J. Astrophys. Astr. (1999) **20,** 91–101

New Light on the Einstein-Hilbert Priority Question

John Stachel, *Department of Physics and Center for Einstein Studies, Boston University, 590 Commonwealth Avenue, Boston, Massachusetts 02215 USA.*
email:stachel@buphyc.bu.edu

1. Introduction

This talk is based on the joint work with Leo Corry and Jürgen Renn.[1] The development of general relativity may be compared to a traditional three-act play:

I (1907-1908): The formulation of the equivalence principle;

II (1912-1913): The choice of the metric tensor field as the appropriate relativistic generalization of the scalar Newtonian gravitational potential;

III (1913-1915): The search for the correct field equations for the metric tensor field.

The first two acts were essentially monologues starring Albert Einstein. The third act was a dialogue between Einstein and David Hilbert, the leading Göttingen mathematician. I have told the story of all three acts from Einstein's point of view elsewhere in some detail,[2] so I shall be brief in reviewing the first two acts. But I must say more about the third act, since this is where Hilbert entered the story and the priority question between them arose.

Acts I and II. In 1907, Einstein prepared a review article on the principle of relativity and its consequences for the various branches of physics.[3] In the course of the review, he discussed the question of a relativistic theory of gravitation.[4] It was clear from the outset that the Newtonian theory would have to be modified, since it is based on the concept of a force between pairs of particles that depends on the distance between them at some moment of time. But according to the special theory of relativity, simultaneity is no longer absolute, but depends on the inertial frame of reference being considered; hence the spatial distance between two particles is now frame -dependent. The example of electromagnetism suggests how to proceed. The electrostatic Coulomb force between two charged particles also seems to depend on their simultaneous positions; but we know how to recast the theory into a field form (Maxwell's equations) that is special-relativistically invariant, and in which interactions between charges actually propagate with a finite speed—the speed of light. The problem was to find a similar field theory of gravitation. Indeed, Newton's theory had already been cast into a field form: the gravitational potential obeys Poisson's equation with the density of matter as its source. The problem was to find a relativistic generalization of Poisson's equation. But here Einstein hit upon a fundamental feature of the gravitational field that has no parallel in electromagnetism. All objects fall with the same acceleration in a given gravitational field (Galileo's principle). Newton had explained (or better reformulated) this observation as a consequence of the equality of gravitational and inertial mass. He verified this equality to one part in a thousand, but offered no further explanation of it. It follows from this equality and Newton's laws of motion that no mechanical experiment can ever distinguish between an inertial frame of reference, in which there is a constant gravitational field + **g,** and an accelerated frame of reference with acceleration –g with respect to the inertial frames, in which there is no gravitational field. Einstein went Newton one better, and proposed what he soon called the principle of equivalence:[5] No experiment, mechanical, optical, electromagnetic, or what have you, can distinguish between these two situations because there is no physical difference between them: inertia and gravitation are essentially just two sides of the same coin ("wesensgleich," as Einstein put it). But this means that, once gravitation is taken into account, the privileged role of the inertial frames and with it the special-relativity principle must be abandoned. Or rather, one must look for a field theory of gravitation that is invariant under some generalization of the special principle, i.e., under a group that includes transformations to (at least some) accelerated frames of reference. Einstein reached this conclusion (which forms the end of Act I) by late 1907, but for a long time he was the only physicist looking for a field theory of gravitation to accept it.[6]

At first Einstein looked for a scalar generalization of Newton's theory, based on the gravitational potential. By the middle of 1912, he had worked out what he regarded as a satisfactory theory for the case of a static gravitational field.[7] He developed a field equation for the gravitational potential, which he identified in this case with a variable speed of light $c(x,y,z)$ instead of the constant speed of the special theory; and worked out the equations of motion for a particle in the static gravitational field. He soon realized that these equations of motion could be derived from a variational principle that implicitly involves a non-flat metric tensor field. Planck had shown that the equation of motion of a particle in Minkowski space-time (i.e., in the absence of a gravitational field) could be derived from a variational principle involving the proper time along a timelike worldline in a flat space-time with Minkowski metric tensor. Einstein found that his equations of motion for a particle in a static gravitational field could be derived from the same variational principle if he just replaced the constant c in the Minkowski metric with $c(x,y, z)$.[8] This yields a non-flat metric tensor field $g_{\mu\nu}$ of a special type; Einstein guessed that the way to proceed beyond the static case was to introduce a general non-flat metric tensor field as the generalization of the scalar Newtonian potential.[9] He presented this idea in the opening section of a paper written in early 1913,[10] concluding Act II of the play.

