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David Hilbert between Mechanical and Electromagnetic Reductionism (1910–1915)

LEO CORRY

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Contents

1. Introduction

In an article entitled "The Basic Equations of Physics", based on a talk delivered in Göttingen on November 1915, David Hilbert presented a new theory intended as a unified foundation of physics at large. This article has attracted the attention of historians of science because of the striking similarity between the field equations of gravitation presented by Hilbert in his talk and those presented by Einstein five days later as the correct, generally-covariant equations of gravitation that lie at the heart of the general theory of relativity. Two main foci of attention are discernible in existing historical accounts of this episode: (1) the possible influence of Hilbert on Einstein's final efforts towards the formulation of his own equations and (2) the inevitable question of priority in the discovery.[1]

Hilbert's incursion into this field has been seen, in one way or another, as an incidental aside from his well-conceived, main mathematical concerns. Still, what is usually considered to be a natural part in an otherwise forced constituent of his career is the fact that Hilbert's unified foundational theory implied an *electromagnetic* reductionistic world-view. After all, when we look at the history of the "electromagnetic field program" or the "electromagnetic world-view", we find many prominent members of the

[1] See Corry, Renn and Stachel 1997; Earman and Glymour 1978; Fölsing 1997, 375–377; Mehra 1974; Pais 1982, Chapt. 14; Vizgin 1994 Chapt. 2.

Göttingen scientific milieu among the leading names traditionally associated with this trend: Max Abraham (1875–1922), Walter Kaufmann (1871–1941), Walther Ritz (1878–1909), Karl Schwarzschild (1873–1916), Emil Wiechert (1861–1928).[2] Moreover, the work in electrodynamics of Hermann Minkowski (1864–1909), Hilbert's closest friend and colleague, has often been interpreted as an attempt to bring to completion the electromagnetic world-view through relativity.[3]

In a series of recent articles I have tried to show that Hilbert's interest in physical questions was a central, organic component of his overall worldview.[4] Seen in this light, Hilbert's attempt to formulate a unified foundational theory of physics appears as a natural step in a long chain that spans Hilbert's entire scientific career. On the other hand, Hilbert's early interest in physics was dominated by a clearly articulated belief in the possibility of reducing all physical phenomena to *mechanical* processes, and therefore it is his later adoption of an electromagnetic reductionism that needs to be explained. I have also discussed Minkowski's involvement with relativity and electrodynamics, showing that the connection between his works and the program for an electromagnetic foundation of physics was a very complex one.[5] Certainly Minkowski did in no sense undertake a completion of that program based on relativity, and thus his possible influence on Hilbert cannot provide a satisfactory explanation of the latter's electromagnetic reductionism either.

In the present article I describe Hilbert's involvement with physical issues between 1910 and 1915, both his published articles and his lectures in Göttingen. I focus particularly on the question how he moved from a strict mechanical reductionistic position to an electromagnetic one. Between 1902 and 1912 Hilbert's main field of mathematical research was the theory of linear integral equations. After Minkowski's unexpected death in 1909, however, he also returned to teach courses on physical issues. Between the years 1910 and 1913 the list of his physical lectures covers an unprecedented variety of topics: mechanics, continuum mechanics, statistical mechanics, radiation theory, molecular theory of matter, electron theory, electromagnetic oscillations. Besides the first two of these, Hilbert had never taught such topics in the past, though from the manuscripts of his 1905 lectures on axiomatization, we already know that they had indeed attracted his attention. In all these lectures Hilbert continued to endorse a mechanical reductionist point of view as the basis for all of physics. Over the years, Hilbert also published a series of works on the foundations of elementary theory of radiation and an important article on the foundations of the kinetic theory of gases. An examination of the evolution of Hilbert's thought along all these intensive years of activity in physics shows how the way was prepared for the formulation of his unified foundational theory.

As will be seen in detail, Hilbert's work on physics over these years continued to be connected with his program for axiomatization first formulated explicitly in 1900. The changes that affected his views are connected, among others, to this steady, central com-

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[2] On the "electromagnetic world-view" see Jungnickel & McCormmach 1986, 227–244; McCormmach 1970.

[3] This is best expressed in Galison 1979, 94.

[4] See Corry 1997, 1998, 1998a.

[5] See Corry 1997a.

ponent of his scientific approach. A second main factor was the increasing mathematical difficulty that affected the treatment of disciplines based on the atomistic hypothesis, and above all the kinetic theory. At some point Hilbert became convinced that this difficulty was so great that it indicated the necessity of changing the most basic assumption behind it.

A main difficulty faced when trying to trace the changes in Hilbert's views stems from his style of presentation. Hilbert never informed his audiences about the changes that had affected his views on any issues, and obviously he never explained the reason behind this change. His pronouncements are always optimistic and very seldom they express doubts about the viability of a given scientific program. Thus when Hilbert adopted the electromagnetic point of view there is no clue whatsoever in his writings or in his lectures, that in the past he held a different view or that his new position may encounter some difficulties in the way to its full implementation. Still, the existing documents provide plenty of information which I'll use here in my attempt to understand when, why and how electromagnetic reductionism came to dominate Hilbert's approach to physics.

2. From mechanics to radiation theory (1910–1913)

In the winter semester of 1910-11 Hilbert taught a course on mechanics, his first one after Minkowski's death. He opened his course by repeating his belief that mechanics should be taken as the foundation of natural science in general (Hilbert 1910-1, 6). As he had done many times in the past, he praised Hertz's and Boltzmann's textbooks for their attempts to present, starting from somewhat different premises, a fully axiomatic derivation of mechanics. At the same time, however, he stressed that this kind of presentation was currently being disputed. After discussing all the basic issues of a standard course in mechanics Hilbert arrived towards the end of his series of lectures to his treatment of the "new mechanics". Hilbert did not use the word "relativity" in this context, nor he mentioned Einstein. Rather, he only mentioned Lorentz and singled out as the main feature of this new mechanics the invariance under the Lorentz transformations of all differential equations that describe natural phenomena. Hilbert remarked that the Newtonian equations of the "old" mechanics do not satisfy this basic principle, which, like Minkowski, he called *Weltpostulate*. These equations must therefore be transformed, so that they become Lorentz-invariant.[6] Hilbert showed that if the Lorentz transformations are used instead of the "Newton transformations", then the velocity of light is the same for every non-accelerated moving system of reference.

[6] Hilbert 1910-1, 292: "Alle grundlegenden Naturgesetzen entsprechenden Systeme von Differentialgleichungen sollen gegenüber der Lorentz-Transformation kovariant sein...Wir können durch Beobachtung von irgend welcher Naturvorgängen niemals entscheiden, ob wir ruhen, oder uns gleichformig bewegen. Diesen Weltpostulate genügen die Newtonschen Gleichungen der älteren Mechanik nicht, wenn wir die Lorentz Transformation zugrunde legen: wir stehen daher vor die Aufgabe, sich dementsprechend umgesalten."

Towards the end of the course Hilbert addressed the unresolved status of gravitation in the framework of this new mechanics, while directly referring to Minkowski's treatment of the question in 1909. Although this treatment had been extremely sketchy, Minkowski had been confident that it would eventually lead to developing a Lorentz-covariant theory of gravitation. Hilbert shared this confidence in his lectures. One should attempt to modify the Newtonian law, he said, in order to make it comply with the World-postulate. However, we must exercise special care when doing this, since the Newtonian law has proved to be in the closest accordance with experience. As Hilbert knew from Minkowski's work, adapting gravitation to the new mechanics would imply that gravitation must propagate with the speed of light. This latter conclusion contradicts the "old theory", but in the framework of the "new mechanics", on the contrary, it finds a natural place. In order to adjust the Newtonian equations to the new mechanics, concluded Hilbert, we proceed, "like Minkowski did, via electromagnetism,"[7] Hilbert did not specify, however, what he meant by this.

Beginning in mid-1911 Hilbert became increasingly interested in the question of the structure of matter, and in the possibility of addressing this question as the key to providing a unified foundation for the whole of physics. This interest was manifest in both his courses and his published work. Thus, in the winter of 1911-12 Hilbert taught for the first time a course specifically devoted to the kinetic theory of gases. In the introduction to the course, he discussed three possible ways of studying different physical theories like hydrodynamics, electricity, etc. First, he mentioned the "phenomenological perspective", often applied to study the mechanics of continua. Under this perspective, the whole of physics is divided into various chapters: thermodynamics, electrodynamics, optics, etc. Each of these can be approached using different assumptions, peculiar to each of them, and deriving from these assumptions different mathematical consequences. The main mathematical tool used in this approach is the theory of partial differential equations.

A much deeper understanding of the physical phenomena involved in each of these domains is reached – Hilbert told his students – when the atomistic theory is invoked. In this case, one attempts to put forward a system of axioms which is valid for the whole of physics, and which enables explaining all physical phenomena from a single, unified point of view. The mathematical methods used when following this point of view are obviously quite different from those adopted in the phenomenological perspective. They can be subsumed, in general, 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. Seen from the point of view of this approach, Hilbert stated, the phenomenological

[7] Hilbert 1910-1, 295 "Wir können nun an die Umgestaltung des Newtonsches Gesetzes gehen, dabei müssen wir aber Vorsicht verfahren, denn das Newtonsche Gesetz ist das desjenige Naturgesetz, das durch die Erfahrung in Einklang bleiben wollen. Dieses wird uns gelingen, ja noch mehr, wir können verlangen, dass die Gravitation sich mit Lichtgeschwindichkeit fortpflantz. Die alte Theorie kann das nicht, eine Fortpfalntzung der Gravitation mit Lichtgeschwindichkeit widerspricht hier der Erfahrung: Die neue Theorie kann es, und man ist berechtigt, das als eine Vorzug derselben anzusehen, den eine momentane Fortpflanzung der Gravitation passt sehr wenig zu der modernen Physik.
Um die Newtonschen Gleichungen für die neue Mechanik zu erhalten, gehen wir ähnlich vor wie Minkowski in der Elektromagnetik."

perspective appears as a palliative, necessary as a primitive stage in the way to real knowledge, which we must however abandon as soon as possible in order to gain entry into the "real sanctuary of theoretical physics" (Hilbert 1911-12, 2). Unfortunately, he said, mathematical analysis is not yet so developed as to enable fulfilling all the demands of this approach. We must therefore do without rigorous logical deductions, in this case, and be temporarily satisfied with rather vague mathematical formulas. Still, Hilbert said, it is amazing that using this method we nevertheless obtain ever new results that are in close agreement with experience.

But yet a third approach, which in Hilbert's view corresponded to the main task of physics, is the study of the molecular theory of matter itself. The study of this theory stands above the kinetic theory, as far as its degree of mathematical sophistication and exactitude is concerned. In the present course, Hilbert intended to concentrate on the kinetic theory, yet he promised to consider the molecular theory of matter in the following semester.

And indeed, in the summer semester of 1912, Hilbert taught a course on the theory of radiation. Connecting this topic with the promise issued at the beginning of the preceding semester, Hilbert declared that he intended to address now the "domain of physics properly said", which is based on the point of view of the atomic theory. Hilbert was clearly very much impressed by recent developments in quantum theory. The importance of these developments had particularly been discussed and highlighted during the First Solvay Conference, held in Brussels in October 1911,[8] echoes of which most likely reached Hilbert through his physicist colleagues. "Never has there been a most propitious and challenging time than now," Hilbert said in the opening lecture of his course, "to undertake the study of the foundations of physics." What seems to have impressed Hilbert more than anything else were the deep interconnections recently discovered in physics, "of which formerly no one could have even dreamed, namely, that optics is nothing but a chapter of the theory of electricity, that electrodynamics and thermodynamics are one and the same, that also energy possesses inertial properties, that physical methods have been introduced into chemistry as well" (Hilbert 1912c, 2). And above all, the "atomic theory", the "principle of discontinuity", as Hilbert said, which today is not hypothesis anymore, but rather, "like Copernicus's theory, a fact confirmed by experiment." Very much like the unification of apparently distant mathematical domains, which played a leading role throughout his career, the unity of physical laws exerted a strong attraction on Hilbert.

