOWSKI's "postulate of relativity" is nothing but a "confidence" (*Zuversicht*) in the plausibility of extending to all of physics, as a general underlying principle, what was a mathematical theorem known to be valid for the laws of electrodynamics (p. 353). As late as 1913, HILBERT reasserted the need to realize the view behind the confidence expressed by MINKOWSKI, and turned it into a central task of his own unified perspective for physics. Nevertheless, he was well-aware of the difficulties of a purely electromagnetic reduction. Lecturing on the theory of the electron he asserted:

> The Maxwell equations and the concept of energy do not suffice to provide a foundation of electrodynamics. The concept of rigidity is thus needed. Electricity should be attached to a stable scaffold, and this scaffold is what we denote as an electron. The electron embodies the concept of a rigid body in Hertz's mechanics. All of the laws of mechanics can be derived, in principle at least, from these three ideas: Maxwell's equations, the concept of energy, and rigidity. From them also all the forces of physics can be derived, and in particular the molecular

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[220] HILBERT 1913, 13: "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."

[221] MC CORMMACH 1970, 476–478.

forces. Only gravitation has evaded until now every attempt at an electrodynamic explanation.[222]

When HILBERT addressed in 1915 the problem of a relativistic theory of gravitation, he was simply following a line of interest that he had systematically pursued since the time of his earliest involvement with physical theories. The existing evidence allows us in fact to say much more about the evolution of HILBERT's view from his 1905 lectures to his 1915 field equations for gravitation, but that would be beyond the scope of the present article and will be left for a later occasion. To conclude this brief sketch of that development, however, I must add that HILBERT's 1915 presentation of general relativity was meant as an axiomatization of the principles of physics in general. The second basic axiom of his theory was the demand that the equations of gravitation be generally covariant (HILBERT 1915, 396). In this way, we can discern a clear line of evolution in HILBERT's thought: in 1905 he acknowledged the need for postulating Galilean-invariance as an axiom of the Newtonian theory of gravitation; later (e.g., in the 1913 course), he adopted a view elaborated by MINKOWSKI (to a certain extent, perhaps, under HILBERT's influence) and included the demand of Lorentz-invariance as a basic principle of all physics, though he was not able to derive gravitation from it. Finally, in 1915, the demand of general covariance was among the axioms from which he was able to derive the desired theory of gravitation. For HILBERT, the general covariance of what he saw as the basic equations of physics always remained the most important achievement of modern science, an opinion he repeatedly expressed in later years. Thus for instance in a lecture held in 1921, HILBERT asserted that no other discovery in history had aroused as much interest and excitement as EINSTEIN's relativity theory, "the highest achievement of the human spirit." This excitement was indeed justified in HILBERT's view since, whereas all former laws of physics were provisory, inexact and special, the principle of relativity (and here HILBERT meant by this the general covariance of physical laws) signified "for the first time, since the world has existed, a definitive, exact and general expression of the natural laws that hold in reality."[223] But in order to appreciate in its proper historical context the meaning of HILBERT's adoption in 1915 of the

[222] HILBERT 1913, 61–62: "Auf die Maxwellschen Gleichungen und den Energiebegriff allein kann man die Elektrodynamik nicht gründen. Es muss noch der Begriff der Starrheit hinzukommen; die Elektrizität muss an ein festes Gerüst angeheftet sein. Dies Gerüst bezeichen wir als Elektron. In ihm ist der Begriff der starrer Verbindung der Hertzschen Mechanik verwirklicht. Aus den Maxwellschen Gleichungen, dem Energiebegriff 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."

[223] HILBERT 1921, 1: ". . . denn das Relativitätsprinzip bedeutet, wie mir scheint, zum ersten Mal, seit die Welt steht, eine definitive, genaue und allgemeine Aussage über die in der Wirklichkeit geltenden Naturgesetze."

demand for covariance as a main foundational axiom of physics, it is necessary to recall the fact that this adoption came after three years of EINSTEIN's failure to embrace general covariance as a leading principle of his own relativistic theory of gravitation. After several unsuccessful attempts to formulate such a theory, and after discarding general covariance as part of these attempts, EINSTEIN had only very recently re-espoused this principle.[224]

