

Hilbert's "World Equations" and His Vision of a Unified Science*

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Abstract

In summer 1923, a year after his lectures on the 'New Foundation of Mathematics' and half a year before the republication of his two notes on the 'Foundations of Physics,' Hilbert delivered a trilogy of lectures in Hamburg. In these lectures, Hilbert expounds in an unusually explicit manner his epistemological perspective on science as a subdiscipline of an all embracing science of mathematics. The starting point of Hilbert's considerations is the claim that the class of gravitational and electromagnetic field equations implied by his original variational formulation of 1915 provides valid candidate 'world equations,' even in view of attempts at unified field theories á la Weyl and Eddington based on the concept of the affine connection. We give a discussion of Hilbert's lectures and, in particular, examine his claim that Einstein in his 1923 papers on affine unified field theory only arrived at Hilbert's original 1915 theory. We also briefly comment on Hilbert's philosophical viewpoints expressed in these lectures.

1 Introduction

In the history of unified field theory, many contributors may be identified [Goldstein and Ritter 2003, Goenner 2004], among them certainly, and perhaps foremost, Einstein. Hilbert's place in the history of the unified field

*To appear in a forthcoming volume of *Einstein Studies*, eds. J. Eisenstaedt and A.J. Kox.

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theory program is also well recognized (see, e.g., the discussion of his work in Vizgin's study [Vizgin 1994]). But we tend to view the history of physics in which Einstein was involved through that scholarship which has focussed exclusively, or at least predominantly, on Einstein's work as such. For the case of Einstein's "later journey," we believe that many physicists as well as historians would subscribe to Pais's verdict that "his work on unification was probably all in vain" [Pais 1982, p. 329]. The dismissal of Einstein's efforts over three decades is to some extent supported by Einstein's own self-image, in his later years, as the "lonesome outsider" working without real appreciation in his golden Princeton cage. Einstein was an original thinker and an influential voice in the debate, and for this reason understanding Einstein's obsession with the problem of a unified field theory over the last thirty years of his life presents as much of a challenge to the historian as understanding the achievements of his early work.

To this purpose, it helps to free one's mind from preconceptions. We then find Hilbert's insights of great advantage since he was both knowledgeable and had a well-founded and original perspective of his own.

Let us make a distinction right at the beginning in order to disentangle different scientific approaches. The problem of a unified field theory, as of the 1920s, can be seen in a more specific sense as the problem of finding a consistent and satisfactory mathematical unification of the gravitational and electromagnetic fields, be it by modified field equations, by a modification of the space-time geometry, or by increasing the number of space-time dimensions. But there is another aspect to the problem that is, we believe, of both historical and philosophical interest. This aspect concerns the way in which contemporary scientists perceived the technical problem of unification in the wider context of a unified corpus of human knowledge and understanding. In this respect, Hilbert's perspective on the mathematical sciences as an integrated whole can contribute to our modern attempts to come to grips with the philosophical implications of an ever increasing specialization in the natural sciences. Hilbert certainly was not the only one who envisaged a unified science at the time. Many contemporary mathematicians shared this concern. Felix Klein's *History of the Development of Mathematics in the 19th century* [Klein 1979] can also be seen as a most interesting attempt to understand the inner organic unity of the corpus of mathematical knowledge. Other names that come to mind immediately are those of Kaluza and Weyl, but the list certainly does not end here.

Einstein, too, shared this interest in understanding the inner unity of our

knowledge of nature, and for him, too, the problem of finding a mathematical representation that would provide a unification of the gravitational and electromagnetic fields was more than just a technical problem. This aspect of his work is expressed most convincingly in Einstein’s own account of his lifelong research concerns as given in his 1949 *Autobiographical Notes* [Einstein 1949]. Einstein, as we will argue, followed in his later work a path that is not at all very different from Hilbert’s. Hilbert himself perceived Einstein as sharing his concern. Of course, there are differences, which we do not deny. But from a broader perspective, both Einstein and Hilbert – and others, one may add – belong to a tradition which attempts to integrate our human knowledge and to perceive an inner unity in science. For today’s philosophers, this tradition seems to belong to the 18th and 19th century rather than to the 20th, or to the 21st, for that matter. In this respect, Einstein and Hilbert are akin more to the encyclopedists and enlightenment natural philosophers than to modern puzzle solvers.

2 Hilbert’s Lectures on Fundamental Questions of Modern Physics of 1923

The document to which we would like to draw attention in this paper is a manuscript extant in the Hilbert archives in Göttingen. It will be published in one of the physics volumes of the Hilbert Edition under the title “Fundamental Questions of Modern Physics.” It is a batch of roughly 100 manuscript pages with notes for a trilogy of lectures that Hilbert delivered at the end of the summer semester of 1923 in Hamburg.¹ The three lectures focus on three different topics: the first deals with what Hilbert called the “World Equations,” where these equations are introduced; the second part

¹The lectures were held in Hamburg on July 26, 27, and 28, 1923. They were announced under the title “Grundsätzliche Fragen der modernen Physik,” see “Hamburgische Universität. Verzeichnis der Vorlesungen. Sommersemester 1923,” Hamburg 1923, p. 41. The third of the three lectures was delivered a second time, with short summaries of the first two lectures, in a lecture held at the “Physikalische Gesellschaft” in Zürich on October 27, 1923. This lecture was announced under the title “Erkenntnistheoretische Grundfragen der Physik,” see “Neue Zürcher Zeitung,” Nr. 1473, Erstes Morgenblatt, 27 October, 1923. The manuscript Cod. Ms. Hilbert 596 in the *Handschriftenabteilung* at the *Niedersächsische Staats- und Universitätsbibliothek* (NSUB) contains the notes for both the Hamburg and Zurich lectures. It will be cited in the following as *Lectures*.

discusses applications and consequences of those equations; and the third lecture contains a discussion of the old problem of theory and experience.

