EINSTEIN AND HILBERT:
THE CREATION OF GENERAL RELATIVITY *

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ABSTRACT

It took eight years after Einstein announced the basic physical ideas behind the relativistic gravity theory before the proper mathematical formulation of general relativity was mastered. The efforts of the greatest physicist and of the greatest mathematician of the time were involved and reached a breathtaking concentration during the last month of the work.

Recent controversy, raised by a much publicized 1997 reading of Hilbert’s proof-sheets of his article of November 1915, is also discussed.

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Introduction

Since the supergravity fashion and especially since the birth of superstrings a new science emerged which may be called “high energy mathematical physics”. One fad changes the other each going further away from accessible experiments and into mathematical models, ending up, at best, with the solution of an interesting problem in pure mathematics. The realization of the grand original design seems to be, decades later, nowhere in sight. For quite some time, though, the temptation for mathematical physicists (including leading mathematicians) was hard to resist. Yuri Manin characterized the situation as “an extreme romanticism of the theoretical high energy physics of the last quarter of our century”.

There does exist, on the other hand, a true example of a happy competition between mathematics and physics which has led to the most accomplished among the three claimed revolutions in our science in the first quarter of the twentieth century: the creation of the general theory of relativity. It illustrates how difficult it has been - even for the founding fathers of the theory - to fully understand and adopt such basic notions as reparametrization invariance, Bianchi identities, the concept of energy, which nowadays enter a student curriculum. The presence of a controversy - if not so much among the participants in the events, at least among the historians of science nearly a century later - could serve one good purpose: to attract a wider audience to this remarkable story.

1. Prologue: Einstein (and Grossmann): 1907–1915

Einstein seemed never happy with what he had achieved. He was not satisfied by the special principle of relativity because it did not incorporate accelerated motion. Since his student years he had absorbed with sympathy Ernst Mach criticism of the “monstrous [Newtonian] notion of absolute space”. (It was his lifelong friend Michele Besso who induced the 18 year old Einstein – back in 1897 – to read Mach’s “History of Mechanics”.)

In his recently reprinted address to the 1904 International Congress of Arts and Science St. Louis, *) as well as in his fundamental paper “Sur la dynamique de l’électron” which appeared in 1906, Poincaré already states the problem of modifying Newton’s gravity theory in order to make it consistent with relativity. The problems of gravity and of relativity of accelerated motion are combined in what Einstein will call 13 years later “the happiest thought in my life” (Pais 82, Chap.9, pp.178–179). Then, in 1907, while working on the review article “Über das Relativitätsprinzip und die aus demselben gezogenen Folgerungen” (“On the principle of relativity and its consequences”), Jahrbuch der

*) H. Poincaré, L’ état actuel et l’avenir de la physique mathématique, Bulletin des Sciences Mathématiques 28 (1904) 302-324; reprinted as: The present and future of mathematical physics, Bull. Amer. Math. Soc. 37 (2000) 25-38.
Radioaktivität und Elektronik 4 (1907) 411–461 (written, ironically, on the request of Johannes Stark who appears as a bitter enemy of relativity during the Nazi period), he has the idea that “for an observer falling freely from the roof of a house there exists – at least in his immediate surroundings – no gravitational field”. In his Kyoto lecture (cited by Pais) Einstein recalls: “I was sitting in a chair in the patent office at Bern when all of a sudden a thought occurred to me: ‘If a person falls freely he will not feel his own weight’. I was startled . . . ”

Thus the celebrated equivalence principle first appears just two years after the formulation of special relativity (although it was only so baptized by Einstein 5 years later). The 1907 paper does not stop at that. It contains a derivation of the gravitational red shift. Einstein also deduces the formula \[ c(\phi) = c\left(1 + \frac{\phi}{c^2}\right) \] for the velocity of light along the direction \( \xi \) of a constant gravitational field (the mass in the gravitational potential \( \phi \) being identified with the unit of mass so that \( \phi/c^2 \) appears to be dimensionless). He infers that *) “the light rays which do not run in the \( \xi \) direction are bent by the gravitational field”. As if all this was not enough for a first probe into relativistic gravity, Einstein wrote to his friend Konrad Habicht on Christmas 1907 (just 3 weeks after submitting the paper): “I hope to clear up the so–far unexplained secular change of the perihelion length of Mercury . . .” (Pais 82, p.182).