HILBERT's brief discussion of electrodynamics in 1905 and the point of view adopted in it are thus of fundamental importance for understanding the main ideas behind HILBERT's program for the axiomatization of physics, as well as his own later contributions to it. We have already seen various passages where HILBERT — following an idea expressly manifest in the introduction to HERTZ's *Principles*[225] — stressed the possibility that new, significant facts would be added in the future to the edifice of mechanics. The axiomatization of this science should be carried out in a way that would allow for the absorption of such eventual discoveries into the existing body of knowledge, without major modifications in the logical structure of the theory, and by adding or deleting specific axioms of relatively circumscribed consequences for that structure. In 1905 HILBERT was faced with the new discoveries brought about by research on electron theory. From his point of view, this new research should and could be easily incorporated into the existing picture of mechanics, by the addition of suitable axioms. This is precisely what he stated in this section of his lectures. At that time, HILBERT was not yet aware of the recent publication of EINSTEIN's special theory of relativity. Yet not even the subsequent development of this theory would present any problem of principle for HILBERT's conception. On the contrary, repeating what he had done in 1905 for the laws of motion of the electron, he would simply be confronted with the need to find the special axioms that would allow the special theory of relativity to be incorporated into the already established — yet open to necessary modifications — logical structure of mechanics. Finally, beginning in the late 1913, HILBERT would again be in the same position with regard to GUSTAV MIE's electrodynamic theory of matter and EINSTEIN's attempt to develop a relativistic theory of gravitation. HILBERT's endeavor to address the challenge posed by the possible incorporation of these two theories into the existing picture of physics initiated a line of development that would eventually lead him to the discovery and publication of his own version of the correct field equations for general relativity.

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[224] See NORTON 1984.

[225] HERTZ 1956, 10: "Our assurance, of course, is restricted to the range of previous experience: as far as future experience is concerned, there will be yet occasion to return to the question of correctness." This passage is quoted extensively above on p. 95.

Psychophysics

The last domain considered by HILBERT in his 1905 account of the role of axiomatization in natural science was psychophysics. HILBERT's account of this domain referred to a recent work on the theory of color perception published by EGON RITTER VON OPPOLZER, a psychologist from Innsbruck (OPPOLZER 1902–3). OPPOLZER's article was a classical representative of the German school of experimental psychology, going back to the work of GUSTAV FECHNER (1801–1887).[226] One of FECHNER's main contributions to this field was the so-called WEBER-FECHNER law concerning the relation between the magnitude of a stimulus and the magnitude of the sensation produced by it. Since the latter cannot be directly measured, FECHNER focused rather on the absence or presence of a sensation, estimating its threshold values, i.e., the minimal amount of stimulus needed to produce that sensation or a noticeable difference between two sensations of the same kind. Before FECHNER, ERNST HEINRICH WEBER (1795–1878), a professor of anatomy and physiology at Leipzig, had experimentally established in 1834, for a light stimulus of intensity I_k and brightness x_k, that the quotient $\frac{I_k + \Delta I_k}{I_k}$ is constant for all values of Δx_k.

Building upon WEBER's result, FECHNER — who had started his career as professor of physics — established in 1860 a more precise quantitative relation: if R denotes the magnitude of the stimulus (*Reiz*) and S denotes the magnitude of the sensation, then

$$S = k \log R.$$

Here, S is measured in multiples of the empirically determined, minimal noticeable difference between two sensations of the same kind, whereas R is measured as multiples of the threshold value of the stimulus.

OPPOLZER took the WEBER–FECHNER law — with certain reservations — as one of the starting points of his work. He also relied on the work of HERMANN VON HELMHOLTZ (1821–1894), who in 1860 had published an analysis of color vision in the second part of his *Handbuch der physiologischen Optik*. HELMHOLTZ's theory, based in turn on THOMAS YOUNG's account of vision, became a most influential source for the study of color vision.[227] OPPOLZER's was only one of a long series of German articles devoted to this question after the publication of HELMHOLTZ's book.[228] Its declared aim was to characterize the

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[226] On FECHNER's contributions see BORING 1929, 265–287. More generally, on the German school, see there, pp. 237–401. OPPOLZER is mentioned neither in BORING's classical account, nor in other, standard similar works.