To Hilbert at that time, the epistemological and philosophical implications of recent developments in physics were of central concern. He himself had contributed substantially to modern mathematical physics in the preceding years, most notably through his two Communications to the Göttingen Academy Proceedings on the Foundations of Physics of November 1915 and December 1916, respectively [Hilbert 1915, Hilbert 1917]. By the summer of 1917 at the latest, however, another problem was increasingly occupying Hilbert's mind, namely the problem of an absolute consistency proof of arithmetic that would provide a sound logical foundation for the whole body of mathematics. Just as in Hilbert's work in physics, the roots of this preoccupation date back to his very early work, at least to his "Mathematical Problems" of 1900 [Hilbert 1900]. This interest resurfaced with a lecture on set theory held in the summer term of 1917.

As a matter of fact, Hilbert's renewed attention to the foundations of mathematics in general, and to a theory of proof in particular, contributed to his taking a broader perspective on the contemporary debates in General Relativity and Field Theory. He had kept an active interest in the development of General Relativity after 1915 but was increasingly concerned with the philosophical implications of the new theories rather than with contributing solutions of some of its outstanding technical problems.² He also began to spend a great deal of energy in popularizing these new developments and in acquainting a larger audience with the results of modern physics. It is therefore no accident that when Hilbert spoke on the same topic a few weeks later in Zurich, but in a single lecture, he chose to center on the third of his Hamburg lectures.³ He used the same manuscript notes for the Zurich

²In this respect, we disagree with the claim made by Renn and Stachel, who characterize Hilbert's work in GRT as the transition from a "Theory of Everything to a Constituent of General Relativity," [Renn and Stachel 1999]. While their assessment may be true in abstraction of its actors, it is certainly not true for Hilbert himself. Rather than beginning to see his own work as a constituent of General Relativity, his main effort with respect to General Relativity in later years was to emphasize his claim that his approach would provide the basis for a true unification of physics.

³The lecture was arranged by Peter Debye following Hilbert's request: "Prof. Hilbert who is presently staying in Switzerland wished to deliver a lecture in the joint Physical and Mathematical Colloquium." ("Herr Prof. Hilbert, welcher zur Zeit in der Schweiz weilt, hatte den Wunsch im zusammengefassten Physik. und Mathematischen Kolloquium einen Vortrag zu halten.") P. Debye to Robert Gnehm, 22 October 1923. Archiv des

lecture, but since he had to cut down the material, he summarized the main points of the first two lectures. This editing of his own manuscript makes it difficult to exactly associate specific phrases with either the Hamburg or Zurich lectures.

3 Hilbert’s “World Equations” of summer and fall of 1923

Hilbert starts his first lecture by introducing what he calls the “World Equations” or the “World Laws” (“Weltgleichungen” or “Weltgesetze”). The way Hilbert introduces these equations is interesting in itself but for the sake of brevity, we shall only say that these equations basically are the same ones that he had proposed in his First Communication on the Foundations of Physics [Hilbert 1915], considering the fact that Hilbert had, originally, not completely specified the Lagrangian I of the variational integral

$$\int I\sqrt{-g}d\tau, \tag{1}$$

where $g = \det(g_{\mu\nu})$ and the integral is over (a domain of) four-dimensional space-time. But both in 1915 and now again in 1923 he pointed out that the fundamental dynamical variables are the ten components $g_{\mu\nu}$ of the metric tensor and the four components φ_l of the electromagnetic potential.⁴

In his Hamburg and Zurich lectures, he takes the Lagrangian to be the sum of a gravitational part K and a matter part L ,

$$I = K + L. \tag{2}$$

Schweizerischen Schulrats, ETH-Bibliothek, Zürich.

⁴As an aside, Hilbert observed in his 1923 lectures that the difference between his own fundamental equations of November 1915 and Einstein’s gravitational field equations pertains to the choice of fundamental variables: “Einstein’s equations of gravitation are, in the sense defined here, the fundamental equations of physics, if one takes in them the gravitational potential $g_{\mu\nu}$ and the energy tensor as fundamental potentials. I proposed, at the same time, fundamental equations of physics, in which only the electromagnetic four-potential φ_k enters in addition to the gravitational potential $g_{\mu\nu}$.” (“Die Einsteinschen Gravitationsgleichungen sind in dem hier definierten Sinne die Grundgleichungen der Physik, wenn man darin das Gravitationspotential $g_{\mu\nu}$ und ausserdem den Energietensor als Grundpotentiale nimmt. Ich habe zur selben Zeit Grundgleichungen der Physik aufgestellt, in denen neben dem Gravitationspotential $g_{\mu\nu}$ nur noch das elektromagnetische Viererpotential φ_k als Grundpotential auftritt.”) *Lectures*, part I, p. 16.