All three observational implications of general relativity were in the mental view of its creator already at this preliminary stage. Einstein’s genius is here manifest with all its flare as well as with its limitations. The limitations are most honestly described by Einstein himself. In his “Autobiographische Skizze” completed in March 1955, a month before his death, he deplors his attitudes towards advanced mathematics during his student years. (Maurice Solovin, Einstein’s close friend during the period just after he graduated from the ETH – the Zürich Polytechnic, remembers that “Einstein . . . often spoke against abusive use of mathematics in physics. Physics, he would say, is essentially a concrete and intuitive science.” “I do not believe in mathematics”, Einstein is reported to have affirmed before 1910 – see Pyenson 85, p.21, and references cited there.) Already in 1907 Einstein is striving for a generally covariant theory, but he is not aware that such a theory, the Riemannian geometry, has been created in the 19th century. The (local) equivalence principle purporting to generalize “the happiest thought” of 1907 to inhomogeneous fields is instrumental in Einstein’s tracing the road to identifying gravity with space time geometry. Yet, nowadays, a mathematically minded student of relativity feels embarrassed if he has to explain what such an “equivalence principle” does actually mean. The presence of a non–zero gravitational field strength is manifested by a non–zero curvature tensor and

*) In predicting the bending of light Einstein has a prominent predecessor. In the first “Query” to his “Opticks” Isaac Newton writes: “Do not Bodies act upon Light at a distance, and by their action bend its Rays; and is not this action strongest at the least distance?”
cannot be eliminated – not even locally – by a coordinate transformation – no matter what acceleration one chooses. (In order to make our point in a few words we are oversimplifying matters. A detailed treatment of Einstein’s principle of equivalence is contained in (Norton 86).)

To summarize: by Christmas 1907 Einstein had all physical consequences of the future gravity theory in his hands, yet, he had another eight years to go and to appeal for mathematicians’ help before arriving at the proper mathematical formulation of general relativity.

At first, though, Einstein behaved much like Michael Atiyah’s physicist who “not being able to solve a problem moves on to the next more difficult one”. For three and a half years, from 1908 to mid 1911, Einstein’s main preoccupation was quantum theory: light quanta, blackbody radiation. (In 1910 he also completed a paper on critical opalescence, his last major work on classical statistical physics.) Characteristically this was before the early (1913) work of Niels Bohr after which quantum theory started becoming popular. (Even later, in 1915, Robert Millikan, who spent 10 years to test Einstein’s prediction of the photoeffect will write: “Einstein’s photoelectric equation . . . appears in every case to predict exactly the observed results . . . Yet the semicorpuscular theory by which Einstein arrived at his equation seems at present wholly untenable.” – see Pais 82, p.357.) Einstein’s own appraisal of his 1909–1910 assault on the light quantum problem was not complimentary either. (In today’s perspective, though, he appears as a true pioneer in the quantum theory of those days - see, e.g., Sec.2 Einstein and the early quantum theory in M.J. Klein’s lectures “The Beginning of Quantum Theory” in History of Twentieth Century Physics, Proceedings of the International School of Physics “Enrico Fermi”, Course LVII, ed. by C. Weiner, Academic Press, New York and London 1977, pp.19-39.)