Act III. The rest of his 1913 paper begins the lengthy final Act III. With the help of his friend and colleague Marcel Grossmann, Einstein succeeded in showing that the effects of gravitation on all other physical processes could be taken into account by means of the non-flat metric tensor field, and that the resulting equations could be put into a generally covariant form. That is, the group of transformations under which these equations are invariant includes all invertible differentiable point transformations. (Einstein, in keeping with the mathematical treatments of the time, gave the transformations a passive interpretation as coordinate transformations; but from the modern viewpoint it is better to give them an active interpretation as differentiable point transformations, or diffeomorphisms.) Thus the group automatically includes all possible transformations to accelerated frames of reference, which seemed to Einstein a very satisfactory solution to the problem of generalizing the relativity principle.

However, one major difficulty remained: He could not make the differential equations for the gravitational field itself generally covariant. If they were based on the Ricci tensor, just about the only generally-covariant candidate for second-order field equations constructed from the metric tensor, the equations relating the latter to its source, the stress-energy-momentum tensor describing all non-gravitational matter and fields, did not seem to allow passage to the well-verified static Newtonian limit (Poisson's equation).[11] The root of the problem lay in a misconception of what the static metric tensor field should be, and how the Newtonian limit should be taken. But this did not emerge until well after Einstein had abandoned generally covariance in favor of a set of non-covariant field equations, now usually called the Einstein-Grossmann field equations, first presented in the 1913 paper.

Being Einstein, he did not rest content with the Newtonian-limit argument against field equations based on the Ricci tensor (indeed he seems to have become suspicious of that argument rather soon). He soon came up with what appeared to be a devastate-ing argument against generally covariant gravitational field equations of any kind, which he dubbed the hole argument:[12] Consider a finite region of space-time (the hole), inside of which there is no matter. Einstein required of any gravitational theory that the specification of all sources of the field outside the hole -together with any appropriate boundary conditions on the hole, at infinity, etc. -should uniquely determine the gravitational field inside the hole. But this seems to be impossible if the gravitational field equations are generally covariant: Suppose we have one solution to the field equations outside and inside the hole. Then let us carry out a diffeomorphism (i.e., a one-one differentiable point transformation)[13] that reduces to the identity outside and on the boundary of the hole, but is different from the identity inside the hole. For generally covariant equations, the diffeomorphic-transform of a solution is also a solution to the field equations. Thus, the same source and boundary conditions outside the hole correspond to two distinct solutions inside the hole - indeed, since the group of diffeomorphisms depends on four arbitrary functions, the number of such distinct solutions is unlimited. Therefore one must look for gravitational field equations that are not generally covariant; yet they must be invariant under a group of transformations that includes at least some non-linear ones (accelerations) –but not so many as to fall foul of the hole argument.

This is how the situation stood, as Einstein saw it, in late 1913, and so it stayed until late 1915. He remained wedded to the Einstein-Grossmann field equations and kept trying to find better and better arguments in their favor; in particular, arguments for their uniqueness and for their invariance under the maximum invariance group compatible with avoiding the hole argument.

It was not until the end of 1915, after he had returned to general covariance, that he found the flaw in his hole argument against it: In modern terms, the hole argument presupposes that some intrinsic physical significance attaches to the points of a four-dimensional manifold *before* the metric tensor field is defined on it. Once this idea is abandoned, and it is accepted that the points of the manifold inherit all their physical properties from the metric tensor field (and any other physical fields that may be present in regions outside the hole), then it is clear that two mathematical solutions inside the hole that differ only by a diffeomorphism transformation represent the *same* gravitational field; so the uniqueness requirement is satisfied. But as noted above, Einstein did not realize this until after he had returned to general covariance for other reasons.