The gravitational part K is understood to be the Riemann curvature scalar. The matter part L is taken to be a sum of a term proportional to the square of the fields, and another term proportional to the square of the potential,

$$L = \alpha\Phi + \beta\varphi, \quad (3)$$

where $\Phi \equiv \sum \Phi_{mn}\Phi^{mn}$ with $\Phi_{mn} \equiv \varphi_{[m;n]}$ ⁵ denoting the electromagnetic field, and $\varphi \equiv \varphi_k\varphi^k$.⁶ As usual, variation with respect to the components of the metric tensor produces the gravitational field equations,

$$K_{\mu\nu} = -\frac{\partial\sqrt{-g}L}{\partial g^{\mu\nu}}, \quad (4)$$

and variation with respect to the components of the electromagnetic four-potential produces generalized Maxwell equations of the form

$$\mathcal{D}iv\Phi^{mn} = \frac{\beta}{\alpha}\varphi^m. \quad (5)$$

The latter equations are determined by the matter term alone. More specifically, the first term in (3) produces the left hand side of the inhomogeneous Maxwell equations, $\alpha\mathcal{D}iv\Phi^{mn}$, while the second term in (3) produces a term proportional to the electromagnetic vector potential, φ^k , the latter acting as the source of the inhomogeneous Maxwell equations. Following Mie's approach, external currents and charges are not part of the theory. The homogeneous field equations,

$$\Phi_{(mn;k)} = 0, \quad (6)$$

⁵We are closely following Hilbert's and Einstein's notation, with the following exceptions: for notational brevity, we denote partial (coordinate) derivatives by a subscript index separated by a semicolon (comma), and indicate (anti)symmetrization by setting the relevant indices in (square) brackets. We also do not use an imaginary x_4 -coordinate, as Hilbert did.

⁶Already in his First Note on the Foundations of Physics [Hilbert 1915], Hilbert had left open the final choice of a matter term in the Lagrangian. It should be diffeomorphism invariant, and it should not depend on the derivatives of the metric. But Mie's example of a term proportional to the sixth power of φ had obviously been unacceptable, and a different specification of the Lagrangian that would allow for solutions of a reasonable physical interpretation had not yet been found, see [Mie 1912] and also the discussion in Hilbert's own lecture notes on "Die Grundlagen der Physik," of the summer of 1916, which are located at the library of the Mathematics Institute of Göttingen University, see especially §§ 27–30. For further discussion of Hilbert's First Communication, see [Sauer 1999].

follow, in the usual way, from the definition of the field and the fact that the connection was assumed to be the symmetric Levi-Civita connection.

4 Hilbert's Comments on Einstein's Recent Work on Affine Field Theory

At this point, Hilbert introduces a remark which at first sight may seem preposterous, or, if you wish, arrogant and self-serving. He claims that Einstein, in his most recent publications, would have arrived, after “a colossal detour,” (“kolossaler Umweg”) at the very same results and equations that Hilbert had put forward in his first note on the Foundation of Physics of November 1915. But before dismissing this claim as a stubborn and senile insistence of a mathematician who “has left reality behind” let us examine his claim more closely and see whether it is conducive to a more nuanced historical interpretation.

The starting point is Hilbert's claim that the invariance of the action integral allows one to interpret the electromagnetic field equations as implicit in the gravitational field equations. Hilbert here reiterates the claim of his first note that this fact would provide the solution to a problem that he traces back to Riemann, namely the problem of the connection between gravitation and light. He goes on to observe that since then many investigators had tried to arrive at a deeper understanding of this connection by merging the gravitational and electromagnetic potentials into a unity. The one example Hilbert mentions explicitly is Weyl's unification of the two fields in a “unified world metric,” as he calls it, by means of Weyl's notion of gauge invariance. Another approach would be Eddington's who proceeded by selecting “certain invariant combinations” as fundamental potentials of the quantities determining the fields. Schouten then had investigated the manifold of possibilities of such combinations and realized that there would be a rich variety of them. At this point, Hilbert inserts his comment on Einstein's recent work. He says explicitly:

Einstein finally ties up to Eddington in his most recent publications and, just as Weyl did, arrives at a system of very coherent mathematical construction.

But, Hilbert goes on,

However, the final result of Einstein’s latest work amounts to a Hamiltonian principle that is similar to the one that I had originally proposed. Indeed, it might be the case that the content of this latest Einsteinian theory is *completely equivalent* to the theory originally advanced by myself.⁷

It is important to note that Hilbert makes his claim somewhat more specific than that. Looking at the variational principle which he explicitly writes down in the form

$$\delta \int \int \int \int \left\{ K + \alpha \Phi + \beta \varphi \right\} \sqrt{-g} dx_1 dx_2 dx_3 dx_4 = 0, \quad (7)$$

he observes that Einstein in his latest note had arrived at the very same Hamiltonian principle

where φ is defined through $\varphi^m = \text{Div } \Phi^{mn}$ and variation with respect to $g_{\mu\nu}$ and Φ^{mn} produces the eqs. $\Phi_{mn} = \text{Rot } \varphi_m$ instead of my eq. [(5)].

Hilbert concludes:

Hence, nothing else than *an exchange of the two series* [of] Max[well] eq[uations].⁸

The emphasis in the last quote is Hilbert’s. He was not only pointing at a vague similarity between his own work and Einstein’s. Rather he had identified the differences in their work as being of a purely nominal nature.

5 Einstein’s “colossal detour”

In view of this remark, let us briefly examine Einstein’s post-1915 work in General Relativity, in particular with regard to the problem of unifying gravitation and electromagnetism (see also [Vizgin 1994, Goenner 2004]).

⁷“Einstein endlich knüpft in seinen letzten Publikationen an Eddington an und gelangt ebenso wie Weyl zu einem mathematisch sehr einheitlich aufgebauten System. Indess mündet das Schlussresultat der letzten Einsteinschen Untersuchung wieder auf ein Hamiltonsches Prinzip, das dem ursprünglich von mir aufgestellten gleich; ja es könnte sein, dass diese Einsteinsche Theorie inhaltlich sich mit der von mir ursprünglich aufgestellten Theorie *völlig deckt*.” *Lectures*, part I, p. 19 (Hilbert’s emphasis).

⁸“... wo φ durch $\varphi^m = \text{Div } \Phi^{mn}$ definiert ist und durch Variation nach $g_{\mu\nu}$ und Φ^{mn} die Gl. $\Phi_{mn} = \text{Rot } \varphi_m$ an Stelle meiner Gl. (5) entstehen. Also Nichts als eine *Vertauschung der beiden Serien* [von] Max. Gl.” *ibid.*, p.20 (Hilbert’s emphasis).