In 1912 Hilbert enrolled an assistant for physics, who was commissioned with the task of keeping him abreast of current developments in the various branches of physics. Paul P. Ewald (1888–1985), who had recently finished his dissertation in Munich, was the first to hold this position, which Hilbert maintained for many years to come. That year Hilbert also published his first article specifically devoted to a physical issue (Hilbert 1912a). It appeared as the last installment of his treatise on the theory of linear integral equations (Hilbert 1912), and it dealt with the foundations of the kinetic theory of gases. Among other things, his work on integral equations implied a solution of the all-important Boltzmann equation, but indeed Hilbert saw his work from a much broader perspective,

[8] See Kormos Barkan 1993.

and in close relation with the call for an axiomatization of physics issued as part of the famous 1900 list of problems. Hilbert was always after the larger picture, searching for the underlying connections among apparently distant fields. In many occasions he stressed the multiple connections of his work on the kinetic theory with other physical domains, and in particular with radiation theory, as he did in the following passage:

> In my treatise on the "Foundations of the kinetic theory of gases", I have showed, using the theory of linear integral equations, that starting alone from the Maxwell-Boltzmann fundamental formula – the so-called collision formula – it is possible to construct systematically the kinetic theory of gases. This construction is such, that it only requires a consistent implementation of the methods of certain mathematical operations prescribed in advance, in order to obtain the proof of the second law of thermodynamics, of Boltzmann's expression for the entropy of a gas, of the equations of motion that take into account both the internal friction and the heat conduction, and of the theory of diffusion of several gases. Likewise, by further developing the theory, we obtain the precise conditions under which the law of equipartition of energies over the intermolecular parameter is valid. A new law is also obtained, concerning the motion of compound molecules, according to which the continuity equation of hydrodynamics has a much more general meaning than the usual one. ...
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> Meanwhile, there is a second physical domain whose principles have not yet been investigated at all from the mathematical point of view, and for the establishment of whose foundations – as I have recently discovered – the same mathematical tools provided by the integral equations are absolutely necessary. I mean by this the elementary theory of radiation, understanding by it the phenomenological aspect of the theory, which at the most immediate level concerns the phenomena of emission and absorption, and on top of which stand Kirchhoff's laws concerning the relations between emission and absorption. (Hilbert 1912b, 217, 218)

As I have described Hilbert's work on kinetic theory elsewhere,[9] I will present here in greater detail that on radiation theory. At any rate, it is worth pointing out from the beginning that the actual, positive contribution of his axiomatic analysis is more limited than what Hilbert's somewhat pretentious declaration in this passage would make us believe.

At the focus of Hilbert's published work on radiation theory we find, indeed, an axiomatic treatment of Kirchhoff's laws of emission and absorption. Gustav Kirchhoff (1824–1887) had established the laws governing the energetic relations of radiation in a state of thermodynamical equilibrium. According to these laws, in the case of purely thermal radiation (i.e., radiation produced by thermal excitation of the molecules) the relation between the emission and absorption capacities of matter is a universal function of the temperature and the wavelength, and it is therefore independent of the substance and of any other characteristic of the body in question. In his work on the theory of radiation, Max Planck (1857–1947) substituted Kirchhoff's concepts of emission and

[9] See Corry 1998.

absorption capacity by the coefficients of emission and absorption, ϵ and α respectively, defined for an element of volume. Planck showed that Kirchhoff's law could be formulated as follows: the ratio $\frac{q^2\varepsilon}{\alpha}$ (q being the speed of light propagation in the body) is independent of the substance of the body involved, and it is a universal function of the temperature and the frequency of radiation.[10]

In his first article on radiation theory (Hilbert 1912b) Hilbert attempted to provide the foundations of this theory, while avoiding the kinds of simplifications usually introduced by physicists (e.g., that the body is homogeneous, simply limited, etc.). Hilbert assumed that the three parameters ε, α and q are given by some arbitrary functions of their spatial location, and showed that the demand for energy equilibrium for each color leads to a separate, homogeneous integral equation of the second type for ϵ, whose unique solution is $\varepsilon = \frac{\alpha}{q^2}K$ (where K is a constant).

Although Hilbert declared from the beginning that his foundational study of radiation theory was axiomatic, it was only in an article published the following year (Hilbert 1913) and especially in his second talk on the topic before the Göttingen Academy (Hilbert 1913a), that he articulated the axioms laying at the basis of his theory and studied more systematically their interrelations. In a formulation that can be found in many other of his publications, Hilbert declared that his presentation of the theory was as strict as that of geometry in the *Grundlagen der Geometrie*. He explained, however, that since the publication of his earlier article, he had realized the need to include some additional axioms. Later, in his last publication in this domain (Hilbert 1914), Hilbert even discussed the consistency of the axioms, a rather uncommon step in his works on physical theories. It is pertinent to describe here cursorily the axioms and what Hilbert claimed to have done with them.

It is worth noticing, in the first place, that in the footnotes and references appearing in his various articles, Hilbert mentioned a considerable number of works in the field: by Planck, Ernst Pringsheim (1859–1917), W. Behrens, Rudolf Ladenburg (1882–1952), Max Born (1882–1970), and S. Bougoslawski. It would appear, however, that at least part of those works Hilbert got to read only after a number of objections to his first article were raised by Pringsheim, leading to a somewhat heated debate between the two. This debate is illustrative of the typical way in which a physicist could have reacted to Hilbert's approach to physical issues, and of how Hilbert's treatment, rather than presenting the systematic and finished structure characteristic of the *Grundlagen*, was piecewise, ad-hoc and sometimes confused or unilluminating.

Pringsheim's objections concerned the general approach adopted by Hilbert, as well as many of the details of his arguments. Pringsheim also stressed the significant differences between Hilbert's successive articles, in spite of the latter's insistence that there were none. It is noteworthy that also in his later work in general relativity, Hilbert published several versions and claimed that they were essentially identical – a claim that is not confirmed by a detailed examination of the various versions.[11] At any rate, Pringsheim claimed that by focusing on the inadequacies of all former proofs of Kirchhoff's theorem Hilbert was assuming, as a basis for his own proof, a fact that Kirchhoff and

[10] For Planck's work see Kuhn 1978.
[11] See Corry 1998a.

all other physicist had considered to be in urgent need of proof, namely, the fact that the radiation at each wavelength is in itself in equilibrium and that no interchange of energy takes place between different spectral regions. In fact, Pringsheim claimed, a main task of Kirchhoff's work was precisely to prove this assertion.[12] Hilbert had to admit the validity of these objections, and his successive articles were in fact attempts to reorganize his thoughts while paying attention to Pringsheim's criticism. Hilbert claimed throughout the articles, however, that the main reason for applying the axiomatic method to study this particular physical theory was precisely the need to introduce some order into the entanglement of physical assumptions and mathematical derivations that, in Hilbert's opinion, affected it.

In order to prove the impossibility of deriving the Kirchhoff–Planck equations starting from the assumption of equilibrium of total energy for all wavelenghts, Hilbert had set the values q and α equal to 1, independently of the value of the wavelength λ. Pringsheim considered this step inadequate, because no actual body in nature has as its absorption coefficient $\alpha = 1$, and at the same time no dispersion whatsoever (i.e., $q = 1$, the velocity of light in vacuum).

A second objection of Pringsheim against Hilbert's work was that the latter had not taken in account the effects of dispersion and reflection. Hilbert's last article was written as an attempt to prove that even when these are taken into consideration, his proof remains valid. Hilbert's 1914 version of the theory of radiation included four axioms, as follows:

> Axiom A (Axiom of the compensation of the total energy): Every optical system admits a state of radiation equilibrium. In this state, the total amount of energy emitted by all colors from any given volume element equals its total absorbed energy.
> Axiom B (Axiom of the compensation of energy for each individual color): Every optical system admits a state of radiation equilibrium. In this state, there is no exchange of radiant energy corresponding to different colors at any given region of matter. Moreover, the radiation corresponding to each color is itself in a state of independent equilibrium.
> Axiom C (Axiom of the physical nature of the radiation density): In the – always possible – state of equilibrium, the density of the radiation energy of every wavelength is uniquely determined by the physical conditions of matter in the region where the matter is found, and by them alone.
> Axiom D (Axiom of the existence of certain differences among substances): There are substances for which the values of α (absorption coefficient) and q (velocity of propagation of light) are such that the quotient α/q^2 equals the value of any arbitrarily function of λ prescribed in advance. (Hilbert 1914, 241)

Hilbert explained that Axiom A was equivalent, in essence, to the energy principle, whereas the other three axioms contained the essence of the principles that Hilbert himself in his first article, Planck in his textbook on radiation (Planck 1906) and Pringsheim

[12] Pringsheim published his objections in Pringsheim 1913, 1913a.

in an earlier work (Pringsheim 1903), had put forward as a foundation for the theory of radiation. He then proceeded to derive Kirchhoff's laws using the four axioms.

In a section entitled "Radiation theory and elementary optics" Hilbert discussed some broader implication of his axiomatic analysis of the theory. Insofar as the consistency of the systems of axioms has not been proved, he said, we cannot know whether not only the actual laws can be derived, but perhaps also their negations. This would imply that the derivations in the former section are indeed correct, but perhaps also meaningless. In fact, Hilbert said, it is by no means obvious *a-priori* why it might not be possible to find a certain arrangement of pieces of matter with different optical properties, such that Axiom A and the laws of refraction and reflection of elementary optics are not satisfied simultaneously. If this were the case, then at least part of the mathematical formulation of the laws derived would be inexact. Thus, the question of the consistency of this system of axioms, he concluded, touches upon the question of the exactitude of the laws of Kirchhoff.

How can this question be decided? Hilbert proposed to find out whether certain theorems about the energy distribution of individual rays under refraction and reflection could be construed as necessary consequences of the four axioms. In doing this he used only axioms A and D (or rather a variant, D*, of the latter), and a single formula of elementary optics, namely, the formula expressing the reflected energy of a perpendicular ray:

$$E_r = ME_e.$$

Here M depends on the values of α and q of the given medium, and its exact expression had been recently derived by Born and Ladenburg (1911). After some elaborate mathematical arguments Hilbert concluded that the following theorem had been proved:

> Assume space to be full with two transparent media, separated by a plane, and let two rays of natural light with arbitrary wavelengths but having the same energy be incident, from different sides on the separation surface, in such a way that the first ray after crossing the surface has the same direction than the second one after being reflected on it. Then the ray created in this process by the composition of the two given ones is itself a ray of natural light with the same given energy. (Hilbert 1914, 252)

Since this theorem, according to Hilbert, had been derived using Axioms A and D*, and since it was a correct and accepted law of elementary optics that could have been independently derived from Fresnel's formula for reflection and refraction, he concluded that this derivation "had not led to any contradiction with the laws of elementary optic." This was the typical way in which Hilbert used to corroborate the value of his axiomatic analysis of physical theories, and we can find it in several of his other physical works.[13]

But the last section of the article also contained what Hilbert claimed to be a definitive proof of the internal consistency of his system of axioms and of the lack of contradiction between the latter and the laws of optics. In doing so, Hilbert was going much farther

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[13] And also in Minkowski's axiomatic analysis of the role of the relativity postulate in physics. See Corry 1997a, esp. pp. 283–285.

here than he had gone in the axiomatic analysis of any other physical domain thus far. In fact, whenever he had discussed axiomatic systems for individual disciplines in the past, he never accompanied his discussion with a detailed analysis of the kind he had performed for geometry, though he very often declared this to be the case. This time he at least included some detailed argument concerning the consistency of his system, although this is far from being a completed proof of consistency. Like for other domains of physics, Hilbert's analysis of the logical interrelations among the basic concepts and the principles of the theory, and of their relations to other physical domains certainly provided a degree of clarity unlike that of any of the previous works in the discipline. However, there seems to be a considerable distance separating his declarations about the strict logical character of his axiomatic analysis – and especially about its similarity with what he had formerly done in geometry – on the one hand, and what he actually did in the article, on the other hand.

