MAX-PLANCK-INSTITUT FÜR WISSENSCHAFTSGESCHICHTE
Max Planck Institute for the History of Science

2004

PREPRINT 271

Jürgen Renn, Matthias Schemmel, and Milena Wazeck

In the Shadow of the Relativity Revolution

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In the Shadow of the Relativity Revolution

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Preface

The four essays presented in this collection are based on lectures given at the Fifth and Sixth International Conferences on the History of General Relativity held at the University of Notre Dame, July 1999, and Amsterdam, June 2002, respectively. Authored by members of the Institute, they share the same approach while dealing with different aspects of Einstein's work. A volume based on these conferences will be published by Birkhäuser in volume 11 of the "Einstein Studies Series," edited by Jean Eisenstaedt and A. J. Kox.

Standing on the Shoulders of a Dwarf

General Relativity
A Triumph of Einstein and Grossmann's Erroneous "Entwurf" Theory

Jürgen Renn

1. General Relativity as an Heroic Achievement

When a single figure plays such a distinctive historical role as Einstein did in the emergence of general relativity it becomes almost unavoidable to tell this story, be it drama or comedy, in theatrical terms. The narratives of the history of general relativity thus refer to blunders and breakthroughs, to fatal errors and the dawning of truth. They characterize this history as the drama of a lonely hero, as a comedy of errors, or even as the irresistible rise of a slick opportunist. Dramatic narratives tend to emphasize the achievements of great heroes and to neglect the minor figures; they favor the mysticism of great ideas (or great failures) and usually ignore their tedious elaboration. In this form, apparent mistakes, while presenting little interest in themselves, provide the contrast that makes the victory of truth appear all the more triumphant.

As an example of such dramatic narratives, let me quote from Kip Thorne's fascinating account of recent developments in general relativity which, however, presents David Hilbert as the true hero of the story (Thorne 1994):[1]

> In autumn 1915, even as Einstein was struggling toward the right law, making mathematical mistake after mistake, Hilbert was mulling over the things he had learned from Einstein's summer visit to Göttingen. While he was on an autumn vacation on the island of Rugen in the Baltic the key idea came to him, and within a few weeks he had the right law—derived not by the arduous trial-and-error path of Einstein, but by an elegant, succinct mathematical route.

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[1] See also (Fölsing 1997).

The work undertaken on the history of general relativity, pursued by participants of a collaborative research project that began in Berlin in 1991, has involuntarily contributed to the thrill of this story:[2] the insight that Einstein already formulated the correct field equations in linearized form in 1912 and then discarded them, his similar treatment of the gravitational lensing effect in the same year (Renn, Sauer, and Stachel 1997), and our finding that Hilbert did not actually discover the field equations, but rather offered a non-covariant version of his theory only *after* the publication of Einstein's general relativity (Corry, Renn, and Stachel 1997) are all results of this joint research effort and add further dramatic turns to an already exciting plot.

The focus here is on what is generally seen as perhaps the most boring period of Einstein's search for a generalized theory of relativity, the time between spring 1913 and fall 1915, in which he firmly stuck to the erroneous "Entwurf" theory he published together with Marcel Grossmann before the end of June 1913 (Einstein and Grossmann 1913). According to the dramatic narratives of the emergence of general relativity, this period was one of stagnation. It was the calm interval between two major storms, Einstein's tragic struggle with and eventual rejection of generally covariant field equations in the winter of 1912/13, and the shocking revelation of fatal errors in the "Entwurf" theory that led immediately to its demise and then to a triumphant, if gradual, return to generally covariant field equations in the fall of 1915.

Based on the results of a joint research effort and an alternative approach to the history of science, this period will be presented here from a new perspective. From the point of view of an historical epistemology, the apparent stagnation between 1913 and 1915 can be considered a period in which new knowledge was assimilated to a conceptual structure still rooted in classical physics. As a result of this assimilation of knowledge, this conceptual structure became richer, both in terms of an increasingly extended network of conclusions that it made possible, and in terms of new opportunities for ambiguities and internal conflicts within this network. It was this gradual process of enrichment that eventually created the preconditions for a reflection on the accumulated knowledge which, in turn, induced a reorganization of the original knowledge structure. The enrichment of a given conceptual structure by the assimilation of new knowledge and the subsequent reflective reorganization of this enriched structure are the two fundamental

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[2] For publications representing the output of this joint research project see, e.g. (Renn and Sauer 1999). For a comprehensive publication see (Renn forthcoming).

cognitive processes which explain the apparent paradox that the preconditions for the formulation of general relativity, which matured under the guidance of a theory that is actually incompatible with it.

The results achieved on the basis of the “Entwurf” theory should therefore perhaps not be understood as so many steps in the wrong direction, whereupon it appears that their only function was to make the deviation from the truth evident, but rather as instruments for accumulating and giving new order to this knowledge. It obviously makes little sense to consider one of these processes as being more central than the other since both are essential to the development of scientific knowledge.

This perspective on the genesis of general relativity, as flowing “out of the spirit of the ‘Entwurf’ theory,” also leads to a new evaluation of what are usually cast as the stepchildren in the heroic narratives of the history of science: namely the “erroneous” approaches and theories, the boring periods of tedious elaboration of such theories, and the faceless minions in their service. It will become clear from my account that it was precisely the insistence with which Einstein laboured to plug the holes of the erroneous “Entwurf” theory that made his approach so much more successful than Hilbert’s. It will hopefully also become clear that the inconspicuous contributions to this theory by Grossmann, Bernays, and Besso were crucial in overturning it.

This counter-story proceeds in five acts. The next act attempts to destroy four legends on the history of general relativity; a breakthrough in late 1915, a pitfall in early 1913, a period of stagnation between 1913 and 1915, and the almost simultaneous discovery of general relativity by Einstein and Hilbert.

2. The Legend of a Breakthrough in Late 1915

After more than two years of intensive work on his “Entwurf” theory, Einstein suddenly abandoned this theory on the 4th of November 1915 with the publication of a short paper in the *Sitzungsberichte* of the Prussian Academy (Einstein 1915). In this and subsequent papers, as well as in his correspondence, Einstein himself gave the reasons for his abandoning of the “Entwurf” theory (CPAE 8, Doc. 153). The “Entwurf” theory could not explain the perihelion shift of Mercury, the earliest astronomical touchstone of general relativity, it did not allow treatment of a rotating system as being equivalent to the state of rest, and hence did not satisfy Einstein's Machian heuristics, and finally, a flaw was discovered in the derivation of the theory.

From the point of view of later general relativity, each of these three arguments seems to represent a major blunder that in itself would have sufficed to reject the "Entwurf" theory. Accordingly, historians of science are disputing which of these arguments was first or decisive in leading to the demise of this theory. On the other hand, they tend to leave unquestioned the assumption that it must have been one of these three or perhaps a fourth stumbling block that led to its downfall. This assumption fits well with the philosophical idea of progress being due to falsification and also to the historical topos of a dramatic turn in early November 1915, initiating the true birth of general relativity.

A closer look at the historical evidence, however, makes this assumption doubtful. Indeed, it can be shown that the "Entwurf" theory survived *all* the blunders listed above. As has become clear from research notes of Einstein and Besso, they knew at least from mid-1913 that the "Entwurf" theory failed to explain the perihelion shift of Mercury.[3] Recently discovered additional notes documenting the Einstein-Besso collaboration in 1913-14 show that Besso warned Einstein in August 1913 that the Minkowski metric in rotating coordinates is not a solution to the "Entwurf" field equations. Einstein seems to have accepted this conclusion for a while but thought in early 1914 that he had found an argument showing that this metric had to be a solution (CPAE 8, Doc. 47). Besso questioned this result (CPAE 8, Doc. 516) but Einstein did not listen.[4] Furthermore, when Einstein found out, around mid-October 1915, that his mathematical derivation of the "Entwurf" theory did not work, he nevertheless continued initially to stick to this theory as is made evident by a new demonstration he sent to H.A. Lorentz (CPAE 8, Doc. 129). The stubbornness with which Einstein held on to the "Entwurf" theory is the same characteristic that guided his entire search for a relativistic theory of gravitation. But, in the face of so many counter arguments, for which reasons did he cling so stubbornly to the "Entwurf" theory and what caused him finally to change his mind?

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[3] See (CPAE 5, Doc. 14) and for a historical discussion, see (Earman and Janssen 1993).

[4] See (Janssen forthcoming, sec. 3) for a detailed discussion of this episode.

In order to answer these questions, a short review of how Einstein reacted to each of the three "Entwurf" theory problems listed above may be appropriate. This is most easily done for the problem of the perihelion shift of Mercury. The same research notes containing Einstein and Besso's calculation of the perihelion shift of Mercury on the basis of the "Entwurf" theory also document them checking whether its main competitor, Nordström's theory of gravitation, would yield the correct result—which turned out not to be the case. Mercury's perihelion shift was thus not a criterion for choosing between the alternative theories available (CPAE 5, Doc. 14).

The situation is more complicated for the question of rotation. The Machian idea that the inertial effects in a rotating system may actually be due to the interaction with distant masses had been an important element of Einstein's heuristics. It motivated his search for a theory with a generalized relativity principle in which a rotating frame of reference can be considered as being equivalent to an inertial frame with gravito-inertial forces. By mid-1913 he knew, however, that the Minkowski metric in rotating coordinates is *not* a solution to the "Entwurf" field equations, and that therefore the state of rotation cannot be considered as being equivalent to the state of rest. Nevertheless, on the basis of general considerations in the course of his further elaboration of the "Entwurf" theory in 1914, Einstein convinced himself that this theory did, after all, comply with his Machian heuristics (CPAE 5, Doc. 514). He believed that he had actually reached the goal of a generally relativistic theory of gravitation in spite of the fact that the "Entwurf" field equations are not generally covariant. In fact, he interpreted the conditions on the covariance properties of the theory that he had meanwhile identified *not* as restrictions on possible solutions for the metric tensor but only as restrictions of the *coordinate systems* for representing a given solution (CPAE 8, Doc. 47, p. 80). Influenced by these general considerations, Einstein tended to forget his earlier finding that the Minkowski metric in rotating coordinates is not a solution of the "Entwurf" field equations and rediscovered this fact only in September 1915 (Earman and Janssen 1993). The fact that this "oversight" did not constitute a sufficient reason for abandoning the "Entwurf" theory is made evident by his development of a new derivation of the theory in October of the same year (CPAE 8, Doc. 129).

The third problem with which the "Entwurf" theory was confronted was the flaw in its derivation from general principles. In the winter of 1912/13, Einstein had developed the theory, jointly with Marcel Grossmann, by starting from a cautious generalization of Newtonian gravitation theory and of a special relativistic expression for energy-momentum conservation. We have called this strategy, along which Einstein hoped to eventually reach an implementation of his Machian heuristics—without ever losing touch with the secure knowledge of classical and special-relativistic physics—his "physical strategy." He turned to this strategy after having first followed what we have called his complementary "mathematical strategy," which started by immediately implementing his Machian heuristics in terms of the absolute differential calculus, and then aimed at recovering, within this framework, a representation of the familiar knowledge on gravitation and energy-momentum conservation.

After the physical strategy had led Einstein to the formulation of the field equations of the "Entwurf" theory, it was natural for him to turn around and attempt to derive these equations following the mathematical strategy, which is precisely what he undertook in 1914. In a 1914 review paper, he published a lengthy and complicated demonstration in which the "Entwurf" field equations were derived from a general variational principle without explicitly introducing the requirement that the resulting theory should incorporate the classical knowledge on gravitation (CPAE 6, Doc. 9).

In October 1915, however, Einstein discovered that this demonstration actually does not uniquely determine the "Entwurf" field equations, but instead only provides a general mathematical framework for formulating a theory of gravitation. While he was naturally disappointed by this discovery, it also clearly did not represent a reason for abandoning the "Entwurf" theory. After all, the flaw in the 1914 demonstration did not affect the earlier justification of the theory in Einstein and Grossmann's original paper. Einstein even found a way of repairing his 1914 demonstration by supplementing it with an assumption representing the classical knowledge on gravitation.

In summary, all three objections to the "Entwurf" theory, which in hindsight appeared to mark its decline, if not its demise, emerge, on closer inspection, as failures only of the more ambitious and more problematic parts of Einstein's heuristics. In particular, these parts included his goal to find an astronomical confirmation of his new theory of gravitation in observations already available, his hope to realize the Machian idea of conceiving rotation as rest, and his expectation that the "Entwurf" theory could also be derived through mathematical strategy. On the other hand, these objections did not touch upon what had been the firm foundation of the "Entwurf" theory from the beginning: its roots in the knowledge of classical and special relativistic physics. It is therefore clear that the abandonment of the “Entwurf” theory in early November 1915 is not properly characterized as a “breakthrough” in the commonly accepted sense: that is, the demise of a faulty theory followed by the gradual dawning of the correct one. Before we return to the question of what it was that eventually changed Einstein’s mind on the “Entwurf” theory, we first have to tackle another legend on the history of general relativity: the legend of a pitfall in early 1913.

3. The Legend of a Pitfall in Early 1913

The legend of a pitfall in early 1913 is structurally related to that of a breakthrough in late 1915. While the previous legend conveys the elimination of errors, this one imparts their introduction. Even before Einstein’s calculations in the Zurich notebook (CPAE 4, Doc. 10) had been reconstructed by the members of the project mentioned previously, it had long been known that as early as 1912/13 Einstein had come close to formulating the final field equations of general relativity, at least for the source-free case. The analysis of the Zurich notebook has made the situation even more dramatic because it revealed the existence of an entry representing the linearized form of the definitive full gravitational field equations. It thus seems that Einstein must have been detracted from the correct path by some error, for otherwise he would have preserved these field equations instead of rejecting them in favor of those of the “Entwurf” theory. Several hypotheses have been advanced concerning the nature of this “error.”

From remarks in the 1913 “Entwurf” paper as well as from later recollections by Einstein it was clear that he must have encountered a problem with recovering Newtonian gravitation theory from a relativistic gravitation theory based on the Riemann tensor.[5] In fact, when one forms the Ricci tensor, which represents a natural candidate for the left-hand side of gravitational field equations, one finds that, for weak static gravitational fields, it does not reduce to a form that immediately lends itself to a recovery of the Newtonian limit. From a page of the Zurich notebook we know that Einstein was indeed disappointed to find what he called “disturbing terms,” in addition to the one from which the Newtonian limit can be derived (CPAE 4, Doc. 10, p. 233). In the modern understanding of general relativity, one can eliminate these additional terms by choosing an appropriate coordinate condition. What is therefore more plausible than to assume

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[5] See, also for the following, the pioneering studies of John Norton (Norton 1989) and John Stachel (Stachel 1989b).

that Einstein was, in 1912/13, not yet aware of the possibility of picking an appropriate coordinate condition allowing the transition to the Newtonian limit – at least if one admits that Einstein could have been guilty of such a trivial error? A glance at the Zurich notebook shows, however, that this explanation cannot work because precisely the coordinate condition that we would introduce today, the harmonic condition, appears only a few pages later (CPAE 4, Doc. 10, pp. 244–245).[6]

Which other "error" is then responsible for Einstein's publication of the "Entwurf" theory, other than a theory based on the Riemann tensor? There can be no doubt that, from a modern point of view, Einstein's thoughts on gravitation during this period were plagued by "errors." He assumed, for instance, that for weak gravitational fields the metric tensor becomes spatially flat and that it can be represented by a diagonal matrix with only one variable component, corresponding to the classical gravitational potential. He was convinced that the Newtonian limit could only be attained for weak static fields of this type.[7] This was a plausible assumption for Einstein for a number of reasons and hence offered historians the opportunity to accuse him of a non-trivial error since, in fact, the Newtonian limit also works fine with off-diagonal terms in the metric tensor because these have no effect on the equation of motion that describes the relevant weak-field effects. Were Einstein's faulty expectations concerning the Newtonian limit the reason why he discarded all candidates for the left-hand side of the field equations that were based on the Riemann tensor and decided in favor of the "Entwurf" theory instead?

For all we know on the basis of the reconstruction of the Zurich notebook, problems with the Newtonian limit were indeed the reason why in 1912 he rejected field equations based on the Einstein tensor. He realized that the trace term of this tensor would give rise to weak static fields incompatible with his expectations. We also know, however, that these expectations cannot have been the reason why he discarded other candidate gravitation tensors based on the Riemann tensor.

In particular, in the Zurich notebook, Einstein examined a tensor, covariant under unimodular coordinate transformations, for which he did not encounter this problem (CPAE 4, Doc. 10, pp. 253–254). And indeed, it was this tensor that constitutes the basis for the new gravitation theory

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[6] This was first noted in (Norton 1989).

[7] For a historical discussion, see (Norton 1989;Stachel 1989b).

with which Einstein replaced the “Entwurf” theory in early November 1915— it has therefore been dubbed the “November tensor.” At this point he had not yet abandoned his expectations concerning the Newtonian limit and obviously found the November tensor in agreement with them. But why then did he discard this candidate in the winter of 1912/13? Did this rejection involve another fatal error that as yet has not been recognized?

Again, various hypotheses have been proposed to explain this apparent “pitfall.” The coordinate condition with the help of which the Newtonian limit of the November tensor is attained turns out to be incompatible with the Minkowski metric in rotating coordinates (Norton 1989). Was this the reason why Einstein rejected this candidate? But why on earth should he have expected that the same coordinate condition could be employed for deriving both the Newtonian limit and the Minkowski metric in rotating coordinates, *if* he really understood coordinate conditions in a modern sense as the freedom to choose coordinates appropriate to a particular physical situation? And what if, in the end, he did *not* understand coordinate conditions in this way? After years of pondering the reasons for Einstein’s rejection of the November tensor, this question seems to have brought us back to square one, namely the hypothesis about Einstein’s ignorance of coordinate conditions in the modern sense.

It therefore comes almost as a shock to realize from indications in the Zurich notebook that Einstein’s understanding of coordinate conditions was indeed different from the modern one. Why else should he have applied coordinate transformations to a coordinate condition, as he did with the condition reducing the November tensor to a form appropriate for the Newtonian limit (CPAE 4, Doc. 10, pp. 252–253)? From a modern point of view this makes no sense.

But which error was it that induced Einstein to perform this strange operation? Did he see coordinate conditions as a set of equations on the same level as the field equations? They would thus guarantee in all admissible coordinate systems that the field equations keep the form that Einstein had recognized as being appropriate for obtaining the Newtonian limit—without, in the words he uses in his notebook, the “disturbing terms.” But is this not just another way of simply incriminating Einstein of being ignorant of the freedom to choose a coordinate system in a generally covariant theory?

Perhaps there is some deeper error involved here, one that cannot simply be identified in the calculations of the notebook because it is more of a conceptual, if not metaphysical nature. What if Einstein had been guilty of believing in the famous hole argument at the time of the Zurich notebook, and, if not guilty of that, then at least of the commitment to the physical reality of coordinate systems underlying this argument?[8]

The evidence available makes it, in my view, implausible that this was indeed Einstein's pitfall in early 1913. If he committed an error conceptually close to the hole argument then it becomes incomprehensible why, as the historical documents indicate, Einstein only formulated this argument as late as summer 1913, and from then on regarded it as the life belt of the "Entwurf" theory, while, before that, he considered its lack of being generally covariant as a shameful dark spot. The recently found document mentioned earlier offers additional documentary evidence suggesting that it was unlikely that he committed such an error. This document, written by Michele Besso and dated 28 August 1913, contains what probably represents the record of an exchange with Einstein and shows the hole argument in *statu nascendi*—without the hole (Janssen forthcoming). The basis of the argument against general covariance is the construction of distinct solutions of the field equations in the same coordinate system—in contradiction to the requirement of uniqueness. If such a construction was available at the end of August 1913 and could be used as an argument against general covariance, why should Einstein not have used it before as a defense of the "Entwurf" theory? In fact, however, he celebrated the discovery of the hole argument in his correspondence later in the year as a new and important achievement.[9]

In summary, the "hole-argument error" was not the original sin, marking the death of the November tensor and the birth of the "Entwurf" theory. More generally speaking, it seems that searching for the fatal error supposedly responsible for the pitfall of early 1913 may be something like the hunt for the white elephant in Mark Twain's famous story: despite successful reports of detectives claiming to have seen the missing elephant all over the country, and in spite of announcements that they were ready to capture it, the poor beast had been lying dead in the cellar of the New York Police headquarters since the beginning of the chase.[10]

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[8] This interpretation has been developed by John Norton in the context of our collaboration and will be expounded in detail in (Norton forthcoming). For discussions of Einstein's Hole Argument see, (Stachel 1989a).