Enter Hilbert. In the summer of 1915, while he was at the stage of the non-covariant theory, Einstein was invited to give a series of lectures on his gravitational theory in Göttingen (he stayed from June 29th to July 7th), then the mathematical capital of Germany if not the world. In addition to Hilbert, it boasted the presence of Felix Klein, the doyen of German mathematics, Emmy Noether, and many other luminaries. Einstein, who had been having his troubles getting the physicists to accept his approach to gravitation, was delighted with the reception of his work by the Göttingers.

> In Göttingen I had the great pleasure of seeing everything understood, down to the details. I am quite enthusiastic about Hilbert. An important man. I am very curious about his opinion.[14]

For his part, Hilbert, who had been working on Mie's nonlinear electrodynamic theory, which aimed at explaining the structure of matter, was delighted with Einstein's approach to gravitation. He decided to combine Mie's four electrodynamic potentials q_u with Einstein's gravitation potentials $g^{\mu\nu}$ in order to forge a theory of matter that would enable him to explain the structure of the electron, as well as its curious non-radiative behavior in the Bohr atom.[15] In the fall of 1915, Einstein and Hilbert entered into an intense correspondence - the only serious one either had on this topic -just as Einstein was reaching the crisis point in his efforts to shore up the Einstein-Grossmann equations, leading to their abandonment.[16] Einstein's letters to Hilbert report such milestones as: his return to general covariance and re-adoption of field equations based on equating the Ricci tensor to the stress-energy tensor in two papers submitted on 4th and 11th November;[17] his solution of the problem of the anomalous precession of the perihelion of Mercury submitted on 18th November;[18] and his adoption of the final form of the field equations submitted on 25th November.[19] These equations are still based on the Ricci tensor, but now with $-1/2 g_{\mu\nu} T$ (where T is the trace of the stress-energy tensor—this term is often referred to as the trace term) subtracted from the stress energy tensor on the right hand side. Meanwhile, Hilbert had not been inactive. He, too had formulated field equations—but for a combined theory of gravitation and Mie's electromagnetism, based on a generally-covariant variational principle.

The accepted account of just what happened during this period has been succinctly formulated in a recent biography of Einstein:

> In the decisive phase [of his work on general relativity] Einstein even had a congenial colleague, though this caused him more annoyance than joy, as it seemed to threaten his primacy. "Only one colleague truly understood it, and he now tries skilfully to 'nostrify' [i.e., appropriate] it," he complained to [Heinrich] Zangger about what he evidently regarded as an attempt at plagiarism. This colleague was none other than David Hilbert[...] What must have irritated Einstein was that Hilbert had published the correct field equations first—a few days before Einstein. [...]
> In November, when Einstein was totally absorbed in his theory of gravitation, he essentially corresponded only with Hilbert, sending Hilbert his publications and, on November 18th, thanking him for a draft of his article. Einstein must have received that article immediately before writing this letter. Could Einstein, casting his eye over Hilbert's paper, have discovered the term which was still lacking in his own equations [i.e., the trace term], and thus 'nostrified' Hilbert?[20]

The author argues that this is improbable, and most authorities agree; but the point is that, on the accepted account, which based on comparison of the published version of Hilbert's paper on the foundations of physics[21] with Einstein's papers, the dating of the papers makes such an act of plagiarism by Einstein *possible.*

Recently, Leo Corry found a set of printer's proofs of Hilbert's paper, marked "First proofs of my first note" in his handwriting.[22] As a result of a study of these proofs and their dating, one can conclude that Einstein *could not* have taken anything pertaining to the field equations from Hilbert's paper, or the summary sent him in November. Indeed, the situation is quite the reverse: The question is whether Hilbert might have taken from Einstein's 25th November paper, because it was *quite possible* for him to do so. Let me explain how this reversal of the direction of possible influence came about.