By June 1911, after a four month stay in Prague, Einstein is again on the general relativity track and on the bending of light. The next important breakthrough comes a year later, in August 1912 when Einstein is back in Zürich and is literally crying for help to his friend and fellow student from ETH: “Grossmann, you must help me or else I’ll go crazy!” (Since 1907 Marcel Grossmann was a professor of geometry in ETH.) As witnessed in Einstein’s correspondence (letter to L. Hopf of 16 August, 1912) at this time the two of them understood that gravity should be described not by a single scalar field (which Einstein related in previous publications with the variation of the velocity of light), but by the symmetric tensor metric field \( g_{\mu\nu}(x) \) which has 10 independent components. Grossmann quickly realized that the generally covariant formalism Einstein was looking for (equivalence of arbitrarily moving frames) was provided by Riemannian geometry. Yet, this was only the beginning of the hard work. Einstein was absorbed to a point at which he refused to talk about quantum theory anymore. On 29 October 1912 he wrote to Sommerfeld: “I assure you that with respect to quantum I have nothing new to say . . . I am now exclusively occupied with the problem of gravitation and I hope to master all
difficulties with the help of a friendly mathematician here. But one thing is certain: in all
my life I have labored not nearly as hard, and I have become imbued with great respect for
mathematics, the subtler part of which I had in my simple-mindedness regarded as pure
luxury until now. Compared with this problem the original relativity is a child’s play.”
(Mehra 73, p.93 and Pais 82, p.216).

It appears that in the subsequent months Einstein still trusts better his (un-
common!) physical intuition, rather than the mathematical wisdom. In their first joint
paper with the glorious title: “Entwurf einer verallgemeinerten Relativitätstheorie und
einer Theorie der Gravitation. I. Physikalischer Teil von Albert Einstein. II. Mathema-
tischer Teil von Marcel Grossmann” (Leipzig und Berlin, B.G. Teubner 1913; reprinted
with added “Bemerkungen” in Zeitschrift für Mathematik und Physik 62 (1914) 225–261).
Grossmann notes in the mathematical part that the Ricci tensor \( R_{\mu\nu} \) may be used for
the formulation of a generally covariant gravity theory - an important step towards the
ultimate formulation of the basic equation of general relativity. (As stressed in (Win 04)
Grossmann deserves more credit for this achievement than usually given.) But the authors
reject this possibility, since it allegedly violates “the physical requirements”. The crucial
mistake comes from Einstein’s “causality requirement”: the metric tensor \( g_{\mu\nu} \) should be
completely determined from the stress energy tensor. This is certainly not correct for the
true equations of general relativity,

\[
G_{\mu\nu} = \kappa T_{\mu\nu} \quad \text{where} \quad G_{\mu\nu} := R_{\mu\nu} - \frac{1}{2} R g_{\mu\nu},
\]

first reported by Hilbert in his paper submitted (to Nach. Ges. Wiss. Göttingen), 20
November 1915. Indeed, \( G_{\mu\nu} \) satisfies the Bianchi identities \( (G^\mu_{\nu};\nu = 0 \) in accord with
the (covariant) energy momentum conservation law. Hence, only six of its ten components
are independent, so that the \( g_{\mu\nu} \), far from being uniquely determined, depend on four
arbitrary functions. We now, sure, understand what it means. General covariance says
that the choice of coordinates is a matter of convention which should not affect physics. The
metric tensor much like the electromagnetic potential is not an observable. To determine
it one needs (on top of \( T_{\mu\nu} \)) four “coordinate conditions” corresponding to the gauge fixing
in electrodynamics. It would be too easy to criticize Einstein on the ground of knowledge
acquired by physics decades later. (The notion of gauge invariance (Eichinvarianz) first
appears six years later in Hermann Weyl’s “Gravitation und Elektrizität” (Sitzungsber. d.
Preuss. Akad. d. Wiss. (1918) 465-478)*) describing a hypothetical dilation symmetry -
in an early attempt to construct a unified field theory. It made its way to where it really
belongs – Maxwell–Dirac electrodynamics – again thanks to Weyl after another 10 years.
In 1912–1915 Einstein was well ahead of his time exploring, in the words of Pais, a “no
man’s land”.)