Until 1923, it is perhaps not too unjust to say that Einstein basically had been reacting to the work of others. He had submitted Kaluza's theory of a five-dimensional metric for publication in the Prussian Academy Proceedings [Kaluza 1921] and had himself done calculations along this approach, partly in collaboration with Jakob Grommer [Einstein and Grommer 1923]. He had also published a couple of notes that further elaborated on Weyl's ideas [Einstein 1921], notwithstanding his critical evaluation of its physical viability. Thirdly, he had lately picked up on Eddington's approach of basing the theory on the affine connection rather than on the metric [Einstein 1923a, Einstein 1923b, Einstein 1923c].

In order to evaluate Hilbert's claim, let us take a closer look at Einstein's work along Eddington's approach, as he had published it in those papers of 1923 to which Hilbert refers. Following Eddington,⁹ Einstein had taken the components of a real, symmetric affine connection $\Gamma_{\lambda\mu}^{\kappa}$ as the basic quantities of the theory instead of the metric tensor field $g_{\mu\nu}$ which provided the dynamical variables in the original theory. From the symmetric connection he had constructed an asymmetric contracted curvature tensor,

$$R_{kl} = -\Gamma_{kl,\alpha}^{\alpha} + \Gamma_{k\beta}^{\alpha}\Gamma_{l\alpha}^{\beta} + \Gamma_{k\alpha,l}^{\alpha} - \Gamma_{kl}^{\alpha}\Gamma_{\alpha\beta}^{\beta}. \quad (8)$$

Since $R_{kl}dx^k dx^l$ is an invariance of the line element, it was tempting to split the Ricci tensor into a symmetric part g_{kl} , to be interpreted as a metric tensor associated with the gravitational field, and an antisymmetric part ϕ_{kl} , to be associated with the electromagnetic field tensor.

In a first note presented to the Berlin Academy on 15 February 1923, Einstein observed that Eddington had not yet solved the problem of finding the necessary equations that would determine the 40 components of the connection. He therefore set out to provide just such equations. He postulated a Hamiltonian principle,

$$\delta \left\{ \int \mathcal{H} d\tau \right\} = 0, \quad (9)$$

with a Lagrangian that would depend only on the contracted curvature tensor, $\mathcal{H} = \mathcal{H}(R_{kl})$.¹⁰ More specifically, he proposed a tentative set of field

⁹In this paper, we will not deal with Eddington's own work but only with Einstein's perception of it.

¹⁰We are using Einstein's and Hilbert's notation, both of whom referred to the Lagrangian as a Hamiltonian function.

equations for the affine connection based on a Lagrangian proportional to the square root of the determinant of the contracted curvature tensor

$$\mathcal{H} = 2\sqrt{-|R_{kl}|}. \quad (10)$$

In his first note, Einstein does not proceed to derive the field equations explicitly from that Lagrangian. Instead, he does the variation for a general Lagrangian \mathcal{H} which gives him

$$\mathfrak{s}^{kl}_{;\alpha} - \frac{1}{2}\delta_{\alpha}^k \mathfrak{s}^{l\sigma}_{;\sigma} - \frac{1}{2}\delta_{\alpha}^l \mathfrak{s}^{k\sigma}_{;\sigma} - \frac{1}{2}\delta_{\alpha}^k \mathfrak{f}^{l\sigma}_{;\sigma} - \frac{1}{2}\delta_{\alpha}^l \mathfrak{f}^{k\sigma}_{;\sigma} = 0, \quad (11)$$

where \mathfrak{s}^{kl} and \mathfrak{f}^{kl} are defined as variations of \mathcal{H} with respect to g_{kl} and ϕ_{kl} , respectively, i.e.

$$\delta\mathcal{H} = \mathfrak{s}^{kl}\delta g_{kl} + \mathfrak{f}^{kl}\delta\phi_{kl}. \quad (12)$$

Solving with respect to $\Gamma_{\mu\nu}^{\lambda}$, he obtains

$$\Gamma_{kl}^{\alpha} = \frac{1}{2}s^{\alpha\beta} \left(s_{k\beta,l} + s_{l,\beta,k} - s_{kl,\beta} \right) - \frac{1}{2}s_{kl}i^{\alpha} + \frac{1}{6}\delta_k^{\alpha}i^l + \frac{1}{6}\delta_l^{\alpha}i^k, \quad (13)$$

where $i^l = \sqrt{-|s_{kl}|}i^{l\sigma} = \mathfrak{f}^{l\sigma}_{;\sigma}$, and indices are raised and lowered by means of s_{kl} and s^{kl} respectively, a fundamental tensor which in turn is defined via $\mathfrak{s}^{kl} = s^{kl}\sqrt{-|s_{kl}|}$ and $s_{\alpha i}s^{\beta i} = \delta_{\alpha}^{\beta}$.

Explicit field equations were given by Einstein in a short follow up note to his paper [Einstein 1923b] published on May 15, 1923. In it he briefly recapitulated the basic equations of his previous note, implicitly introducing a change of notation by denoting the Ricci tensor as r_{kl} , and denoting the Ricci tensor formed from the fundamental tensor s^{kl} only as R_{kl} . The field equations were now given as the symmetric and antisymmetric parts of

$$r_{kl} = R_{kl} + \frac{1}{6} \left[\left(i_{k,l} - i_{l,k} \right) + i_k i_l \right]. \quad (14)$$

These field equations would not hold up for long. Already two weeks after the publication of the second note, Einstein presented a third note to the Prussian Academy dealing with the affine theory [Einstein 1923c], published in the Academy's Proceedings on 28 June. While Einstein in the introductory paragraph of that paper announced that "further considerations" ("Weiteres Nachdenken") had led him to a "perfection" ("Vervollkommnung") of the

theory laid out in the previous two notes, he was, in fact, going to present some major revisions, including a new set of field equations.