The published article by Hilbert is dated, "submitted to the session [of the Göttingen Academy of Sciences] of 20 November 1915." Einstein's conclusive paper, in which he gave the final form of his generally-covariant field equations was submitted to the session of the Prussian Academy of Sciences in Berlin of 25th November 1915. Bearing in mind that Hilbert sent a summary or draft of his paper to Einstein, to which the latter replied on November 18th, it seems that Einstein had available at least the essence of Hilbert's work a week before he submitted his conclusive paper of 25th November. And so he did—but it was Hilbert's work as presented in the proofs. These proofs are dated exactly the same way as the published version – "submitted to the session of 20 November 1915" – and bear a printer's date stamp –"6th December 1915" – indicating that is when they were typeset. These dates would mean nothing if the proofs were essentially the same as the published version. But they are not: they differ in several important respects, the most crucial being that:

(**1**) The theory that they present is not generally covariant; in addition to generally covariant field equations, there are four equations whose purpose is precisely to limit the coordinate system.

(**2**) The generally covariant gravitational field equations are not written down explicitly but merely as the variational derivative, which is not evaluated, of a Lagrangian. In particular, there is no hint of a trace term.

Since the proofs are dated November 20th, they represent the status of Hilbert's work submitted on that date to the Academy and presumably previewed in his earlier communication to Einstein. Since they bear the printer's stamp dated December 6th, any changes Hilbert made before that date are incorporated in the proofs (indeed, he had probably made no changes since he marked them "first proofs"). Thus, there is no chance that Einstein could have learned about the need for a trace term in his equations from a perusal of Hilbert's work before Einstein submitted his paper of November 25th including the trace term.

On the other hand, Hilbert did not start correcting the proofs until 6th December at the earliest (he finally gave up the revision and completely rewrote the article, as we shall see); and Einstein's 25th November paper was published on 2nd December. Since Hilbert's paper was not actually published until March 1916, there *was* plenty of opportunity for Hilbert to see Einstein's paper before publishing his own. Indeed, there is no doubt that he did: Hilbert's paper, which was actually published in the issue of the *Göttinger Nachrichten* dated 31st March, includes references to all of Einstein's November papers including that of 25th November.

Since we now know that Hilbert drastically rewrote his original (proofs) version sometime between December 1915 and March 1916, the question is no longer what could Einstein have gotten from Hilbert, but: What could Hilbert have gotten from Einstein?

Hilbert's proofs: We shall only discuss the two major points of difference between the proofs and the published paper that are most relevant to the priority question; for more detailed comparisons one can consult the references in note 15. First of all, as noted above, in the proofs Hilbert asserts that the theory he is developing cannot be generally covariant. He bases this assertion on a slighly more sophisticated version of Einstein's hole argument (he cites Einstein's 1914 discussion of that argument), involving the Cauchy problem on an initial hypersurface rather than boundary conditions on a hole:

> Since our mathematical theorem [a version of what later became known as Noether's theorem, which guarantees the existence of four identities between generally covariant field equations] shows that the previous axioms I [the existence of a Lagrangian for the four electromagnetic and ten gravitational equations] and II [the general covariance of that Lagrangian] can only provide ten essentially independent equations for the 14 potentials [of gravitation and electromagnetism]; and further, maintaining general covariance makes quite impossible more than ten essentially independent equations for the 14 potentials $g_{\mu\nu}$, q_s; then, in order to keep the deterministic character of the fundamental equations of physics, in correspondence with Cauchy's theory of differential equations [that is, to have a well-posed Cauchy problem], the requirement of four further non-invariant equations to supplement [the generally covariant gravitational equations] is unavoidable. In order to find these equations I start out by setting up a definition of the concept of energy (pp. 3–4 of the proofs, translation from reference in note 1).