[9] See Einstein to Ludwig Hopf, 2 November 1913 (CPAE 5, Doc. 480), Einstein to Paul Ehrenfest, before 7 November 1913 (CPAE 5, Doc. 481); Einstein to Paul Ehrenfest, second half of November 1913 (CPAE 5, Doc. 484); Einstein to Paul Ehrenfest, second half of November 1913 (CPAE 5, Doc. 484), p. 568.

4. The Legend of a Period of Stagnation between 1913 and 1915

We may now take a leaf from Twain's book and turn away from where the action is apparently taking place and instead take a closer look at an area where apparently nothing is happening: the supposed period of stagnation between early 1913 and late 1915. The traditional picture of this period can, with little exaggeration, be summarized in a single sentence: Einstein wasted his time elaborating the erroneous "Entwurf" theory and invented misleading arguments to support it. How does this picture change if we assume that neither the November tensor nor the "Entwurf" theory were ever definitively rejected in this period? The supposed period of stagnation would then become, objectively, a period of contest between two rival theories, even though, during this period, the contest did not surface openly. While it is clear which contender was, for Einstein, the stronger candidate in the beginning and which was stronger in the end, the question of what changed this balance of power becomes the decisive one and can only be answered by reconsidering what happened during this period.

From our reconstruction of the calculations in the Zurich notebook, Einstein's criteria for comparing candidate gravitational tensors have become clear. He checked whether the Newtonian limit could be obtained from them, whether they allow for energy-momentum conservation, and whether and to which extent they imply a generalization of the relativity principle (Renn and Sauer 1999). These heuristic guidelines did not, however, function like knockout criteria since their precise expression depended on the formalism used and on the degree of its elaboration. It was hence necessary for Einstein to check, on the concrete level of his calculations, whether his various criteria were compatible with each other, or whether, for instance, one had to be restricted or modified in order to allow for the implementation of the other. What was the situation in this respect for the November tensor in the Zurich notebook? It apparently allowed for an extension of the relativity principle since the November tensor is covariant under unimodular coordinate transformations. The requirement of energy-momentum conservation turned out, on the level of the weak field approximation, to be compatible with and even equivalent to the coordinate condition necessary for obtaining the Newtonian limit: a test which previous candidates had failed.

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10 See (Renn, Damerow, and Rieger 2001;Twain 1882).

In spite of this positive record, however, some problems remained. For instance, does the requirement of energy-momentum conservation imply a restriction on the admissible coordinate transformations, as Einstein expected on the basis of earlier experiences? In the weak field approximation, this could be easily checked by exploring the transformational behavior of the expression which represents energy-momentum conservation and also the coordinate condition for the Newtonian limit (CPAE 4, Doc. 10, pp. 252–253). Here then is a plausible explanation for the curious fact that Einstein seems to have explored the behavior of the coordinate condition for the November tensor under coordinate transformations. What he actually explored was the transformational behavior of an equation implied by energy-momentum conservation, a procedure that is also familiar from his work on the "Entwurf" theory (CPAE 6, Doc. 2 and Doc. 9). The outcome of this exploration was not very promising since it indicated that not even simple standard cases were included in the class of admissible coordinate transformations.

Naturally, however, a calculation in the weak field limit must remain inconclusive so that what Einstein really needed to do at some point was to formulate energy-momentum conservation for the full November tensor field equations. But, as his calculations in the Zurich notebook indicate, in view of its technical challenges he did not actually pursue this task. Eventually, instead of deriving an expression for energy-momentum conservation from the field equations, he turned around and took such an expression as the starting point for his search for appropriate field equations. Following this strategy, he finally arrived at the "Entwurf" field equations. This reconstruction is confirmed by Einstein's later recollections referring to difficulties with establishing energy-momentum conservation for candidates based on the Riemann tensor, recollections which until now found no place in the reconstruction of Einstein's discovery of the field equations.[11]

But let us return to the supposed period of stagnation. On closer inspection, it turns out that practically all the technical problems Einstein had encountered in the Zurich notebook with candidates derived from the Riemann tensor were actually resolved in this period, in the course of his examination of problems associated with the "Entwurf" theory. In order to deal with the issue of its unclear transformational properties, for instance, Einstein and Grossmann developed, at the suggestion of the mathematician Paul Bernays, a variational formalism for this theory

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[11] See, e.g., Einstein to Michele Besso, 10 December 1915 (CPAE 4, Doc. 14, p. 392) and Einstein to H. A. Lorentz, 1 January 1916.

(CPAE 6, Doc. 2 and Doc. 9). As a by-product, this variational formalism made it possible to derive an expression for energy-momentum conservation for any given Lagrangian and hence also for a theory based on the November tensor, provided that it can be reformulated in terms of a Lagrangian formalism.

But there was more. When Einstein and Besso calculated the perihelion shift for the “Entwurf” theory, Besso found that only the 4-4 component of the metric tensor for a weak static field is relevant in the equations of motion and made a note to that effect on the back of a letter from Guye to Einstein, dated 31 May 1913.[12] Besso thus effectively removed *the* major stumbling block that had prevented Einstein in 1912/13 from accepting the correct field equations of general relativity. If our so-called “period of stagnation” had any heroes, then their names were Grossmann, Bernays, and Besso.

With so many new tools at his disposal, why did Einstein not immediately turn back to the candidates based on the Riemann tensor? This was simply because the open contest between his candidates was temporarily suspended. It was a matter of perspective. With the publication of the “Entwurf” theory in early 1913, he had concluded an exploratory phase and entered a defensive phase of his work. Only when problems began to accumulate for the “Entwurf” theory did his perspective change causing him to switch back to the explorative stance. The effect of these problems was hence not to refute the “Entwurf” theory, but to trigger a process of reflection in which the new technical possibilities could now be brought to bear on a reevaluation of the candidates that had earlier been excluded from the contest. The suddenness of the apparent breakthrough of late 1915 was hence not induced by new factual insights that somehow popped up like a Jack-in-the-box, but was generated by a process of reflection on results that had accumulated over the past two years.

5. The Legend of Hilbert’s Discovery of the Field Equations

In the introduction it was claimed that the genesis of general relativity can be described as the result of a double process, an assimilation of knowledge to a structure largely shaped by ideas from classical physics and the subsequent reflective reorganization of this structure. But before

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[12] This is documented by a page in the Einstein-Besso manuscript, written by Michele Besso on the back of a letter to Einstein; the page can be dated to June 1913 when both worked together in Zurich (CPAE 4, Doc. 14, p. 392).

this hypothesis may become acceptable as explanatory account of a scientific revolution, we must, in conclusion, tackle yet another legend, that of Hilbert's almost simultaneous discovery of the field equations as described in the beginning. If it were indeed true that there was a royal road to general relativity, paved by superior mathematical competence, then whatever the period of stagnation might have meant for Einstein's achievements, *sub specie eternitatis* it would be nothing but an unnecessary detour.

Recent findings (based on an analysis of the proofs of Hilbert's first paper) have shown that Hilbert did not actually anticipate Einstein in finding the field equations of general relativity.[13] But what is much more important, the theory which Hilbert expounded in his proofs is remarkably similar in structure to Einstein's "Entwurf" theory. In particular, both in the "Entwurf" theory and in the proof version of Hilbert's theory, covariance properties are determined by the requirement of energy-momentum conservation. But while Hilbert's theory may be mathematically more sophisticated, Einstein's "Entwurf" theory turns out to be a much more realistic theory of gravitation. Its notion of energy-momentum, for instance, is based on special-relativistic continuum dynamics, while Hilbert's notion of energy merely represents an attempt to establish a connection with Mie's speculative theory of matter. The "Entwurf" theory was supported by the classical knowledge on gravitation by way of its Newtonian limit, while this question was not even tackled by Hilbert. In short, as far as its support by the available physical knowledge is concerned, Hilbert's theory is at best comparable to some of the early candidates in the Zurich notebook that Einstein decided not to publish.

Einstein's elaboration of his "Entwurf" theory, on the other hand, not only extended the formalism and hence the network of possible conclusions but also augmented the occasions for confronting these conclusions with the physical knowledge incorporated into the theory. The tensions thus created during the so-called stagnation phase gave Einstein the opportunity to reflect upon a reorganization of his theory, while for Hilbert, as the subsequent revisions of his theory testify, a similar tension was created not by internal conflicts but by the challenge with which Einstein's results confronted his framework. Rather than representing a detour, the stagnation period was hence precisely what justifies the claim that it was Einstein and his collaborators and not Hilbert who founded general relativity.

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[13] See (Corry, Renn, and Stachel 1997) and for a detailed account (Renn and Sauer 1999).

Acknowledgements

This paper was presented at the Fifth International Conference on the History and Foundations of General Relativity held at the University of Notre Dame, July 8-11, 1999. As mentioned in the beginning, it is based on the results of a joint research project with Michel Janssen, John Norton, John Stachel, and Tilman Sauer. In addition it has substantially benefited from insights, comments, and advice from Michel Janssen, Matthias Schemmel, and Peter Damerow.

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Before the Riemann Tensor

THE EMERGENCE OF EINSTEIN'S DOUBLE STRATEGY

Jürgen Renn

1. The Paradox of General Relativity

This paper represents the third in a series based on joint work with Michel Janssen, John Norton, Tilman Sauer, and John Stachel on the genesis of general relativity as documented by the Zurich Notebook and other sources. The first paper (Renn and Sauer 1999) identifies the heuristics guiding Einstein's search for the gravitational field equation between 1912 and 1915, in particular what we call his "double strategy." The second paper, also published in this volume, analyzes the sense in which Einstein's work between 1913 and 1915 on the erroneous "Entwurf" theory created, paradoxically, the preconditions for formulating his final theory. This paper returns to the beginning of this development and will show how the crucial heuristic strategy that guided Einstein's work throughout those years emerged in the first place. I claim that this strategy actually took on its specific form before Einstein encountered the most important mathematical tool in his search for the field equation: the Riemann tensor. In order to substantiate this claim, it is necessary to examine Einstein's heuristics once more, and interpret it from a point of view that owes much to ongoing research at the Max Planck Institute for the History of Science on an historical epistemology of scientific knowledge, as well as to a theoretical framework originally developed with Tilman Sauer, and to an intensive collaboration with Michel Janssen on the early part of the Zurich notebook (Renn forthcoming).

1.1 The Epistemological Framework

From the perspective of an historical epistemology, the genesis of general relativity confronts us with a paradox: How was it possible for Einstein to formulate a theory that turned out to be amazingly suited to interpreting empirical knowledge which was unknown at the time of its creation (such as the expansion of the universe) and that involved substantial conceptual novelties (such as understanding gravitation as the curvature of space-time) and all this on the basis of knowledge still anchored in the older conceptual foundation of classical physics? Such a development can hardly be described in terms of formal logic. In fact, if the knowledge on gravitation relevant to the emergence of general relativity were structured as a deductive system in the sense of formal logic, it would suffice for one of the premises to be wrong for the entire building to collapse. But as Einstein's investigative pathway strikingly illustrates, in contrast to the inferences of formal logic, scientific conclusions can be corrected. Even when knowledge is subjected to major restructuring, science never starts from scratch as would be the case for a system structured according to formal logic and whose premises are no longer acceptable. In fact, scientific knowledge and also, of course, what can be termed the "shared knowledge" of large domains of human experience, transmitted over generations, is not simply lost when scientific theories are restructured. In the case at hand, it was mainly the shared knowledge of classical physics that needed to be preserved and exploited in a conceptual revolution that gave rise to a relativistic theory of gravitation whose far-reaching physical consequences, which eventually changed our understanding of the universe, were largely unknown at the time of the theory's creation.

What is therefore required in order to adequately describe the cognitive dynamics of the genesis of general relativity is an account of the underlying shared knowledge that illuminates, first, how past experiences can shape inferences about a matter on which only insufficient information is available, and, second, how conclusions can be corrected without always having to start from scratch. In order to satisfactorily account for these features in the case of Einstein's search for the gravitational field equation, it has turned out to be useful to introduce concepts from cognitive science, in particular the concepts of "mental model" and "frame."[1] A mental model is conceived here as a knowledge structure that possesses slots that can be filled not only with empirically gained information but also with "default assumptions" resulting from prior experience. These default assumptions can be substituted by updated information so that inferences based on the

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[1] These concepts have been adopted from cognitive science (see e.g. (Davies 1984; Gentner and Stevens 1983; Minsky 1987, and also Damerow 1996) to historical research on both individual and shared scientific knowledge in the ongoing work at the Max Planck Institute for the History of Science (see the Institute's Research Report 2000–2001 which is available at http://www.mpiwg-berlin.mpg.de/resrep00_01/index.html). For historical case studies making use of this approach, see e.g. (Büttner, Damerow, and Renn 2001; Büttner, Renn, and Schemmel 2003; Damerow, Renn, and Rieger 2002; Renn 2000).

model can be corrected without abandoning the model as a whole. Information is assimilated to the slots of a mental model in the form of “frames” which are understood here as “chunks” of knowledge with a well-defined meaning anchored in a given body of shared knowledge.

Conceiving the shared knowledge of classical and special relativistic physics in terms of mental models and frames makes clear how this knowledge could serve as a resource in Einstein’s search for the gravitational field equation. In fact, essential relations between fundamental concepts such as field and source largely persist, even though the concrete applications of these concepts may differ considerably, as in the case of a classical vs. relativistic field equation. This structural stability turned the concepts and principles of classical and special relativistic physics into heuristic orientations when Einstein entered unknown terrain, for instance, when encountering a new expression generated by the elaboration of a mathematical formalism. None of these expressions in themselves constituted a new theory of gravitation. Only by complementing them with additional information based on the experience accumulated, not only in classical and special relativistic physics, but also in the relevant branches of mathematics, could such expressions become candidates for a gravitational field equation embedded in a full-fledged theory of gravitation. In the language of mental models, such past experience provided the default assumptions necessary to fill the gaps in the emerging and necessarily incomplete framework of a relativistic theory of gravitation. It was precisely the nature of these default assumptions that allowed them to be discarded again in the light of novel information—provided, for instance, by the further elaboration of the mathematical formalism—without, however, having to abandon the underlying mental models which could thus continue to function as heuristic orientations.

2. The Lorentz Model

2.1 The Mental Model of a Field Theory

The mental model that was crucial in Einstein’s search for the gravitational field equation was shaped largely by prior experiences with the classical gravitational potential governed by the Poisson equation and by the treatment of electromagnetic fields, which had taken on its most developed form in Lorentz’s theory of electromagnetism (Lorentz 1904a, 1904b). For this reason it may be called the “Lorentz model.” The Lorentz model describes in terms of a field how the environment is affected by the matter considered to be the “source” of the field, and how this field then determines, in turn, the motion of matter, now conceived as a “probe” exposed to the field. A mathematical representation of physical processes interpreted according to this model therefore necessarily comprises two parts, a field equation describing how a localized source creates the global field, and an equation of motion describing how the global field determines the motion of a localized probe.

In order to apply the Lorentz model of a field theory to the case of a relativistic theory of gravitation, one had to identify an appropriate mathematical representation of both the gravitational field and of its source, and to find a generalization of the classical Poisson equation that was at least Lorentz-invariant. The structure of the as yet unknown field equation, as suggested by the Lorentz model, may be represented by the symbolic equation **GRAV(POT) = MASS.** The familiar quantities from classical physics, the scalar gravitational potential and the gravitational mass, represented the original defaults for the slots **POT** and **MASS**, respectively, while the classical default setting for the operator **GRAV** was the Laplace operator.

When Einstein took up his systematic search for a relativistic gravitational field equation in mid-1912, however, it had become clear from both his own research and that of contemporaries such as Abraham and Laue that these original default settings were no longer acceptable.[2] Indeed, the experience of the years between 1907 and 1912 had suggested new default assumptions. In particular, the metric tensor was now taken to represent the gravitational potential and hence became the canonical concretization of **POT** in the Lorentz model. The default setting for the metric tensor was, in turn, the spatially flat metric suggested by Einstein's experiences with his theory of static gravitational fields.[3] Similarly, the energy-momentum tensor became the standard setting for **MASS**, which in turn took the special case of dust as its default case (CPAE 4, Doc. 10, p. 10). These two key ingredients of the gravitational field equation had the

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[2] In 1912 Max Abraham was the first person, in the context of a controversy with Einstein, to suggest generalizing the line element of Minkowski's four-dimensional spacetime to include a variable speed of light (as it occurred both in his and in Einstein's gravitational field theories), see (Abraham 1912). This suggestion soon became the basis for Einstein's introduction of the metric tensor as the representation of the gravitational tensor, see (Einstein 1912) and (CPAE 4). Max Laue's work on a relativistic continuum theory, on the other hand, suggested taking the energy-momentum tensor as the default-filling of the source slot of the Lorentz model, see (Laue 1911a, 1911b as well as CPAE 4) and, for historical discussion (Norton 1992).

[3] See (Einstein 1912) and (CPAE 4, Doc. 10, p. 201), and for historical discussion (Norton 1989).

appealing feature of being generally covariant objects and therefore embodied the expectation that the field equation itself would also take the form of a generally covariant tensorial equation, thus allowing Einstein to realize his ambition of creating a generalized relativity theory.

It is only for the third component of the Lorentz model, the differential operator **GRAV**, that the situation was more complicated. At the beginning of his search, not only was Einstein largely ignorant of the mathematical techniques necessary for constructing suitable candidates, but the many requirements to be imposed on an acceptable candidate effectively prevented the selection of an obvious default assumption for a differential operator **GRAV** compatible with all these requirements.

2.2 The Heuristic Requirements[4]

In fact, finding a field equation turned out to be the most challenging task Einstein ever tackled in his struggle for a relativistic theory of gravitation. First of all, he was confronted with the daunting mathematical problem that the representation of the gravitational potential by the metric tensor requires a field equation not for a single function but for a ten-component object. Second, Einstein could not avoid taking into account that the action of the gravitational field under ordinary circumstances was well known and satisfactorily described by Newton’s law of attraction. The relativistic field equation of gravitation therefore had to yield the same results as this law under appropriate circumstances, a requirement we have referred to as the “correspondence principle.” Third, the new field equation obviously had to be compatible with the well-established knowledge on energy and momentum conservation as well, a requirement we have labeled the “conservation principle.”

2.3 Double Strategy and Default Settings

How did Einstein’s heuristics actually bring these resources together, and how did they effectuate the process of knowledge integration required to identify an acceptable field equation? How could the Lorentz model ever be harmonized with the physical requirements embodied in the correspondence principle, the conservation principle, and, of course, in his generalized principle of relativity? And how could these structures of physical knowledge ever be matched with the representational tools offered by mathematics? The response that emerged from our earlier

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[4] See, also for the following, (Renn and Sauer 1999).

work was that Einstein pursued two complementary heuristic strategies, one physical and the other mathematical. Einstein's "physical strategy" took the Newtonian limiting case as its starting point and then turned to the problem of the conservation of energy and momentum in order to finally examine the degree to which the principle of relativity is satisfied. His "mathematical strategy," on the other hand, is characterized by the fact that he took the principle of relativity as a starting point in order to then check the question of the Newtonian limiting case, and to finally make sure that the conservation of energy and momentum was also satisfied.

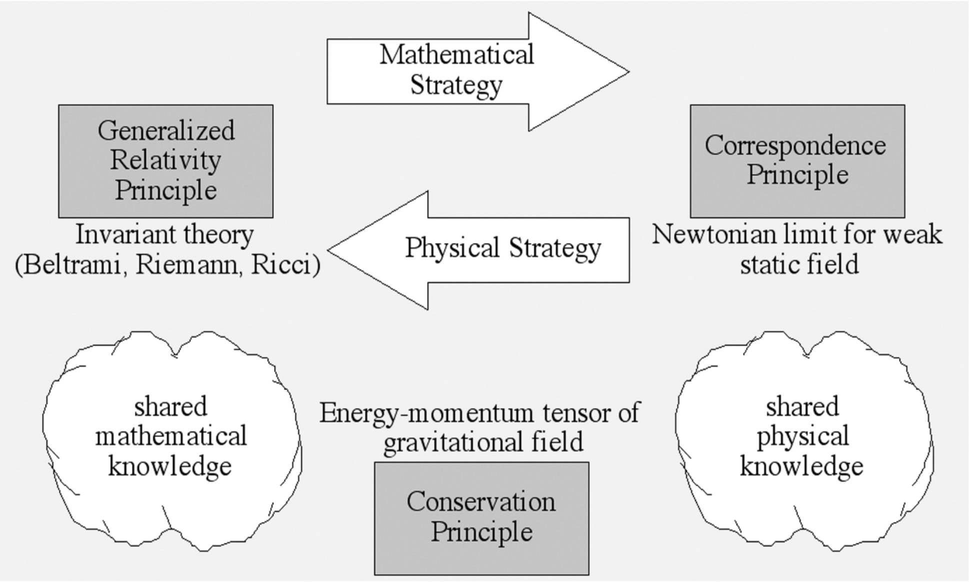

Figure 1: Einstein's heuristic strategies

What is the meaning of this double strategy from the viewpoint of historical epistemology? The answer to this question can be found in the nature of the Lorentz model as a mental model embedded in the shared physical knowledge of the time. Due to its epistemic architecture, the Lorentz model is in fact not just an abstract scheme into which knowledge about the properties of the gravitational field could be more or less successfully pressed. Rather it represents the crucial cognitive motor of Einstein's heuristics. The default settings of the Lorentz model made it possible to complement, if only by as yet uncertain and in hindsight often problematic information grounded in classical knowledge, the dim and shaky picture emerging in the course of Einstein's attempt to construct a relativistic field equation of gravitation.