One change in his understanding is reflected in an implicit overall change of notation. While he had previously regarded the symmetric and antisymmetric parts g_{kl} and ϕ_{kl} of the Ricci tensor $R_{kl} = R_{kl}(\Gamma_{\mu\nu}^\lambda)$ as the “metric and electromagnetic field tensors,” he now attaches this physical meaning to different quantities. Hence he now denotes the symmetric part of $R_{\mu\nu}$ as $\gamma_{\mu\nu}$ and uses the letter g resp. \mathfrak{g} to denote the quantities that he had previously denoted by s resp. \mathfrak{s} ,

$$\delta\mathcal{H} = \mathfrak{g}^{kl}\delta\gamma_{kl} + \mathfrak{f}^{kl}\delta\phi_{kl}. \quad (15)$$

It is the quantities \mathfrak{g}^{kl} and \mathfrak{f}^{kl} that were now “regarded as tensor densities of the metric and electric field.” Einstein also pointed out that he no longer would assume the Lagrangian \mathcal{H} to depend on $R_{\mu\nu}$, i.e. only on the sum of $\gamma_{\mu\nu} + \phi_{\mu\nu}$ but would now allow for the possibility that it depend on $\gamma_{\mu\nu}$ and $\phi_{\mu\nu}$ independently.

Thirdly, Einstein does not simply proceed to discuss restrictive conditions or other motivations for a definite choice of \mathcal{H} in order to fix the field equations. Instead, he argues that since, by assumption, eq. (15) is a complete differential,

$$\gamma_{\mu\nu}d\mathfrak{g}^{\mu\nu} + \phi_{\mu\nu}d\mathfrak{f}^{\mu\nu} \quad (16)$$

is a complete differential of another scalar density \mathcal{H}^* where \mathcal{H}^* is a function of the tensor densities of the metric and electric fields, $\mathcal{H}^* = \mathcal{H}^*(\mathfrak{g}^{\mu\nu}, \mathfrak{f}^{\mu\nu})$. For the choice of a definite \mathcal{H}^* Einstein then gives some arguments. It should be a function of the two invariants of the electromagnetic fields, and specifically, he argues that, “according to our present knowledge, the most natural ansatz”¹¹ would be

$$\mathcal{H}^* = 2\alpha\sqrt{-g} - \frac{\beta}{2}f_{\mu\nu}\mathfrak{f}^{\mu\nu}. \quad (17)$$

The resulting field equations, after a rescaling of the electromagnetic field, read

$$R_{\mu\nu} - \alpha g_{\mu\nu} = -\left[\left(-f_{\mu\sigma}f_{\nu}^{\sigma} + \frac{1}{4}g_{\mu\nu}f_{\sigma\tau}f^{\sigma\tau}\right) + \frac{1}{\beta}i_{\mu}i_{\nu}\right] \quad (18)$$

$$-f_{\mu\nu} = \frac{1}{\beta}i_{[\mu}i_{\nu]}. \quad (19)$$

¹¹“Der im Sinne unserer bisherigen Kenntnisse natürlichste Ansatz” [Einstein 1923c, p. 139].

For us, the last half-page of his note, following immediately after equations (18), (19) is most interesting. Einstein observed that the field equations derived along the lines sketched above may also be derived, in fact quite easily, from a different Hamiltonian principle. He conceived of \mathcal{H} as a function of $\mathfrak{g}^{\mu\nu}$ and $\mathfrak{f}^{\mu\nu}$ which Einstein, as was mentioned, in this third note took to be the tensor densities of the metric and electromagnetic fields, $\mathcal{H} = \mathcal{H}(\mathfrak{g}^{\mu\nu}, \mathfrak{f}^{\mu\nu})$. The Lagrangian whose variation with respect to $\mathfrak{g}^{\mu\nu}$ and $\mathfrak{f}^{\mu\nu}$ would produce the field equations (18), (19) directly then reads

$$\mathcal{H} = \sqrt{-g} \left[R - 2\alpha + \kappa \left(\frac{1}{2} f_{\sigma\tau} f^{\sigma\tau} - \frac{1}{\beta} i_{\sigma} i^{\sigma} \right) \right]. \quad (20)$$

Here R denotes the Riemannian curvature scalar formed from the metric tensor $g_{\mu\nu}$. Notwithstanding the cosmological constant term -2α , the Lagrangian already looks familiar. But we need one more little step. In the penultimate paragraph of his paper, Einstein suggests that for a physical interpretation it would be most useful to introduce the “electromagnetic potential”

$$-f_{\mu} = \frac{1}{\beta} i_{\mu}, \quad (21)$$

a step that would eventually turn the field equations into those that were identical - up to the sign of the constant β - to field equations proposed by Weyl.

Let us now pause and look at Einstein’s result through Hilbert’s eyes. If we substitute the electromagnetic potential (21) for i_{μ} , we get the variational principle in the form

$$\delta\mathcal{H} = \delta \int \left\{ R - 2\alpha + \kappa \left(\frac{1}{2} f_{\sigma\tau} f^{\sigma\tau} - \beta f_{\sigma} f^{\sigma} \right) \right\} \sqrt{-g} d\tau = 0. \quad (22)$$

Comparing this variational principle with the variational integral (7) given by Hilbert in his lectures, we see that Hilbert’s interpretation actually does capture Einstein’s result of his third note on the affine theory, provided we make the following identifications. Hilbert’s K would be Einstein’s $R - 2\alpha$, i.e. Hilbert ignored the cosmological term. However, such a term would fit easily into Hilbert’s original scheme. We would also identify Hilbert’s $\alpha\Phi$ with Einstein’s $\frac{\kappa}{2} f_{\sigma\tau} f^{\sigma\tau}$. Finally, we would identify Hilbert’s $\beta\varphi$ with Einstein’s $\kappa\beta f_{\sigma} f^{\sigma}$.