Without going into the details of the resulting energy theorem, I shall only cite Hilbert's:

> Axiom III (axiom of space and time). The space-time coordinates are those specific world parameters [Hilbert's name for arbitrary coordinates] for which the energy theorem . . . is valid.
> The validity of [the energy] equation ... is a consequence of Axiom III; these four differential equations .. supplement the [generally-covariant] gravitational equations . . . to yield a system of 14 equations for the 14 potentials $g^{\mu\nu}$, q_s: *the system of the fundamental equations of physics* (original emphasis; p. 7 of the proofs, translation from reference in note 1).

Thus, Hilbert was adopting Einstein's line of reasoning from 1913–mid 1915 just at the time that Einstein was abandoning it, in favor of a return to general covariance. Of course, Hilbert did include ten generally covariant gravitational equations in his "system of the fundamental equations of physics," but Einstein was not impressed by this move. Immediately after receiving Hilbert's account of his new theory, Einstein wrote him on 18th November:

> The difficulty was not to find generally covariant equations for the $g^{\mu\nu}$; this is easy with the help of the Riemann tensor. What was difficult instead was to recognize that these equations form a generalization, and indeed a simple and natural

> generalization of Newton's law. I only succeeded in doing so in the last few weeks (I sent you my first communication); while I had already considered the only possible generally covariant equations, which have now proven to be the correct ones, three years ago with my friend Grossmann. Only with a heavy heart did we give them up, because the physical discussion appeared to show me their incompatibility with Newton's law (my translation).[23]

That Einstein's claims about his earlier work are quite accurate is proved by an examination of his 1912 Zürich research notebook.[24]

The second major point about Hilbert's proofs concerns the form of his gravitational field equations. In the proofs, these equations do not appear explicitly. Hilbert's Lagrangian contains a gravitational term, which is probably $\sqrt{-g}R$ in modern terms,[25] where R is the Ricci scalar (unfortunately, the few lines where the Lagrangian is defined are missing from the proofs), and he indicates that the gravitational field equations result from taking the variational derivative of this term with respect to the metric tensor. But he does not evaluate this variational derivative at all.

Why did Hilbert decide to publish his work before he had accomplished his original aim: to explain the behavior of the electron? He submitted his work to the Göttingen Academy two days after Einstein's letter of 18th November,[26] which opened with a sentence I have not yet quoted:

> The system given by you agrees - as far as I can see – completely with what I have found in the last few weeks and sent to the Academy;

and closed with the news that:

> I have today handed in a work to the Academy, in which without any supplementary hypothesis I have quantitatively derived the perihelion motion of Mercury, discovered by Leverrier, from general relativity (my translation).[27]

As Tilman Sauer suggests,[28] Hilbert may well have felt some urgency to establish his own claims in the light of Einstein's successes.

It was shortly after receiving Hilbert's summary of his theory, which presumably contained both points discussed above, that Einstein complained about Hilbert's "nostrification" of his theory. The letter, cited earlier, continues:

> In my personal experience, I have hardly ever learned to know better the wretchedness of human beings than on the occasion of this theory and what is connected with it. But I don't give a damn (my translation).[29]

Hilbert was apparently aware of Einstein's unhappiness. When he got the proofs of his paper (presumably on or about 6th December), he added an additional complimentary reference to Einstein among several other handwritten insertions; but then he seems to have realized that he would have to completely rewrite the paper in the light of Einstein's four November papers (as noted above, they are all referenced in the published version of Hilbert's paper).[30]

The published paper: Hilbert removed all references to his argument about the need for four non-generally covariant equations to supplement the ten generally-covariant gravitational field equations. He did not discuss the subject of causality for the field equations in this paper at all. Only in 1917, in his second paper on the foundations of physics, did he return to it.[31] He simply asserted Axioms I and II, and dropped Axiom

III and all other references to preferred coordinate systems. He completely changed his energy discussion to take into account the full general covariance of the present version of his theory.

He also now wrote down the explicit form of the gravitational field equations that follow from the variational derivative of his Lagrangian: the trace term $1/2g_{\mu\nu}R$, where R is the trace of the Ricci tensor, is subtracted from the Ricci tensor, giving what we now call the Einstein tensor as the lefthand side of the field equations. (His ighthand side is the stressenergy tensor for Mie's electrodynamic theory.)