*) After over half a century Paul Dirac (Proc. Roy. Soc. A333 (1973) 403-418) still views
this paper of Weyl as “unrivalled by its simplicity and beauty”.

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In short, in the “Entwurf” (“Outline”) Einstein and Grossmann back down from general covariance and settle for a set of not quite geometric equations only invariant under linear coordinate transformations. Einstein is not happy with it. In August 1913 he writes to Lorentz: “The gravitational equations unfortunately do not have the property of general covariance . . . However, the whole faith in the theory rests on the conviction that acceleration of the reference system is equivalent to a gravitational field. Thus, if not all equations of the theory . . . admit transformations other than linear ones, then the theory contradicts its own starting point . . . all is then up in the air” (Pais 82, p.228).

In early 1914 Einstein and Adriaan Focker (who had just received his Ph.D under Lorentz) restored general covariance but at a high price. They derived the scalar equation \[ R = -\kappa T( R = R_{\nu}^{\nu}, T = T_{\nu}^{\nu}) \] assuming that the metric is conformally flat, \[ g_{\mu\nu} = \psi^2 \eta_{\mu\nu} \], i.e., returning, essentially, to the scalar theory of gravity. In October 1914 Einstein completes a 56-page long paper “Die formale Grundlage der allgemeinen Relativitätstheorie” which goes back to the Einstein–Grossmann theory. (Einstein’s strength is not in the mathematical formalism: his 1914 treatment of the covariance properties of the field equations, criticized by Levi-Civita, would hardly have impressed educated geometers - see Sect. 4 and footnote 124 of (Sau 99); it does not please its author either.) In the beginning of 1915 Einstein appears fed up (if not fully satisfied) with general relativity, and he goes ahead to do some experimental work with the Dutch physicist Wander de Haas (they discover a new effect: the torque of a suspended iron cylinder as a consequence of an abrupt magnetization).

2. Berlin – Göttingen (1915)*

On 29 November 1971 Eugene Wigner writes to Jagdish Mehra asking him one of those questions, people, who have come to know Wigner, can easily imagine: “. . . I was under the impression that, simultaneously with Einstein, Hilbert also found the now accepted equations of general relativity. Is this correct? If so, is there a reason no one seems to mention this now? I realize that the basic idea was due to Einstein but it is interesting that, even after the promulgation of the basic idea, it took a rather long time to find the correct equations incorporating that idea – even though both Einstein and Hilbert seem to have worked on it.” Mehra replies to Wigner within two weeks by a long letter and later publishes an 87-page paper on the subject (Mehra 73). But the real answer to Wigner’s question comes another five years later in an article by Earman and Glymour who have dug into the Einstein Papers at Princeton University.

David Hilbert, whose 23 “Honors Class” problems (Yan 02) occupy mathemati-

*) A well researched and lively account of Einstein’s Berlin years is provided in (Goen 05); (Reid 96) is a standard source for Hilbert’s life (1862-1943), most of which (since 1895) is spent in Göttingen.
cians throughout the 20th century, is, in his fifties (after Poincaré, 58, dies in 1912), the uncontested leader of the world of mathematics. Having published a (by now, classic) book, “Foundations of Geometry” Hilbert states his sixth problem (Paris, 1900): “To treat in the same manner, by means of axioms, those physical sciences in which already today mathematics plays an important part...”. His lifelong belief that every scientific problem can - and will - be solved is reflected in the words engraved on his tombstone in Göttingen: “Wir mussen wissen, wir werden wissen.” (“We must know, we shall know.”). Starting with 1912, after completing his book on linear integral equations, Hilbert’s main preoccupation becomes mathematical physics: the realization of the program encoded in his sixth problem. He thinks of unifying within the axiomatic approach the new electromagnetic theory of the electron, put forward in 1912 by Gustav Mie (1869-1957), with the Einstein-Grossmann theory (reported to the Göttingen Mathematical Society in 1913, shortly after its publication - see (Sau 99), Sect. 2.1). Hilbert tries to have Einstein visiting Göttingen (he invites him more than once - first in 1912) and this time he succeeds.