[227] HELMHOLTZ's theory is discussed in detail in KREMER 1993, 237–258.

[228] According to TURNER 1987, 44, research into color vision was the single topic that attracted the greatest number of publications in physiological optics between 1870 and 1885. It continued to be at the center of attention of German vision research until 1920. See KREMER 1993, 257–258.

sensation of light in "total colorblind systems" by means of a single, purely psychological parameter — the brightness (*Helligkeit*) — as opposed to the physically characterizable concept of intensity (*Intensität*). The problem addressed by OPPOLZER, as HILBERT presented it in his lectures, was to express the magnitude of this parameter as a function of the intensity and wave-length of light.[229]

As in the case of BOHLMANN's work on probabilities, the axioms mentioned by HILBERT for the case of psychophysics can be found only retrospectively in OPPOLZER's own article. OPPOLZER himself described his basic assumptions discursively, sometimes loosely, and not only in the opening sections, but throughout his article. Needless to say, he did not analyze the independence, consistency or any other property of his "axioms". Yet precisely because the unsystematic way in which OPPOLZER discussed principles and ideas drawn from works as diverse as those of GOETHE and the German psychologists, NEWTON and THOMAS YOUNG, this work seems to have presented HILBERT with a further, unexplored territory in which the axiomatic approach could usefully be applied. In fact, OPPOLZER's article was in this sense symptomatic of a more general situation in contemporary research in psychophysics,[230] and was therefore well-suited to exemplify HILBERT's claims concerning the careless introduction of new assumptions into existing physical theories.

The manuscript of the lectures makes no mention of the differences between HILBERT's formulation and OPPOLZER's own. HILBERT simply put forward his axioms, which are defined for a collection of "brightnesses" $x_1, x_2, \ldots$. The axioms postulate the following properties that the brightnesses are required to satisfy:

1. To every pair of brightnesses x_1, x_2, a third one $[x_1, x_2]$ can be associated, called "the brightness of the mixed light of x_1, x_2." Given a second pair of brightnesses x_3, x_4, such that $x_1 = x_3$ and $x_2 = x_4$, then $[x_1, x_2] = [x_3, x_4]$.
2. The "mixing" of various brightnesses is associative and commutative.
3. By mixing various homogeneous lights of equal wave-lengths, the brightness of the mixed light has the same wave length, while the intensity of the mixed light is the sum of the intensities.

Experience, said HILBERT, amply confirms these three axioms. The first one contains what HILBERT called the law of GRASSMANN, namely, that intensities

[229] [189] Das Hauptproblem ist, diese Helligkeit x als Funktion der Bestimmungstücke der das Licht physisch (sic) zusammensetzenden homogenen Lichter (d.i. Intensität und Wellenlänge eines jeder) darzustellen.

[230] As KREMER 1993, 257, describes it: "For a variety of philosophical, institutional and personal reasons, color researchers between 1860 and 1920 simply could not agree on which color experiences are quintessential or on what criteria are appropriate to evaluate hypothetical mechanisms for a psychoneurophysiological system of sensation."

that are psychically equal (but may be physically different), remain equivalent at the psychical level, after they are physically mixed.[231]

If one calls the uniquely determined number $[x_1, x_2]$, x_{12}, one can then write it as a function of the two parameters

$$x_{(12)} = f(x_1, x_2).$$

From the second axiom, one can derive the functional equation:

$$f(f(x_1, x_2), f(x_3, x_4)) = f(f(x_1, x_3), f(x_2, x_4)) = f(f(x_1, x_4), f(x_2, x_3)).$$

One can then introduce a new function F that satisfies the following relation:

$$F(x_{12}) = F(f(x_1, x_2)) = F(x_1) + F(x_2).$$

From axiom 3, and assuming the by now well-known general postulate of continuity, it follows that the function F, for homogeneous light, is proportional to the intensity. This function is called the "stimulus value" (*Reizwert*), and once it is known, then the whole theory becomes, so HILBERT claimed, well-established. One notices immediately, HILBERT went on to say, the analogy with the previously studied domains, and especially with the theorem of existence of a function $l(x)$ in life-insurance mathematics. This very analogy could suffice to show, he concluded, that in this latter domain also, so far removed from the earlier ones, the approach put forward in the whole course would become fruitful.[232]