Hilbert's articles on radiation theory, at any rate, attracted only scarce attention from physicists. Max Born explained the reason for this neglect adducing the fact that new works appeared soon, dealing with deeper problems of radiation theory (especially the law of spectral energy distribution of the black body) which became far more important than the issues dealt with in Hilbert's articles. These new works, Born claimed, uncovered many interesting connections with the foundations of physics, that had led to a turning point in our understanding of radiation.[14]

3. The structure of matter

The atomistic hypothesis was a main physical assumption underlying all of Hilbert's work from very early on, and also in the period that starts in 1910. This hypothesis, however, was for him secondary to more basic, mathematical considerations of simplicity and precision. A main justification for the belief in the validity of the hypothesis was the prospect of a more precise and detailed explanation of natural phenomena, once the tools will be developed for a comprehensive mathematical treatment of theories based on it. Already in his 1905 lectures on the axiomatization of physics Hilbert had stressed the problems implied by the combined application of analysis and the calculus of probabilities as the basis for the kinetic theory, an application which is not fully justified on mathematical grounds.[15] In his physical courses after 1910, as we have seen, he expressed again similar concerns. Yet, the more Hilbert became involved with the study of the kinetic theory itself, and at the same time with the deep mathematical intricacies of the theory of linear integral equations, these concerns did not diminish. Rather, they only increased. This situation, together with his growing mastery of specific physical issues from diverse disciplines, help understanding Hilbert's increasing interest in questions related to the structure of matter that occupied him from mid-1912 on.

Lecturing in the winter semester of 1912-13 on the "Molecular Theory of Matter," Hilbert suggested that in order to overcome the deep mathematical difficulties implied by

[14] See Born 1922, 592–593.
[15] See Corry 1997, 167–168.

the atomistic hypothesis, one must adopt a “physical” point of view, i.e., one must make clear, through the use of the axiomatic method, those places in which physics intervenes into mathematical deduction. In this way, it may be possible to separate three different components of the specific physical domain considered: first, what is arbitrarily adopted as definition or taken as an assumption of experience; second, what one a-priori expects should follow from these assumptions, but which the current state of mathematics does not yet allow us to conclude with certainty; and third, what is truly proven from a mathematical point of view.[16] The awareness of the need to implement this separation – which interestingly brings to mind Minkowski’s earlier discussion on the status of the principle of relativity –[17] will certainly manifest itself in Hilbert’s reconsideration of his view of mechanics as the ultimate explanation of physical phenomena.

In the summer semester of 1913 we find interesting, additional evidence of Hilbert current interest in physical questions. In May, the Göttingen Royal Academy of Sciences organized a series of lectures on the current state of research in the kinetic theory. The invited lecturers included some of the leading physicists of the time. Max Planck, whose work on radiation Hilbert had studied in great detail when writing his own articles, lectured on the significance of the quantum hypothesis for the kinetic theory. Peter Debye (1884–1966) had become in 1914 professor of physics in Göttingen; his talk dealt with the equation of state, the quantum hypothesis and heat conduction. Walther Nernst (1864–1941), whose work on thermodynamics Hilbert had been following with interest,[18] spoke about the kinetic theory of rigid bodies. Marian von Smoluchowski (1872–1917) came from Krakow and lectured on the limits of validity of the second law of thermodynamics. Arnold Sommerfeld (1868–1951) came from Munich to talk about problems of free trajectories. Lorentz was invited from Leyden; he spoke on the applications of the kinetic theory to the study of the motion of the electron. That the meeting was an initiative of Hilbert is clear from the fact that it was sponsored by the *Wolfskehlstiftung*, whose chair was Hilbert himself. Hilbert wrote a report on the lectures delivered in the meeting,[19] as well as the introduction to the published collection of lectures, in which he expressed the hope that it would stimulate further interest, especially among mathematicians, and lead to additional involvement with the exciting world of ideas created by the new physics of matter. [20]

That semester Hilbert also taught two courses on physical issues, one of them on the theory of the electron and the second one on the principles of mathematics, quite

[16] 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 dritten, das, was beweisene mathematische Folgerung ist.”

[17] See Corry 1997a, 280.

[18] In January 1913, Hilbert had lectured on Nernst’s law of heat at the Göttingen Physical Society. The manuscript of the lecture is preserved in Hilbert’s *Nachlass*, Ms Cod 590. See also a remark added in Hilbert’s handwriting in Hilbert 1905, 167 (quoted in Corry 1997, 182)

[19] See the announcement in *Jahresberichte* DMV Vol. 22 (1913), pp. 68–69.

[20] See the proceedings of the meeting in Planck et al. 1914.

similar to his 1905 course on the axiomatic method and including a long section on the axiomatization of physics as well. Hilbert's lectures on electron theory emphasized throughout the importance of the Lorentz transformations and of Lorentz covariance, and continually referred back to the works of Minkowski and Born. Hilbert also stressed once again in this course his views concerning the need to formulate unified theories in physics, and to explain all physical processes in terms of motion of points in space and time.[21] From this reductionistic point of view, the theory of the electron would appear as the most appropriate foundation of all of physics.[22] However, given the difficulty of explicitly describing the motion of, and the interactions between, several electrons, Hilbert indicated that the model provided by the kinetic theory has to be brought to bear here. He thus underscored the formal similarities between mechanics, electrodynamics and the kinetic theory of gases, to which he had dedicated much effort over the preceding years. In order to describe the conduction of electricity in metals, he developed a mechanical picture derived from the theory of gases, which he then later wanted to substitute by an electrodynamical one.[23] Hilbert stressed the methodological motivation behind his quest after a unified view of nature, and the centrality of the demand for universal validity of the Lorentz covariance, in the following words:

> But if the relativity principle [i.e., invariance under Lorentz transformations] is valid, then it is so not only for electrodynamics, but also for the whole of physics. We would like to consider the possibility of reconstructing the whole of physics in terms of as few basic concepts as possible. The most important concepts are the concept of force and of rigidity. From this point of view the electrodynamics would appear as the foundations of all of physics. But the attempt to develop this idea systematically must be postponed for a later opportunity. In fact, it has to start from the 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.[24]

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

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[21] Hilbert 1913, 1: "Alle physikalischen Vorgänge, die wir eine axiomatische Behandlung zugängig machen wollen, suchen wir auf Bewegungsvorgänge an Punktsystem in Zeit und Raum zu reduzieren."

[22] Hilbert 1913, 13: "Die Elektronentheorie würde daher von diesem Gesichtpunkt aus das Fundament der gesamten Physik sein."

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

[24] Hilbert 1913, 13: "Die wichtigsten Begriffe sind die der Kraft und der Starrheit. Die Elektronentheorie würde daher von diessem Gesichspunkt 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."

his attention. Thus, for instance, Hilbert explained that in the existing theory of electrical conductivity in metals, only the conduction of electricity — which itself depends on the electron movements – has been considered, while assuming that the electron satisfies both Newton's second law, $F = ma$, and the law of collision as a perfectly elastic spherical body (like in the theory of gases).[25] This approach assumes that the magnetic and electric interactions between the electrons are described correctly enough in these terms as a first approximation. [26] However, Hilbert said, if we wish to investigate with greater exactitude the movement of the electron, while at the same time preserving the basic conception of the kinetic theory based on colliding spheres, then we should also take into account the field surrounding the electron and the radiation that is produced with each collision. We are thus led to investigate the influence of the motion of the electron on the distribution of energy in the free ether, or in other words, to the study of the theory of radiation from the point of view of the mechanism of motion of the electron. In his 1912 lectures on the theory of radiation, Hilbert had already considered this issue, but only from a "phenomenological" point of view. This time he referred to Lorentz's work as the most relevant one. [27] From Lorentz's theory, he said, we can obtain the electrical force induced on the ether by an electron moving on the x-axis of a given coordinate system.

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

In discussing the behavior of the single electron, Hilbert referred to the possibility of an electromagnetic reduction of all physical phenomena, freely associating ideas

[25] Hilbert 1913, 14 (Emphasis in the original): "In der bisherigen Theoire der Elektricitätsleitung in Metallen haben wir nur den Elektrizitätstransport, der durch die Bewegung der Elektronen selbst bedingt wird, in Betracht gezogen; unter der Annahme, dass <u>die Elektronen erstens dem Kraftgesetz $K = mb$ gehorchen und zweitens dasselbe Stossgesetz wie vollkommen harten elastischen Kugeln befolgen</u> (wie in der Gastheorie)."

[26] Hilbert 1913, 14: "Auf die elektrischen und magnetische Wirkung der Elektronen aufeinander und auf die Atome sind wir dabei nicht genauer eingegangen, vielmehr haben wir angenommen, dass die gegenseitige Beeinflussung durch das Stossgesetz in ester Annäherung hinreichend genau dargestellt würde."

[27] Hilbert 1913, 14 (Emphasis in the original): "Wollte man die Wirkung der Elektronenbewegung genauer verfolgen – jedoch immer noch unter Beibehaltung des der Gastheorie entlehnten Bildes stossender Kugeln – so müsste man <u>das umgebende Feld der Elektronen und die Strahlung in Rechnung stezen, die sie bei jedem Zusammenstoss aussenden</u>. Man wird daher naturgemäss darauf geführt, den Einfluss der Elektronenbewegung auf die Energieverteilung im freien ther zu untersuchen. Ich gehe daher dazu über, die Strahlungstheorie, die wir früher vom phänomenologischen Standpunkt aus kennen gelernt haben (SS 1912), aus dem Mechanismus der Elektronenbewegung verständlich zu machen. Eine diesbezügliche Theorie hat H.A. Lorentz aufgestellt."

developed earlier in their respective works by Mie and by Abraham. The Maxwell equations and the concept of energy, Hilbert said, do not suffice to provide a foundation of electrodynamics; the concept of rigidity has to be added to them. Electricity has to be attached to a steady scaffold, and this scaffold is what we denote as an electron. The electron, he explained to his students, embodies the concept of a rigid connection of Hertz's mechanics. In principle at least it should be possible to derive all the forces of physics, and in particular the molecular forces, from these three ideas: Maxwell's equations, the concept of energy, and rigidity. However, he stressed, one phenomena has evaded so far every attempt at a electrodynamic explanation: the phenomenon of gravitation.[28] Still, in spite of the mathematical and physical difficulties that he considered to be associated with a conception of nature based on the model underlying the kinetic theory, Hilbert did not change at this stage his basic mechanistic approach, and in fact he asserted that the latter is a necessary consequence of the principle of relativity.[29]

4. Axiomatization of physics (1913)

Side by side with his more advanced course on electron theory, Hilbert gave an introductory course entitled "Elements and Principles of Mathematics." This course was very similar in spirit to the one he taught back in 1905 under the same name, and like its predecessor, it also contained a long section on the axiomatization of physical theories. The opening page of the manuscript mentions three main parts that the lectures intended to cover:

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

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

[28] Hilbert 1913, 61-62 (Emphasis in the original): "Auf die Maxwellschen Gleichungen und den Energiebegirff 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 bezeichnen wir als Elektron. In ihm ist der Begriff der starrer Verbindung der Hertzschen Mechanik verwirklicht. Aus den Maxwellschen Gleichungen, dem Energiebegriff und dem Starrheitsbegriff lassen sich, im Prinzip wenigstens, die vollständigen Sätze der Mechanik entnehmen, auf sie lassen sich die gesamten Kräfte der Physik, im Besonderen die Molekularkräfte zurückzuführen. Nur die Gravitation hat sich bisher dem Versuch einer elektrodynamischen Erklärung widersetzt."

[29] Hilbert 1913, 65: "Es sind somit die zum Aufbau der Physik unentbehrlichen starren Körper nur in den kleinsten Teilen möglich; man könnte sagen: das Relativitätsrpinzip ergibt also als notwendige Folge die Atomistik."

number and its axioms, and impossibility proofs. The details of the contents of this last part, though interesting in themselves, are beyond our present concerns here.