In late June - early July 1915 Einstein spends a week in Göttingen where (as he witnesses in a letter to Zangger of 7 July) he “gave six two–hour lectures there”. By all accounts he seems happy with the outcome: “To my great joy, I succeeded in convincing Hilbert and Klein completely” (E. to de Haas) “I am enthusiastic about Hilbert” (E. to Sommerfeld). The feelings appear to be mutual. Hilbert recommends Einstein for the third Bolyai Prize in 1915 for “the high mathematical spirit of his achievements” (the first and the second recipients of the Bolyai prize have been Poincaré and Hilbert – see Mehra 73). Nevertheless, the Göttingen discussions seem to have reinforced Einstein’s uneasiness about the lack of general covariance of his (and Grossmann’s) equations. He is reluctant (he writes to Sommerfeld in July 1915) to include his papers on general relativity in a new edition of “The Principle of Relativity”, “because none of the presentations to date is complete”. After the November race Einstein will state more precisely (in letters to friends) the grounds for his discontent with the old theory: (1) its restricted covariance did not include uniform rotations; (2) the precession of the perihelion of Mercury came out 18" instead of the observed 45" per century; (3) his proof of October 1914 of the uniqueness of the gravitational Hamiltonian is not correct.

In the meantime Einstein receives a letter by Sommerfeld (perhaps in late October 1915 – the letter is lost) from which he learns that he is not the only one dissatisfied with his 1914 theory. Hilbert also has objections to it and is working on his own on “Die Grundlagen der Physik” originally conceived as “Die Grundgleichungen /basic equations/ der Physik” - see (Sau 99) footnotes 73 and 90). Will Einstein let someone else, be it Hilbert himself, share with him the fruit of years of hard work and great inspiration? Not he! At 36, he can still fight. The Einstein papers reveal an unprecedented activity in November 1915.

Einstein submits four communications to the “Preussische Akademie der Wis-
senschaften”: on 4, 11, 18 and 25 November – no Thursday is skipped! These are not different parts of a larger work. The first, “Zur allgemeine Relativitäts-theorie” rejects his formulation of 1914 and proposes a new fundamental equation. The second, with the same title, rejects the first and starts anew. The fourth, “Die Feldgleichungen der Gravitation” rejects the first two and finally contains the right equations. It is like in a movie when the film is turned on a high speed. Nothing similar has happened either before or after in Einstein’s life.

But this is not all. Einstein only answers (the lost) Sommerfeld’s letter on 28 November (three days after his last talk at the Academy). “Don’t be angry with me” he writes “for only today answering your friendly and interesting letter. But last month I had one of the most exciting, most strenuous times of my life, also one of the most rewarding. I could not concentrate on writing”. Indeed, from late October to late November Einstein stops writing to any of his habitual addressees: Besso, Ehrenfest, Lorentz, . . . But he does write letters (or, rather, postcards). He only replaces all his regular correspondents by a single new one – Hilbert. Four postcards are preserved from Einstein to Hilbert dated 7, 12, 15, 18 November and two of the four Hilbert answers.

On 7 November Einstein sends to Hilbert the proofs of his November-four paper and in the accompanying card writes “I recognized four weeks ago that my earlier methods of proof were deceptive”. He alludes to the above mentioned letter of Sommerfeld which reports on Hilbert’s objections to the October 1914 paper; and closes by saying: “I am curious whether you will be well disposed towards this solution”.

Hilbert would have hardly been well disposed towards the new equation, since it assumes that the determinant of the metric tensor is a constant (-1) and is hence still not generally covariant. Probably, after having Hilbert’s criticism (which has been lost) Einstein opted on 11 November for the generally covariant equation

$$R_{\mu\nu} = \kappa T_{\mu\nu}$$

which Grossmann and he have rejected two years earlier. It only coincides, however, with the correct equation (1) if $T_{\mu\nu}$ (and hence also $R_{\mu\nu}$) is traceless. This is the case of Maxwell electrodynamics and Einstein speculates that it may be more general.