As a mental model anchored in an elaborate body of physical and mathematical knowledge, the Lorentz model also offered the resources for constructing a mathematical representation of a candidate field equation, embodying both the structure of the model and its default assumptions. If such a candidate then turned out to be incompatible with other requirements to be imposed on a gravitational field equation, a fact that could now be checked on the level of an explicit representation, it did not necessarily follow that the theory built upon this candidate was thus falsified such that a new attempt had to begin from scratch. Instead it usually sufficed to merely adjust one or the other default assumptions, replacing them with the information newly gained in the course of exploring the given candidate field equation.

The most obvious starting point for constructing and exploring a candidate gravitational field equation was, in any case, an object with a well-defined physical meaning, constructed on the basis of the Lorentz model and with default settings rooted in classical and special relativistic physics. The approach of starting from such an object and then modifying it according to the heuristic requirements to be imposed on a candidate gravitational field equation is precisely what we have called the “physical strategy.” Given the nature of its starting point, the physical strategy complies immediately with the correspondence principle, but it is not obviously clear whether a candidate constructed according to this strategy also satisfies the conservation principle and the generalized principle of relativity. The physical strategy is thus comparable to the synthetic approach of traditional Euclidean geometry, proceeding from what is “known” (in this case: gravitation as understood in classical physics) to the construction of the “unknown” (in this case: gravitation as it must be understood according to a generalized theory of relativity).[5]

The complementary “mathematical strategy” starts with embedding the Lorentz model within a higher-order mathematical knowledge structure, from which it then inherits default assumptions of a different kind. These assumptions are rooted in the shared mathematical knowledge of the time, e.g. the derivation of such expressions as the Ricci tensor from the metric tensor. Also in this context, the model may thus serve to guide the construction of concrete candidate gravitational field equations that now, from the outset, make perfect sense as mathematical objects and comply, in particular, with the generalized principle of relativity. It remains to be seen, however, whether these candidates satisfy the other requirements placed on a field equation, in partic-

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[5] This comparison was suggested to me by Peter Damerow.

ular the heuristic expectations rooted in physical knowledge as represented by the principles of correspondence and conservation. The mathematical strategy is hence comparable to analytic geometry, which proceeds from the “unknown” (in this case: a mathematical object whose physical meaning is unclear) to the “known” (in this case: a physically meaningful field equation of gravitation).

In summary, in both the case of the physical and of the mathematical strategy, the available knowledge led, via default assumptions of the Lorentz model, to concrete mathematical representations of candidate field equations which made it possible to check whether they fulfilled and were compatible with the criteria an acceptable field equation has to satisfy. The alternation between physical and mathematical strategy fostered the assimilation of both physical and mathematical resources to the basic model of a field equation. The eventual success of Einstein’s search for the gravitational field equation was thus the result of a particularly efficient way of exploiting these shared knowledge resources.

3. Learning from a Dilemma

3.1 The Role of Reflection

How did this efficient heuristics, Einstein’s double strategy, take on the specific form we see at work in the main part of the Zurich Notebook where Einstein deals with the Ricci tensor, the Einstein tensor, the so-called November tensor, and finally with the Entwurf operator? The emergence of this truly pathbreaking strategy can only be understood if yet another cognitive mechanism is taken into account, a mechanism that, in a sense, is complementary to the one involved in the application of this strategy. In fact, the mechanism by which the exploitation of shared knowledge resources took place has been considered as yet only from a single perspective, that of incorporating physical and mathematical resources into the basic model of a field equation. While the assimilation of physical and mathematical knowledge to the Lorentz model is basically a top-down process guided by the relatively stable and high-level cognitive structures at the core of Einstein’s heuristic criteria, a reflection on the experiences resulting from such an assimilation, including its failures, could trigger a corresponding bottom-up process. Such a process could induce an accommodation of the high-level structures, including the Lorentz model itself, to the outcome of these experiences, or could result in new higher-level structures operating on a strategic level, that is, guiding the implementation of Einstein’s heuristic requirements in terms of what in cognitive science is called “procedures.” It is, as will be argued here, precisely such a process that triggered the emergence of Einstein’s successful double strategy in an early “tinkering phase” of the research documented in his Zurich Notebook.

3.2 The Tinkering Phase in Einstein’s Zurich Notebook

The earliest notes on gravitation in the Zurich Notebook represent a stage of Einstein’s search for the field equation in which he had hardly any sophisticated mathematical tools at hand that would allow him to construct candidates fitting the framework provided by the Lorentz model (CPAE 4, Doc. 10, p. 201 ff.). Even his knowledge of the metric tensor and its properties were still rudimentary. Only gradually did he find ways of exploiting his knowledge of vector analysis for his search. Eventually he familiarized himself with the scalar Beltrami invariants as another instrument that allowed him to tinker with the few building blocks at his disposal, that is, the metric as a representation of the gravitational potential, the four-dimensional Minkowski formalism, and his theory of the static gravitational field. In spite of the staggering lack of mathematical sophistication characterizing this early tinkering phase, not to mention his failure to produce a promising candidate for the field equation, it is precisely in this period that Einstein acquired essential insights that shaped his research in subsequent phases of work, in particular his double strategy.

In the following, Einstein’s attempt to assimilate knowledge about the static gravitational field to a metric formalism will first be outlined. I will then concentrate on the dilemma resulting from his construction of two incompatible default settings in the Lorentz model. It will be argued that the experience he made when attempting to resolve this dilemma caused him to devise a procedure for constructing and examining candidate gravitation tensors, a procedure that was to become essential for the mathematical prong of his double strategy. This procedure involves, on one hand, the identification of a physically meaningful default setting for the left-hand side of the gravitational field equation, an object we have called the “core operator” (Renn and Sauer 1999, 102). In the weak-field limit, this reduces to the ordinary d’Alembertian and allows the construction of a weak-field equation compatible with the correspondence principle. The procedure involves, on the other hand, a method for turning a mathematically meaningful default setting into a physically acceptable candidate gravitation tensor. This method makes use of coordinate restrictions limiting the validity of the relativity principle (in contrast to “coordinate conditions" in the modern understanding of general relativity). While such a method seems strange from a modern perspective, it actually determined Einstein's understanding of his theory of gravitation until the fall of 1915 (Renn forthcoming).

How did Einstein's procedure emerge in the course of his research? Here it will be argued that it was his reflection on the experiences of the tinkering phase that led to what one might describe as a "chunking" of his trials, alternately using physically and mathematically plausible default settings, in the procedure at the heart of his double strategy.

3.3 Assimilating Knowledge about the Static Gravitational Field to a Metric Formalism[6]

Before explaining the emergence of this procedure, let us look briefly at the beginning of Einstein's research on gravitation as documented by the notebook. His first attack constitutes an attempt at assimilating knowledge about the static case to a metric formalism, concentrating on two slots of the Lorentz model for a field equation, the slot for the gravitational potential and the slot for the differential operator. Einstein's key problem was that the default-settings for these two slots, representing his earlier experiences with implementations of this model, did not match. While the default setting for the gravitational potential was represented by a spatially-flat metric *tensor*, the default setting for the differential operator was the left-hand side of his 1912 field equation involving merely a *scalar* gravitational potential. Was there any way of bridging this gap between a scalar differential operator and a tensorial potential?

3.4 A Mathematical Toy Model as a New Starting Point[7]

The mismatch between the default-settings for two of the slots of the Lorentz model of a field equation, that for the differential operator and that for the gravitational potential, left Einstein with two principal options for proceeding. He could try to somehow build, from whatever knowledge was at his disposal, an appropriate differential operator applicable to the metric tensor. Alternatively, he could tentatively explore variations of the default-setting for the gravitational potential, thus creating "toy-models" in the sense of manifestations of the Lorentz model with purposefully simplified default-settings. Even if that could mean temporarily shelving the

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[6] See (CPAE 4, Doc. 10, pp. 201–203).

[7] See (CPAE 4, Doc. 10, pp. 214–217).

insight that the gravitational potential is represented by the metric tensor, it might still be possible to gain knowledge from exploring such “toy-models” that could help to construct a real candidate field equation.

When, at some point during his work on the notebook, Einstein became familiar with the second Beltrami invariant as a generalization of scalar differential operators, it must have immediately appealed to him as a mathematically plausible setting for the differential operator slot, since a field equation formulated with its help would satisfy the heuristic requirement of the generalized principle of relativity from the outset. But choosing this setting also posed a problem: it was incompatible with filling the potential slot by the metric tensor as the Beltrami invariant was applicable only to scalar functions. In a sense, a scalar field equation formulated in terms of the second Beltrami invariant represents the counterpart to the scalar field equation of Einstein’s 1912 static theory: while the latter constitutes an initial, physically plausible default-setting for the Lorentz model, the former represents an equally plausible initial default-setting rooted in mathematical knowledge. In both cases, the resulting field equations were merely starting points for further investigations that had to establish contact with knowledge not yet embodied in these initial default-settings.

It therefore comes as no surprise that Einstein attempted to understand the conditions under which a generally covariant scalar field equation, formulated in terms of the second Beltrami invariant, reduces to the ordinary Poisson equation. In fact, such a reduction must be possible if the candidate (or rather “toy”) field equation is to comply with the correspondence principle. It turned out that the implementation of this heuristic principle requires an additional hypothesis on the choice of the coordinates supplementing the field equation. Essentially by inspection, Einstein was able to identify the harmonic coordinate restriction as a condition that would ensure that the Beltrami field equation would reduce to the ordinary Poisson equation for weak gravitational fields, eliminating disturbing terms. In other words, the exploration of a toy field equation taught Einstein that a candidate field equation obtained from a mathematical default-setting might require an additional coordinate restriction in order to be viable from a physical point of view as well.

This sequence—to first pick a generally covariant candidate and then reduce it to a familiar physical format, that is, to get rid of disturbing terms by imposing a coordinate restriction—was to become the basic procedure of Einstein's mathematical strategy. What was still lacking for this strategy to emerge fully was a more realistic target than the classical Poisson equation, a target that involved the true setting for the potential-slot: the metric tensor. This missing piece was found after Einstein had made a fresh attempt directed at creating a physically more meaningful candidate.

3.5 A Physical Toy Model as a New Starting Point[8]

Einstein's exploration of the Beltrami invariant left him, in the end, uncertain about how to get from a mathematically plausible scalar differential equation to a tensorial field equation that was both mathematically *and* physically plausible. Reflecting on this problem, he now started out from a physically-plausible "toy" field equation. Instead of taking a simplified default-setting for the potential-slot of the Lorentz model in order to explore a mathematical toy model, he chose a simplified default-setting for the differential operator slot while keeping the realistic setting for the potential slot, i.e. the metric tensor.

Einstein's broad experience with the tools of vector analysis and their use in physics in fact made it easy for him to write down a straightforward translation of the ordinary Laplacian operator into a differential operator capable of acting on the metric tensor, the core operator. But while even his limited familiarity with Beltrami invariants must have made it obvious that the core operator could hardly represent a generally covariant object, the way in which it was constructed made it equally clear that a field equation based on it satisfies the correspondence principle. For this reason, the core operator became the default-setting for all Einstein's subsequent attempts to implement this principle and with it both the starting point for his physical strategy and the target of his mathematical strategy.

The challenge was now to confront the core operator with the other heuristic requirements to be imposed on a field equation and, in particular, to explore its relation to mathematical knowledge about coordinate transformations. Einstein therefore began to check the transformational behavior of the core operator. But he quickly discovered that the analysis of explicit coordinate transformations of the core operator became rather involved and offered hardly any general insight

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[8] See (CPAE 4, Doc. 10, pp. 216–220).

into its covariance properties. What he learned from this attempt was merely the possibility of also taking into account coordinate transformations that explicitly depend on the metric tensor—again a heuristic insight with far-reaching implications for his further research.

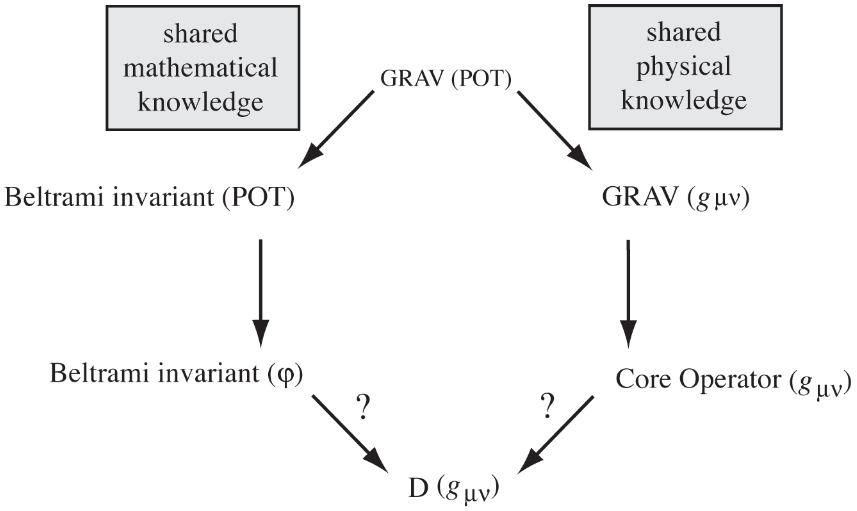

Figure 2: The Dilemma: The Beltrami Invariant and the Metric Tensor as incompatible fillings of the slots of the Lorentz Model

3.6 Identifying the Core Operator as the Target of the Mathematical Strategy[9]

Einstein’s attempt to start from a physically satisfactory candidate gravitation tensor had thus failed to yield any tangible results. He therefore turned again to the Beltrami invariants, this time, however, under new conditions. Earlier, he had unsuccessfully attempted to connect the second Beltrami invariant with physical knowledge on static gravitational fields. At that point, however, he was not yet in possession of the core operator, which now offered a new and more promising target for a transition from a mathematically well-defined object to a physically acceptable candidate gravitation tensor. But first of all, he had to establish a relation between the Beltrami invariants, applicable to scalar functions, and the realistic setting of the potential slot: the metric tensor. For this purpose, he focused on the determinant of the metric tensor. Indeed, if unimodular coordinate transformations are assumed, the determinant of the metric becomes a scalar function and can hence be inserted into the Beltrami invariants.

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[9] See (CPAE 4, Doc. 10, pp. 220–223).

Einstein tried next to extract the core operator from the scalar expression resulting from inserting the determinant of the metric into the Beltrami invariant. Apart from a term involving the contraction of the core operator, however, he also found an additional term that was difficult to interpret. This disturbing remaining term posed a problem analogous to the one Einstein first encountered when comparing a mathematical toy model based on the second Beltrami invariant with the ordinary Laplace operator. This analogy thus suggested taking up the idea of introducing a coordinate restriction as an additional hypothesis under which a mathematically plausible expression reduces to a physically acceptable one.

In any case, Einstein must have hoped to infer the transformational behavior of the core operator by analyzing the behavior of the remaining term under coordinate transformations. In this way, a bridge would have been built between the transformational behavior of the mathematically well-defined second Beltrami invariant and the physically plausible core operator. Unfortunately, however, although the remaining term essentially representing the difference between the Beltrami invariant and the contracted core operator was a simpler expression than the core operator itself, it turned out to be still too complex for an evaluation of its transformational behavior. While this unsuccessful attempt terminated Einstein's use of the Beltrami invariants in the course of his research documented in the Zurich notebook, it did establish a heuristic procedure that, as puzzling as it may appear from the perspective of modern general relativity, was consistently applied by Einstein even when he eventually learned about the Riemann tensor from Marcel Grossmann.

The procedure that resulted from this experience takes a covariant object as its starting point and then attempts to extract a candidate gravitation tensor compatible with the correspondence principle by imposing an additional coordinate restriction. Typically, such a candidate gravitation tensor would be represented by the core operator plus some harmless correction terms. Examining the transformational behavior of the coordinate restriction then allows, together with knowledge about the covariant starting point of the procedure, the inference of the transformational behavior of the candidate gravitation tensor.

The genesis of this procedure from the challenge of filling the slots of the Lorentz model of a field equation in a mutually compatible way illustrates a typical learning experience encountered by Einstein in his search, making it evident that this search did not simply consist of the elimination of unworkable alternative candidates. In fact, both the identification of the core operator and its conjointment with a covariant object were results that substantially changed the conditions of his further search quite independently from accepting or discarding a specific candidate. The role of the Beltrami invariants illustrates this seemingly paradoxically feature of Einstein's research: While the Beltrami invariants played no role whatsoever in formulating the final field equation, they were crucial in triggering the higher-order heuristic insights that paved Einstein's way to this solution.

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An Astronomical Road to General Relativity

THE CONTINUITY BETWEEN CLASSICAL AND RELATIVISTIC COSMOLOGY IN THE WORK OF KARL SCHWARZSCHILD

Matthias Schemmel

1. Karl Schwarzschild—Pioneer of Relativistic Astronomy

Only a few weeks after Einstein had presented the successful calculation of Mercury's perihelion advance on the basis of his new theory of general relativity in late 1915, the German astronomer Karl Schwarzschild (1873–1916) published the first non-trivial exact solution of Einstein's field equations (Schwarzschild 1916). The solution describes the spherically symmetric gravitational field in a vacuum and holds a central place in gravitation theory, comparable to that of the Coulomb potential in electrodynamics. It was not only an important point of departure for further theoretical research but also, up to recent times, the basis for all empirical tests of general relativity that proved not only the principle of equivalence but also the field equations themselves. Schwarzschild made a further substantial contribution to the theory when he found another exact solution describing the interior gravitational field of a sphere of fluid with uniform energy density (Schwarzschild 1916a). It is in this communication that the quantity, which under the name *Schwarzschild radius* would later play an important role in the theory of black holes, makes its first appearance.[1] But even long before the final theory of general relativity was established, Schwarzschild had already occupied himself with possible implications of its predecessors for astronomy; in 1913 he carried out observations of the solar spectrum in order to clarify if the gravitational red shift predicted by Einstein on the basis of the equivalence principle was detectable (Schwarzschild 1914).

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[1] For a thorough analysis of the early history of the interpretation of Schwarzschild's solutions and the Schwarzschild radius in particular, see (Eisenstaedt 1982; 1987; 1989). See also (Israel 1987), in particular Section 7.7 on the *Schwarzschild 'Singularity'*.

In view of the fundamental role played by general relativity in astronomy, astrophysics, and cosmology today, it appears quite natural that an astronomer would engage in the study of this theory. Astronomical objects of all scales ranging from supermassive stars via galaxy nuclei and quasars to the universe as a whole are described on its basis. However, at the time when Schwarzschild made his contributions, the situation was quite different. None of the spectacular objects nowadays so successfully described by general relativity were in the focus of research, most of them not even known at all. Rather, the deviations from Newtonian theory which general relativity predicted were so small that in most cases they lay on the verge of detectability, even on astronomical scales. General relativity could thus easily be considered a physical theory—it was developed in the attempt to solve problems in physics such as the incompatibility of Newtonian gravitation theory and special relativity—with little implications on astronomy. And even as a physical theory it was still controversial, as is strikingly illustrated by the case of the physicist Max von Laue who as late as 1917 preferred Nordström's theory of gravitation to Einstein's.[2] Accordingly, at the time, astronomers showed little interest in general relativity. Einstein's plea to put the theory to an empirical test went unheard by most of them. In his attempts to provide empirical evidence for the theory, Erwin Freundlich, an outsider to the astronomical community, even met with hostility among Germany's most prominent astronomers.[3] Why was Schwarzschild an exception to this? What put him in the position to recognize so early the significance of general relativity?

The clue for answering these questions lies in the study of work Schwarzschild had done long before the rise of general relativity. In the course of the late 19th century, foundational questions surfaced in classical physics that had implicit consequences for astronomy: consequences that were often of a cosmological dimension. Mach's critique of Newton's absolute space, for example, immediately led to the question of an influence of distant stars on terrestrial physics. The deviation of the geometry of physical space from Euclidean geometry, to give another example, had become a possibility with the work of Gauss and Riemann and could be imagined to be measurable on cosmological scales. A further example is provided by the various attempts to modify Newton's law of gravitation. Such a modification would have consequences not only for planetary motion but also touches upon questions concerning the stability of the whole universe and

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[2] See (Laue 1917).