Indeed, he was the first to give the field equations in this form. As noted above, Einstein, in his paper of 25th November had the Ricci tensor on the left hand side, with the trace of the stressenergy tensor subtracted on the righthand side. The two forms are completely equivalent, of course; but whether Hilbert or Einstein immediately recognized this is not clear; nor if Hilbert did recognize it, is it clear whether it influenced him in any way. What is clear is that the argument he offers for the form of the left hand side of the field equations is fallacious. Instead of evaluating the Einstein tensor he simply says:

> ...which follows easily without calculation from the fact that, except for $g_{\mu\nu}$, $K_{\mu\nu}$ [the Ricci tensor] is the only tensor of second order and K [the Ricci scalar] is the only invariant that can be constructed from only the $g^{\mu\nu}$ and its first and second order partial derivatives. . . (pp. 404–405 of ref. 20, translation from ref. in note 1)

The argument is fallacious, of course, because many other tensors of second order in the derivatives of the metric and many other invariants can be constructed from the Riemann tensor. Even if one requires linearity in the second derivatives, the fact that the coefficient of the trace term is exactly 1/2 remains quite undetermined by this argument. Hilbert himself seems to have recognized its untenability: When he reprinted the paper in 1924,[32] he dropped it and simply sketched a correct method for evaluating the variational derivative.

Hilbert added several clear aknowledgements of Einstein's priority in constructing a generally-covariant gravitational theory based on the metric tensor, and continued to acknowledge it on many later occasions. If he had only added a few words to the dateline of the published paper: "revised version submitted on any date after 6th December," the whole later priority question could have been avoided.[33]

At any rate, the priority issue between Einstein and Hilbert was happily resolved quite soon. On 20th December 1915, well before Hilbert's paper appeared in print, Einstein wrote him:

> There has been a certain resentment between us, the cause of which I do not wish to analyze. I have fought against the feeling of bitterness associated with it, and indeed with complete success. I again think of you with unclouded friendliness and I ask you to attempt the same with me. It is objectively a pity if two genuine chaps, who have liberated themselves to some extent from this sorry world, do not give each other mutual pleasure (my translation).[34]

Notes

1 Leo Corry, Jürgen Renn and John Stachel, "Belated Decision in the Hilbert-Einstein Priority Dispute," *Science 278* 1270–1273 (1997).

[2] See John Stachel, "The Genesis of General Relativity," in H. Nelkowski *et al,* eds., *Einstein Symposion Berlin, Lecture Notes in Physics 100* (Berlin/Heidelberg/New York: Springer-Verlag, 1980), pp. 428–442; "Einstein and the Rigidly Rotating Disc," in A. Held ed. *General Relativity and Gravitation One Hundred Years After the Birth of Albert Einstein* (New York: Plenum, 1980), pp. 1–15, reprinted in D. Howard and J. Stachel, eds., *Einstein and the History of General Relativity/Einstein Studies, vol. 1* (Boston/Basel/Stuttgart: Birkhäuser, 1989), pp. 48-62; "How Einstein discovered general relativity: a historical tale with some contemporary morals," in M. A. H. MacCallum, ed., *General Relativity and Gravitation/Proceedings of the 11th International Conference on General Relativity and Gravitation* (Cambridge: Cambridge University Press, 1987), pp. 200-208; "Einstein's Search for General Covariance, 1912-1915," in D. Howard and J. Stachel, eds *Einstein and the History of General Relativity/Einstein Studies, vol. 1* (Boston/Basel/Stuttgart: Birkhäuser, 1989), pp. 63–100.

[3] Albert Einstein, "Über das Relativitätsprinzip und die aus demselben gezogenen Folgerungen," *Jahrbuch der Radioaktivität und Elektronik 4,* 411–462 (1907); reprinted in John Stachel et al., eds. *The Collected Papers of Albert Einstein,* vol. 2, *The Swiss Years: Writings 1900–1909,* (Princeton University Press, 1989), pp. 433-488. At that time he still referred to the principle, rather than the theory, of relativity, when referring to what was later called special relativity.