The next day, 12 November, Einstein sends a second postcard to Hilbert announcing that he had finally achieved generally covariant field equations. He also thanks Hilbert for his “kind letter” (which is lost). Hilbert replies on 14 November a long message on two postcards. He is excited about his own “axiomatic solution of your grand problem”. In a postscript Hilbert adds that his theory is “wholly distinct” from Einstein’s and invites Einstein to come to Göttingen and hear his lecture on the subject. The tone is cordial: Hilbert urges Einstein to come to Göttingen the day before the lecture and pass the night at Hilbert’s home. The next day, Monday, 15 November, Einstein already answers Hilbert’s
cards. (One cannot fail to notice how accurately the mail service is working in Germany in
the midst of the European war.) “The indications on your postcards lead to the greatest
expectations”. He apologizes for his inability to attend the lecture, since he is overtired
and bothered by stomach pains. Asks for a copy of the proofs of Hilbert’s paper. Appar-
ently, he does receive the requested copy within three days, because on 18 November, the
day of his third talk at the Academy, Einstein writes his fourth postcard: “The system [of
equations] given by you agrees – as far as I can see – exactly with what I found in recent
weeks and submitted to the Academy”. Then Einstein remarks that he has known about
Eq.(2) “for three years” but that he and Grossmann have rejected it on the grounds that
in the Newtonian limit they are not compatible with “Newton’s law” (meaning Poisson’s
field equation). Finally, Einstein informs Hilbert that he is finally explaining the advance
of the perihelion of Mercury from general relativity alone without the aid of any subsidiary
hypotheses.

Two remarks are in order.

First, it is not true that Hilbert’s Eq.(1) is equivalent to Einstein’s Eq.(2) of the
paper submitted to the Academy on 11 November. (It will be equivalent to the equation
Einstein is going to write a week later. It seems, however, that Einstein does have in mind
his Eq.(2) in this postcard since he is adding the priority claim that he knew it for three
years.) The two equations are only consistent with one another for $T(= T_{\nu\nu}) = 0$, the case
Einstein has been mostly interested in at the time.

Second, Einstein does derive the correct value for the advance of the perihelion
of Mercury in his third communication “Erklärung der Perihelbewegung des Merkur aus
der allgemeinen Relativitätstheorie” from his not exactly correct equation. This is possible
since he is actually solving the homogeneous equation (with $T_{\mu\nu} = 0$) in the post Newtonian
approximation (allowing for point singularities). - In seeing the physical implications of
the theory Einstein has no competitor.

The next day, Friday the 19th, Hilbert congratulates Einstein for having mastered
the perihelion problem and adds cheerfully: “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” (Pais
82, p.260).

On 20 November Hilbert presents to the Gesellschaft der Wissenschaften in
Göttingen his work. He derives the correct equations from the variational principle assum-
ing general covariance (we would say today reparametrization invariance) and a second
order equation for $g_{\mu\nu}$. He gives full credit to Einstein’s ideas. On the first page of his
article he writes: “Einstein . . . has brought forth profound thoughts and unique con-
ceptions, and has invented ingenious methods for dealing with them . . . Following the
axiomatic method, in fact from two simple axioms, I would like to propose a new system
of the basic equations of physics. They are of ideal beauty and I believe they solve the problems of Einstein and Mie at the same time”. In the published version Hilbert refers to all Einstein November papers. About the one of 25 November, submitted after his talk, he says: “It seems to me that [our] differential equations of gravitation are in agreement with the noble theory of general relativity proposed by Einstein in his later memoire”.