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

> The axiomatic method consists in choosing a domain and putting certain facts on top of it; the proof of these facts does not occupy us anymore. The classical example of this is provided by geometry.[30]

Hilbert also repeated the main questions that should be addressed when studying a given system of axioms for a determined domain: Are the axioms consistent? Are they mutually independent? Are they complete?[31] The axiomatic method, Hilbert declared, is not a new one; rather it is deeply ingrained in the human way of thinking.[32]

D. Hilbert.

Elemente u. Prinzipien der Mathematik.

— 1913. —

Hilbert's treatment of the axioms of physical theories repeats much of what he did in 1905 (the axioms of mechanics, the principle of conservation of energy, thermodynamics, calculus of probabilities, and psychophysics) but at the same time it comprises some new sections: one on the axioms of radiation theory, containing Hilbert's recently published

[30] Hilbert 1913a, 1: "Die axiomatische Methode besteht darin, daß man ein Gebiet herausgreiff und bestimmte Tatsachen an die Spitze stellt u. nun den Beweis dieser Tatsachen sich nicht weiter besorgt. Das Musterbeispiel hierfür ist die Geometrie."

[31] Like in the past, Hilbert is not referring here to the model-theoretical notion of completeness. See Corry 1997, 112.

[32] Hilbert 1913a, 5: "Die axiomatische Methode is nicht neu, sondern in der menschlichen Denkweise tief begründet."

ideas on this domain, and one on space and time, containing an exposition of special relativity. This latter section is especially interesting and I will consider it here in some detail. Still it is relevant to first comment briefly on a remark that appears in the section on mechanics.

In his 1905 course Hilbert had considered the possibility of introducing alternative systems of mechanics defined by alternative sets of axioms. One of the intended aims of Hilbert's axiomatic analysis of a given physical theory was to prepare it for the eventuality of new empirical discoveries that will compel us to introduce modifications: in an axiomatically built theory we will be able to introduce a new axiom or to modify an existing one, and the theory will retain its basic logical structure but at the same time it will be able to accommodate the new discovery. Yet if back in 1905, Hilbert saw the possibility of alternative systems of mechanics more as a mathematical exercise than as a physically interesting task, obviously the situation was considerably different in 1913 after relativity theory became established. This time Hilbert seriously discussed such possibility in the framework of his presentation of the axioms of Newtonian mechanics. Like in geometry, Hilbert said, one could imagine for mechanics a set of premises different from the usual ones and, from a logical point of view, one could think of developing a "non-Newtonian Mechanics."[33] More specifically, he used this point of view to stress the similarities between mechanics and electrodynamics. He had already done something similar in 1905, [34] but now his remarks had a much more immediate significance. I quote them here in some extent:

> One can now drop or partially modify particular axioms; one would then be practicing a *non-Newtonian, non-Galileian, or non-Lagrangian mechanics*. This has a very special significance: electrodynamics has compelled us to adopt the view that our mechanics is only a limiting-case of a more general one. Should anyone in the past have thought by chance of defining the kinetic energy as:
>
> $$T = \mu \frac{1 - \nu^2}{\nu} \log \frac{1 + \nu}{1 - \nu},$$
>
> he would have then obtained the [equation of] motion of the electron, where m is constant and depends on the electrons mass. If one ascribes to all of them [i.e., to the electrons] kinetic energy, then one obtains the theory of the electron, i.e., an essential part of electrodynamics. One can then formulate the Newtonian formula:
>
> $$ma = F$$
>
> But now the mass depends essentially on the velocity and it is therefore no more a physical constant. In the limit case, when the velocity is very small, we return to the classical physics. ...

---

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

[34] See Corry 1997, 173–174.

Lagrange's equations show how a point moves when the conditions and the forces are known. How these forces are created and what is their nature, however, this is a question which is not addressed.
Boltzmann attempted to build the whole of physics starting from the forces; he investigated them, and formulated axioms. His idea was to reduce everything to the mere existence of central forces of repulsion or of attraction. According to Boltzmann there are only mass-points, mutually acting on each other, either attracting or repelling, over the straight line connecting them. *Hertz* was of precisely the opposite opinion. For him there exist no forces at all; rigid bonds exist among the individual mass-points. Neither of these two conceptions has taken root, and this is for the simple reason that electrodynamics dominates all.
The foundations of mechanics, and especially its goal, are not yet well established. Therefore it has no definitive value to construct and develop these foundations in all detail, as it has been done for the foundations of geometry. Nevertheless, this kind of foundational research has its value, if only because it is mathematically very interesting and of an inestimably high value.[35]

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[35] Hilbert 1913a, 105–108 (Emphasis in the original): "Man kann nun gewisse Teile der Axiome fallen lassen oder modifizieren; dann würde an also "Nicht-Newtonsche", od. "Nicht-Galileische", od. "Nicht-Lagrangesche" Mechanik treiben.
Das hat ganz besondere Bedeutung: Durch die Elektrodynamik sind wir zu der Auffassung gezwungen werden, daß unsere Mechanik nur eine Grenzfall einer viel allgemeineren Mechanik ist. Wäre jemand früher zufällig darauf gekommen die kinetisch Energie zu definieren als:

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

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

$$mb = K$$

Nun hängt aber die Masse ganz wesentlich von der Geschwindigkeit ab und ist keine physikalische Constant mehr. Im Grenzfall, daß die Geschwindigkeit sehr klein ist, kommt man zu der alten Mechanik zurück. (Cf. H. Stark "Experimentelle Elektrizitätslehre", S. 630).
Die Lagrangesche Gleichungen geben die Antwort wie sich ein Punkt bewegt, wen man die Bedingungen kennt und die Kräfte. Wie diese Kräfte aber beschaffen sind und auf die Natur die Kräfte selbst gehen sie nicht ein.
Boltzmann hat versucht die Physik aufzubauen indem er von der Kräften ausging; er untersuchte diese, stellte Axiome auf u. seine Idee war, alles auf das bloße Vorhandensein von Kräften, die zentral abstoßend oder anziehend wirken sollten, zurückzuführen. Nach Boltzman gibt es nur Massenpunkte die zentral gradlinig auf einander anzieh. od. abstoßend wirkend.
Hertz hat gerade den entgegengesetzten Standpunkt. Für ihm gibt es überhaupt keine Kräfte; starre Verbindungen sind zwischen dem einzelnen Massenpunkten.
Beide Auffassungen haben sich nicht eingebürgert, schon aus dem einfachen Grunde, weil die Elektrodynamik alles beherrscht.
Die Grundlagen der Mechanik und besonders die Ziele stehen noch nicht fest, so daß es auch noch nicht definitiven Wert hat die Grundlagen in den einzelnen Details so auf- und ausbauen wie

This passage is very illuminating concerning Hilbert's conceptions by 1913. At the basis of his approach to physics stands, as always, the axiomatic method as the most appropriate way to examine the logical structure of a theory and to decide what are the individual assumptions from which all the main laws of the theory can be deduced. This deduction, however, as in the case of Lagrange's equation, is independent of questions concerning the ultimate nature of physical phenomena. Hilbert mentions again the mechanistic approach promoted by Hertz and Boltzmann, yet he admits, perhaps for the first time, that it is electromagnetism that pervades all physical phenomena.[36] Finally, the introduction of Lagrangian functions from which the laws of motion may be derived, yet more general than the usual ones of classical mechanics was an idea that in the past might have only be considered as a pure mathematical exercise; now – Hilbert cares to stress – it has become a central issue in mechanics, given the latest advances in electrodynamics. An approach of this kind will also provide the framework within which Hilbert's involvement with general relativity would develop in the near future.

The last section of Hilbert's discussion of the axiomatization of physics addressed the issue of space and time, and as its sub-title has it, it was in fact a discussion of the principle of relativity.[37] What Hilbert did in this section provides the most detailed evidence of his conceptions concerning the principle of relativity, mechanics and electrodynamics, before his 1915 paper on the foundations of physics. It is therefore pertinent to discuss it in some detail.

The latest research in physics, especially in the domain of electrodynamics – said Hilbert in the opening passages of this section, echoing Minkowski's 1907 lectures on the same issue – has led us to abandon the old Newtonian conception of space and time, and more precisely, to see the old laws governing the motion of a body as a special case of more general ones.[38] One of the alternatives Hilbert proposed for axiomatizing classical dynamics in the relevant section consisted in defining space axiomatically, using the already established axioms of geometry, and then expanding this definition with some additional axioms defining time. In principle, something similar could be done for the

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die Grundlagen der Geometrie. Dennoch behalten die axiomatischen Untersuchungen ihren Wert, schon deshalb, weil sie mathematisch sehr interessant und von unschätzbar hohen Werte sind."

[36] Although it is doubtful whether Hilbert's historical assessment on this respect fits what was actually the case by this time.

[37] The following bibliographical list appears in the first page of this section (p. 119):

M. Laue Das Relativitätsprinzip 205 S.

M. Planck 8 Vorlesungen über theoretische Physik

8. Vorlesung p. 110–127

A. Brill Das Relativitätsprinzip: ein Einführung in die Theorie 28 S.

H. Minkowski Raum und Zeit XIV Seiten

Beyond this list, together with the manuscript of the course, in the same binding, we find some additions, namely, (1) a manuscript version of Minkowski's famous work (83 pages in the same handwriting as the course itself), (2) the usual preface by A. Gutzmer, appearing as an appendix, and (3) two pages containing a passage copied from Planck's *Vorlesungen*.

[38] Hilbert 1913a, 120: "Man ist durch die neueren Untersuchungen in der Physik, von allem auf dem Gebiete der Elektrodynamik veranlaßt worden mit der alten Newtonschen Auffassung von Raum und Zeit zu brechen, oder genauer gesagt: Man hat für die Bewegungen eines Körpers allgemeinere Gesetze gefunden, von denen die fruheren Gesetze nur eine Spezialfall sind."

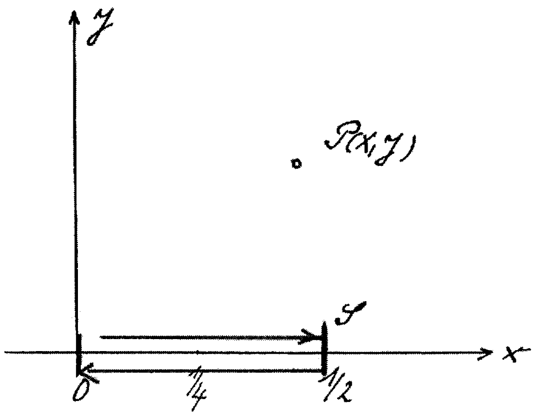

Figure 1

new conception of space and time, but the specific axioms defining time would clearly have to change. Hilbert thus assumed the axioms of Euclidean geometry and proceeded to redefine the concept of time. He introduced a unit of time based on the already defined concept of space, together with a light ray. He did it as follows: Take the point $x = 1/2$, on the x-axis, and lay a mirror on it. Send a ray of light from the origin O to that point, and let it be reflected again to the origin of coordinates. The velocity of light is set, once and for all, to be equal to 1. The time it takes the light-ray to cover the distance from the origin to the mirror and back is taken as the time-unit.[39] The following axiom is then postulated:

> If a light-ray sent from the origin is reflected by a mirror placed on a point $x = 1/4$ on the x-axis, and it covers twice the distance $\overline{O1/4}$ (back and forth), then the elapsed time equals 1. Moreover, light expands evenly in all directions, i.e., if a light-ray sent from the origin is reflected by a mirror placed at a distance 1/2, but not on the x.-axis (e.g., at a point $P(x, y)$, such that $\overline{OP} = 1/2$), then the time needed for the ray to return to O equals 1. [40]

In this way, said Hilbert, by means of a "light-pendulum" we determine the pace of the clock at O and therefore the time. [41] In the same way we can take any point in the plane, think of it as being equipped with a milestone (which is given by its coordinates), and

[39] Hilbert 1913a, 121 (Emphasis in the original): "Die Zeit, die ein Lichtstrahl braucht um von O nach dem Spiegel S und wieder zurück zu gelangen wollen wir als Zeiteinheit einführen."