[3] For an account on Freundlich's work on empirical tests of general relativity and the astronomers' reaction to it, see (Hentschel 1997).

the large-scale distribution of matter therein.[4] These foundational questions were, despite their astronomical implications, not on the agenda of contemporary astronomical research. Nevertheless, they were studied by a few individual scientists, among them Karl Schwarzschild.

In this paper it is argued that a continuity exists between Schwarzschild's prerelativistic work on foundational problems on the borderline of physics and astronomy and his occupation with general relativity.[5] After a brief biographical introduction (Section 2), Schwarzschild's prerelativistic considerations on the relativity of rotation (Section 3) and on the non-Euclidean nature of physical space (Section 4) are presented as they are documented in his publications as well as in his unpublished notes. On this background, Schwarzschild's reception of general relativity will then be shown to have been shaped to a large extent by his earlier experiences. In fact, what at first sight may appear to be a rather technical contribution to a physical theory—Schwarzschild's derivation of an exact solution of Einstein's field equations—turns out to have been motivated by Schwarzschild's concern for a consolidation of the connection between astronomy and the foundations of physics as established by Einstein's successful calculation of Mercury's perihelion motion (Section 5). What is more, Schwarzschild was reexamining his prerelativistic cosmological considerations in the framework of the new theory of relativity as hitherto neglected manuscript evidence reveals for the case of the problem of rotation (Section 6, a manuscript page from Schwarzschild's Nachlass is reproduced with annotations in the Appendix). Furthermore it turns out that, prepared by his earlier cosmological considerations, Schwarzschild was the first to consider a closed universe as a solution to Einstein's field equations (Section 7). Summing up, Schwarzschild's road to general relativity may be called an astronomical one. Concluding this paper it will be argued that it was no coincidence that Schwarzschild of all astronomers took this road, but that this was the natural outcome of his interdisciplinary approach to the foundations of the exact sciences (Section 8).

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[4] For a discussions of fundamental problems arising in Newtonian cosmology, see (Norton 1999).

[5] The work presented here was done in the context of a project on the genesis of general relativity conducted at the Max Planck Institute for the History of Science in Berlin. In this project the role that the knowledge of classical science played in the emergence of general relativity is explored. Here it is attempted to show how the knowledge potential inherent in classical astronomy became effective in the early history of general relativity through the work of Karl Schwarzschild.

2. Karl Schwarzschild—Astronomer, Physicist and Astrophysicist

Schwarzschild was born on October 9, 1873 in Frankfurt am Main, the eldest of seven children of a Jewish businessman.[6] He studied astronomy in Strasbourg and in Munich, where he obtained his doctoral degree in 1896 under Hugo von Seeliger (1849–1924), one of the most prominent German astronomers at the time. After having worked for three years at the Kuffner Observatory in Ottakring near Vienna, Schwarzschild obtained his post-doctoral degree (*Habilitation*) in Munich in 1899. On this occasion, Schwarzschild had to defend five theses, mostly concerned with foundational questions, that inspired him, as we will see, to much of the work relevant to our discussion. It is therefore interesting to question the extent to which Schwarzschild's teacher, von Seeliger, was involved in formulating these theses. While it may well be the case that Schwarzschild himself played some role in their creation, their exact wording makes it plausible that they were formulated by von Seeliger (see the discussion below). Thus this sheds some light on von Seeliger's ambivalent role in the early history of relativity. On one hand he was known to be very sceptical of relativity theory. For example, he severely criticized Erwin Freundlich's attempts to provide empirical evidence supporting general relativity. On the other hand he was interested in foundational questions of theoretical astronomy and apparently inspired Schwarzschild to much of the work discussed here.

In 1901, Schwarzschild was appointed professor of astronomy and director of the observatory of Göttingen University. He became closely associated with the circle of mathematicians and natural scientists around Felix Klein and furthered the integration of Göttingen astronomy with general scientific life.[7] Schwarzschild left Göttingen in 1909 and became director of the *Astrophysikalisches Institut* in Potsdam, but, for the short remainder of his life, he maintained the personal and scientific relationships established in Göttingen. Thus it was his Göttingen colleagues and acquaintances who, on several occasions, wrote him about the latest developments of Einstein's theory and pointed out the importance of its astronomical verification.[8] On May 11, 1916, Schwarzschild died an untimely death from a skin disease he contracted while serving at the Russian front.

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[6] For a short biographical account on Schwarzschild, see (Schwarzschild 1992, 1–25).

[7] See (Blumenthal 1918).

Schwarzschild's scientific work is characterized by its rare breadth. The range of topics from physics and astronomy covered by his more than one hundred publications is hardly surpassed by any other single scientist of the twentieth century. Schwarzschild is further known to be one of the founders of astrophysics in Germany and was its most prominent exponent at the time. While disciplinary astrophysics itself was a rather specialized enterprise—using physical instruments for astronomical observation and applying physical theory to astronomical objects—Schwarzschild's interdisciplinary outlook on the foundations of science[9] enabled him to overcome the constraints imposed by specialization and deal with foundational problems on the borderline of physics and astronomy that were not in the focus of mainstream research.

3. Schwarzschild's Prerelativistic Considerations on the Relativity of Rotation

In 1897, while he was assistant at the Kuffner Observatory in Ottakring, Schwarzschild published a popular article entitled *Things at Rest in the Universe* (*Was in der Welt ruht,* Schwarzschild 1897). In this paper he discusses the relativity of motion and the problem of finding appropriate reference frames. In particular, he is concerned with the question of how fixed directions in space can be defined.

Schwarzschild's starting point is the observation that the motion of an object can only be perceived relative to other objects and that therefore any object may be considered at rest. The question of what thing is at rest in the universe should therefore be reformulated in a historical manner as "[w]hat things in the universe did one find useful to treat as being at rest, at different times [in history]?".[10] In the Copernican system, Schwarzschild explains, fixed directions in space were defined by reference to the system of fixed stars. Towards the end of the 17th century it became clear however that the Copernican stipulation is not unambiguous: the stars per-

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[8] See, in particular, Schwarzschild's correspondence with David Hilbert. On a postcard to Schwarzschild from October 1915, for example, Hilbert wrote: "The astronomers, I think, should now leave everything aside and only strive to confirm or refute Einstein's law of gravitation." ("Die Astronomen, meine ich, müssten nun Alles liegen lassen u. nur danach trachten, das Einsteinsche Gravitationsgesetz zu bestätigen oder widerlegen!") Hilbert to Schwarzschild, October 23, 1915, N Briefe 331, 6r. This and all following translations are my own. Examples of this kind are also found in Schwarzschild's correspondence with Arnold Sommerfeld.

[9] See Section 8.

[10] "[w]as in der Welt hat man zu verschiedenen Zeiten als ruhend zu betrachten für gut befunden?" (Schwarzschild 1897, 514). All page numbers cited for this text refer to Vol. 3 of the *Collected Works* edition (Schwarzschild 1992).

form motions relative to one another, the so-called proper motions. Schwarzschild therefore next considers the electromagnetic ether as a candidate for a material reference of rest but comes to the conclusion that the ether too cannot serve such a purpose since it is affected by ponderable matter moving through it. Schwarzschild concludes that there are no material objects in the universe that one could reasonably consider at rest and that one can only take resort to “certain conceptually defined points and directions that may serve as a substitute to a certain extent”.[11]

In order to explain how fixed directions in space may be defined on the basis of the law of inertia, Schwarzschild refers to Foucault’s pendulum. By accurate observations of the rotation of the pendulum’s plane of oscillation, Schwarzschild explains, one could calculate the speed of rotation of the earth, and would then have to describe as fixed the direction with respect to which the earth rotates with the calculated speed.

In following this idea further, Schwarzschild establishes an interesting connection between inertia and gravitation in the following way. In regarding the planets orbiting around the Sun as gigantic, diagonally pushed pendulums, he conceives an astronomical realization of the physical model of the pendulum. In analogy to Foucault’s pendulum, fixed directions in space are then given by the aphelia (or perihelia) of the orbits of the different planets. However, Schwarzschild explains, astronomical observations since the middle of the 19th century reveal that the directions singled out by the orbits of the different planets rotate with respect to each other at a very slow rate, so that they “impossibly can all be considered as fixed”.[12] Although Schwarzschild was aware of possible astronomical explanations, such as interplanetary friction, he considered it more probable that an explanation of these small anomalies has to go further, requiring a revision of the classical law of gravitation.

In this way, Schwarzschild established a relation between the two physical phenomena, inertia and gravitation, the integration of which was later to lie at the basis of Einstein’s theory of general relativity. Moreover, the observational fact by which Schwarzschild links the two phenom-

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[11] “[...] gewisse begrifflich definierte Punkte und Richtungen, die einigermaßen als Ersatz eintreten können [...]” (Schwarzschild 1897, 516).

[12] “[...] unmöglich alle als fest betrachtet werden können.” (Schwarzschild 1897, 520.)

ena—the perihelion shift of the inner planets—was later to play a crucial role in the establishment of general relativity, for some years being the only empirical fact suggesting a superiority of general relativity over the Newtonian theory.

There are, of course, fundamental aspects of general relativity that have no analogue in Schwarzschild's prerelativistic considerations. Most notably, Schwarzschild did not consider a field theory of gravitation that unifies gravitation and inertia in one single field. In fact, in this text, Schwarzschild does not even question the origin of inertia. Unlike Mach and Einstein, he does not search for a physical cause of inertia but rather assumes inertia to be given and, on its basis, defines fixed directions in space. Most probably he therefore thought of modifications of the Newtonian law of gravitation that do not affect inertial frames. For example, it was well known at the time that the change of the exponent in Newton's inverse square law yields perihelion motions.[13] Such a motion could have easily been subtracted from the observed motions in order to obtain the "true" inertial directions in space given by the planets' orbits. There are however notes found in Schwarzschild's manuscripts that show that he was concerned with the question of the origin of inertia and that, in this context, he considered the possibility of local inertial frames rotating with respect to one another. These notes, in which Schwarzschild was again using orbits of celestial bodies in order to determine inertial directions, shall now be discussed.

As explained in Section 2, Schwarzschild had to defend five theses, probably formulated by his teacher von Seeliger, in order to obtain his post-doctoral degree in 1899. One of these theses read: "The existence of centrifugal forces is comprehensible only under the assumption of a medium pervading all of space."[14] In a notebook of 1899 (N 11:17),[15] we find Schwarzschild's tentative defense of this thesis. In a thought experiment reminiscent of Einstein's later ones, at-

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[13] Thus, Schwarzschild's teacher Hugo von Seeliger wrote in a letter to Arnold Sommerfeld: "that the law of attraction $1/r^n$ $(n \neq 2)$ [causes] perihelion shifts, that is known to any astronomer since time immemorial. [...] *Newton* already treated this case, or a quite similar one, in his 'Principia.'" ("[...] daß das Anziehungsgesetz $1/r^n$ $(n \neq 2)$ Perihelbewegungen [hervorruft], das ist jedem Astronomen seit jeher bekannt. [...] Schon *Newton* hat diesen oder einen ganz ähnlichen Fall in den 'Prinzipien' behandelt.") Hugo von Seeliger to Arnold Sommerfeld, May 25, 1902, Arnold Sommerfeld Nachlass, Deutsches Museum, Munich, HS 1977-28/A, 321, 1-1.

[14] "Die Existenz von Centrifugalkräften ist nur unter der Annahme eines den ganzen Raum erfüllenden Mittels zu begreifen." A document naming the five theses can be found in N 21 (for an explanation of this notation see the next footnote).

[15] Here and in the following, references to Karl Schwarzschild's Nachlass in the Niedersächsische Staats- und Universitätsbibliothek Göttingen are indicated by an archival number following an 'N' (e.g. N 11:17).

tempting to clarify the nature of rotation, Schwarzschild imagines two planets of identical constitution rotating with different angular velocity and having atmospheres that are so dense that the outer world cannot be observed. An inhabitant of one of the planets travelling to the other would have no chance to understand how the difference in the "gravitational conditions" (*Schwereverhältnisse*) arises, since he would not notice the rotation. This shows clearly, Schwarzschild explains,

> that not only the internal relative circumstances but also the relations to the surrounding space have an influence on the processes in a system of bodies. Following Newton we could state that there is an absolute space and that the relation of motions to this absolute space has an influence on the forces appearing through this motion. Or, in other words: absolute space has an effect on the bodies. Now, we are used to thinking of anything having an effect as something real, namely something material, and from this it follows that, if we want to stick to the usual way of thinking, we have to imagine space, Newton's absolute space, filled with a substance.[16]

The hypothetical identification of space with a substance now puts Schwarzschild in a position to discuss the global validity of the locally distinguished directions:

> This substance does not have to be at absolute rest, but only in a state of motion that in some way distinguishes three fixed directions in space [...]. Then it is comprehensible that the centrifugal forces are based on a relation of the motion of the usual bodies to the motion of this substance.[17]

From the observation that the perihelia of double stars are at rest with respect to the directions that seem fixed inside the solar system, Schwarzschild concludes that the directions distinguished in their region of space have to be the same as in the solar system.

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[16] "[...] daß auf die Vorgänge in einem Körpersystem nicht nur die inneren relativen Verhältnisse, sondern auch die Beziehungen zum Raum, der sie umgiebt, von Einfluß sind. Wir könnten mit Newton sagen, daß es einen absoluten Raum giebt und daß das Verhalten der Bewegungen zu diesem absoluten Raum auf die bei der Bewegung auftretenden Kräfte von Einfluß ist. Oder in anderen Worten: der absolute Raum hat eine Wirkung auf die Körper. Nun pflegen wir uns aber alles, was eine Wirkung hat, als etwas wirkliches, nämlich als etwas Materielles zu denken, und daraus folgt, daß wir, wenn wir überhaupt in der üblichen Denkweise bleiben wollen, uns den Raum, Newtons absoluten Raum durch einen Stoff erfüllt denken müssen." (N 11:17, 8v–9r.) There are no page numbers in this notebook. The page numbers given here refer to my pagination.

[17] "Dieser Stoff muß nicht absolut ruhen, sondern nur eine Bewegungsform haben, welche auf irgend eine Weise drei besondre feste Richtungen auszeichnet [...]. Dann ist begreiflich, daß die Centrifugalkräfte auf einer Beziehung der Bewegung der gewöhnlichen Körper zur Bewegung dieses Stoffes beruhen." (N 11:17, 9r.)

To sum up, while in the previous example, Schwarzschild had established a relation between inertia and gravitation, here he relates inertia to the structure of space, considering the possibility that the local inertial directions may vary on cosmological scales.

4. Schwarzschild's Prerelativistic Considerations on Non-Euclidean Cosmology

A second example for Schwarzschild's prerelativistic treatment of foundational questions having cosmological implications is provided by his application of non-Euclidean geometry to physical space. Again, this work appears to have been inspired by one of the five theses Schwarzschild had to defend in order to attain his degree. This thesis reads: "The hypothesis that our space is curved should be rejected".[18] It is plausible to assume that this thesis too was formulated by von Seeliger. In fact, the thesis seems to reflect von Seeliger's attitude toward the application of non-Euclidean geometry to physics and astronomy which was extremely sceptical as may be illustrated by the following passage from a talk by von Seeliger entitled *Remarks on the So-Called Absolute Motion, Space, and Time*:

> [...] the common and therefore very fatal misapprehension has emerged that one believed to be able to decide by measurement which geometry is the "true" one, or even, which space is the one in which we live. From the stand point taken here the latter formulation is by far the more dangerous one, since space in itself has no properties at all.[19]

In the above-mentioned notebook of Schwarzschild we find an entry in which the thesis is slightly reformulated as follows: "The assumption of a curvature of our space is without any advantage for the explanation of the structure of the system of fixed stars".[20] The note is accompanied by considerations and calculations in which Schwarzschild examines empirical consequences of a curvature of space, for example, on the parallaxes of stars. One year later, Schwarzschild published a more detailed account of these considerations, though now under a different perspective. While the notebook entries aimed at a rejection of the curvature of

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[18] "Die Hypothese einer Krümmung unseres Raumes ist zu verwerfen" (N 21).

[19] "[Es] ist der verbreitete, aber gerade darum sehr verhängnisvolle Irrtum entstanden, daß man glaubte durch Messungen entscheiden zu können, welche Geometrie die "wahre" ist, oder gar, welcher Raum der ist, in dem wir leben. Von dem hier vertretenen Standpunkt aus ist die letztere Fassung die bei weitem gefährlichere, da der Raum an sich überhaupt keine Eigenschaften hat." (Seeliger 1913, 200–201.)

[20] "Die Annahme einer Krümmung unseres Raumes ist ohne jeden Vorteil für die Erklärung des Baues des Fixsternsystems." (N 11:17, 4r.)

space—understandably so in view of their context, the defense of a thesis—the purpose of the published article is to estimate the degree of curvature that can be assumed without contradicting observation. The article bears the title *On the Permissible Scale of the Curvature of Space* (*Über das zulässige Krümmungsmaass des Raumes,* Schwarzschild 1900).[21]

In his article Schwarzschild mainly discusses two cases: hyperbolic space, having constant negative curvature, and spherical space, having constant positive curvature. He makes the assumption that light travels along geodesics.

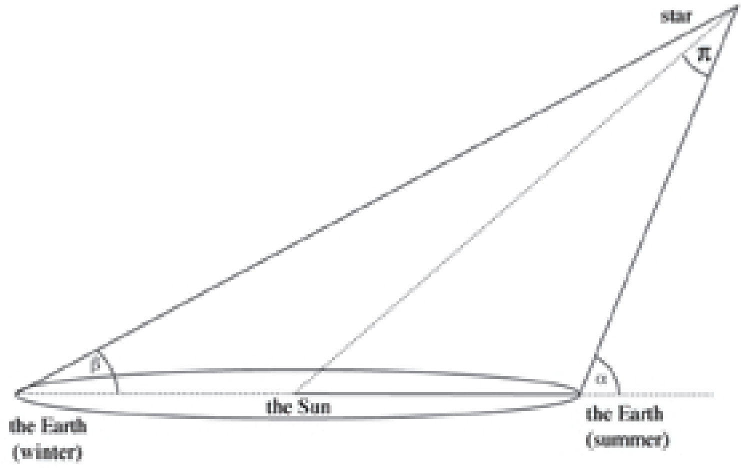

Figure 1: The annual parallax of a star nearly perpendicular to the ecliptic.

As far as hyperbolic space is concerned, Schwarzschild is able to estimate a minimal radius of curvature with the help of the parallax. As is well known, the parallax of a star, for simplification assumed to be nearly perpendicular to the ecliptic, is defined as half the difference of the two angles under which the star is seen in an interval of half a year, $\pi = (\alpha - \beta)/2$ (see Fig. 1). (In Euclidean space this coincides with the angle under which the radius of the Earth's orbit is seen from that star.) In Euclidean space, therefore, a parallax of exactly zero implies that the star is infinitely far from the Earth, since parallel geodesics in Euclidean space intersect at infinity. In hyperbolic space, in contrast, neighboring geodesics diverge. Thus even stars infinitely remote from the Earth possess a certain parallax. This minimal parallax decreases with an increase in the radius of curvature. Since for most stars no parallax can be observed, the minimal parallax

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[21] The article is based on a talk Schwarzschild held at the Heidelberg meeting of the *Astronomische Gesellschaft* in 1900.

is given by the accuracy of observation. This is given by Schwarzschild as $0.05''$. From this he concludes that the curvature radius of hyperbolic space must be at least 4 million times the radius of the Earth's orbit.

Figure 2: Two-dimensional hyperbolic space. Two geodesics are drawn as dotted lines.

As concerns spherical space, Schwarzschild discusses the special case of an elliptic space. The latter can be obtained from usual spherical space by identifying antipodal points. As a consequence, two geodesics going around the world intersect at only one point. Schwarzschild's reason for preferring elliptic to spherical space is that, in the latter, light emitted at one point in space in different directions would converge on the antipodal point, a rather artificial-looking consequence which, according to Schwarzschild, one would not accept without being forced to.

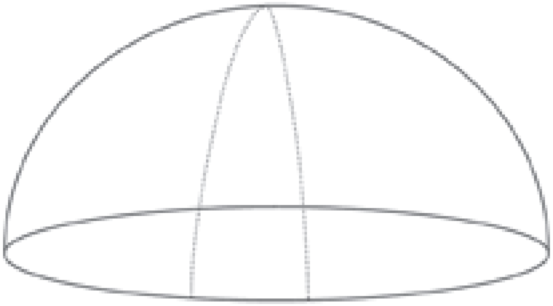

Figure 3: Two-dimensional elliptic space (the antipodal points on the circle c are to be identified). Two geodesics are drawn as dotted lines.