On 25 November Einstein proposes without derivation the equation

\[ R_{\mu\nu} = \kappa \left( T_{\mu\nu} - \frac{1}{2} T g_{\mu\nu} \right) \]

which is exactly equivalent to Hilbert’s Eq.(1), since they both imply \( R + \kappa T = 0 \). He chooses not to mention Hilbert’s name in the published paper. Later commentators have a hard time to understand what was Einstein’s argument at the time to include the trace term. Only Norton makes a well documented (59 pages long) case (including the study of a Zürich notebook of Einstein) for an independent Einstein’s road to the correct equations.

Aside: Today’s student will easily find the \(-\frac{1}{2} R\) term (or equivalently, the \(-\frac{1}{2} T\) term) using the Bianchi identity. The trouble was, Einstein did not know them. We should not be too hard on him on that account. Hilbert, too, does not know them: he derives four identities among the fields in his theory - anticipating, three years in advance, a special case of Noether’s theorem (Viz 94) (complemented with a stronger statement - see (Sau 99), Sect. 3.3 and footnote 120) but he conjectures erroneously that they will enable him to express the electromagnetic potential in terms of the gravitational field. He corrects his error in a later version of the paper.

Felix Klein who (in 1918) reduces the vanishing of the covariant divergence,

\[ \left( R^{\mu\nu} - \frac{1}{2} g^{\mu\nu} R \right)_{;\nu} = 0, \]

to Noether’s theorem as well, does not realize that it is a consequence of the Bianchi identities for the Riemann curvature tensor either. “Bianchi identities” are known before Bianchi to Aurel Voss (1880) and to Ricci (1889); Luigi Bianchi (a pupil of Klein’s!) rediscovers them in 1902.

3. Aftermath. Controversy among historians of science

In his speech on the occasion of Planck’s 60th birthdate (in 1918) Einstein talks about different categories of people that have devoted themselves to science. For some science is a sport which allows them to satisfy their pride or vanity. If the angel chases all such people from the temple of science, he continues, then the temple would remain almost empty, but Planck will be among the precious few who will remain.
The chronicle of the last month of the creation of general relativity demonstrates that the spirit of competition has not been alien to Einstein himself (as it was not to Leibniz and Newton). It is to the credit of both Hilbert and Einstein that their November 1915 rivalry did not grow into a public argument. Yet the outcome of the November events resulted in some uneasy feelings between the two men. On 20 December 1915, Einstein writes to Hilbert: “I want to take the opportunity to say something to you which is important to me. There has been a certain spell of coolness between us, the cause of which do not want to analyze. I have, to be sure, struggled against any resentment, and with complete success. I think of you once again with untroubled friendliness, and ask you to try to think of me in the same way. It is really a shame when two such real fellows [zwei wirkliche Kerle], whose work has taken them above the shaby world, give one another no pleasure”. (EG 78, p.306; a slightly different reading of the German original the reader will find in Pais 82, p.260.)

In his expository paper “Die Grundlage der allgemeinen Relativitätstheorie”, Annalen der Physik 49, 769-822 (received 20 March, 1916) Einstein already refers (albeit superficially) to Hilbert’s work. In May 1916 he gives a colloquium in Berlin on Hilbert’s paper. On that occasion he again writes to Hilbert asking him to explain his work (and complaining about its obscurity).

Hilbert’s appreciation of Einstein appears unequivocal. His biographer, (Reid 96), attributes to him the words: “Every boy in the streets of Göttingen understands more about four-dimensional geometry than Einstein. Yet, . . . Einstein did the work and not the mathematicians”.

The story does not end here, however: it is continued by the next generation of Einstein biographers and students of science history.

In 1997 a noteworthy addition to existing Einstein’s biographies, (FL 97), appeared in English, providing a nice complement to (Pais 82).

Summing up the decisive phase of his work on general relativity (FL 97) quotes Einstein’s letter to Heinrich Zangger (see also an earlier discussion of this letter in (Med 84)) which says: “Only one colleague truly understood it, and he now tries skilfully to `nostrify' it” [i.e. appropriate (‘make it ours’)]. We already know that the colleague in question was none other than David Hilbert. Fölsing justly refutes the accusation on the basis of available evidence.