One technical difference remains. Hilbert is doing the variation with respect to the electromagnetic potential φ_{μ} whereas Einstein is doing the

variation with respect to the electromagnetic tensor density $\mathfrak{f}^{\mu\nu}$. In Hilbert's theory, the electromagnetic field was *defined* as $\Phi_{mn} \equiv \varphi_{[m;n]}$ and the variation *produced* the generalized Maxwell equations (5). In Einstein's theory, the variation is done with respect to the field $\mathfrak{f}^{\mu\nu}$. The variation of the term $\beta f_\sigma f^\sigma$ makes use of the *definition* $f^\mu = -(1/\beta) f_{;\nu}^{\mu\nu}$ and *produces* the relation $f_{\mu\nu} = f_{[\mu;\nu]}$ as an electromagnetic field equation. Taking into account that for symmetric connections the homogeneous Maxwell equations (6) follow from the fields being given as the rotation of a vector, we can now see the point of Hilbert's remark.

Regardless of how Einstein had derived his field equations in the first place, he himself had cast them into a form that was technically equivalent to Hilbert's initial framework of 1915. The resulting equations were essentially equivalent to Hilbert's with the only difference that what appeared as a definition and a field equation in one framework turned out to be the resulting field equations and the defining relation in the other. In Hilbert's words, the difference amounted to an "interchange of the two series of Maxwell equations." To be sure, the identification involves some amount of interpretation but essentially we can see why Hilbert rejoiced:

And if on a colossal detour via Levi-Civita, Weyl, Schouten, Eddington, Einstein returns to this result, then this certainly provides a beautiful confirmation.¹²

It also becomes conceivable that Hilbert's reprint of his 1915 and 1917 notes on the *Grundlagen der Physik* in 1924 as a single paper in the *Mathematische Annalen* was not motivated by his desire to revise his original theory (as has been argued in [Renn and Stachel 1999]). His lectures of 1923 in Hamburg and Zurich rather suggest that the true motivation for Hilbert becomes visible on the background of his perception of Einstein's latest work on the affine theory. He saw Einstein's work as a confirmation of his original approach. Hence, there is no reason to assume that Hilbert did not believe what he wrote about his original 1915 theory in the new introduction to the 1924 reprint:

I firmly believe that the theory which I develop here contains a core that will remain and that it creates a framework that leaves

¹²“Und wenn auf dem kollossalen Umweg über Levi Civita, Weyl, Schouten, Eddington Einst. zu dem Resultat zurückgelangt, so liegt darin sicher eine schöne Gewähr.” *Lectures*, part I, p. 20.

enough room for the future construction of physics along the field theoretic ideal of unity.¹³

6 Accessorial Laws of Nature?

As we have seen, Hilbert meant what he said, even though he was deliberately formulating his claim as a hypothesis. Having established that his “world equations” are confirmed, if only by his own perception of a convergence of related research efforts, Hilbert in his second lecture became somewhat more speculative. Of central importance for the argument of his second lecture is the notion of “accessorial laws.”¹⁴ While Hilbert does use the term “accessorial” in a contemporary lecture course,¹⁵ we are not aware of any other usage of the term, neither in Hilbert’s own Oeuvre nor in any of his contemporaries’ writings. Our guess is that Hilbert created a neologism based on the Latin “accedere” — in its meaning “to add.”¹⁶ What notion then does Hilbert want to capture by the term “accessorial”? He says:

Anything that needs to be added to the world equations in order to understand the events (“Geschehnisse”) of inanimate nature, I will briefly call “accessorial.”¹⁷

An obvious candidate for something “accessorial” with respect to the “world equations” immediately comes to mind. These equations being differential equations, require for the explanation of “events” certainly the determination of initial or boundary conditions. Indeed, Hilbert concedes that initial or boundary conditions are necessary in order to allow for definite solutions

¹³“Ich glaube sicher, daß die hier von mir entwickelte Theorie einen bleibenden Kern enthält und einen Rahmen schafft, innerhalb dessen für den künftigen Aufbau der Physik im Sinne eines feldtheoretischen Einheitsideals genügender Spielraum da ist.” [Hilbert 1924, p. 2].

¹⁴For another discussion of this concept, see [Majer and Sauer 2003].

¹⁵See lecture notes for course on “Über die Einheit in der Naturerkenntnis,” held in winter 1923/24. NSUB Cod. Ms. Hilbert 568, p. 247.

¹⁶We realize that the English word “accessorial” is not a neologism and its meaning of auxiliary, supplementary makes good sense in the present context.

¹⁷“Ich möchte Alles, was noch zu den Weltgleichungen hinzugefügt werden muss, um die Geschehnisse in der leblosen Natur zu verstehen, kurz *accessorisch* nennen.” *Lectures*, part II, p. 1.

of the “world equation,”¹⁸ but, obviously he has something more demanding than “initial conditions” in mind, because he does not qualify them as “accessorial.” Hence, the question arises, what else does he want to capture with the term “accessorial.” To answer this question, we have to explain how he proceeds in the second lecture.