In the case of elliptic space there is no minimal parallax and physical considerations are required in order to determine a minimal radius of curvature. Schwarzschild offers the following reasoning. In elliptic space neighboring geodesics converge and thus intersect already at a finite distance, namely at the distance $(\pi/2)R$, where R denotes the curvature radius. Stars having a parallax smaller than a certain given value, say $0.1''$, therefore have all to be located within a finite volume. Now, there are approximately 100 stars of parallax above $0.1''$. All other stars are thus to be found in this finite volume. If one assumes a uniform distribution of stars, one can determine a certain minimal radius of curvature. A weaker requirement, however, is that the stars with parallax less than $0.1''$ occupy a volume large enough so that they do not influence each other in a way that could not have escaped observation. Schwarzschild does, for instance, calculate that if the elliptic space had a curvature radius of about 30,000 times the radius of the Earth's orbit, stars at great distances from the Earth would be separated from one another by only about 40 times the radius of the Earth's orbit. The physical interactions between the stars resulting from this could hardly be concealed from observation. From these considerations Schwarzschild concludes that the minimal radius of curvature of elliptic space is of the order of 100 million times the radius of the Earth's orbit. Schwarzschild further argues that such a relatively small radius of curvature (roughly 1600 light years) is only a realistic possibility if one further assumes an absorption of the starlight of about 40 magnitudes in one circulation around the universe because it is only under this assumption that the appearance of a counter image of the Sun can be avoided.

In this article, as in a later one (Schwarzschild 1909), Schwarzschild expresses his preference for the elliptic space over the hyperbolic or even the Euclidean one, because its finiteness would make it possible in principle to investigate the macroscopic world exhaustively. This idea would have a soothing effect on the mind.

While the differences between Schwarzschild's application of non-Euclidean geometry to physical space on one side and modern cosmology on the other are obvious (application to three-dimensional space rather than to four-dimensional spacetime, no dynamics of geometry, consideration of scales that today are hardly considered cosmological), Schwarzschild's consideration also contains striking parallels to the modern treatment of the problem such as the idea that light proceeds along geodesics and, most notably, the possibility that the universe is spatially closed.[22]

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[22] In an addendum to his article, Schwarzschild mentions a further possibility that later became a debated subject in relativity theory: the possible application of different topologies to physical space.

5. The Perihelion Breakthrough

In Section 3 we have seen how Schwarzschild put the perihelion anomalies of the inner planets into the context of the fundamental physical phenomena of inertia and gravitation. In this section it will be argued that Einstein's successful calculation of Mercury's perihelion motion—that first established Schwarzschild's speculated relation between physics and astronomy—was the trigger for Schwarzschild's devotion to general relativity. It turns out that even Schwarzschild's derivation of his first exact solution was motivated by his concern to consolidate Einstein's result.

As early as 1912 Schwarzschild had been confronted with the question of the observability of astronomical consequences of general relativity and its predecessors—in particular consequences of the principle of equivalence. Interestingly, in view of Schwarzschild's correspondence, it was not Einstein himself who first confronted Schwarzschild with the question of astronomical consequences of such a theory but rather one of his antagonists in the search for a new theory of gravitation: the theoretical physicist Max Abraham (1875–1922). Abraham, at that time holding a post as professor of rational mechanics at the University of Milan, was himself working on a new theory of gravitation on which he had already published.[23] Although Einstein and Abraham were in severe disagreement about the foundations the new theory of gravitation should build upon, some empirical consequences of Abraham's theory coincided with those Einstein had derived from his more general considerations. Thus, in his first publication on the matter, Abraham discusses the bending of light in a gravitational field that follows from Huygens' principle whenever the speed of light is assumed to be variable, and, in a footnote, points out that Einstein has drawn the astronomers' attention to the fact that the bending of star light in the gravitational field of the Sun may be observable (Abraham 1912, 2).

There are two remnants of Schwarzschild's correspondence with Abraham found in Schwarzschild's Nachlass. The first is a draft in Schwarzschild's hand of a letter most probably addressed to Abraham,[24] the second is a letter from Abraham to Schwarzschild, dated October 13, 1912 (N Briefe 5). From Schwarzschild's draft it becomes apparent that Abraham had previously raised the question whether there would be an effect recognizable through astronomical observation if the Sun's loss of inertial mass was proportional to the energy it radiates away, while

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[23] His first publication on that matter being (Abraham 1912).

the gravitational mass did not change in this proportion. In his letter from October 13, 1912, Abraham formulated another idea, arguing that the energy loss of the planets when cooling down in the process of the genesis of the solar system must have diminished the inertial and the gravitational mass in equal proportion, since otherwise Kepler's third law of planetary motion could not be valid. Finally, in their correspondence, the two discussed the possible shift of spectral lines in a gravitational field. Here, as elsewhere in his correspondence and writings prior to Einstein's perihelion result, Schwarzschild is very sceptical about the astronomical detectability of the predicted effects, although he appears to regard the nascent theory of relativity with openness. Thus, as concerns the red shift in the solar spectrum, Schwarzschild writes:

> The shift of the wavelengths on the Sun that Einstein demands, exists [...] due to a strange coincidence in exactly the right magnitude. There is, however, no doubt that it is to be blamed partly on pressure and partly on downwards motions in the solar atmosphere. To see more clearly in this respect, one has to study the lines at the different points of the solar disk. Until now, this has only been done in a really sufficient manner for the lines 3933 Å of calcium (St. John, Astrophysical J[ournal).] His results do in fact speak *against* the existence of the sought-after shift. Despite this, I do not want to claim that this is already an absolute veto against the theory. Beforehand, more lines would have to be equally well investigated.[25]

In 1913 Schwarzschild himself started an investigation of exactly the kind he had spoken of in his letter to Abraham, performing a series of observations of the band at 3883 Å in the solar spectrum. The continuation of these observations was foiled by the outbreak of war in 1914, but

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[24] N Briefe 846. The draft is dated in another hand as "1912" and commented on as "possibly [to] W. Lorey." The contents however makes it most probable that it is the draft of a letter to Max Abraham. It may be dated September 29, 1912 (or a little earlier) on the basis of Abraham's letter to Schwarzschild from October 13, 1912, mentioning a letter from Schwarzschild from September 29: most probably the letter that was written on the basis of the draft.

[25] "Die Verschiebung der Wellenlängen auf der Sonne, die Einstein fordert, besteht [...] durch einen merkwürdigen Zufall genau in der richtigen Größe. Es ist aber kein Zweifel, daß dieselbe zum Teil auf Druck, zum Teil auf absteigende Bewegungen in der Sonnenatmosphäre zu schieben sind. Um Klarheit darüber zu bekommen, muß man die Linien an den einzelnen Punkten der Sonnenscheibe studieren. Das ist in wirklich ausreichender Weise bisher nur für die Linien 3933 A. E. des Calciums geschehen (St. John, Astrophysical J. dessen Resultate sprechen durchaus *gegen* die Existenz der gesuchten Verschiebung. Trotzdem möchte ich nicht behaupten, daß hiermit schon ein absolutes Veto gegen die neue Theorie gegeben ist. Es müßten doch erst noch mehr Linien gleich gut untersucht werden." Draft of a letter from Schwarzschild to Abraham, probably September 29, 1912, N Briefe 846, 27v, 28r. St. John's publications in the *Astrophysical Journal* on the motion of calcium vapour in the solar atmosphere are (St. John 1910; 1910–11). For an account on St. John's later work on line shifts and its relation to general relativity, see (Hentschel 1993).

Schwarzschild reported on the results so far obtained in a communication that was presented to the Prussian Academy of Sciences by Einstein on November 5, 1914 (Schwarzschild 1914). In the introduction to this communication, Schwarzschild points out that the observation of the solar spectrum is of interest not only for the sake of solar physics, but "can, according to Mr. Einstein, inform us about the relativity of the world".[26] By referring to Einstein's article on the influence of gravitation on the propagation of light (Einstein 1911), Schwarzschild explicitly relates the gravitational red shift to the principle of equivalence. However, once more Schwarzschild does not conclude affirmatively, describing his preliminary results and other astronomers' observations he reports on as still being indecisive concerning the gravitational red shift.

In his correspondence with Max Planck in 1913, Schwarzschild even more clearly expresses his doubts concerning an astronomical verification of Einstein's theory. In a letter from January 31, 1913, Planck had asked Schwarzschild for an assessment of the feasibility and the expenses of the eclipse expedition that Erwin Freundlich was planning for the year 1914 and for which he was going to apply to the *Preussische Akademie der Wissenschaften* for funding (N Briefe 593, 2r, v). Freundlich intended to search for a deflection of starlight near the solar disk as predicted by Einstein. Schwarzschild commented on the observational side of the problem in the following way:

> In the problem itself I also have no particular confidence. The diminution of the frequency on the Sun and the shift to red of all spectral lines on the Sun that Einstein assumes can be regarded as refuted by the observations. The last word has not yet been spoken, but the shifts which for single lines are also to violet, can be too well interpreted as being due to pressure. Since this whole thing looks rather fishy, it won't be much different for the deflection of light rays by the Sun's gravitation.[27]

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[26] "[...] kann nach Hrn. Einstein auch Auskunft über die Relativität der Welt geben." (Schwarzschild 1914, 1201.)

[27] "Auch zum Probleme selbst habe ich kein besonderes Fiduz. Die Verminderung der Schwingungszahl auf der Sonne und die [...] Verschiebung aller Spektrallinien nach Rot auf der Sonne, die Einstein annimmt [...] kann als durch die Beobachtungen als widerlegt angesehn werden. Das letzte Wort ist noch nicht gesprochen, aber die [...] Verschiebungen, die [...] bei einzelnen Linien auch nach Violett gehen, lassen sich zu gut als Druckverschiebungen deuten. Da es hiermit ziemlich faul [?] steht, wird es mit der Ablenkung der Lichtstrahlen durch die Sonnengravitation auch nicht viel anders sein." Draft of a letter from Schwarzschild to Planck, after January 31, 1913, N Briefe 593, 6r. The passages omitted are crossed-out in Schwarzschild's manuscript.

When Einstein succeeded in deriving the correct value for Mercury's perihelion shift from his theory, Schwarzschild's appraisal of the new theory of relativity changed drastically. Einstein presented his calculation of Mercury's perihelion advance to the Prussian Academy of sciences on November 18, 1915. Schwarzschild was on leave from his military duties at the Russian front and attended the meeting.[28] Back in Russia, Schwarzschild wrote to Einstein:

> It is quite a wonderful thing that from such an abstract idea the Mercury anomaly emerges so stringently.[29]

In a letter of the same day to Arnold Sommerfeld, Schwarzschild even explicitly states that to him the perihelion result was much more convincing than the empirical consequences of Einstein's theory discussed earlier:

> Did you see Einstein's paper on the motion of Mercury's perihelion in which he obtains the observed value correctly from his last theory of gravitation? That is something much closer to the astronomers' heart than those minimal line shifts and ray bendings.[30]

It is a matter of course that the quantitatively appropriate explanation of an anomaly that had been detected by astronomers more than half a century earlier provided a stronger argument for the new theory than the hardly detectable effects that it also predicted. However, Einstein's successful calculation of the perihelion shift did not convince everybody to the same degree as Schwarzschild. More than one year after Einstein's calculation, Max von Laue still described the result as a "coincidence of two single numbers"[31] which

> remarkable as it may be, does not seem to us to give sufficient reason to change the whole physical world picture in its foundations, as Einstein's theory does.[32]

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[28] See the minutes of the meeting on November 18, 1915, *Archiv der Berlin-Brandenburgischen Akademie der Wissenschaften* II–V, Vol. 91, 64–66.

[29] "Es ist eine ganz wunderbare Sache, daß von so einer abstrakten Idee aus die Erklärung der Merkuranomalie so zwingend herauskommt." Schwarzschild to Einstein, December 22, 1915, CPAE 8, Doc. 169.

[30] "Haben Sie Einstein's Arbeit über die Bewegung des Merkurperihels gesehen, wo er den beobachteten Wert richtig aus seiner letzten Gravitationstheorie heraus bekommt? Das ist etwas, was den Astronomen viel tiefer zu Herzen geht, als die minimalen Linienverschiebungen und Strahlenkrümmungen." Schwarzschild to Sommerfeld, December 22, 1915, München, Deutsches Museum Archiv NL 89, 059, p. 1.

[31] "Übereinstimmung zwischen zwei einzelnen Zahlen"

[32] "[...] scheint uns, so bemerkenswert sie ist, doch kein hinreichender Grund, das gesamte physikalische Weltbild von Grund aus zu ändern, wie es die Einsteinsche Theorie tut." (Laue 1917, 269.)

In view of Schwarzschild's earlier contextualization of perihelion motions, it becomes understandable why, for him, Einstein's result signified much more than the "coincidence of two single numbers." Furthermore, on the background of Schwarzschild's prerelativistic cosmological considerations that included the application of non-Euclidean geometry to physical space and the idea of mutually accelerated inertial systems, the changes brought about by Einstein's theory must have appeared less drastic to Schwarzschild than to most others, including von Laue.

Einstein's derivation of the perihelion advance which was based on an approximation had, however, one blemish: the uniqueness of the solution remained questionable. In order to consolidate Einstein's result, Schwarzschild tried to prove the uniqueness of the solution. In the above-mentioned letter to Sommerfeld, Schwarzschild reports:

> In Einstein's calculation the uniqueness of the solution remains doubtful. In the first approximation, which Einstein makes, the solution, when carried out completely, is even apparently ambiguous—one additionally gets the beginning of a divergent expansion. I have tried to derive an exact solution, and that was unexpectedly easy.[33]

And even in his publication that is today known for containing the first derivation of an exact nontrivial solution of Einstein's field equations, Schwarzschild emphasizes that, rather than the quest for an exact solution, it is the consolidation of Einstein's result which is of primary concern:

> It is always convenient to possess exact solutions of a simple form. More important is that the calculation yields, at the same time, the uniqueness of the solution about which Mr. Einstein's treatment remained doubtful and which arguably, in view of the way in which it emerges below, could hardly have been proven by such an approximative method.[34]

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[33] "Bei Einstein's Rechnung bleibt die Eindeutigkeit der Lösung noch zweifelhaft. In der ersten Annäherung, die Einstein macht, ist die Lösung sogar, wenn man sie vollständig macht, scheinbar mehrdeutig — man bekommt noch den Anfang einer divergenten Entwicklung herein. Ich habe versucht, eine strenge Lösung abzuleiten, und das ging unerwartet einfach." Schwarzschild to Sommerfeld, December 22, 1915, München, Deutsches Museum Archiv NL 89, 059, 1. In his letter to Einstein from December 22, 1915, Schwarzschild reports in even more detail about the motivation that led him to his exact solution, see (CPAE 8, Doc. 169).

[34] "Es ist immer angenehm, über strenge Lösungen einfacher Form zu verfügen. Wichtiger ist, daß die Rechnung zugleich die eindeutige Bestimmtheit der Lösung ergibt, über die Hrn. Einsteins Behandlung noch Zweifel ließ, und die nach der Art, wie sie sich unten einstellt, wohl auch nur schwer durch ein solches Annäherungsverfahren erwiesen werden könnte." (Schwarzschild 1916, 190.)

6. The Relativity of Rotation Revisited

After his consolidation of the connection between general relativity and observational astronomy established by Einstein's perihelion calculation, Schwarzschild turned to other questions of theoretical astronomy for which general relativity appeared to provide the adequate framework. One of these questions concerned the relativity of rotation, a question we already encountered in Schwarzschild's prerelativistic work.

The major source documenting Schwarzschild's work on this question in the context of general relativity is a formerly unrecognized manuscript page in Schwarzschild's Nachlass in the University Library of Göttingen (N 2:2, 12r). A reproduction with explanations of the page is given in the Appendix. The page is full of calculations and contains hardly any text. It is found among a few similar pages, some of which contain notes on general relativity the purpose of which however is not obvious. The notes on the page under discussion are undated but obviously stem from the short period between Einstein's successful perihelion calculation in November 1915 and Schwarzschild's death in May 1916.

In these notes, Schwarzschild distinguishes an "inner" and an "outer" metric. The inner metric describes a Minkowski spacetime in a coordinate system rotating with constant angular velocity n. Using cylindrical coordinates, the outer metric can be written as

$$g_{\mu\nu_{\mathrm{outer}}} = \begin{bmatrix} -f_1 & 0 & 0 & 0 \\ 0 & -f_2 & 0 & f \\ 0 & 0 & -1 & 0 \\ 0 & f & 0 & f_4 \end{bmatrix},$$

where x_1 is a radial coordinate, x_2 is an angle, x_3 is parallel to the symmetry axis of the cylinder, and x_4 is a timelike coordinate. f and f_i, $i = 1, 2, 4$, are functions of x_1 only. For the radial coordinate becoming infinitely large, Schwarzschild imposes the condition that the outer metric tends to the (non-rotating) Minkowski metric, rescaled in such a way that it satisfies the determinant condition, $|\det(g_{\mu\nu})| = 1$.[35] The spacetime Schwarzschild attempts to investigate thus consists of a cylindrical section of Minkowski space, the inner space, rotating with constant angular velocity n relative to an inertial frame at radius infinity and surrounded by an outer space, becoming Minkowskian for $x_1 \to \infty$.

Schwarzschild then obviously tries to find general expressions for the metric functions f and f_i, $i = 1, 2, 4$. In this he follows exactly the procedure he elaborated in his publication on the field of a point mass (Schwarzschild 1900). First, he constructs the Lagrangian of a point particle

$$F = \frac{1}{2}\sum g_{\mu\nu}\frac{dx_\mu}{ds}\frac{dx_\nu}{ds}.$$

Next, he calculates $\partial F/\partial x_i$ and $\partial F/\partial \dot{x}_i$ for $i = 1, 2, 3$ and puts the resulting terms into the variational equation

$$\frac{\partial F}{\partial x_i} - \frac{d}{ds}\left(\frac{\partial F}{\partial \dot{x}_i}\right) = 0.$$

He then manipulates the resulting equations—further using the determinant condition $f_1(f_2 f_4 + f^2) = 1$—in such a way that he may read off the field strengths $\Gamma^\lambda_{\mu\nu}$ by a comparison of the coefficients with those appearing in the equations of motion of a point particle. He then puts the expressions for the field strengths into the field equations and manipulates them in order to determine the functions f and f_i by integration. However, while in the case of the spherically symmetric vacuum field this procedure led to a simple result, in this case the differential equations become rather involved and no solution is obtained. The calculations on the page discussed end with a second order differential equation coupling f and f_1 (see the equation at the bottom right hand side of Schwarzschild's manuscript page reproduced in the Appendix).

What physical situation is Schwarzschild trying to describe here? One might think of an infinitely long rotating cylindrical mass shell which Schwarzschild could have considered in order to address the conceptual problem of the relativity of rotation in general relativity.[36] (Note that

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[35] As in (Schwarzschild 1916), Schwarzschild here employs the condition that the determinant of the metric tensor be unity. Einstein had introduced this condition in an addendum to his publication *Concerning the Theory of General Relativity* (*Zur allgemeinen Relativitätstheorie*, Einstein 1915a), and continued to use it in his paper on the perihelion motion of Mercury (Einstein 1915b). The left-hand-side of the field equations Einstein presented and used in these papers consists of the Ricci Tensor only and is thus not the one of the final theory. Since in vacuum these older field equations coincide with those of the final theory, Einstein and Schwarzschild's solutions still hold there.

[36] The interior field of an infinitely long, rotating, cylindrical shell of matter is indeed Minkowskian; see (Davies et al. 1971). The exterior vacuum metric of such a matter distribution, including the dragging of inertial frames close to the shell, is discussed in (Frehland 1972). Non-local effects of such a matter distribution, corresponding to the Aharonov-Bohm effect in electrodynamics, are discussed in (Stachel 1983). The problem of the relativity of rotation was addressed in 1918 by Thirring who considered a rotating spherical mass shell rather than a cylindrical one (Thirring 1918, see also Lense and Thirring 1918).

Schwarzschild sets the g_{33}-component of the outer metric to -1.) Possibly, this spacetime should function as a model for the spatially two-dimensional situation of a rotating disk of Minkowski space. One may then interpret Schwarzschild's calculations as the exploration of a simple model for the spacetime within and outside the rotating system of fixed stars.[37]

The fact that Schwarzschild considers the spacetime within the system as approximately Minkowskian is consistent with his earlier observation that the perihelia of remote double stars do not rotate relative to the directions defined by the planetary orbits in the solar system and that therefore inertial frames inside the Galaxy do not rotate with respect to one another. Now, on one hand, Schwarzschild contended that, by analogy to other celestial motions, it must be assumed that the system of fixed stars as a whole rotates.[38] Yet, on the other hand, such a rotation has hardly been observed, as Schwarzschild explained in an earlier text:

> [...] it turns out that the average of those few thousand stars, whose proper motions are known, displays no evidence of rotation with respect to [the] directions [defined by the planetary orbits] [...].[39]

General relativity now provided a possible explanation of this phenomenon, if it was assumed that, together with the stars themselves, the global inertial system within the Galaxy was rotating. In searching for the functions f and f_i describing the outer metric, Schwarzschild would then have attempted to clarify in what sense one may speak in general relativity of a rotation of the system of fixed stars as a whole.