[4] See ref. in note 3, pp. 476–484.

[5] See Albert Einstein, "Lichtgeschwindigkeit und Statik des Gravitationsfeldes," *Annalen der Physik 38,* 355–369 (1912), p. 365.

[6] Some physicists looking for a relativistic theory of gravitation continued to adhere to the special-relativistic principle (Gunnar Nordström and Gustav Mie); one physicist abandoned the relativity principle entirely in his search for a field theory of gravitation (Max Abraham).

[7] See ref. in note 5, and Albert Einstein, "Zur Theory des statischen Gravitationsfeldes," *Annalen der Physik 38,* 443–458 (1912); both papers are reprinted in Martin J. Klein *et al,* eds, *The Collected Papers of Albert Einstein,* vol. 4, *The Swiss Years: Writings 1912–1914,* (Princeton University Press, 1995), pp. 130–144,147–162.

[8] See second reference in note 7, p. 458.

[9] The consideration of stationary gravitational fields, and in particular the gravita tional field on a rotating disc, also played a role in convincing Einstein of the need to go beyond flat space to solve the gravitational problem. See John Stachel, "Einstein and the Rigidly Rotating Disc," reference in note 2.

[10] See Albert Einstein and Marcel Grossmann, *Entwurf einer verallgemeinerten Relativitätstheorie und einer Theorie der Gravitation,* (Leipzig/Berlin, B.G. Teubner, 1913). Part I, "Physikalische Teil," was written by Einstein, Part II, "Mathematischer Teil," by Grossmann. The paper is reproduced in Martin J. Klein *et al,* eds , *The Collected Papers of Albert Einstein,* vol. 4, *The Swiss Years: Writings 1912 1914,* (Princeton University Press, 1995), pp. 303–339.

[11] See John Stachel, "Einstein's Search for General Covariance, 1912–1915," reference in note 2.

[12] For more detailed discussions, see John Stachel, "The Meaning of General Covariance: The Hole Story,"in John Earman et al., eds., *Philosophical Problems of the Internal and External World/Essays on the Philosophy of Adolf Grünbaum*

(Konstanz: Universitätsverlag/Pittsburgh: University of Pittsburgh Press, 1993), pp 129–160; and "Einstein's Search for General Covariance, 1912–1915," reference in note 2.

[13] By speaking of diffeomorphisms instead of coordinate transformations, I am modernizing the argument, but am not being unfaithful to the essence of Einstein's argument.

[14] Einstein to Arnold Sommerfeld, 15 July 1915, in Robert Schulmann *et al.,* eds. *The Collected Papers of Albert Einstein,* vol. 8, *The Berlin Years: Correspondence, 1914–1918 Part A: 1914–1917,* (Princeton University Press, 1998), Doc. 96, p.147.

[15] For a discussion of Hilbert's work during this period, see Leo Corry, "From Mie's Electromagnetic Theory of Matter to Hilbert's Unified Foundation of Physics," *Studies in History and Philosophy of Modern Physics* **30B,** 159-183 (1999); Tilman Sauer, "The Relativity of Discovery: Hilbert's First Note on the Foundations of Physics," *Archive for History of Exact Sciences* 53, 529–575 (1999); and Jürgen Renn and John Stachel, "Hilbert's Foundations of Physics: From a Theory of Everything to a Constituent of General Relativity," (Berlin, Max-Planck-Institut für Wissenschaftsgeschichte Preprint 118, 1999).

[16] For discussions of the reasons for his abandonment of the Einstein-Grossman equations and his return to general covariance, see John Stachel, "Einstein's Search for General Covariance," reference in note 2; John Norton, "How Einstein Found His Field Equations , 1912–1915," *Historical Studies in the Physical Sciences 14,* 253–316 (1984); Michel Janssen, "Rotation as the Nemesis of Einstein's Entwurf Theory," in Hubert Goenner *et al.* (eds), *The Expanding Worlds of General Relativity, Einstein Studies,* vol. 7, (Boston/Basel/Berlin, Birkäuser 1999), pp. 127–157.