Conceding that the world equations are in need of initial or boundary conditions, the main point of Hilbert’s second Hamburg lecture is to argue for another and non-trivial meaning of “accessorial.” Even with initial conditions, the equations, being differential equations with respect to some time coordinate, would only predict the future from the past, but would they also teach us something about the present which after all, as Hilbert argues, is what we really want? If the answer is no, then we are in need of “accessorial” *laws*, that can tell us something about the present state of nature. Now the interesting point is, as we will see in a moment, that Hilbert argues that no such accessorial laws of nature exist, for the simple reason that precisely that which we want to capture with such laws is either inconsistent with the world equations or already contained in them.

A first argument supporting his claim is a discussion of the irreversibility of thermodynamics. He looks at the example of the mixing of a gas that is initially distributed over two separate halves of a container and emphasizes that the apparent asymmetry with respect to past and future is exclusively a consequence of the choice of the initial states and the initial conditions, and hence that the irreversibility is not one that exists objectively in inanimate nature and its lawfulness but is only an apparent irreversibility, arising from what he called our anthropomorphic point of view.

The argument is interesting in itself, especially with respect to Hilbert’s epistemological position.¹⁹ While Hilbert is unambiguous about his claim that there are no accessorial laws introduced in statistical mechanics, he himself brings up an obvious objection. The example of the diffusion of a gas in a container in the theoretical context of kinetic gas theory presupposes the assumption that there exist atoms and molecules, and that these are the fundamental constituents of the diffusing gas. This argument leads him to a

¹⁸For further discussion Hilbert would assume the world to be Euclidean-Newtonian at infinity, but with respect to contemporary cosmological debates Hilbert added a disclaimer to the effect that this choice was only motivated by formal simplicity and was made only to fix the ideas.

¹⁹For a more detailed discussion of the non-objective but anthropomorphic character of certain apparently irreversible processes in inanimate nature, see [Majer 2002].

discussion of the question whether the principle of atomism is an accessorial law of nature. Hilbert's position on this issue is just as unambiguous as is his position on the issue of irreversibility. He argues that the world equations, possibly after necessary elaborations or corrections, suffice to explain the existence, and even the structure, and properties, of matter. In order to justify this claim, Hilbert refers to Bohr's quantum theory and to the explanation of basic features of the periodic system of elements (such as its periodicity and the chemical stability of the inert gases) on the grounds of the electron orbit model.

Hilbert's conclusion from this discussion is that the field equations and laws of motion suffice to derive the deepest properties of matter including the characteristic details of the chemical elements as particular mathematical integrations of the field equations. It is important to note that in this respect the "world equations" differ fundamentally from Newton's laws, including gravity, because the latter do not imply anything about the existence of atoms and molecules. Of course, Hilbert would take it for granted, among other things, that particle-like solutions of the field equations would exist whose dynamics would then be governed by the field equations as well, rather than by independent equations of motion.

Hilbert's belief that the world equations can tell us something about the present presupposes that we accept only those solutions to the equations that correspond to constant or periodic processes in nature. Hence we have to qualify the assertion about the non-existence of accessorial laws by admitting that there are accessorial *ideas* and *principles*, such as stability and periodicity. But the crucial difference, according to Hilbert, is that these accessorial ideas and principles do not have the character of new equations but are of a more general nature that is connected to our thinking as such and to our attitude towards nature.

It so happened that a number of the assumptions made by Hilbert, both explicitly and implicitly, turned out to be highly problematic, if not false. This is the case, e.g, with the violation of gauge invariance implied by accepting an explicit dependence of the Lagrangian on the electromagnetic potential. But before dismissing Hilbert as a bad speculative physicist, let us take seriously the fact that he himself in a most enthusiastic manner pointed to the rapid development of the natural sciences and the rapid succession of fundamental discoveries. It seems to us that his perhaps premature acceptance of results which had yet to be confirmed appears to us today naïve for a very specific reason. Hilbert's optimism was fuelled by his unwillingness

to accept the fact that the modern development of the natural sciences no longer allows for a conceptual unity of knowledge. In this respect, by the way, he was not alone. Indeed, the purpose of the first two lectures of his trilogy was to provide the scientific underpinning for a more philosophical claim that he made in the third lecture.

7 Hilbert's Position between Kantian Apriorism and Poincaré's Conventionalism

Let us therefore return now to Hilbert's epistemological position.²⁰ In his third lecture, Hilbert addresses the ancient question as to the sources of our knowledge, or, in his own words:

We are dealing here with a decision of an important philosophical problem, namely the old question as to the portion of our knowledge that comes from our thinking, on the one hand, and from experience, on the other hand.²¹

In the remainder of this paper we want to say a few words about Hilbert's answer to the question of the borderline between knowledge a priori and knowledge by experience. Hilbert's position is based on two fundamental presuppositions. The first of these is the distinction between two different domains of the natural sciences, the domain of "inanimate" nature, which is the proper domain of physics in the widest sense, and the domain of living beings including "man as such" which is the domain of biology, including the social and cultural sciences. Even though the distinction seems problematic from a physicalistic point of view, it has not been shown to this day whether the laws of physics, as we know them today, suffice to deduce the phenomena of life, or whether we need in fact some accessorial laws or principles.²² But for our context, it is sufficient to take this distinction as a warning that the claim that there exist "world equations" in the strong sense, i.e. that we do

²⁰See [Majer and Sauer 2003] for a more extensive discussion.

²¹"Wir stehen da vor der Entscheidung über ein wichtiges philosophisches Problem, nämlich vor der alten Frage nach dem Anteil, den das Denken einerseits und die Erfahrung andererseits an unserer Erkenntnis haben." *Lectures*, part III, p. 1.

²²For a discussion of this intricate question in connection with the supposed irreversibility of living processes, see [Majer 2002].

not need any accessorial laws, is certainly more difficult to establish if the life sciences were included in the claim.