That Schwarzschild indeed considered the question of the rotation of the Galaxy in the context of general relativity is made evident by a letter from Einstein dated January 9, 1916.[40] In a preceding letter by Schwarzschild which is lost, Schwarzschild must have raised several questions, which Einstein answers one by one. Einstein's second point reads as follows:

> The statement that "the system of fixed stars" is free of rotation may retain a relative meaning, which is to be fixed by a comparison.

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[37] The spacetime of an axially symmetric distribution of particles revolving with constant angular velocity was later derived by (Stockum 1937).

[38] See, for example, (Schwarzschild 1897, 519).

[39] "[...] zeigt sich, dass der Durchschnitt aus jenen paar Tausend Sternen, deren Eigenbewegung man kennt, [...] keine Rotation gegen diese Richtungen aufweist." (Schwarzschild 1897, 520.)

[40] Einstein to Schwarzschild, January 9, 1916, N 193, 3-5, see also (CPAE 8, Doc. 181).

> The surface of the Earth is irregular, as long as I regard very small sections of it. However, it approaches the flat elementary shape when I regard larger sections of it, whose dimensions are still small in comparison to the length of the meridian. This elementary shape becomes a curved surface when I regard even larger sections of the Earth's surface.
>
> For the gravitational field things are similar. On a small scale the individual masses produce gravitational fields that, even with the most simplifying choice of the reference system, reflect the character of the quite irregular matter distribution on the small scale. If I consider larger regions, like astronomy presents them to us, the Galilean reference system provides me with the analogue to the flat elementary shape of the Earth's surface in the previous comparison. But if I consider even larger regions, there probably will be no continuation of the Galilean system to simplify the description of the universe to the same degree as on a small scale, that is, throughout which a mass point sufficiently remote from other masses moves uniformly in a straight line.[41]

Schwarzschild's response is consistent with the calculations as interpreted above:

> As concerns the inertial system, we are in agreement. You say that beyond the Milky Way system conditions may arise under which the Galilean system is no longer the simplest. I only hold that within the Milky Way system such conditions do not arise.[42]

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[41] "Die Aussage, dass "das Fixsternsystem" rotationsfrei sei, behält wohl einen relativen Sinn, der durch ein Gleichnis festgelegt sei.
Die Oberfläche der Erde ist, solange ich ganz kleine Teile derselben ins Auge fasse, unregelmässig. Sie nähert sich aber der ebenen Grundgestalt, wenn ich grössere Teile ins Auge fasse, deren Abmessungen aber immer noch klein sind gegen die Länge des Meridians. Diese Grundgestalt wird zu einer gekrümmten Fläche, wenn ich noch grössere Teile der Erdoberfläche ins Auge fasse.
So ähnlich ist es auch mit dem Gravitationsfeld. Im Kleinen liefern die einzelnen Massen Gravitationsfelder, welche auch bei möglichst vereinfachender Wahl des Bezugssystems den Charakter der ziemlich regellosen Verteilung der Materie im Kleinen widerspiegeln. Betrachte ich grössere Gebiete, wie sie uns die Astronomie bietet, so bietet mir das Galileische Bezugssystem das Analoge zu der ebenen Grundgestalt der Erdoberfläche beim vorigen Vergleich. Betrachte ich aber noch grössere Gebiete, so wird es wohl keine Fortsetzung des Galileischen Systems geben, welche in solchem Masse wie im Kleinen die Beschreibung der Welt einfach gestaltet d.h. in welchem überall der von anderen Massen hinlänglich entfernte Massenpunkt sich gradlinig gleichförmig bewegt." Einstein to Schwarzschild, January 9, 1916, CPAE 8, Doc. 181.

[42] "Was das Inertialsystem angeht, so sind wir einig. Sie sagen, daß jenseits des Milchstraßensystems sich Verhältnisse einstellen können, in denen das Galilei'sche System nicht mehr das einfachste ist. Ich behaupte nur, daß sich innerhalb des Milchstraßensystems solche Verhältnisse nicht einstellen." Schwarzschild to Einstein, February 6, 1916, N 193, 7–8, see also CPAE 8, Doc. 188.

In view of this exchange between Schwarzschild and Einstein, it appears obvious that Schwarzschild's calculations are related to the question he must have posed to Einstein: Does it make sense to speak of a rotation of the system of fixed stars? The calculations then document the attempt to explore the "even larger regions" outside the system of fixed stars, in which "there probably will be no continuation of the Galilean system."

7. A Closed Universe as a Solution of Einstein's Field Equations

In his calculations, Schwarzschild had assumed the Universe to be asymptotically Minkowskian. In his correspondence with Einstein on the question of global frames of inertia, Schwarzschild mentions a further possibility. In direct continuation of the passage quoted above, he explicates:

> As concerns very large spaces, your theory has a quite similar position as Riemann's geometry, and you are certainly not unaware that one obtains an elliptic geometry from your theory, if one puts the entire universe under uniform pressure (energy tensor $-p, -p, -p, 0$).[43]

Thus, Schwarzschild was the first to entertain the possibility of a closed universe with an elliptic geometry as a solution to Einstein's field equations. Schwarzschild's remark that Einstein's theory had a similar position as Riemann's geometry thereby alludes to his prerelativistic application of elliptic geometry to the universe on the background of Riemannian geometry discussed in Section 4.

Contrary to Schwarzschild's assumption, Einstein was, at the time, most probably unaware of such cosmological implications of his theory.[44] It was only through a debate with the Dutch astronomer Willem de Sitter (1872–1934) beginning in fall 1916 that Einstein was led to consider

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[43] "Was die ganz großen Räume angeht, hat Ihre Theorie eine ganz ähnliche Stellung, wie Riemann's Geometrie, und es ist Ihnen gewiß nicht unbekannt, daß man die elliptische Geometrie aus Ihrer Theorie herausbekommt, wenn man die ganze Welt unter einem gleichförmigen Druck stehen läßt (Energietensor $-p, -p, -p, 0$)." Schwarzschild to Einstein, February 6, 1916, N 193, 7–8, see also CPAE 8, Doc. 188. This energy tensor actually does not yield a spherical static universe. It does, however, yield the universe Schwarzschild is talking of when the trace term in the field equations is neglected, i.e. in the context of the older field equations, the left hand side of which equals the Ricci tensor. It may be the case that Schwarzschild originally conceived of the tensor on the basis of these field equations and later did not modify it as the new field equations would have demanded. Schwarzschild continues his letter by explaining the solution inside a sphere of fluid with uniform energy density (energy tensor $-p, -p, -p, \rho_0$). Here, as well as in the corresponding publication (Schwarzschild 1916a, 431–432), Schwarzschild points out that inside the sphere spherical geometry applies.

a closed universe which he hesitantly proposed in 1917 (Einstein 1917).[45] The distinction between spherical and elliptic space had thereby remained obscure to him. De Sitter pointed the distinction out to Einstein, referring to Schwarzschild's 1900 paper on the curvature of space and the argument for preferring elliptic to spherical space given therein (de Sitter to Einstein, June 20, 1917, CPAE 8, Doc. 355).

In Einstein's debate with de Sitter, the question of the global geometry of the universe emerged from a discussion of the relativity of inertia. Strikingly, in Schwarzschild's correspondence with Einstein, the question of the global geometry of the Universe is brought up in exactly the same context.[46] It is therefore tempting to see here the commencement of an Einstein–Schwarzschild debate foreshadowing the later Einstein–de Sitter debate. Einstein appears, however, to have not yet been prepared to consider the cosmological implications of his theory at that time. And by the time he was slowly pushed into that direction in his exchange with the astronomer de Sitter, Schwarzschild had already died. Nevertheless, in view of Schwarzschild's deliberations discussed here, it seems safe to say that, had Schwarzschild lived longer, he could have made a substantial contribution to the cosmological debates emerging later.

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[44] In November 1916, Einstein still calls the question of the boundary conditions of the metric field "purely a matter of taste that will never attain a scientific meaning." ("eine reine Geschmacksfrage, die nie eine naturwissenschaftliche Bedeutung erlangen wird.") Einstein to de Sitter, November 4, 1916, CPAE 8, Doc. 273.

[45] On the Einstein–de Sitter debate, see (CPAE 8, 351–357) and the references given therein, in particular (Kerszberg 1989; 1989a).

[46] In Einstein's letter to Schwarzschild from January 9, there is, in fact, a passage in which he expresses exactly the kind of strong Machian claims concerning his theory which later sparked off his debate with de Sitter. In direct continuation of the passage quoted above, Einstein explains: "According to my theory, inertia is an interaction between masses, in the end, not an effect in which, besides the mass under consideration, 'space' itself would be involved. The essence of my theory is precisely that no independent properties are attributed to space itself.

Jokingly one may put it this way. If I let all things in the world disappear, according to Newton the Galilean inertial space remains, according to my perception, however, *nothing* remains."

("Die Trägheit ist eben nach meiner Theorie im letzten Grunde eine Wechselwirkung der Massen, nicht eine Wirkung bei welcher ausser der ins Auge gefassten Masse der 'Raum' als solcher beteiligt ist. Das Wesentliche meiner Theorie ist gerade, dass dem Raum als solchem keine selbständigen Eigenschaften gegeben werden.

Man kann es scherzhaft so ausdrücken. Wenn ich alle Dinge aus der Welt verschwinden lasse, so bleibt nach Newton der Galileische Trägheitsraum, nach meiner Auffassung aber *nichts* übrig.") Einstein to Schwarzschild, January 9, 1916, (CPAE 8, Doc. 181).

8. Schwarzschild's Interdisciplinary Approach to the Foundations of Science

Let us come back to the question raised at the beginning: Why did Schwarzschild recognize the significance of general relativity at such an early stage? Here it has been attempted to show that, already in his early astronomical work, Schwarzschild did not act as a specialist but attempted to meet the challenges resulting from the implications of foundational questions in physics on astronomy. Thus it comes as no surprise that Schwarzschild was also among the first to recognize that Einstein had—without being aware of it—provided the astronomers with the adequate framework for treating their questions. As a result, a clear continuity can be perceived in Schwarzschild's work on cosmology, prerelativistic and relativistic. In this context it is interesting to question the extent to which parallel cases are provided by the work of other pioneers of relativistic astronomy such as Willem de Sitter and Arthur Eddington (1882–1944). The study of this question, however, does not lie in the scope of this contribution. Here, in conclusion, it shall only be pointed out that it was no coincidence that Schwarzschild took an astronomical road to general relativity, but that this may rather be seen as the natural outcome of his interdisciplinary approach to the foundations of the exact sciences.

Indeed, not only is interdisciplinarity the hallmark of Schwarzschild's scientific work, but he also was quite aware of the general significance of interdisciplinarity for the progress of science. On many occasions Schwarzschild explained how he saw scientific progress emerging from the interplay of the different branches of science, for instance when, on the occasion of his admission speech at the Prussian Academy of Sciences, he stated that "the greatest yet unsolved problem of celestial mechanics, the so-called many-body problem, most closely touches a problem of physics that concerns the foundations of its newest developments".[47] As a further example, consider the following passage from the same speech in which Schwarzschild describes the establishment of special relativity:

> [...] an important source for the electron and relativity theory lay in an astronomical problem. The astronomical aberration results from the finite propagation speed of light through the ether in combination with the Earth's motion in space. H.A. Lorentz occupied himself many times with the theory of aberration and searched for a satisfying picture of the ether's behavior when large masses, like the

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[47] "[...] berührt sich das höchste noch ungelöste Problem der Himmelsmechanik, das sogenannte Vielkörperproblem, aufs engste mit einem Problem der Physik, das an die Fundamente ihrer neuesten Entwicklung greift." (Schwarzschild 1913, 597.)

> Earth, move through it, until he finally cut the knot by consistently implementing Fresnel's assumption that the ether is absolutely rigid and cannot be brought to flow by any force acting on it. In this way the path was cleared for the electron theory. Furthermore, the completely rigid ether stepped out of the circle of the objects that can be influenced and thus can be more closely perceived, so much so that relativity theory became possible, in which the concept of the ether only appears as a space-time concept deepened by new experience.
>
> Electron theory and relativity theory in turn have already posed various problems to astronomy as a consequence of the modifications of celestial mechanics they necessitate.[48]

Clearly Schwarzschild was equipped to take up these challenges posed to astronomy by its neighboring disciplines. He knew that scientific progress is not a matter of the advancement of isolated disciplines, as both the previous and the following quotations make clear:

> Mathematics, physics, chemistry, astronomy march in line. Whichever lags behind is pulled forward. Whichever hastens ahead pulls the others forward. The closest solidarity exists between astronomy and the whole circle of exact sciences.[49]

Acknowledgements

I am indebted to Jürgen Renn who inspired and supported my work from its inception up to its present state. For helpful discussions and comments I would also like to thank Dieter Brill, Giuseppe Castagnetti, Peter Damerow, Jürgen Ehlers, Hubert Goenner, Michel Janssen, John

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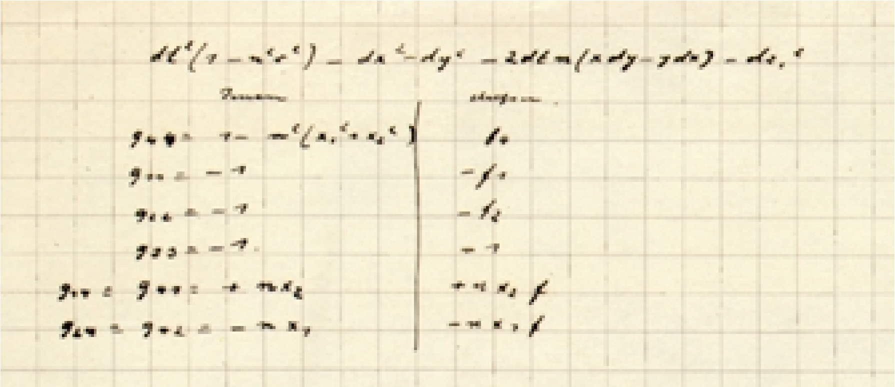
48 "[...] eine wichtige Quelle für die Elektronen- und Relativitätstheorie in einem astronomischen Probleme lag. Die astronomische Aberration ist eine Folge der endlichen Ausbreitungsgeschwindigkeit des Lichtes im Äther verbunden mit der Bewegung der Erde im Weltraum. H.A. Lorentz hat sich vielfach mit dem Problem der Aberration beschäftigt und nach einer befriedigenden Anschauung über das Verhalten des Äthers, wenn große Massen, wie die Erde, sich durch ihn hindurchbewegen, gesucht, bis er schließlich den Knoten zerhieb durch völlig konsequente Durchführung der alten Fresnelschen Annahme, daß der Äther absolut starr und durch keine auf ihn wirkende Kraft zum Fließen zu bringen sei. Dadurch war die Bahn frei geworden für die Elektronentheorie. Der völlig starre Äther trat ferner so sehr aus dem Kreis der beeinflußbaren und damit näher erkennbaren Objekte heraus, daß auch die Relativitätstheorie möglich wurde, bei welcher der Begriff des Äthers nur als ein durch neue Erfahrungen vertiefter Raum-Zeitbegriff erscheint.
Elektronentheorie und Relativitätstheorie haben auch Rückwärts der Astronomie schon wieder mancherlei Probleme gestellt infolge der Modifikationen der Himmelsmechanik, die sie notwendig machen." (Schwarzschild 1913, 598.)

49 "Mathematik, Physik, Chemie, Astronomie marschieren in einer Front. Wer zurückbleibt, wird nachgezogen. Wer vorauseilt, zieht die anderen nach. Es besteht die engste Solidarität der Astronomie mit dem ganzen Kreis der exakten Naturwissenschaften." (Schwarzschild 1913, 599.)

Norton, and John Stachel. For permission to reproduce a page of Schwarzschild's Nachlass I am grateful to the Manuscript Department of the Niedersächsische Staats- und Universitätsbibliothek Göttingen.

Appendix:
A Manuscript Page from Schwarzschild's Nachlass

Annotated reproduction of manuscript page N 2:2, 12r (for technical reasons the reproduction is divided into three parts).