Later the same year an article in the 14 November issue of Science, (CRS 97) made the news. This paper has a direct bearing on our topic. It points out that a lately discovered proof-sheet of Hilbert’s paper, with a publisher’s stamp of 6 December 1915, i.e. after the publication of the fourth of Einstein’s communications, involves substantial changes in the manuscript. The fact that Hilbert modified his paper after its submission
has been known before: as we noted he had cited all four Einstein’s November papers and had commented on the last one (submitted after his) in the published version of his November 20 article. The authors strive to attribute a great significance to the fact that the original text only involves the Hilbert action, while the field equations, which are derived from it, appear to be first inserted at the stage of the proofreading. Their attempt to support on this ground Einstein’s accusation of “nostrification” goes much too far. A calm, non-confrontational reaction was soon provided by a thorough study (Sau 99) of Hilbert’s route to the “Foundations of Physics” (see also the relatively even handed survey (Viz 01)). A direct critical comment on the unfounded accusations in (CRS 97), (Win 04), originally rejected by the editors of Science, *) finally appears in a more specialized journal (Win 04).**) 

The polemics is getting rough. A new book, (Wuensch 05), is advertised with a question mark: “Ein Kriminalfall in der Wissenschaftsgeschichte?” (“A criminal case in the history of science?”). The author asserts - already in the abstract to the book - that a missing fragment (also discussed in (Sau 99) and in (Win 04)) of the text on pages 7 and 8 of Hilbert’s proof-sheets, used in (CRS 97), contained “in all probability ... the explicit form of the field equations...” She further argues that “the passage ... was not excised originally but rather ... it must have been deliberately removed in more recent times in order to falsify the historical truth.”

It is quite clear from the November correspondence (and from recently discovered letters of Max Born to Hilbert of the fall of 1915 - see (Som 05)) - without appealing to criminal proceedings - that Hilbert’s competitive influence was crucial for Einstein’s acceptance of general covariance - in spite of his long time reservations and doubts. The analysis of the new evidence, detailed in (Sau 99) and in (Viz 01), indicates, on the other hand, that Hilbert appears to have been misled for a while, during the final race, in the

**) There seems to be a concerted effort to present the view of (CRS 97) as a final generally accepted “Decision”. In a little more than a 3-page long article on Hilbert (by J.J. O’Connor and E.F. Robertson), available at [http://www-groups.dcs.st-and.ac.uk/~history/Mathematicians/Hilbert](http://www-groups.dcs.st-and.ac.uk/~history/Mathematicians/Hilbert), the authors have found it necessary to devote a paragraph citing the (CRS 97) accusation of inappropriate behaviour against Hilbert, preceded by the words “the authors /of (CRS 97)/ show convincingly ...”. It is this type of overtly prejudiced attitude that provokes uncommonly angry reactions as (LMP 04) and gives credibility to extremist publications as (Bje 03).

**) In their “Response” (CRS 04) to this comment the authors of (CRS 97) continue to assert that taking the variational derivative of the Hilbert action (a routine 3-line exercise for an average graduate student) is something Hilbert was not able to do by himself in 1915, and even compare it with the calculation of “the one-billionth digit of π” (that would require a supercomputer and the dedication of someone - like the Chudnovsky brothers (see R. Preston, The Mountains of PI, New Yorker, March 2, 1992) - to program it).
opposite direction. After formulating the generally covariant action principle he appeals, in
his original text, to Einstein’s long-promoted “causality principle” and restricts the general
covariance by a (non-covariant formulation of) the energy momentum conservation law.
Only at the stage of proofreading does Hilbert suppress all extra conditions and recognize
the unqualified physical relevance of the covariant equation (1).

Einstein and Hilbert had the moral strength and wisdom - after a month of intense
competition, from which, in a final account, everybody (including science itself) profited
- to avoid a lifelong priority dispute (something in which Leibniz and Newton failed). It
would be a shame to subsequent generations of scientists and historians of science to try
to undo their achievement.

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