The second fundamental distinction that plays a role here is a distinction between three different levels of experience: (i) a level of every day experience, (ii) a level of scientific experience in the broadest sense of the term, and (iii) a level of totally objective knowledge that is achieved by an emancipation from what Hilbert calls our anthropomorphic point of view. The principle of objectivity that Hilbert had introduced earlier in his first lecture illustrates what Hilbert has in mind by the emancipation from the anthropomorphic point. This principle states

A sentence about nature, expressed in coordinates, is only then a proposition about the objects in nature, if the sentence has a content which is independent of the coordinates.²³

According to Hilbert, this emancipation from the coordinate system can be achieved in three different ways that correspond to the three forms of singular, particular, and general judgment: First, by showing or presenting a concrete object, in respect to which the coordinate system has to be fixed; second, in the form of an existential assertion by saying: there exists a coordinate system in which all the formulated relations between the objects considered are valid; third, by formulating the proposition in a form that is valid in every coordinate system. Evidently, this distinction implies that the introduction of coordinates in the first place is a compromise to our human way of looking at nature, and the third way of emancipating from a coordinate system therefore represents the most far-reaching “emancipation from the anthropomorphic point of view.” A certain view of the actual and proper development of science is implicit in this latter assumption, and Hilbert’s epistemology is indeed a philosophy of graded progress [Majer and Sauer 2003].

Hilbert emphatically points out that the Kantian question is ripe for an answer for two reasons, (1) the spectacular progress in the sciences of the time and (2) the advent of the axiomatic method. Much more needs to be said about Hilbert’s epistemological position in general and the interrelations of these two moments in particular. But for the sake of brevity, let us here only point to the role of the world equations. Hilbert says:

²³“Ein in Koordinaten ausgedrückter Satz über die Natur ist nur dann eine Aussage über die Gegenstände in der Natur wenn er von den Koordinaten unabhängig einen Inhalt hat.” *Lectures*, part I, p. 3.

If now these world equations, and with them the framework of concepts, would be complete, and we would know that it fits in its totality with reality, then in fact one needs only thinking and conceptual deduction in order to acquire all physical knowledge.²⁴

Leaving aside the difficult question concerning completeness of physical theories, we only wish to emphasize that Hilbert, contrary to what one might expect from this quote, by no means wants to take an idealistic position. He emphasizes

I claim that precisely the world equations can be obtained in no other way than from experience. It may be that in the construction of the framework of physical concepts manifold speculative view points play a role; but whether the proposed axioms and the logical framework erected from them is valid, experience alone can decide this question.²⁵

In the sequel to the lecture, Hilbert refined this somewhat crude position by taking issue with Kantian apriorism and with Poincaré's conventionalism. The upshot is

The opinion advocated by us rejects the absolute Apriorism and the Conventionalism; but nevertheless it does in no way retreat from the question of the precise validity of the laws of nature. I will instead answer this question in the affirmative in the following sense. The individual laws of nature are constituent parts of the total conceptual framework, set up axiomatically from the world-equations. The world-equations are the precipitation of a long, in part very strenuous, experimental inquiry and of experience, often delayed by going astray. In this way we come to the idea

²⁴“Wenn nun diese Weltgleichungen und damit das Fachwerk vollständig vorläge, und wir wüssten, *dass es* auf die Wirklichkeit in ihrer Gesamtheit passt und dann bedarf es tatsächlich nur des *Denkens* d.h. der begrifflichen *Deduktion*, um alles phys. Wissen zu gewinnen.” *Lectures*, part III, pp. 20f. (Hilbert's emphasis).

²⁵“... behaupte ich, dass gerade die Weltgesetze auf keine andere Weise zu gewinnen sind, als aus der Erfahrung. Mögen bei der Konstruktion des Fachwerkes der phys. [Begriffe] mannigfache spekulative Gesichtspunkte mitwirken: *ob* die aufgestellten Axiome und das aus ihnen aufgebaute logische Fachwerk stimmt, das zu *entscheiden*, ist allein die *Erfahrung* im Stande.” *ibid.*, p. 21 (Hilbert's emphasis).

that we approximate asymptotically a Definitivum by continued elaboration and completion of the world-equations.²⁶

Whatever may be said about this position from a historical and philosophical standpoint, we hope to have at least shown that Hilbert's work along the unified field theory program is embedded in a broader perspective of epistemological and methodological concerns that well deserves to be taken seriously, even on today's philosophical horizon.

Acknowledgments

One of us (T.S.) would like to thank Jim Ritter for illuminating discussions about Einstein and the unified field theory program during our common stay at the Max Planck Institute for the History of Science in Berlin. We also thank Diana Buchwald, Dan Kennefick, and Stephen Speicher for helpful comments on an earlier draft of this paper. Hilbert's lectures are quoted by kind permission of the *Niedersächsische Staats- und Universitätsbibliothek (Handschriftenabteilung)*.

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²⁶ "Die von uns vertretene Meinung verwirft den unbedingten Apriorismus und den Konventionalismus; aber sie entzieht sich trotzdem keineswegs der vorhin aufgeworfenen Frage nach der genauen Gültigkeit der Naturgesetze. Ich möchte diese Frage vielmehr bejahen und zwar in folgendem Sinne. Die einzelnen Naturgesetze sind Bestandteile des Gesamtfachwerkes, das sich aus den Weltgleichungen axiomatisch aufbaut. Und die Weltgleichungen sind der Niederschlag einer langen zum Teil sehr mühsamen und oft durch Irwege aufgehaltenen experimentellen Forschung und Erfahrung. Wir gelangen dabei zu der Vorstellung, da wir uns durch fortgesetzte Ausgestaltung und Vervollständigung der Weltgleichungen asymptotisch einem Definitivum nähern." *ibid.*, pp. 42f.

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