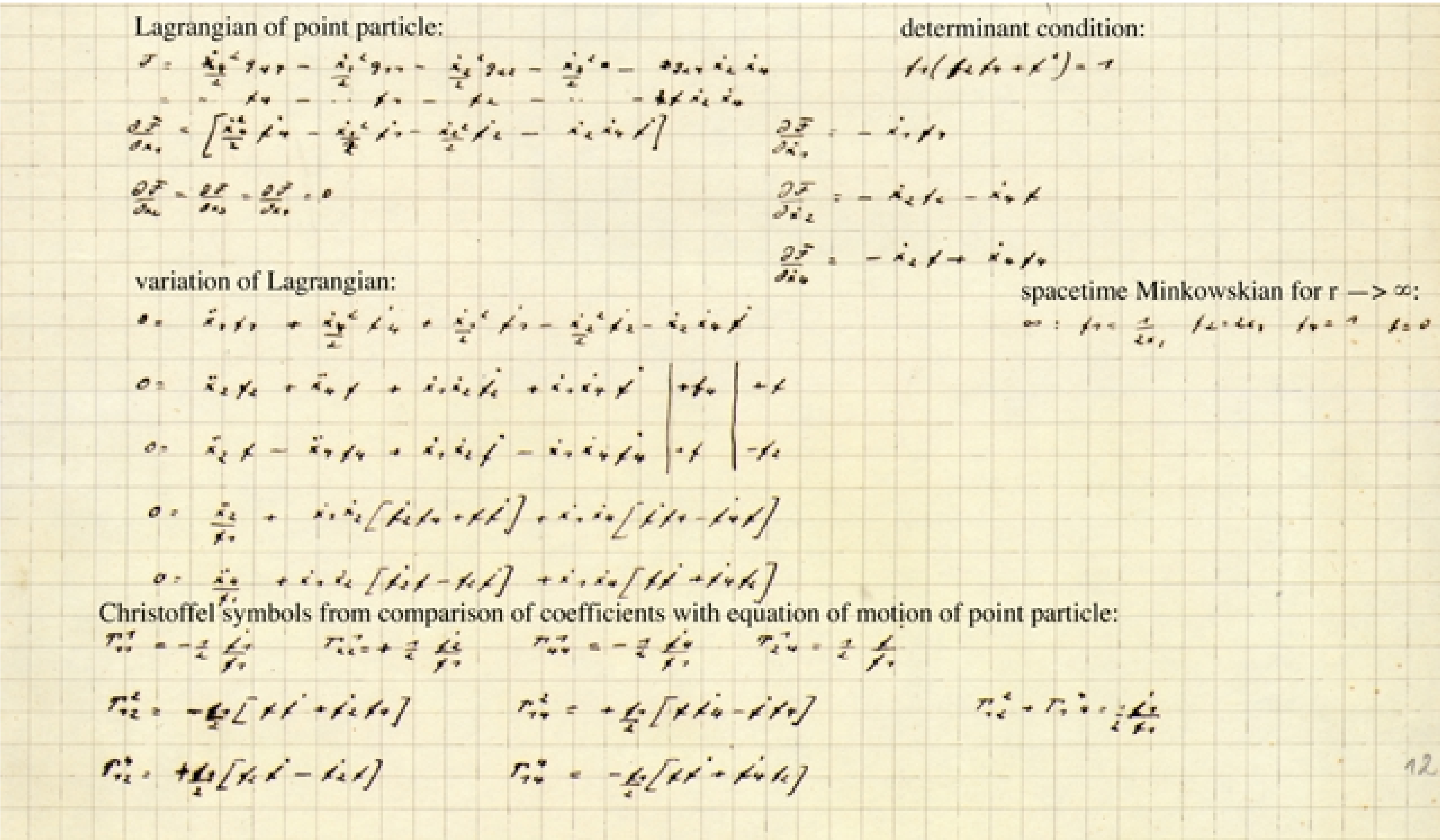


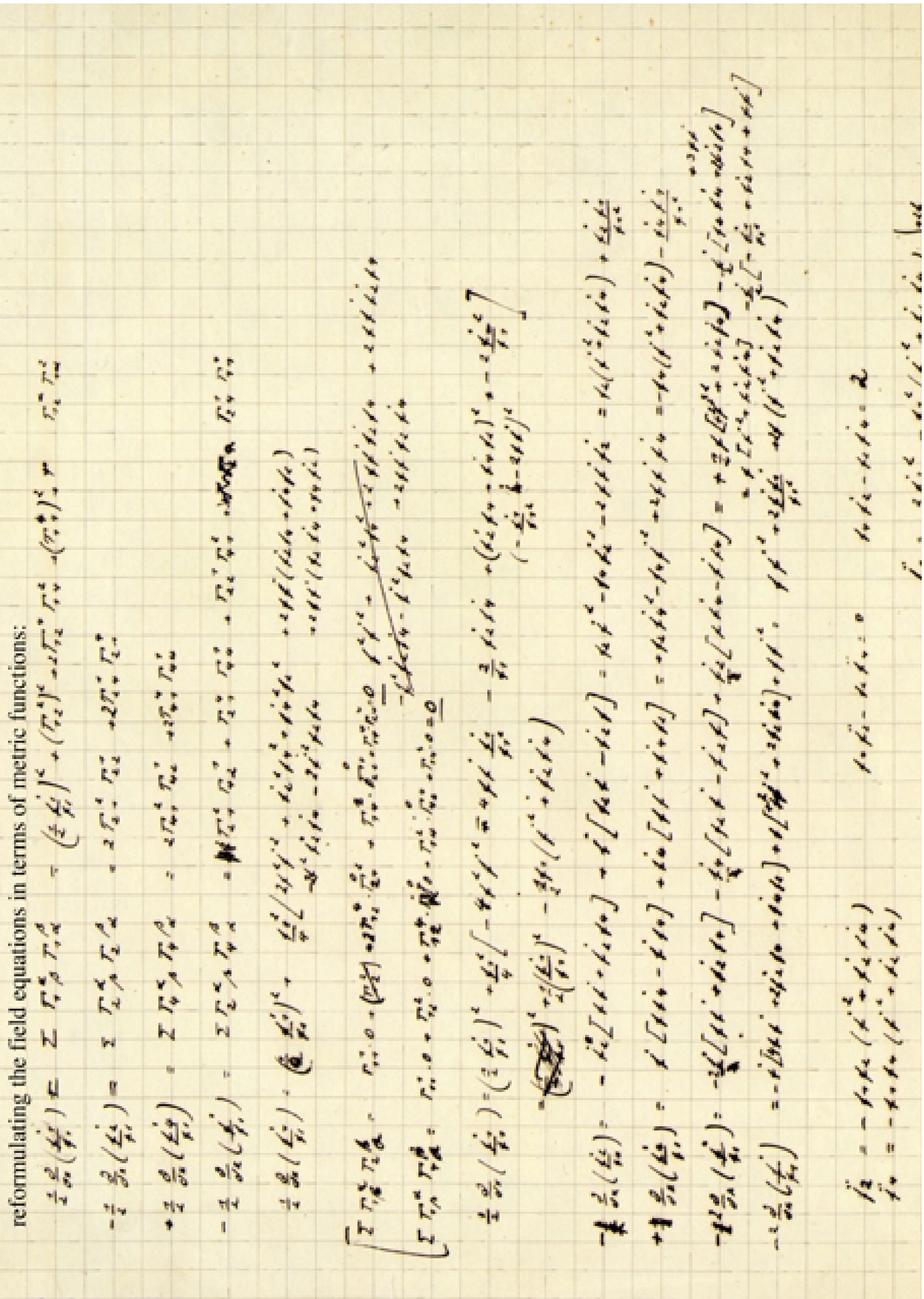
$$g_{\mu\nu_{\text{inner}}} = \begin{bmatrix} -1 & 0 & 0 & nx_2 \\ 0 & -1 & 0 & -nx_1 \\ 0 & 0 & -1 & 0 \\ nx_2 & -nx_1 & 0 & 1-n^2r^2 \end{bmatrix} \qquad g_{\mu\nu_{\text{outer}}} = \begin{bmatrix} -f_1 & 0 & 0 & nx_2 f \\ 0 & -f_2 & 0 & -nx_1 f \\ 0 & 0 & -1 & 0 \\ nx_2 f & -nx_1 f & 0 & f_4 \end{bmatrix}.$$

For the following calculation, Schwarzschild changes from the cartesian coordinates used above to cylindrical coordinates and absorbs the factors of n in function f. The outer metric then reads:

$$\downarrow$$

$$g_{\mu\nu_{\text{outer}}} = \begin{bmatrix} -f_1 & 0 & 0 & 0 \\ 0 & -f_2 & 0 & f \\ 0 & 0 & -1 & 0 \\ 0 & f & 0 & f_4 \end{bmatrix}.$$


Lagrangian of point particle:
determinant condition:
variation of Lagrangian:
spacetime Minkowskian for r —> ∞:
Christoffel symbols from comparison of coefficients with equation of motion of point particle:
12



reformulating the field equations in terms of metric functions:

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Einstein in the Daily Press:

A GLIMPSE INTO THE GEHRCKE PAPERS

Milena Wazeck

1. Introduction

The Max Planck Institute for the History of Science has recently acquired what has been preserved of the Ernst Gehrcke papers.[1] All that is known about the history of these papers is that the rest of them were lost in World War II. The following will provide an overview of this material and a glimpse into the papers, in particular into the Gehrcke newspaper article collection. The focus will be on Einstein's opponents in the daily press during the run-up to the centennial of the German Society for Natural Scientists and Physicians in the summer of 1922.

Ernst Gehrcke is known as a fervent critic of Einstein and a leading figure among Einstein's German opponents. In particular his name is linked to a meeting at the Berlin Philharmonic Hall in August 1920, which was organized by the anti-Semitic agitator Paul Weyland and set up chiefly to oppose Einstein.[2]

From 1902 until 1946, Gehrcke was employed at the *Physikalisch-Technische Reichsanstalt*, and became director of the department of optics in 1926. Although Gehrcke, an experimentalist and specialist in optics, is not one of the well-known physicists of the time, his work is recognized through the Lummer-Gehrcke plate, the cathode-ray oscilloscope and the multiplex interference spectroscope.

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[1] These papers will be digitized and made accessible within the framework of a research project at the Max Planck Institute for the History of Science. As part of this project, my dissertation deals with amateur scientists' opposition to the theory of relativity in the early 20th century.

[2] See (Gönner 1993, 107–133; Rowe 2002) for further references. For Weyland see (Kleinert 1993, 198–232). The typescript of Gehrcke's talk with handwritten corrections is preserved in the Gehrcke papers.

Gehrcke's interests outside physics were broad, ranging from patent law and the Paleolithic age to climatic research, which during the 1930s became an increasingly important part of his work. He developed an artificial healing climate, which was applied as therapy for tuberculosis and other respiratory diseases in the Gehrcke climate institutes, which he founded. In fact, the majority of the papers contain material concerning Gehrcke's medical interests, for example, correspondence with patients or medical magazines.

Furthermore, the papers contain:

- numerous offprints and booklets presenting unorthodox theories of space, time and gravitation, some explicitly opposing the theory of relativity,
- correspondence with the physicists Philipp Lenard, Stjepan Mohorovičić, Ludwig Glaser, Hermann Fricke, Johannes Stark, Otto Lummer and the philosophers Oskar Kraus, Melchior Palagyi, Leonore Frobenius-Kühn, and others,
- some drafts and manuscripts by Gehrcke, for example, *Über das Uhrenparadoxon in der Relativitätstheorie* (Gehrcke 1921, 428) and *Die erkenntnistheoretischen Grundlagen der verschiedenen physikalischen Relativitätstheorien* (Gehrcke 1914, 481–487), and
- in addition, all the parts which were rescued from the Gehrcke newspaper article collection.

2. The Newspaper Article Collection

In 1924 Gehrcke's booklet, *Die Massensuggestion der Relativitätstheorie*, appeared. With this he aimed to reveal the theory of relativity as a "suggestion to the masses" introduced by propaganda in the daily newspapers. The newspaper article collection is the material upon which Gehrcke based his booklet (Gehrcke 1924, 1).

To acquire the articles, Gehrcke subscribed to clipping services that sent him all articles containing the keywords "Einstein" or "relativity." Although these clipping services were very representative, they did not present every single newspaper. Other articles from the Gehrcke collection were sent from friends. Since most research on the reception of relativity in the press has so far focused on articles appearing in the major newspapers,[3] the Gehrcke collection emerges as an unusual source due to its extraordinary richness of articles, which also came from small and regional newspapers.

In the following a glimpse is given at parts of this collection. The article collection is made up of twenty-one folders, eight of which were lost during the war. The collection contains altogether about 3000 articles—from the over 5000 constituting the original collection—(Gehrcke 1924, 1). Most of them were mounted and pasted, some were loose, and the majority published in the years 1921 – 1923. Gehrcke organized the folders more or less thematically in preparation for his booklet, in which he focuses on articles covering specific events, such as Einstein's trips to France, Italy, England, America, and Japan, as well as his various lectures.

2.1 The French Folder

What may be called the French Folder is the most extensive, with over 650 articles covering Einstein's trip to France in March 1922, and reflects the overwhelming reception of this trip among the general public. The press (mostly French and German) covered Einstein's schedule in great detail; his arrival, his lectures at the Collège de France, his visit to World War I battlefields, and his cancellation of the meeting at the Académie des Sciences due to a planned boycott by its members. In addition, the daily newspapers provided popular accounts of the theory of relativity, the positions of French scientists on Einstein's theory, as well as anti-German or anti-Semitic sentiments.[4]

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[3] Pais, for example, focused on the *New York Times*. See (Pais 1994); Crelinsten concentrated on the *Times* (London) and the *New York Times*. See (Crelinsten 1980, 115–122; 1980a, 187–193); Elton focused on leading German newspapers, particularly on the *Vossische Zeitung*. See (Elton 1919–1920, 95–102).

[4] For the reaction in France, see in particular (Biezunski 1991).

QUESTIONS DU JOUR

Einstein à Paris

L' « Einsteinisme » et sa théorie

MON REPORTAGE

LE TEMPS N'EST PLUS!

Aimez-vous Einstein ?

ALBERT EINSTEIN.

EINSTEIN SPEAKS BEFORE SORBONNE ON RELATIVITY

Plans Reply To Those Who Attack His Theory

Figure 1: Headlines from the French Folder. *La France*, March 24, 1922; *Presse*, April 10, 1922; *Telegramme*, April 9, 1922; *Chicago Tribune*, April 1, 1922.

Einstein's late arrival, for instance, led to various speculations and anecdotes reported in the press. Under headings such as "The False Einstein"[5] and "The Fuss over Einstein in Paris"[6] it was announced:

> The arrival of Einstein, the first German scholar for whom the Collège de France has given an honorable reception, attracted numerous journalists, photographers and spectators to the Gare du Nord; but the gentleman who stepped off the train was not Einstein but a Polish minister received by members of the embassy. Neither the public nor the photographers recognized the mistake in time. Thus the Polish minister was admired and photographed with an interest he had not expected. A woman from the crowd shook her head and said to me: And all this for a German! It was indeed a surprise to see the alleged German being received by the Polish military attaché in uniform. In fact, Einstein had been in Brussels.[7]

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[5] "Der falsche Einstein", *12 Uhr Mittagszeitung*, March 31, 1922.

[6] "Das Einsteintheater in Paris", *Allgemeine Zeitung für Mitteldeutschland*, April 2, 1922.

Even a purely ironic article such as this is referring to the anti-German sentiments omnipresent at that time in French society. In fact, Einstein was the first German scholar to be officially received in France after World War I when anti-German attitudes were still high. The question of Einstein's nationality (Swiss or German) was extensively discussed by the newspapers after numerous French newspapers presented Einstein to the public as a Swiss mathematician evidently to avoid anti-German sentiments:

> The Société Française de Physique has just invited the celebrated mathematician Einstein to give a series of lectures on the special and general theory of relativity at the Collège de France. Mr. Einstein will arrive in Paris on March 28th. He will give six lectures, one of which will take place at the Société de Physique and one at the Société de Philosophie. He will remain in Paris for ten days. At the Académie des Sciences, Mr. Painlevé will comment on the theories of Einstein in the presence of the Swiss mathematician.[8]

This announcement immediately provoked the German papers to react, regardless of their political affiliation—a point to which I shall return later. The social democratic newspaper *Vorwärts* comments:

> The *Temps* calls Einstein a Swiss mathematician, of course, a kraut would not be allowed to appear in Paris.[9]

And the national conservative *Leipziger Neueste Nachrichten*:

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[7] "Die Ankunft Einsteins als des ersten deutschen Gelehrten, dem das Collège de France einen ehrenvollen Empfang bereitet, hatte zahlreiche Journalisten, Photographen und Zuschauer nach der Gare du Nord gebracht; aber der Herr, der dem Zuge entstieg, war nicht Einstein, sondern ein polnischer Minister, der von Mitgliedern der Gesandtschaft empfangen wurde. Weder das Publikum noch die Photographen erkannten den Irrtum rechtzeitig. So wurde der polnische Minister mit einem Interesse bestaunt und photographiert, das er nicht erwartet hatte. Eine Frau aus dem Volke meinte zu mir kopfschüttelnd: Und das alles für einen Deutschen! Es war in der Tat eine Ueberraschung, den vermeintlichen Deutschen vom polnischen Militärattaché in Uniform empfangen zu sehen. In Wirklichkeit hatte sich Einstein in Brüssel aufgehalten." "Der falsche Einstein", *12 Uhr Mittagszeitung*, March 31, 1922.

[8] "La société française de physique vient d´inviter le célèbre mathématicien Einstein à venir faire, au Collège de France, une série de conférences sur les théories de la relativité simple et généralisée. M. Einstein arrivera à Paris le 28 mars. Il donnera six conférences, dont une à la Société de Physique et une à la Société de Philosophie. Il restera à Paris une dizaine de jour. M. Painlevé fera un commentaire, à l´Académie des Sciences, sur les théories d`Einstein, en présence du savant mathématicien suisse." *Le Temps, Ère Nouvelle, Victoire, Èclair, Petit Parisien*, *Rappel*, March 21, 1922.

[9] "Der "*Temps*" nennt Einstein einen Schweizer Mathematiker, natürlich, ein Boche dürfte in Paris nicht auftreten." *Vorwärts*, March 31, 1922.

> The French hatred of the Germans has now come to a point where it can only be seen as comical. The derogation of everything German can, in the end when faced with the truth, find no other way out than to revert to falsification. This is no longer unspeakably cowardly, but just ridiculous. There is no future for this nation.[10]

The collection also includes folders containing articles on Einstein's trip to America (90 articles), to England in June 1921 (174 articles), to Italy in October 1921 (68 articles), and to Japan in November 1922 (98 articles).[11] As nationalism is a major topic in the reactions to these trips, the articles from the Gehrcke collection provide a hitherto unexploited source for insights into nationalism in science.

2.2 The Movie Folder

In 1922, the Colonna Movie Company in cooperation with a group of scientists[12] produced a movie on the theory of relativity for the general public. It is unclear whether this has been preserved. In any case, one can get an impression of the film's content and impact by the over 70 articles in Gehrcke's folder on the "Einstein movie."

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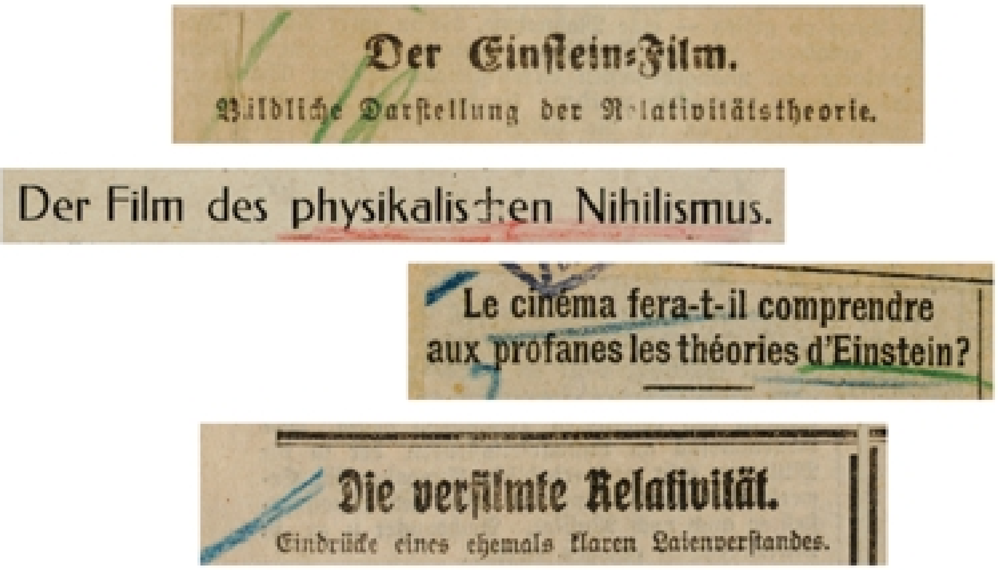
[10] "Hiermit ist der Deutschenhaß der Franzosen an dem Punkt angelangt, wo er nur noch komisch wirkt. Die Verkleinerung alles dessen, was aus Deutschland kommt, die sich schließlich vor der Wahrheit nicht anders helfen kann als dadurch, daß sie fälscht. Das ist nicht mehr unsäglich erbärmlich, das ist nur noch lächerlich. Dieser Nation kann nicht die Zukunft gehören." *Leipziger Neueste Nachrichten*, March 22, 1922

[11] The small Swedish Folder is unusual in so far as it mainly contains material from the 1930s and 1950s, including articles on the occasions of Einstein's 75th birthday (on March 14, 1954) and his death (on April 18, 1955), some caricatures and cartoons.

[12] Georg Nicolai, Rudolf Lämmel, Otto Buek, Otto Fanta.



Der Einſtein-Film.

Bildliche Darſtellung der Relativitätstheorie.

Der Film des physikalischen Nihilismus.

Le cinéma fera-t-il comprendre aux profanes les théories d'Einstein?

Die verfilmte Relativität.

Eindrücke eines ehemals klaren Laienverſtandes.

Figure 2: Headlines from the Movie Folder. *Vossische Zeitung*, April 6, 1922; *Kino-Rat* No. 9/10, 1922; *Avenir* May 4, 1922; *Berliner Lokal-Anzeiger*, May 8, 1922.

In particular, this early approach to the "Public Understanding of Science" with the help of modern media (with animation and special effects) provoked many satirical reactions such as "Relativity filmed: Impressions of a previously clear layman's mind":[13]

> The accurate clock ... plays an important role in the explanation of the theory of relativity. We are shown that a clock on the street indicates a completely different time than a clock carried by a man riding the subway ... . And if I mention finally that the same train can, at the same time, be twelve, eighteen and then finally even twenty-four meters long, and that all these measurements are correct, I will probably, like the film, have given the layman an illustrative description of the most famous of all theories.[14]

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[13] "Die verfilmte Relativität. Eindrücke eines ehemals klaren Laienverstandes." (Aros 1922.)

[14] "Eine große Rolle bei der Erklärung der Relativitätstheorie spielt ... die richtiggehende Uhr. Es wird uns gezeigt, daß eine Uhr, die auf der Straße geht, ganz andere Zeiten anzeigt, wie eine Uhr, die ein Mann bei sich hat, wenn er mit der Untergrundbahn zu fahren hat. ... Wenn ich schließlich noch erwähne, daß ein und derselbe Eisenbahnzug einmal zwölf, dann wieder achtzehn und schließlich sogar vierundzwanzig Meter lang sein kann, und daß all diese Maße richtig sind, werde ich wohl ebenso wie der Film dem Laien eine anschauliche Darstellung der berühmtesten aller Theorien gegeben haben." (Aros 1922.)

This article is somewhat unclear about whether the satire applies to the film medium or to the theory of relativity, while other statements about the film are clearly primarily intended to go against Einstein's theory. Polemics such as "The Film of Physical Nihilism" [15] (*Kino-Rat* 1922) equated the theory of relativity with ethical relativism:

> Einstein creates a universe using the imperfection of our sensory perception. He preaches to us: All your perceptions are relative, therefore you must construct a relative universe following my recipe. That is nothing but the most unproductive scientific nihilism and in accordance with the political past of the professor, who belongs to political parties, which intend to relativize the national sense of honor... All Einsteinians with their comprehension-simulating bolshevik-zionist clique cannot deny the fact that time, space and matter exist infinitely and that, from a given center, one can indeed develop an absolute world-view.[16]

This fabrication of a close connection between Einstein's "dubious character" and the "relativism" of the theory of relativity is the basic structure of argumentation in the anti-Semitic attacks against Einstein, which are already rife in the early 20s.[17]

2.3 The Eclipse Folder

The Eclipse Folder is of particular interest with regards the public discussions of the three experimental tests for general relativity: the precession of the perihelion of Mercury, the gravitational red shift, and the gravitational bending of light near a massive body.[18] At that time, the latter was only observable during a total eclipse of the sun. The test was carried out by the British astronomer Arthur Eddington during the eclipse of May 29, 1919, and his results confirmed general relativity's prediction.[19]

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[15] "Der Film des physikalischen Nihilismus."