[40] Hilbert 1913a, 121-122: "Wenn wir einem von O ausgehenden Lichtstrahl an einen im Punkt $x = 1/4$ aufgestellten Spiegel reflektieren und ihn die Strecke $\overline{O1/4}$ 2 mal durchlaufen lassen (hin und zurück), so soll auch die Zeit 1 verstrichen sein. Ferner soll sich das Licht nach allen Richtungen gleichmäßig ausbreiten d.h. wenn wir einen von O ausgehenden Lichtstrahl an irgend einem Spiegel reflektieren lassen, der von O der Abstand 1/2 hat aber nicht auf der x-Achse steht (z.B. im Punkte $P(x, y)$, wo $\overline{OP} = 1/2$), so soll er um nach O zurückzukehren ebenfalls die Zeit 1 brauchen".

[41] Hilbert 1913a, 122: "Auf diese Art haben wir durch die 'Lichtpendel' den Gang der Uhr in O und damit die Zeit bestimmt."

set up a clock on it.[42] All these clocks are totally independent form one another. They are set as follows: At time t, send from the origin O to P a light-ray. When it arrives at P, the clock at P should show the time $(OP + t)$. A similar process is applied at each point and the following axiom is then postulated:

> One always obtains identical time settings, irrespective of what point in space is chosen as "regulative point".[43]

Consequently, time can be established starting from any point in space and it should make no difference. Simultaneity is defined as follows:

> Two events at two different points P_1 and P_2 occur simultaneously, if the clocks at P_1 and P_2 show the same time when the event takes place.[44]

In this way, concluded Hilbert, we can actually determine any position and any time in space.[45] The next step is to investigate the properties of motion under this new conception. In order to do this, a second system of coordinates (ξ, η) is introduced having the same units as the original system (x, y), and such that the origin-point Ω of the second system moves with uniform velocity v in the direction of the old x-axis, and that the ξ-axis is always superposed on the x-axis. Assuming that time is equal for both systems of coordinates, the transformation laws between them can be written as follows:

$$\begin{aligned} x &= \xi + vt \quad & \xi &= x - vt \\ y &= \eta & \eta &= y \\ & & t = \tau & \end{aligned}$$

Using these transformations we can now regulate the time in the moving system of reference, as we did above for the stationary one. Given any point P in the plane, if a light-ray is sent from the origin, forming a angle ϑ with the x-axis, then its coordinates in the moving system are given by:

$$\begin{aligned} \xi &= x - vt = t\cos\vartheta - vt \\ \eta &= y \qquad\quad = t\sin\vartheta \end{aligned}$$

[42] Hilbert 1913a, 123: "Auf dieselbe Art können wir uns in jedem Punkt der Ebene, den wir uns mit einem Kilometerstein versehen denken, auf dem seine Coordinate angegeben sind, eine Uhr aufzustellen."

[43] Hilbert 1913a, 123: "Bei der Wahl eines anderem Punktes P als Regulativespunktes (bis jetzt war O der Punkt von dem die Regulierung ausging) erhalten wir genau die gleiche Zeiteinstellung."

[44] Hilbert 1913a, 124: "Zwei Ereignisse in P_1 und P_2 treten gleichzeitig ein, wenn die Uhren im Moment des Geschehens in P_1 und P_2 die gleiche Zeit angeben."

[45] Hilbert 1913a, 124: "So haben wir nun im Raum Ort und Zeit wirklich festgestellt."

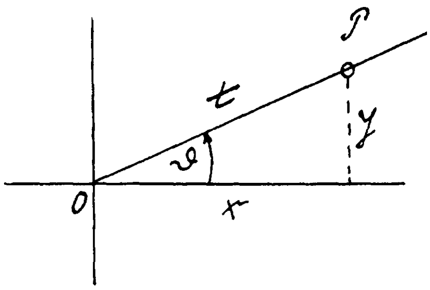

Figure 2

In this system the velocity of the ray-light γ_ϑ is given by:

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

At this point Hilbert connected the axiomatically constructed theory with the additional empirical consideration it was meant to account for, namely, the outcome of the Michelson-Morley experiment when the values of the latter formula for $\vartheta = 0, \pi/2, \pi$, are measured. Hilbert stressed the similarities between the situation in this case, and that in geometry, when one invokes Gauss's measurement of angles in the mountain triangle for determining the validity of Euclidean geometry. Hilbert said:

> Michelson set out to test the correctness of these relations, which were obtained working within the old conception of time and space. The [outcome of his] great experiment is, that these formulas do not work, whereas Gauss had experimentally confirmed (i.e., by measuring the Hoher Hagen, the Brocken, and the Inselsberg) that in Euclidean geometry, the sum of the angles of a triangle equals two right ones.[46]

From the negative result of Michelson's experiment, Hilbert concluded that the assumption implied by the old conception – according to which, the velocity of light measured in a moving system has different values in different directions – leads to contradiction. We are thus led to adopt the opposite assumption, namely to postulate that the velocity

[46] Hilbert 1913a, 124: "Diese aus der alten Auffassung von Raum und Zeit entspringende Beziehung hat Michelson auf ihre Richtigkeit geprüft. Das große Experiment ist nun das, daß diese Formel nicht stimmt, während bei der Euklidischen Geometrie Gauss durch die bestimmte Messung Hoher Hagen, Brocken, Inselsberg bestätigte, daß die Winkelsumme im Dreieck 2 Rechte ist."

On p. 128 Hilbert explained the details of Michelson's calculations, namely, the comparison of velocities at different angles via the formula:

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

where the remaining terms are of higher orders.

of light behaves with respect to moving systems like it had been already postulated for stationary ones. Hilbert expresses this as a further axiom:

> Also in a moving system, the velocity of light is identical in all directions, and in fact, identical to that in a stationary system. The moving system has no priority over the older one. [47]

Now the question naturally arises: what is then the true relation between t and τ? Hilbert answered this question by introducing the Lorentz transformations, which he discussed in some detail, including the limiting properties of the velocity of light,[48] and the relations with a third system, moving with yet a different uniform velocity.

A further question discussed by Hilbert in this context concerns the meaning of "simultaneity" under the new conception of time. He considered three different cases. First, if two events take place at the same place in a moving system, the interval of time between them, as observed from the stationary system, is longer than the one observed from the moving system. Second, if two events take place at the same time at different places in a moving system, then the length interval between them, as observed from the stationary system, is longer than the one observed from the moving one. This is the Lorentz contraction hypothesis.[49] Third, if two events in a stationary system take place at different places and at different times, then there are various possible relationships between their locations, times and speeds, which Hilbert discussed using the Lorentz transformations. Finally he drew the following conclusion, as a recapitulation of the last section:

> There is no absolutely determined system, since no system is to be preferred over another. From any given system, it is always possible to introduce a new one, by means of the Lorentz transformation:
>
> $$x^2 + y^2 + z^2 - t^2 = \xi^2 + \eta^2 + \varsigma^2 - \tau^2$$
>
> As the supreme *world-principle* we establish the following: *The laws of the world are independent of the Lorentz transformations -Postulate of Relativity*.[50]

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

[48] Hilbert 1913a, 132: "Eine größen Geschwindigkeit als die Lightgeschwindigkeit kann nicht vorkommen."

[49] Hilbert 1913a, 137 (Emphasis in the original): "Die Lorentzsche Kontraktionshypothese besagt also: 'Die Abmessung eines Körpers parallel zur Bewegungsrichtung wird also vom mitbewegten System eins größer beurteilt als von jedem andern'. Diese 'Ruhelange' ist also größer als alle andern Längen."

[50] Hilbert 1913a, 139–140 (Emphasis in the original): "Es gibt kein absolut festes System, denn kein System ist vor irgend einem anderem bevorzugt. Zu einem gegebenem System können wir immer ein neues einführen durch die Lorentztransformation.

$$x^2 + y^2 + z^2 - t^2 = \xi^2 + \eta^2 + \varsigma^2 - \tau^2$$

At this point Hilbert concluded his exposition of the axiomatic method, and in particular of the axioms of physics. It is worth noticing that one particular discipline he did not consider in his exposition was gravitational theory.

5. Mie's theory and electromagnetic reductionism

The preceding sections described Hilbert's consistent confidence in mechanical reductionism, at least until late 1913, as the key to attain a unified picture of all physical phenomena. This confidence obviously eroded at some point, since in 1915 Hilbert presented his well-known papers on the foundations of physics, where he developed a unified theory based on an *electromagnetic*, reductionistic point of view. A main factor behind this change of conception was the electromagnetic theory of matter developed by the German physicist Gustav Mie (1868–1957).

Beginning in 1912 Mie developed a theory at the center of which stood an articulate attempt to develop the idea that the electron cannot be ascribed physical existence independently of the ether. Of course, Mie was not the first to advance such an attempt, but his theory was certainly much more mathematically elaborate than most of the earlier ones. Mie had hoped that in the framework of his theory, the existence of the electron with finite self-energy could be derived from the field in purely mathematical terms. Mie also sought to explain the phenomenon of gravitation as a necessary consequence of his theory of matter; he intended to show that both the electric and the gravitational actions were a direct manifestation of the forces that account for the very existence of matter.[51]

Mie's theory was discussed in Göttingen by the end of 1912.[52] As we have seen, by that time Hilbert was deeply immersed in his research on the kinetic theory and on radiation theory. On the face of it, then, the questions addressed by Mie in his article must have strongly attracted Hilbert's attention. However, the lecture notes of the courses he taught in the winter semester of 1912–13 ("Molecular Theory of Matter"), or in the following semester ("Electron Theory") – in spite of their obvious, direct connection with the issue – show no evidence of a sudden interest in Mie's theory or in the point of view developed in it. Perhaps Mie's strong electromagnetic reductionism, which run contrary to Hilbert's current views, contributed to the latter's lack of interest in the theory. It was Max Born, among Göttingen physicists the closest to Hilbert, who took a direct interest in Mie's theory and further developed some of its main ideas. On December 1913 Born presented the results of his work on Mie's theory (Born 1914). This time he

Als oberstes Weltprinzip stellen wir folgendes auf: Die Weltgesetze sind unabhängig von jeder Lorentz-tranformationen - Relativitätspostulat."

Hilbert also added a remark at the end of the page, concerning the magnitude of the change in the earth's dimensions due to the Lorentz's contraction:

"Bem: Die Maxwellschen Gleichungen sind invarianten gegenüber der Lor. – Transf. Die Erde erscheint von der Sonne aus gemessen 6 cm. verkürtzt."

[51] The theory was published in Mie 1912, 1912a, 1913.

[52] See the announcement in the *Jahresberichte* DMV Vol. 22 (1913), 27.

seems to have done it in a way that did attract Hilbert's attention, who presented himself the work to the Göttingen Academy.[53]

Still, Hilbert did not rush to adopt Mie's theory and its underlying electromagnetic world-view, as we see from the manuscript of the course taught during the winter semester of 1913–14. This course dealt with electromagnetic oscillations and it was his last one on physical issues before he began developing his unified theory. In a certain sense it was a continuation of the course on electron theory taught during the previous summer semester of 1913. The manuscript of this course contains the first documented instance where Hilbert seems to allude to Mie's ideas and, indeed, the first explicit evidence of Hilbert's pondering to adopt electrodynamics, rather than mechanics, as the possible foundation to which all of physical explanation might be reduced. "It appears – Hilbert said – as if theoretical physics has finally and totally been absorbed by electrodynamics, to the extent that every special question should be solved, in the last instance, by appealing to electrodynamics."[54]

In the course itself, however, Hilbert did not actually address in any concrete way the kind of electromagnetic reduction suggested in its introduction. He claimed that the task of reducing all physical phenomena to the n-electron problem was still very far away from being actually achieved,[55] and suggested that in order to make some progress towards it, one should rely on a suitable application of the axiomatic method to the physical theories involved. Thus Hilbert said:

> Instead of providing a mathematical foundation starting from the equations of motion of the electron, it is still necessary to introduce some arbitrary assumptions, some temporary hypotheses that should later be substantiated, and also some assumptions of a very fundamental nature that must certainly be later modified. This inconvenience will remain insurmountable for a long time. However, what must nevertheless characterize our account is the fact that the truly necessary assumptions are all explicitly specified and that they are never confused with the consequences [of other assumptions].[56]

Hilbert did not specify what assumptions he meant to include under each of the kinds mentioned above. Yet, it would seem quite plausible to infer that the "assumptions of a

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[53] For a detailed account of Mie's theory and the crucial role played by Born in developing and transmitting the theory, see Corry 1999.