[17] Albert Einstein, "Zur allgemeinen Relativitätstheorie," *Königlich Preußischen Akademie der Wissenschaften* (Berlin), *Sitzungberichte,* 778–786 (1915), "Zur allgemeinen Relativitätstheorie (Nachtrag), *ibid.,* 799–801 (1915).

[18] Albert Einstein, "Erklärung der Perihelbewegung des Merkur aus der allgemeinen Relativitätstheorie," *Königlich Preußischen Akademie der Wissenschaften* (Berlin), *Sitzungberichte,* 831–839 (1915).

[19] Albert Einstein, "Die Fieldgleichungen der Gravitation," *Königlich Preußischen Akademie der Wissenschaften* (Berlin) *Sitzungsberichte,* 844–847 (1915). The papers cited in notes 17, 18 and this note are reprinted in Martin J. Klein et al, eds. *The Collected Papers of Albert Einstein,* vol. 6, *The Berlin Years: Writings 1914-1917* (Princeton University Press, 1996), pp. 215–223, 226–228, 234–242, and 245–248.

[20] Albrecht Fölsing, *Albert Einstein: a biography* (Viking, New York, 1997), pp. 375–376. The translation has been slightly modified and is taken from ref. 1.

[21] David Hilbert, "Die Grundagen der Physik (Erste Mitteilung)," *Nachrichten von der Königl. Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse,* 395–407 (1915).

[22] The proofs are in the Handschriftenabteilung of the Staats- und Universitätsbibliothek Göttingen, Cod. Ms. D. Hilbert 634.

[23] Albert Einstein to David Hilbert, 18 November 1915, in Robert Schulmann *et al,* eds. *The Collected Papers of Albert Einstein,* vol. 8, *The Berlin Years: Correspondence, 1914–1918 Part A: 1914–1917,* (Princeton University Press, 1998), Doc. 148, pp. 201–202.

[24] Albert Einstein, “Research Notes on a Generalized Theory of Relativity,” in Martin J. Klein *et al.,* eds. *The Collected Papers of Albert Einstein,* vol. 4, *The Swiss Years: Writings 1912–1914,* (Princeton University Press, 1995), pp. 201–269 for a transcription, pp. 630–682 for a facsimile. For a brief review of its significance, see Jürgen Renn and Tilman Sauer,” Einsteins Züricher Notizbuch,” *Physikalische Blätter 52,* 865–872 (1996).

[25] Hilbert does not introduce a minus sign since he is working with one imaginary coordinate, and he writes (K) for what we now usually designate by (R).

[26] It is a sad reflection on the state of the mails today that in wartime Germany, mail between Berlin and Göttingen seems to have taken only a day or so.

[27] Reference in note 23.

[28] See the reference in note 15.

[29] Albert Einstein to Heinrich Zangger, 26 November 1915, in Robert Schulmann *et al.,* eds. *The Collected Papers of Albert Einstein,* vol. 8, *The Berlin Years: Correspondence, 1914–1918 Part A: 1914–1917,* (Princeton University Press, 1998), Doc. 152, pp. 204–205; citation from p. 205.

[30] References in notes 17, 18 and 19.

[31] David Hilbert, “Die Grundlagen der Physik (Zweite Mitteilung),” *Nachrichten von der Königl. Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse,* 53–76 (1917).

[32] It was reprinted, with extensive changes, in *Mathematische Annalen 92,* 1 (1924).

[33] It was not impossible to do this in the *Göttinger Nachrichten.* Related papers by Klein and Noether, for example, carry such revised datelines (see the paper by Renn and Stachel cited in note 15).

[34] Albert Einstein to David Hilbert, 20 December 1915 in Robert Schulmann *et al.,* eds. *The Collected Papers of Albert Einstein,* vol. 8, *The Berlin Years: Correspondence, 1914–1918 Part A: 1914–1917,* (Princeton University Press, 1998), Doc. 167, pp. 222.