HILBERT's treatment of psychophysics, at least as it appears in the manuscript, was rather sketchy and its motivation was far from obvious, since he did not provide any background for understanding the current research problems of this domain. Moreover, as in the case of probabilities, HILBERT did not examine the logical interrelations among the axioms, beyond the short remarks quoted in the preceding paragraphs. Yet, in the context of his treatment of other physical domains and of the confused state of affairs in contemporary psychological research, one can grasp the breadth of application that HILBERT envisaged for the axiomatic method in science. HILBERT's ideas seem not to have influenced in any tangible way the current research of German psychologists, and one wonders whether or not there was any personal contact between him and his psychologist colleagues, at least in Göttingen.

In the years following this series of lectures, HILBERT himself became gradually involved in actual research in mathematical physics. To conclude the present discussion, it is interesting to notice that several years after having taught this course, HILBERT returned to the manuscript and added some remarks in his own handwriting on the front page, in which he mentioned two

[231] [189] Psychisch gleich Erscheinendes (was [190] aber physisch verschieden sein kann), bei der physischen Operation der Mischung wieder psychisch Gleiches gilt.

[232] [190] Das mag zur Kennzeichnung genügen, wie auch in diesem von den früheren so ganz verschiedenen Gebiete unsere Gedankengänge fruchtbar werden.

more recent works he thought relevant to understanding the use of the axiomatic method in physics. First, he simply 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 published in 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 it contains on the significance of his axiomatic method (HAMEL 1909, 358), and, more importantly perhaps, from its account of a new system of axioms for mechanics.[233]

Second, in a formulation that condenses in a very few sentences what HILBERT saw as the principles and goals of axiomatization, as applied to geometry and to various domains of physics, he 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 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.[234]

This remark may reflect some kind of contact of HILBERT with the ideas of PAUL EHRENFEST, either personally or through HILBERT's reading of the latter's *Encyclopädie* article (written in collaboration with his wife TATYANA). In fact, the two last terms used here by HILBERT (*Umkehr- oder Wiederkehreinwand*) were

[233] 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 by HAMEL (1927) that appeared in Vol. 5 of the *Handbuch der Physik.*

[234] ⟨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 bewussten 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.⟩

introduced only in 1907 by the EHRENFESTS,[235] and were made widely known only through the *Encyclopädie* article that appeared in 1912. 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 EHRENFEST's article definitely contributed to clarify.[236] 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 EHRENFEST's central contributions.[237]

Concluding Remarks

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 colleague at Königsberg, PAUL VOLKMANN, show that towards the end of the century questions of this kind were also discussed in the circles HILBERT moved in. From his earliest attempts to treat geometry in an axiomatic fashion in order to solve the 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.

The lecture notes of HILBERT's 1905 course on the axiomatic method provide the earliest encompassing evidence of HILBERT's own picture of physical science in general and, in particular, of how he thought that the axiomatic analysis of individual theories should be carried out. This interesting document shows that HILBERT's interests covered a very wide range, and he seems to have been well

[235] On November 13, 1906, PAUL EHRENFEST gave a lecture at the Göttingen Mathematical Society, at which HILBERT was most likely present, on BOLTZMANN's H-theorem and some of the objections (*Einwände*) commonly raised against it. This lecture is reported in the *Jahresbericht der Deutschen Mathematiker-Vereinigung*, Vol. 15 (1906) p. 593.

[236] See KLEIN 1970, 119–140.

[237] See KLEIN 1970, 230–257.

aware of the main open questions being investigated in most of the domains addressed. HILBERT's 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 was not a characteristic trait of HILBERT's working style to study 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. From his repeated, enthusiastic reference to HERTZ's textbook we cannot safely infer that he had read that book thoroughly, or even cursorily. 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 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 was interested, study the topic more thoroughly and develop his own ideas. It is thus hard to determine with exactitude how far he really commanded all the details of each theory and each topic discussed in his lectures.

It is also important to qualify properly the extent to which HILBERT carried out a true 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 no case, in the framework of the lectures, did HILBERT actually prove the independence, consistency or completeness of the axiomatic systems he introduced. In certain cases, like vector addition, he quoted works in which such proofs could be found (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. Many times 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 the *Grundlagen*.