[54] Hilbert 1913–14, 1: "Es scheint indessen, als ob die theoretische Physik schliesslich ganz und gar in der Elektrodynamik aufgeht, insofern jede einzele noch so spezielle Frage in letzter Instanz an die Elektrodynamik appellieren muss."

[55] Hilbert 1913–14, 87: "Von der Verwirklichung unseres leitenden Gedankens, alle physikalischen Vorgänge auf das n-Elektronenproblem zurückzuführen, sind wir freilich noch sehr weit entfernt."

[56] Hilbert 1913–14, 87–88: "An Stelle einer mathematischen Begründung aus den Bewegungsgleichungen der Elektronen müssen vielmehr noch teils willkürliche Annahmen treten, teils vorläufige Hypothesen, die später einmal begründet werden dürften, teils aber auch Annahmen ganz prinzipieller Natur, die sicher später modifiziert werden müssen. Dieser Übelstand wird noch auf lange Zeit hinaus unvermeidlich sein. Unsere Darstellung soll sich aber gerade dadurch auszeichnen, dass die wirklich nötigen Annahmen alle ausdrücklich aufgeführt und nicht mit ein Folgerungen vermischt werden."

very fundamental nature, that must certainly be later modified," referred in some way or another to physical, rather than purely mathematical assumptions, and more specifically, to the atomistic hypothesis, on which much of his own physical conceptions had been based hitherto. An axiomatic analysis of the kind he deemed necessary for physical theories could indeed compel him to modify even his most fundamental assumptions if necessary. The leading principle should remain, in any case, to separate as clearly as possible the assumptions of any particular theory from the theorems that can be derived in it. Thus, the above quotation suggests that if by this time Hilbert had not yet decided to abandon his commitment to the mechanistic reductionism and its concomitant atomistic view, he was certainly preparing the way for that possibility, should the axiomatic analysis convince him of its necessity.

Hilbert referred in this course to ideas that are evidently connected to those of Mie's theory, yet he did not explicitly mention Mie's name (at least according to the record of the manuscript). He discussed the Maxwell equations not only outside the electrons, as was customary, but also inside them as Mie's theory postulated (Hilbert 1913–14, 89), and he formulated all the equations following Mie's train of thought.

Finally, Hilbert also addressed the problem of gravitation, and he did so from a rather peculiar, and sometimes hard to follow, point of view, that again would seem to allude to the themes discussed by Mie, without however explicitly mentioning his name. Hilbert explained that the problem that had originally motivated the consideration of what he called "diffuse electron oscillations" (a term he did not explain) was the attempt to account for gravitation. In fact, he added, it would be highly desirable – from the point of view pursued in the course – to explain gravitation based on the assumption of the electromagnetic field and the Maxwell equations, together with some auxiliary hypotheses, such as the existence of rigid bodies. The idea of explaining gravitation in terms of "diffuse radiation of a given wave length" was, according to Hilbert, closely related to an older idea first raised by Le Sage. The latter was based on the assumption that a great number of particles move in space with a very high speed, and that their impact with ponderable bodies produces the phenomena of weight.[57] However, Hilbert explained, more recent research has shown that an explanation of gravitation along these lines is impossible; Hilbert was referring to an article published by Lorentz in 1900, showing that no force of the form $1/r^2$ is created by "diffuse radiation" between two electrical charges, if the distance between them is large enough when compared to the wavelength of the radiation in question.[58]

And yet in 1912, Erwin Madelung, who taught physics at that time in Göttingen, had retaken Lorentz's ideas in order to calculate the force produced by radiation over short distances and, eventually, to account for the molecular forces in terms of radiation phenomena (Madelung 1912). Hilbert considered that the mathematical results obtained by Madelung were very interesting, even though their consequences could not be completely confirmed empirically. Starting from the Maxwell equations and some

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[57] LeSage's corpuscular theory of gravitation, originally formulated in 1784, was reconsidered in the late nineteenth century by J.J. Thomson. On the Le Sage-Thomson theory see North 1965, 38–40; Roseaveare 1982, 108–112.

[58] Lorentz 1900. On this theory, see McCormmach 1970, 476–477.

simple, additional hypotheses, Madelung determined the value of an attraction force that alternatively attains positive and negative values as a function of the distance.

After teaching a course on electromagnetic oscillations in the winter of 1913–14, Hilbert kept his interest on physical issues alive, although perhaps less intensively than in past years. In June 1914 Hilbert published the third part of his work on the foundations of the theory of radiation (Hilbert 1914). That same year two of his students completed doctoral dissertations on physical topics: Bernhard Baule ("Applications of the theory of integral equations to the theory of the electron and the theory of dilute gases") and Kurt Schelenberg ("The applications of integral equations to the theory of electrolysis"). And in the summer semester of 1914 Hilbert lectured again on statistical mechanics.

The beginning of the war, however, altered the normal course of activities in Göttingen, and in particular the presence of students and young docents there over the following years. On November 3, 1914, the meeting of the Göttingen Mathematical Society (GMG) was devoted to discussing the consequences of war on the society's activities.[59] In the summer semester of 1915 Hilbert lectured on the structure of matter, focusing mainly on Born's theory of crystals. Hilbert claimed once again that providing a solid theoretical foundation to explain the structure of matter was one of the main tasks of physics. Surprisingly, however, he did not mention in this course Mie's theory or any other, similar, electromagnetic theory of matter. Rather, Hilbert asserted that the theory of crystals and the theory of dilute gases complement each other as basic elements of any desirable, general account of the properties of matter. [60]

The summer of 1915 was also the time when Einstein visited Göttingen, following an invitation of Hilbert to present the current state of his research on the general theory of relativity. This was Einstein's first trip to Hilbert's city but the latter had certainly been aware of Einstein's raising prominence among physicist long before that, and had actually invited him in the past.[61] Moreover Einstein's *Entwurf* theory, [62] published in mid-1913 in collaboration with Marcel Grossmann, had been discussed in December 1914, at the GMG.[63] And yet, in the manuscripts of Hilbert's courses prior to 1915 we find no mention whatsoever of Einstein, or of any of the issues in which he was currently involved. When Hilbert spoke of relativity in his lectures on physics and explained its centrality for modern physics, he referred to Lorentz, rather than to Einstein, and he never suggested that the requirement of Lorentz covariance was a restricted one that needed to be extended.

Einstein's visit to Göttingen, between Between June 29 and July 7, 1915, came after more than two years of intensive struggle with the attempt to formulate a generalized theory of relativity. This struggle did not necessarily imply, however, that, in Einstein's views, general covariance as such was always the most important single aim to be attained. As a matter of fact, he had temporarily abandoned the demand of general

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[59] See the announcement in the *Jahresberichte* DMV Vol. 23 (1914), 126.

[60] Hilbert 1915, 1.

[61] See, e.g., a letter of Einstein to Hilbert, October 4, 1912, in Klein et al. (eds.) 1993, Doc 417.

[62] Einstein & Grossmann 1913.

[63] See the announcement in the *JDMV*, Vol. 22 (1913), p. 207. Unfortunately, the contents of this lecture are not documented.

covariance as part of his theory, after coming to the conclusion that generally covariant field equations would necessarily lack any physical interest, because they would contradict the principle of causality. The ground for this conclusion was the so-called “hole argument”, which he introduced first in the *Entwurf* paper of 1913, and later articulated most clearly in a summary of the latter, presented in October 1914 to the Berlin Academy of Sciences (Einstein 1914).[64]

Einstein’s quest for a relativistic theory of gravitation was crowned with success only after he abandoned completely the ‘hole argument’, and adopted general covariance again as a leading principle of that theory. Einstein’s confidence on the validity of the argument, however, did not begin to erode until October 1915. But when he came to Göttingen in June 1915, Einstein seems to have been satisfied with the current state of his theory – including the ‘hole argument’ and the conclusions derived from it – and, most likely, his presentation in Göttingen did not contain any significant departure from his October 1914 version. [65]

There seems to be no direct evidence indicating who attended Einstein’s Göttingen lectures in 1915; yet, it is nevertheless possible to reconstruct at least a partial list of the few professors and students who were there at that time, and who perhaps attended the lectures. Klein and Hilbert were obviously among the audience. Emmy Noether (1882–1935) had been recently invited from Erlangen as a specialist in invariant theory. She had arrived in Göttingen in the spring of that year. Constantin Carathéodory (1873–1950), who had received his doctorate in Göttingen in 1904, and habilitated there in 1905, had returned to lecture in Göttingen in 1913. He was a Greek citizen and therefore he had not been drafted. Neither had Peter Debye, a Dutch citizen, who had been collaborating with Hilbert in Göttingen on physical issues since 1913. F. Böhm (1885–1965) was a mathematician from Munich who visited Göttingen between 1913 and 1916.[66] In December 9, 1913, he had presented at the meeting of the GMG the report on the recently published Einstein-Grossmann *Entwurf* paper.[67] A physicist named Louise Lange was Hilbert’s assistant for physics over those years. She annotated the manuscript of Hilbert’s lectures on statistical mechanics during the summer semester of 1914.[68] Finally, Paul

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[64] Very detailed analyses of Einstein’s way to general relativity, and particularly of the hole argument and its striking significance, appear in Norton 1984 (esp. 126–137) and Stachel 1989 (esp. 71–81).

[65] See Pais 1982, 250 & 259.

[66] I thank Professor Bernd Heinzmann, in Augsburg, for communicating me biographical details concerning Böhm. These appear in a short biographical note Professor Heinzmann wrote for the forthcoming *Biographisches Handbuch des Lehrkörpers der Universitäts Ingolstadt-Landstuhl-München*.

[67] As already mentioned above. See footnote 63.

[68] I haven’t been able to find complete biographical details about Lange. She worked at the “Oxford Female College,” a small women’s college in Oxford, Ohio, between 1921 and 1926. The local students newspaper says that she “had been doing mathematical computations for manufacturers of physics instruments” (probably Siemens, in Germany) before coming to the college. She then taught at the Miami University, in Oxford, Ohio, The Alumni directory of the institution reports as her address the Woodrow Wilson College, Chicago, Ill., which does not operate anymore.

Hertz was also then at Göttingen (he was mobilized only towards the end of 1915) and he later corresponded with Einstein over the summer of 1915 on the problems surrounding the 'hole argument.'[69] Other well-known physicists who were close to the Hilbert circle in Göttingen had already been mobilized before the summer of 1915: Max Born, P.P. Ewald and Alfred Landé (1888–1975).

Also the exact content of Einstein's 1915 lectures in Göttingen is unknown to us, unfortunately.[70] Still, it is clear that he considered those lectures to have been a complete success, as he wrote to Sommerfeld in an often-quoted letter upon his return to Berlin. [71] Some among the Göttingen mathematicians, however, may have had some reservations concerning Einstein's mathematical knowledge and abilities. Back in 1908 Minkowski had argued in this direction, when he reportedly expressed his surprise that Einstein had been able to create the theory of relativity. As a former professor in Zürich, Minkowski asserted with full authority that Einstein's mathematical background was certainly incomplete.[72] Much later, in 1916, Klein expressed a similar opinion, when lecturing on relativity. Referring to the connections between relativity and differential geometry he said:

> [T]here are here, in Einstein's work, imperfections, which do not impair the great ideas in his new theory, but hide them from view.
> This is connected with the repeatedly mentioned circumstance that Einstein is not innately a mathematician, but works rather under the influence of obscure, physical-philosophical impulses. Through his interaction with Grossmann and on the basis of the Zurich tradition he has, to be sure, gradually become acquainted with Gauss and Riemann, but he knows nothing of Lagrange and overestimates (parenthetically) Christoffel, under the influence of the local Zürich tradition. [73]

Apparently Hilbert did not share this qualified opinion about Einstein; at least we have no direct evidence that he did. If anything, the enthusiasm expressed by Einstein after his visit was to a considerable extent reciprocated by Hilbert. [74] As he wrote to Karl Schwarzschild on July 17 commenting on the visit:

[69] For more details on Hertz, see Howard and Norton 1993.

[70] I have made some efforts to gather documents related to this visit, so far without much success. What I did find in Hilbert's *Nachlass* in Göttingen, nevertheless, are the handwritten notes taken by an unidentified person at he first of Einsteins lectures (Staats- und Universitäts Bibliothek, Göttingen, Cod Ms D Hilbert 724). These notes have now been published in Kox et al (eds.) 1996, App. B, 586–590.

[71] See Hermann (ed.) 1968, 30; Pais 1982, 259.

[72] See Pyenson 1977, 81.

[73] Quoted from Howard and Norton 1993, 36.

[74] In fact, in a letter dated November 19, 1915, after reading Einstein's solution to the problem of the perihelion of Mercury in the framework of general relativity, Hilbert wrote: "If I could calculate as quickly as you, then the electron would have to capitulate in the face of my equations and at the same time the hydrogen atom would have to offer its excuses for the fact that it does not radiate." See Pais 1982, 260.

> During the summer we had here as guests the following: Sommerfeld, Born, Einstein. Especially the lectures of the last on gravitation theory were an event. [75]

But in spite of the enthusiasm for the reception accorded to him in Göttingen and notwithstanding the confidence Einstein had felt for the current version of his theory at that time, doubts about having abandoned the requirement of general covariance gradually arose in his mind over the fall of 1915. Finally, by mid-October 1915, Einstein had recognized the need to return to that requirement and he embarked in the effort that led him to present four consecutive papers at the weekly meetings of the Berlin Academy, starting on November 4. The fourth paper, presented on November 25, contained his final version of the generally covariant field equations of gravitation. [76] Five days before that, on November 20, Hilbert had presented a communication in Göttingen dealing with closely related issues, and presenting his own unified foundational theory of physics. This theory combined ideas taken from Einstein's and Mie's theories, and with the latter Hilbert shared in particular the underlying assumption of the primacy of electromagnetic processes over mechanical ones.

6. The foundations of physics

Hilbert's paper "The Foundations of Physics" was published in March 1916 in the Proceedings of the Royal Academy of Sciences in Göttingen (Hilbert 1916). This published version, however, differs in many respects from the original communication delivered by Hilbert on November 1915, as we learn by examining the original proof galleys of the printed version, dated December 6, 1915. [77] Nor was it the last version of the theory. Hilbert republished the two parts of his communication in 1924 in the *Mathematische Annalen* with some additional, substantial changes, and yet once again with additional editorial comments in 1932, in the third volume of his collected works. Typically, Hilbert never mentioned any of the major changes he introduced between the various versions. In 1924, for instance, he explained – somewhat misleadingly – that he was basically reprinting what had appeared in the past in two parts, with only

[75] Quoted in Pyenson 1979, 193. According to the announcement in the *JDMV* Vol. 24, 68, Sommerfeld lectured at the GMG "On Modern Physics" in June 15 (but no lecture of Born is announced there.) Incidentally, in a letter sent to Hermann Weyl on July 3, 1918, (and cited in Sigurdsson 1991, 159–160) Sommerfeld "praised Weyl for being the first really to unify gravitation and electrodynamics. He felt that the efforts of Mie in this direction had been unsatisfactory, because he had glued (*ankleben*) gravitation onto electrodynamics in an inorganic manner. He credited himself with having recognized before Hilbert the importance of Mie's electrodynamics and the lack of significance (*Bedeutungslosigkeit*) of his work on gravitation". In view of this letter, and the proximity of Sommerfeld's and Einstein's visits to Göttingen in 1915, one wonders what the contents of his lecture were and what possible effect they could have had on Hilbert's views. Sommerfeld appears several times in the background of this story, without actually coming into the limelight.

[76] See Norton 1984, 138–152.

[77] *Nachlass* David Hilbert, NSUB Göttingen, Cod Ms 634.

minor editorial changes.[78] All these interesting changes and the interrelation between Hilbert's various versions and Einstein's work deserve a close analysis which goes beyond the scope of the present article. They are thus left for a forthcoming opportunity. [79] In the present section I limit myself to discuss how Hilbert's current favoring of an electromagnetic reductionism manifested itself in the theory.

Hilbert's theory took from Einstein the account of the structure of spacetime in terms of the metric tensor. Mie's theory served as a basis for explaining the structure of matter in terms of the electromagnetic field. To these two elements Hilbert applied powerful mathematical tools taken from the calculus of variations and from Riemannian geometry. The theory was presented in an axiomatic formulation and Hilbert thought to have accomplished here for physics what he had done for geometry in 1899 with his *Grundlagen der Geometrie*, or at least so he declared consistently.

The first axiom of Hilbert's theory of gravitation (which he called: "Axiom I: Mie's axiom of the world-function") is based on a variational argument formulated for a scalar Hamiltonian function [80] $H(g_{\mu\nu}, g_{\mu\nu l}, g_{\mu\nu lk}, q_s, q_{sl})$, whose parameters are the ten gravitational potentials $g_{\mu\nu}$, together with their first and second derivatives

$$g_{\mu\nu l} = \frac{\partial g_{\mu\nu}}{\partial\omega_l}, g_{\mu\nu lk} = \frac{\partial^2 g_{\mu\nu}}{\partial\omega_l\partial\omega_k} \quad (l, k = 1, 2, 3, 4)$$

and the four electromagnetic potentials q_s, together with their first derivatives q_{sl}. The gravitational potentials $g_{\mu\nu}$ are the components of a symmetric tensor and, as in Einstein's theory, they constitute the metric tensor of a four-dimensional manifold. The electromagnetic potentials behave like vectors with respect to the four world-parameters ω_l (l = 1, 2, 3, 4). The Hamiltonian is used to derive the basic equations of the theory, starting from the assumption that, under infinitesimal variations of its parameters, the variation of the integral

$$\int H\sqrt{g}d\omega$$

(where $g = |g_{\mu\nu}|$, and $d\omega = d\omega_1 d\omega_2 d\omega_3 d\omega_4$) vanishes for any of the potentials. In fact, instead of the covariant magnitudes $g_{\mu\nu}$ and their derivatives, Hilbert used consistently the contravariant tensor $g^{\mu\nu}$ and their derivatives throughout the argument.[81] The second basic axiom of the theory (Axiom II: axiom of general invariance) postulates that H is invariant under arbitrary transformations of the coordinates ω_l.

Besides the two basic axioms, the core of Hilbert's derivation is based on a central mathematical result (Theorem I), which Hilbert initially described as the *Leitmotiv* of

[78] Hilbert 1924, 1.

[79] But for a preliminary analysis see Corry, Renn & Stachel 1997.

[80] At this point a terminological clarification may be in order. In present-day terms, this function would be more properly called a Lagrangian function, while the term "Hamiltonian" usually refers to functions involving momenta and representing the total energy of the system considered. See, e.g., Lanczos 1970, Chapt. IX. For the purposes of the present article and for the sake of historical clarity, however, it seems more convenient to abide by the original terminology.

[81] In a course taught at Göttingen in 1916–17, Hilbert explicitly explained that this is done for reasons of convenience. See Hilbert 1916–17, 109.

the theory. According to this theorem the number of equations that can be obtained from the variational integral is in fact smaller than the fourteen that one would expect to attain on the face of it. More specifically, in the first printed version of the theory Hilbert formulated the theorem as follows:

> Theorem I. Let J be a scalar expression of n magnitudes and their derivatives that is invariant under arbitrary transformations of the four world-parameters, and let the Lagrange variational equations corresponding to the n magnitudes be derived from the integral
>
> $$\delta \int J\sqrt{g}d\omega = 0.$$
>
> Then, in the system of n differential equations on n variables obtained in this way, four of these equations are always a consequence of the other $n-4$, in the sense that four linearly independent combinations of the n differential equations and their total derivatives are always identically satisfied. (Hilbert 1916, 397)

The variational principle introduced above yields ten equations for the gravitational potentials and four for the electromagnetic ones:

$$\frac{\partial\sqrt{g}H}{\partial g^{\mu\nu}} - \sum_k \frac{\partial}{\partial\omega_k}\frac{\partial\sqrt{g}H}{\partial g_k^{\mu\nu}} + \sum_{k,l}\frac{\partial^2}{\partial\omega_k\partial\omega_l}\frac{\partial\sqrt{g}H}{\partial g_{kl}^{\mu\nu}} = 0 \ (\mu, \nu = 1, 2, 3, 4) \qquad (1)$$

$$\frac{\partial\sqrt{g}H}{\partial q_h} - \sum_k \frac{\partial}{\partial\omega_k}\frac{\partial\sqrt{g}H}{\partial q_{hk}} = 0 \quad (h = 1, 2, 3, 4). \qquad (2)$$

Hilbert denoted the left-hand sides of these equations as $[\sqrt{g}H]_{\mu\nu}$ and $[\sqrt{g}H]_h$, and called them the fundamental equations of gravitation and of electrodynamics respectively. Theorem I was obviously conceived with the intention of being applied to these equations, thus leading to the claim that four of them are in fact consequences of the other ten. In particular, Hilbert concluded, the four equations $[\sqrt{g}H]_h = 0$, are a consequence of the ten gravitational ones, $[\sqrt{g}H]_{\mu\nu} = 0$. This latter conclusion amounted, Hilbert suggested, to nothing less than a definitive explanation of the intimate interconnection between the two kind of physical phenomena involved:

> Based on the above theorem we can advance the following claim: *in the already indicated sense the electrodynamic phenomena are an effect of gravitation.* By recognizing this, I discern the simple and very surprising solution of the problem of Riemann, who was the first to search for a theoretical connection between gravitation and light. (Hilbert 1916, 397–398)[82]

Hilbert was presumably referring here to a short paper on gravitation and light taken from Riemann's *Nachlass* (Riemann *Werke*, 532–538), and thus connecting himself with evident pride to a deep-rooted Göttingen tradition.

[82] A similar assessment appears in Hilbert 1916–17, 168.

Hilbert was thus deducing, based on a chain of purely mathematical results, physical conclusions of the deepest significance. His theory appeared to be "doubly reductionistic:"[83] Hilbert assumed from the outset, following Mie, that matter could be reduced to the electromagnetic field, and, now, by virtue of a formal mathematical theorem, the electromagnetic field appeared as reducible to the gravitational one. One can certainly understand Hilbert's excitement in view of these results.

But things turned out to be a bit more complicated than this. Hilbert had not proved his Theorem I as part of his exposition of the theory in 1915, yet all the same he claimed that the necessary proof would appear in a different place. The mathematical conclusions he drew from the theorem were, at any rate, erroneous: in fact, the validity of the theorem would imply that four among the equations are dependent on the other ten, but this in no way warranted that precisely the four electromagnetic ones are dependent on the gravitational ones, as Hilbert asserted here.

Hilbert's Theorem I was, in fact, an early version of what later came to be known as Noether's theorem (Noether 1918), but his conclusions went way beyond what the theorem actually allows. Over the coming years, Hilbert's theory gave rise to a vivid debate among the Göttingen mathematicians, and the problematic status of his Theorem I and its implications were a focal point of it. I will not enter into all the details of this debate in the present article,[84] but we should at least notice that in the 1924 version of the theory, Hilbert changed many of his early formulations in attention to the reactions of his colleagues to his theory in general, and in particular concerning the implications of Theorem I over the significance of the electromagnetic reductionism.