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 heretofore unexamined 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 a detailed fashion 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 of 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 the newly discovered empirical data. As I have suggested in various places above, HILBERT's own future research in physics would be increasingly guided by this conception. The details of his efforts in this area call for additional research which I intend to undertake in the future.

In HILBERT's treatment of physical theories we find diverse kinds of axioms that reflect a classification previously found in the writings of PAUL VOLKMANN. In the first place, every theory is assumed to be governed by specific axioms that characterize it. These axioms usually express mathematical properties establishing relations among the basic magnitudes involved in the theory. Then, there are certain general mathematical principles that HILBERT thought should be 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 more thoroughly, while on others he simply declared that such a derivation should be possible.

There is yet a third type of axiom for physical theories, however, which HILBERT avoided addressing in his 1905 lectures. That type comprises claims about the ultimate nature of physical phenomena, an issue which was particularly controversial during the years preceding these lectures. Although HILBERT's sympathy for the mechanical world-view is apparent throughout the manuscript of the lectures, his axiomatic analyses of physical theories contain no direct reference to it. The logical structure of the theories is thus intended to be fully understood independently of any particular position in this debate. HILBERT himself, as I suggested above, would later adopt a different stance. His work on general relativity was based directly on his adoption of the electromagnetic world-view 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. That is to say, MINKOWSKI was exploring the implications of the adoption of the postulate of relativity as a general principle of physics (comparable to HILBERT's principle of continuity), while at the same time *avoiding* the debate between the mechanical and the electromagnetic world views.[238]

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 them 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. 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 to 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. I mentioned above the works of BORN[239] and CARATHÉODORY on thermodynamics, and of MINKOWSKI on electrodynamics. Then there were the many dissertations written under Hilbert, as well as the articles written under the influence of his lectures and seminars. I also suggested a possible influence on EHRENFEST's style of conceptual clarification of existing theories, especially as manifest in the famous *Encyclopädie* co-authored by PAUL and TATYANA EHRENFEST article on the kinetic theory of gases. HILBERT's actual influence on the various disciplines of physics is an issue that merits further investigation. On the other hand, we can say that relatively little work on physical theories was published along the specific lines of axiomatic analysis suggested by HILBERT in the *Grundlagen*. It seems, in fact,

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[238] In CORRY 1997 I present this interpretation in greater detail.

[239] In fact, BORN claimed in his autobiography (1978, p. 99) that HILBERT's lectures on physics, and in particular the lectures on kinetic theory of gases, deeply influenced all his work, including his contributions to the establishment of quantum mechanics between 1920 and 1925.

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 — whose name I mentioned above in relation with the axioms of vector addition — 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.

All in all, HILBERT's work on physics did not gain widespread acceptance among physicists. For instance, it is well known that EINSTEIN, in a letter to HERMANN WEYL, judged HILBERT's approach to the general theory of relativity to be "childish . . . in the sense of a child that recognizes no malice in the external world."[240] WEYL himself considered that, compared to HILBERT's work in pure mathematics, his work in physics — and especially his application of the axiomatic method — was of rather limited value. A valuable contribution to physics, WEYL thought, required skills of a different kind from those in which Hilbert excelled. In one of his obituaries of HILBERT, WEYL wrote:

> The maze of experimental facts which the physicist has to take in account is too manifold, their expansion too fast, and their aspect and relative weight too changeable for the axiomatic method to find a firm enough foothold, except in the thoroughly consolidated parts of our physical knowledge. Men like Einstein and Niels Bohr grope their way in the dark toward their conceptions of general relativity or atomic structure by another type of experience and imagination than those of the mathematician, although no doubt mathematics is an essential ingredient.[241]

Be that as it may, and regardless of the actual influence of his ideas about the axiomatization of physics on subsequent developments in this discipline, it is important to bear in mind that a full picture of HILBERT's own conception of mathematics cannot be complete without taking into account his views on physical issues and the relationship between mathematics and physics. Hence the importance of studying the physical background to HILBERT's axiomatic conception and the contents of the lecture notes of 1905.

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[240] In a letter of November 23, 1916. Quoted in SEELIG 1954, 200.
[241] Quoted in SIGURDSSON 1994, 363.

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