Hilbert opened his 1924 version with a new introductory passage in which he explained that the mechanistic ideal for unifying physics had finally been abandoned in favor of an electromagnetic one. In this sense he was more incisive that in the 1915 version. He also stressed again the central role of the axiomatic analysis in his presentation. A similar stress had appeared back in the 1915 version, in the following words:

> In what follows I would like to derive – in the sense of the axiomatic method – essentially from two axioms, a new system of fundamental equations of physics that display an ideal beauty, and which in my opinion simultaneously contain the solutions to the problems of both Einstein and Mie. (Hilbert 1916, 27)

But this time he expressed a more cautious attitude, using the following formulation:

> I am convinced that the theory I present here contains an enduring core (*ein bleibender Kern*) and provides a framework within which there is enough room (*Spielraum*) for the future construction of physics in the sense of a field-theoretical unifying ideal. In any case, it is epistemologically interesting to see how the few simple assumptions that I express as axioms I, II, II, and IV, suffice to reconstruct the whole theory. (Hilbert 1924, 2)

Besides the noticeably more cautious assessment of the value of the axiomatic analysis, Hilbert spoke this time of four, rather than two axioms necessary for building the

[83] This term is used in Vizgin 1994, 61, to describe the theory.
[84] But for details see Rowe 1998.

theory. In fact, the two additional assumptions, embodied here in Axioms III and IV, had already appeared in the earlier versions, but Hilbert did not single them out specifically as axioms of the theory. Axiom III is the assumption that the Hamiltonian function comprises two parts, $H = K + L$, each of which satisfies certain specific properties. Axiom IV is the "axiom of space and time", that Hilbert introduced in relation with the issues raised by Einstein's 'hole argument'.[85] We already saw above that the various versions of Hilbert's work on the foundations of radiation theory were characterized by similar changes in the number and the contents of the basic axioms of the theory. In both cases Hilbert did not point out any of these changes.

But the changes in the 1924 go beyond style of presentation. Unlike in the earlier versions, the problematic Theorem I did not open the mathematical argument of this version anymore. Rather, Hilbert alluded to the theorem when necessary for proving a result concerning the dependence between the electromagnetic and the gravitational equations. He explained that the mathematical core of this result is provided by a "general mathematical theorem that has been the *Leitmotiv* for constructing the theory." Hilbert added a footnote with a reference to Noether's 1918 paper for a proof of the general theorem, but he did *not* indicate the difficulties involved in his earlier versions of the theorem nor in his interpretation of it. In 1915 he had concluded from Theorem I that "the electrodynamic phenomena are an effect of gravitation". This conclusion is totally absent from the 1924 version. At a different place of the article Hilbert had concluded in 1915 that a certain formula embodies "*the exact mathematical expression of the claim formulated above in general terms, concerning the character of electrodynamics as a phenomena derived from gravitation*." In tune with the general spirit of the 1924 version, Hilbert formulated this connection more cautiously when he concluded from the same formula:

> This is the exact mathematical expression of the *interrelation* (*Zusammenhang*) between gravitation and electrodynamics that dominates the entire theory. (Hilbert 1924, 10. Italics added)

In 1924 Hilbert also became much more cautious concerning the conclusions one could draw from the theory in relation to the structure of matter. In 1915 he closed his article by stressing that he had just showed that a sensible interpretation of the basic axioms suffices to construct the theory completely. Moreover – he added – by doing this

> . . . not only our conceptions of space, time and motion have been modified from their foundation in the direction suggested by Einstein, but I am also convinced that starting from the basic equations established here, the innermost – and so far concealed – processes occurring inside the atom will be finally illuminated. In particular, a general reduction of all physical constants to mathematical ones must be possible, and with it the possibility must be brought closer, that in principle physics be transformed into a science of the kind of geometry: this is certainly the greatest glory of the axiomatic method that, as we see in this case, makes use of the powerful tools of analysis, namely, the variational calculus and the theory of invariants. (Hilbert 1916, 407)

[85] See Corry, Renn & Stachel 1997.

In the *Mathematische Annalen* version, this passage – that reflects the typical Hilbert irrepressible optimism – is also missing. Instead we find a short and very cautious one in the opening pages, where Hilbert stated:

> Whether the field-theoretical unifying ideal is indeed a definitive one, or what additions and modifications will eventually be necessary in order to allow for the theoretical foundation of the existence of negative and positive electrons, as well as the logically consistent construction of the laws that are valid inside the atom – to answer these questions remain a task for the future. (Hilbert 1924, 2)

Thus by 1924 Hilbert's confidence on the validity and the sweepingness of his unified theory as an overall foundations of physics had considerably diminished. Hilbert never looked backwards and he did not inform his readers how big had been his hopes for the theory back in 1915.

7. Concluding remarks

Beyond the specific changes in Hilbert's positions on the role of electromagnetic reductionism and its details within his own theory, a more general change in this direction can be discerned, whereby the whole issue of reductionism becomes less and less important in Hilbert's pronouncements on physics after 1916.

Hilbert was deeply and increasingly impressed by Einstein's achievements and by the implications of his work, and in fact he spared no efforts over the years to indicate unambiguously that Einstein's general theory of relativity was *one of the greatest achievements of the human spirit ever!*[86] No more and no less. In a lecture delivered in Göttingen in 1920, for instance, Hilbert described the general theory of relativity as the culmination of an impressive scientific endeavor of the highest value, that was initiated by Pythagoras, was later followed up by Newton, and had only recently been brought to conclusion by Einstein.[87] Whereas all former laws of physics had been provisory, inexact, and particular, for the first time in history – Hilbert asserted in a different lecture – Einstein's theory provided a definitive, exact and general expression of the laws of nature that are truly valid in the real world.[88]

What made Einstein's theory so special in Hilbert's eyes was nothing like reductionism, but rather the new conception of objectivity it entailed, an objectivity of a higher degree than had ever been attained in the past. Hilbert liked to identify now progress in a scientific discipline in terms of the ability to disengage itself from whatever anthropomorphism it might still contain. General relativity, he thought, had signified a true and definitive revolution in this sense. In fact, if, with the rise of modern physics, true progress in science had been initially sparked by the willingness to abandon the immediate data of our sensorial *perceptions* in order to explain phenomena by means

[86] For one among many pronouncements of Hilbert in this spirit, see Hilbert 1992, 51.

[87] See Hilbert 1920, 120 (Emphasis in the original): "Die Aufstellung der allgemeinen Relativitätstheorie ist m.E. eine der grössten Leistungen in der Geschichte der Wissenschaften. Den von Pythagoras begonnenen, von Newton ausgestalteten, Bau hat Einstein zum Abschulss gebracht."

[88] See for insatnce Hilbert 1921, 1.

of a network of abstract concepts (*Fachwerk von Begriffen*), general relativity had gone much farther and had proposed to relinquish as well our most basic *intuitions* regarding space and time. [89] Further, a second element in the estrangement of general relativity from anthropomorphism was its generally covariant character; a representation of natural phenomena, said Hilbert, can only be considered "once and for all to be free of subjectivity and arbitrariness, if it is independent of the way in which the world-points are denoted (through coordinates) in it" (Hilbert 1992, 49).

But not only the objectivity embodied in general relativity was impressive in Hilbert's view; also, of course, was so the unity it had conveyed to our understanding of nature. In fact, Einstein's theory of relativity came to reinforce in an unprecedented way one of the most basic philosophical notions that had traditionally underlay the whole scientific enterprise in Göttingen, namely, the notion of a "pre-established harmony between nature and the human mind". Thus, for instance, in the framework of a series of lectures delivered in 1919–1920, in which Hilbert presented to a general audience his views on physics and mathematics, he said:

> The success of the principle of mathematical simplicity in physics is just astonishing. If one realizes the surprisingly simple form that the basic equations of Maxwell's theory attain in the formal language of four-analysis, and if one further sees how in Einstein's equations of gravitation, the appeal to the simplest differential invariants yields the accurate correction of Newton's law of gravitation, then one is lead to the impression of a pre-established harmony. We face the remarkable fact that, apparently, matter completely and fully abides by the formalism of mathematics. A previously unseen correspondence between being and thought (*Sein und Denken*) is manifest here, that we must provisorily accept (*hinnehmen*) as a miracle. (Hilbert 1992, 69)[90]

Important as the notion of a pre-established harmony was, it must be stressed, Hilbert, like all his colleagues in Göttingen, was never really able to explain, in coherent philosophical terms, its meaning and the possible basis of its putative pervasiveness. He therefore contented himself with asserting once and again its existence and its validity. He believed, at any rate, that the "current state" of epistemological knowledge did not help understanding this fundamental, and perhaps strange but certainly evident, correspondence.[91] And in any case, the achievements of general relativity had certainly helped to corroborate the belief in its indisputable validity.

The innovative sense of objectivity entailed by Einstein's theory, as well as its reinforcing of the sense of a pre-established harmony had little to do for Hilbert – and this is a remarkable point – with *direct* experimental verification. In the last passage quoted above, Hilbert stressed the significance of the Newtonian limit of Einstein's theory. To be sure, this is an issue that Hilbert repeatedly stressed in his lectures and in his writings.

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[89] See Hilbert 1921, esp. pp. 13–14; Hilbert 1992, 50–51.

[90] See also Hilbert 1921a, 3 (Emphasis in the original): "Man ist direkt versucht, von einer prästabilierten Harmonie zwischen Denken und Sein zu reden.' For a broader discussion of the place of this notion in the Göttingen scientific tradition, see Pyenson 1982.

[91] Pronouncements to this effect appear, e.g., in Hilbert 1922–23, 98.

But at the same time, it is remarkable how little, almost nil, reference Hilbert made to the empirical confirmations of Einstein's theory, e.g., by the Eddington expedition of 1919 or in connection with red-shift measurement. In his 1919–1920 public lectures, Hilbert dedicated one talk specifically to explain the complex relationship between experiment and theory in science (Hilbert 1992, 57–60). This is perhaps one of the few instances where he connected Einstein's theory with its empirical confirmations (other than the Newtonian limit), and in any case, these confirmations were not invoked in order to underscore the new kind of objectivity implicit in Einstein's theory. Rather, this new sense of objectivity was for Hilbert purely a product of the theory's intrinsic, mathematical characteristics, and above all, of its generally covariant character and its challenge to our basic intuition of space and time.

Similar methodological and philosophical underpinnings underlay Hilbert's later enthusiasm for general covariance and his earlier support of reductionism in physics, first mechanical and then electromagnetic. But now Hilbert not only stopped mentioning reductionism with the same kind of unqualified eagerness he had done in the past; we can even find an interesting case in which he openly opposed it. This happened after the publication in 1918 of Hermann Weyl's unified field theory (Weyl 1918). In a series of public lectures delievered in 1919–1920 Hilbert adduced that Weyl's recent theory was a typical case of "extreme idealizing", and dubbed it "Hegelian physics". Hilbert used the term to designate those theories in which given their equations and well-determined initial conditions, not only the future values of the variables involved can be determined, but also all specific quantities appearing in nature (e.g., the number of planets, the number of continents) can be derived mathematically from general laws (Hilbert 1992, 71). In Weyl's theory the values of the $g_{\mu\nu}$'s and the q_s's could be determined intrinsically from a mass prescription, and this is what Hilbert deemed exaggerated (p. 99).

Given the close proximity between Hilbert's and Weyl's theories, Hilbert's criticism here looks somewhat out of place, and one cannot but wonder if a possible explanation for it may perhaps be found in a different arena. In 1918, after returning from the war, Weyl had become involved in the debates concerning the foundations of mathematics and–to Hilbert's rage–he took sides with the Dutch intuitionist L.E.J. Brouwer (1881–1960), who was to become Hilbert's anathema over the coming years. Never before or after in his life did Hilbert take such an activist, and outright personal, position in a scientific debate as he did with Brouwer, and his attitude in this sense led him sometimes to a frankly absurd behavior.[92] Perhaps, then, one can understand his strong criticisism of Weyl's theory as an early manifestation of his attitude towards Brouwer and his followers.

Be that as it may, the fact is that the role of physical reductionsim underwent a series of changes as part of Hilbert's physical conceptions but at any given time Hilbert would speak of his current views with full authority, with no hesitations about their validity, without mentioning his earlier, now discarded views, and with great optimism as to the possibilities that adhering to these views opened before science.

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[92] See van Dalen 1990.

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Cohn Institute for History and Philosophy of Science
Tel Aviv University
Ramat Aviv 69978, Israel
e-mail:corry@post.tau.ac.il

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