[16] "Einstein baut ein Weltgebäude auf den Unvollkommenheiten unserer Sinneswahrnehmungen auf. Er predigt uns: Alle deine Wahrnehmungen sind relativ, folglich mußt du dir ein relatives Weltall nach meinem Rezept zurechtzimmern. Das ist nichts anderes als wissenschaftlicher Nihilismus unfruchtbarster Art im Einklang mit der politischen Vergangenheit des Professors, der Parteien angehört, die die Relativität des nationalen Ehrgefühls auf ihre Fahne geschrieben haben. ... Alle Einsteinler mitsamt ihrem Verständnis heuchelnden bolschezionistischen Klüngel können die Tatsache nicht aus der Welt schaffen, daß Zeit, Raum und Materie unendlich bestehen und daß man von einem gegebenen Mittelpunkt aus sehr wohl ein absolutes Weltbild konstruieren kann." (*Kinorat* 1922.)

[17] See (Rowe 2002; Gönner 1993a; Grundmann 1967; Grundmann 1998, esp. p. 142ff.).

[18] The Einstein Tower is represented in the Gehrcke Collection in a folder with 31 articles, mainly featuring well-known photographs. For a historical discussion of the role of the Einstein Tower in the experimental verification of general relativity, see (Hentschel 1992).

[19] For details and a modern assessment see (Earman and Glymour 1980).

The announcement in the media of the results from the Eddington expedition triggered a public Einstein controversy. Beside the celebration of the “New Giant in World History,”[20] there were immediate doubts in the press about the accuracy of measurement and the significance of the results. These doubts did not diminish for many years and were voiced in particular by scientists who aimed to gain public support for their dispute with relativity.

The eclipse in September 1922 and the Dutch-German Solar Eclipse expedition to Christmas Island are covered by 250 articles in the Gehrcke collection. Their tone ranges from enthusiastic to polemic, from “Einstein’s Triumph” (*Vorwärts* 1923) to “Einstein’s Fantasies” (Riem 1923b).

Nachprüfung der Relativitätstheorie.

Die Einſtein-Expedition nach der Weihnachtsinſel.

Einſteins Triumph.

Die neue Beſtätigung der Relativitätstheorie.

8 - APR. 1923

Wieder eine Beſtätigung der Einſteinſchen Theorie.

Die Einſteinſchen Phantaſien.

Sonnenfinſternisbeobachtung und Einſteineffekt.

Figure 3: Headlines from the Eclipse Folder. *Berliner Morgenzeitung*, July 20, 1922; *Volksrecht*, April 23, 1923; *Der Tag*, April 8, 1923; *Deutsche Zeitung*, April 27, 1923.

The science popularizer Rudolf Lämmel wrote in the Swiss social democratic newspaper *Volksrecht*:

> One of the most controversial theories of the Einstein school can be regarded as being finally confirmed by this result. ... The meaning of this confirmed result reaches far beyond the theory of general relativity and interferes deeply with our traditional physical knowledge. It is not merely the confirmation of the deflection of a light ray in a gravity field, but also the unchallengeable fact that

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[20] A headline from the *Berliner Illustrierte Zeitung*, December 14, 1919.

> a ray of light is of a material nature. The acceptance of the hypothetical world ether is no longer required for explaining the physical characteristics of phenomena such as light and electromagnetism ... space is empty, and the only visible thing arriving from the infinite depths of the universe to our planet is light ... .[21]

Numerous newspapers paraphrase the explanations given by Astronomer Royal Sir Frank Dyson at a press conference on the meaning of the eclipse observation. Here the difficulties in communicating complex scientific ideas in a way that is understandable to the general public become evident. His attempt to summarize the gist of relativity culminates in the statement of a rather meaningless "general theorem." The articles, closely paraphrasing Dyson, all conclude that:

> Even if Einstein's whole theory cannot be expressed by a simple formula, the general theorem is accepted as being valid, that the characteristics of space, which until now were considered as absolute, are related to special circumstances and thus depend on special circumstances.[22]

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[21] "Eine der umstrittensten Theorien der Einsteinschen Lehre darf durch dieses Ergebnis wohl endgültig als bestätigt gelten. ... Die Bedeutung des nunmehr gefundenen Ergebnisses geht weit hinaus über die allgemeine Relativitätstheorie und greift aufs Tiefste in unsere bisherige physikalische Erkenntnis ein. Es handelt sich ja nicht allein um den bloßen Nachweis der Ablenkung des Lichtstrahls in einem Schwerefeld, sondern um die nun nicht mehr zu bestreitende Erkenntnis, daß der Lichtstrahl materieller Natur ist. Es bedarf fortan nicht mehr der Annahme des hypothetischen Weltäthers, um die physikalischen Erscheinungen des Lichtes und der Elektrizität zu erklären ... der Raum ist leer, und das einzige, was aus den unendlichen Tiefen des Universums wahrnehmbar bis zu unserem Planeten gelangt, das Licht ..." (Lämmel 1922.)

[22] "Wenn auch die ganze Theorie Einsteins sich durch eine einfache Formel nicht ausdrücken lasse, so werde der allgemeine Lehrsatz als gültig angenommen, daß die Eigenschaften des Raumes, die bisher als absolut gegolten haben, in einem Verhältnis zu besonderen Umständen stehen, daß sie also sich nach besonderen Umständen richten." "Die Bestätigung der Relativitätstheorie," *Berliner Börsen-Courier*, April 16, 1923; "Der Triumph der Einsteinschen Theorie," *Vossische Zeitung*, April 10, 1923; "Einsteins Relativitätstheorie endgültig bestätigt?" *Neue Preussische* (*Kreuz*) *Zeitung*, April 16, 1923; "Bestätigung der Einsteinschen Relativitätstheorie," *Berliner Börsen-Zeitung*, April 17, 1923; "Die Bestätigung der Einstein-Theorie," *Berliner Tageblatt*, April 18, 1923; "Bestätigung der Einstein-Theorie," *Vorwärts* April 16, 1923; "Einstein hat Geltung," *Dresdener Anzeiger*, April 17, 1923; "Die Einstein-Theorie bestätigt," *Elbinger Zeitung*, April 16, 1923; "Bestätigung der Einsteinschen Theorie," *Königsberger Hartungsche Zeitung*, April 16, 1923; "Einsteins Relativitätstheorie. Neue Mitteilungen," *Ostsee-Zeitung*, April 16, 1923; "Die Bestätigung der Einstein-Theorie," *Neuer Görlitzer Anzeiger*, April 17, 1923; "Einsteins Lob bei den Engländern und Amerikanern," *Generalanzeiger für Stettin*, April 17, 1923; *Schlesische Zeitung*, April 23, 1923; "Die Bestätigung der Relativitätstheorie," *Bote aus dem Riesengebirge*, April 17, 1923; "Bestätigung der Einstein-Theorie," *Lübecker Generalanzeiger*, April 22, 1923; "Englische Bestätigung der Einsteinschen Relativitätstheorie," *Der Gesellige*, April 23, 1923; "Die Bestätigung des "Einstein-Effektes"," *Dresdener Neueste Nachrichten*, April 20, 1923; "Der Triumph der Einsteinschen Theorie," *Pester Lloyd*, April 20, 1923.

More than 20 articles in the Eclipse Folder are explicitly anti-Einstein. Among these 20 articles were, for instance, several published by the Potsdam astronomer and Einstein opponent Johannes Riem (Riem 1922, 1922a, 1923, 1923a, 1923b, 1923c). In this series of articles Riem defends Johann Georg Soldner's formula for the gravitational bending of light on the base of classical physics (Lenard 1921) and accuses Einstein of plagiarism.[23] Riem emphasizes that:

> As shown by Soldner's activities, this effect [the deflection of light] has nothing to do with the theory of relativity. He [Soldner] indicated a physical cause, while the theory of relativity is nothing but a scientifically implausible and also philosophically impossible speculation developed on an extremely dubious basis.[24]

Riem's articles all appear in right-wing papers, while Einstein's Triumph was celebrated in the liberal papers. We shall look at this connection between political affiliation and attitude for or against relativity in more detail in the following section.

2.4 The Leipzig Folder

The Leipzig Folder contains articles related to the centennial celebration of the German Society of Natural Scientists and Physicians in Leipzig in September 1922. Following the first wave of polemics in August 1920, the run-up to this celebration prompted the second anti-Einstein wave. As these over 100 articles in the Leipzig folder provide a valuable source illustrating the course this anti-Einstein wave took, we shall look at this in somewhat closer detail.

On August 5th, an article with an apparently harmless headline appeared in the *Leipziger Neueste Nachrichten*: "Is Professor Einstein coming to Leipzig?" But the message was anything but harmless. After foreign minister Walter Rathenau was murdered by right-wing extremists on June 24, Einstein was warned that he would be one of the next victims. He therefore decided to withdraw from public life for some time and cancelled his plans to give the plenary lecture on the theory of relativity at the centennial celebration.[25] These events had great resonance in the press, for example, there are nearly 60 articles in the Gehrcke collection referring to Einstein's murder threat.

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[23] Philipp Lenard reprinted Soldner's work in 1921 (Lenard 1921). See also (Jaki 1978).

[24] "Mit der Relativitätstheorie hat die Sache (der Lichtablenkung) nichts zu tun, wie der Vorgang Soldners zeigt. Dieser hat einen physikalischen Grund angegeben, während die Relativitätstheorie nicht ist als eine auf höchst zweifelhafter Grundlage aufgebaute naturwissenschaftlich unmögliche und philosophisch ebenso unmögliche Spekulation." (Riem 1923).

Max Planck, the chairman of the German Society of Natural Scientists and Physicians, was shocked that a gang of murderers could dictate the itinerary of a scientific society and expressed this in a letter to Max von Laue on July 9.[26] On the other hand, he also saw a positive effect, as he wrote in a letter to Wilhelm Wien on the same day:

> Taken purely objectively, this switch [von Laue speaking instead of Einstein] perhaps even has the advantage that those who still believe that the principle of relativity is at bottom Jewish advertising for Einstein will be set right.[27]

Apparently Planck was convinced that it made sense to separate the theory from Einstein's person. The dangers of such a separation became more obviously apparent much later after the Nazi's rise to power when Planck felt compelled to consent to Einstein's exclusion from the Prussian Academy.[28] The articles in the Leipzig folder are all centered on the three topics Planck addressed in his letter:

- the murder threat,
- Einstein's opponents who believed there was nothing but propaganda behind his theory,
- the centennial and its highlight: the plenary lectures by Max von Laue and Moritz Schlick on the theory of relativity.

2.4.1 The Democratic and the Right-Wing Press

In the following, some reactions to the three topics mentioned above will be shown from the democratic and the right-wing press in Germany. Of course, the journalistic landscape was much more differentiated than is suggested by these two camps, but it is nevertheless possible to make a rough distinction between newspapers generally supporting and newspapers general-

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[25] "... ich bin nämlich von seiten durchaus ernst zu nehmender Menschen—von mehreren unabhängig—davor gewarnt worden, mich in der nächsten Zeit in Berlin aufzuhalten und insbesondere davor, irgendwie in Deutschland öffentlich aufzutreten. Denn ich soll zu der Gruppe derjenigen Personen gehören, gegen die von völkischer Seite Attentate geplant sind." Einstein to Planck, July 6 1922, quoted in (Seelig 1954, 213f.).

[26] "So weit haben es die Lumpen wirklich gebracht, daß sie eine Veranstaltung der deutschen Wissenschaft von historischer Bedeutung zu durchkreuzen vermögen." Planck to Laue, July 9, 1922, quoted in (Hermann 1994, 281).

[27] "Rein sachlich genommen hat dieser Wechsel vielleicht sogar den Vorteil, daß diejenigen, welche immer noch glauben, daß das Relativitätsprinzip im Grunde eine jüdische Reklame für Einstein ist, eines besseren belehrt werden." Planck to Wien, July 9, 1922, quoted in (Heilbron 1988, 127).

[28] "Einstein [hat] selber durch sein politisches Verhalten sein Verbleiben in der Akademie unmöglich gemacht ..." (Kirsten and Treder 1979, 267). See also (Renn et al. 1999).

ly rejecting the democratic system of the Weimar republic as such.[29] This division between the democratic and right wing press corresponds to the division between general support and rejection of the theory of relativity in the press. Thus the "democratic press" includes, for instance, the well-known liberal papers *Vossische Zeitung* and *Berliner Tageblatt* as well as the semi-official *Deutsche Allgemeine Zeitung*.[30]

The right-wing press is often affiliated with the right-wing parties of the opposition, namely the *Deutschnationale Volkspartei* (German National Peoples' Party) via its member Alfred Hugenberg, the press and movie-industry tycoon. Among the more familiar Hugenberg papers are the *Rheinisch-Westfälische Zeitung* and the nationalist newspaper *Der Tag*. The *Deutsche Zeitung* and *Die Wahrheit* are particularly well known as coming from the anti-Semitic camp. We shall begin in chronological order with the reactions to the murder threat as the first event.

2.4.2 The Murder Threat

After the murder threat was made known, there was an immediate and clear expression of solidarity with Einstein on the democratic side. Thus the *Berliner Tageblatt* speaks of a: "moral degeneration which … prevails in broad circles of right-wing radicalism.[31] And the *Dresdener Volkzeitung* comments:

> It is a disgrace for all of Germany that a world-famous scholar can be put on the list for assassination and chased out of the country by unthinking, reactionary scoundrels … .[32]

The *Nationalzeitung*, a national liberal newspaper, was skeptical about the reliability of the source:

> Only by hearsay has it been mentioned that Professor Einstein also belongs to the various prominent republicans against whom assassinations have been planned.[33]

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[29] See (Mendelssohn 1982, 371f.) and (Moores, 76ff.).

[30] The following categorization of newspapers is based on (Mendelssohn 1982) and (Stöber 2002, esp. 202–237).

[31] "… moralischen Verwilderung, die … in weiten Kreisen des Rechtsradikalismus eingerissen ist." *Berliner Tageblatt*, August 5, 1922.

[32] "Es ist eine Schande für ganz Deutschland, dass ein weltberühmter Gelehrter von einem ungeistigen reaktionären Halunkentum auf die Attentatliste gesetzt und außer Landes gehetzt werden kann …" *Dresdener Volkszeitung*, August 5, 1922.

[33] "Es sei lediglich gerüchteweise erwähnt worden, dass zu den verschiedenen prominenten Republikanern, gegen die Attentate geplant seien, auch Professor Einstein gehöre." *Nationalzeitung*, August 6, 1922.

At first sight, the *Nationalzeitung* seems to have been correct in speaking about "*only* hearsay": According to the statements in the trial against members of the murder gang "Organisation Consul," various names and lists circulated among the extremists, but never Einstein's.[34] The great resonance of this "hearsay" on the murder threat in the press shows that there was no doubt at all among the public that there *could* be a murder threat to Einstein. Characteristically, only the *right-wing press* voiced such doubts. *Die Wahrheit* comments:

> Einstein should not have taken such nonsense seriously; then the intended "honor" [the plenary lecture] in the grand manner would not have eluded him; for it is not believable that such crazy people who toy with murderous intentions actually exist.[35]

In view of the contemporary context, characterized by more than 350 cases of political murder motivated by right-wing radicalism from 1919 to 1922,[36] this seemingly innocuous comment can actually be understood as an attempt to downplay the real danger to Einstein's life at this time.

Another view from the right: The *Rheinisch-Westfälische Zeitung* under the heading "The Fugitive Relativity"[37] reports that:

> … the flight he [Einstein] staged is to be interpreted as advertising, intended to make his by now considerably faded star shine in new glory, and is hardly the gist of the matter in this affair.[38]

For the right-wing press the situation was clear: Einstein's escape was not to be taken seriously and was—ultimately—nothing but propaganda.

Following the course of events, we shall now discuss the other two topics Planck mentioned: Einstein's opponents and the centennial celebration.

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[34] The name Einstein never appears in the comprehensive study of Martin Sabrow. See (Sabrow 1994).

[35] "Einstein hätte solchen Blödsinn nicht ernst nehmen sollen, dann wäre er der ihm zugedachten "Ehrung" [der Festvortrag] in großem Stil nicht entgangen; denn, dass es wirklich verrückte Menschen geben sollte, die sich mit dergleichen Mordabsichten tragen, ist nicht glaubhaft." *Die Wahrheit*, September 23, 1922.

[36] See (Gumbel 1922, 78).

[37] "Die flüchtige Relativität," *Rheinisch-Westfälische Zeitung*, August 5, 1922.

[38] "… die von ihm [Einstein] in Szene gesetzte Flucht als Reklame auszulegen ist, die seinen schon merklich verblassten Stern in neuem Glanze erstrahlen lassen soll, dürfte wohl des Pudels Kern in dieser Affäre bedeuten." *Rheinisch-Westfälische Zeitung*, August 5, 1922.

2.4.3 The Protest Declaration of Einstein's Opponents in the Run-up to the Centennial Celebration

The criticism of the theory of relativity in the 1920s is interwoven with personal attacks on Einstein—the pacifist, the democrat, the internationalist, the Jew.[39] This combination is at the core of the "joint effort" by his opponents in September 1922, the "protest declaration" in the run-up to the centennial celebration.

Among the nineteen signatories of the declaration,[40] all of them doctors or professors of physics, mathematics or philosophy, are the physicists Philipp Lenard, Ernst Gehrcke, Hermann Fricke and Ludwig Glaser. Five of the signatories[41] would later contribute to the pamphlet 100 *Autoren gegen Einstein*, published in 1931 (Ruckhaber et al. 1931; Gönner 1999b). The declaration was labeled a scientific statement, but intended and understood as a political statement. Not surprisingly, it was supported by the right-wing press, as will be shown. The declaration rests upon the shared assumption of what I will call the "oppression theory," according to which criticism of the theory of relativity is oppressed by the organized use of propaganda in the scientific and public spheres. The declaration reads as follows:

> [The undersigned] deplore most deeply the deceiving of public opinion, which extols the theory of relativity as the solution to the riddle of the universe, and which keeps people in the dark about the fact that many … scholars ... reject the theory of relativity … as fundamentally misguided and logically untenable fiction. The undersigned regard it as being irreconcilable with the seriousness and dignity of German science, when a theory disputable in the highest degree is conveyed to the layman so prematurely and in such a charlatan manner, and when the Society of German Natural Scientists and Physicians is used to support such efforts.[42]

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[39] See, for instance, (Gönner 1993a, Grundmann 1967, Grundmann 1998, Rowe 2002).

[40] Johannes Riem, M. Wolff, A. Krauße, Josef Kremer, Ernst Gehrcke, Rudolf Orthner, Stjepan Mohorovičić, Hermann Fricke, Philipp Lenard, Melchior Palagyi, E. Hartwig, Leonore Kühn-Frobenius, Ludwig Glaser, Karl Strehl, R. Geißler, Karl Vogtherr, Sten Lothigius, Vincenz Nachreiner, and Friedrich Lipsius.

[41] Karl Strehl, R. Geißler, Karl Vogtherr, Sten Lothigius, and Vincenz Nachreiner.

[42] "[Die Unterzeichneten] beklagen aufs tiefste die Irreführung der öffentlichen Meinung, welcher die Relativitätstheorie als Lösung des Welträtsels angepriesen wird, und welche man über die Tatsache im Unklaren halt, dass viele … Gelehrte ... die Relativitätstheorie … als eine im Grunde verfehlte und logisch unhaltbare Fiktion ablehnen. Die Unterzeichneten betrachten es als unvereinbar mit Ernst und Würde der deutschen Wissenschaft, wenn eine im höchsten Maße anfechtbare Theorie voreilig und marktschreierisch in die Laienwelt getragen wird, und wenn die Gesellschaft Deutscher Naturforscher und Ärzte benutzt wird, um solche Bestrebungen unterstützen." *Die Wahrheit*, September 23, 1922.

The intention of the declaration is outlined once more by one of the initiators, physicist and patent clerk Hermann Fricke, in the right-wing newspaper *Der Tag* on September 28th:

> It appears as if any resistance against the theory is to be vigorously suppressed from the start … .[43]

Evidently, Fricke felt that the Einstein opponents were being oppressed. *Die Wahrheit* prints the pamphlet, introducing it as a "protest of German and foreign scholars against the propaganda for the benefit of Professor Einstein."[44]

J.E.G. Hirzel[45] in the *Luzerner Neueste Nachrichten*, also openly argues anti-Semitically in his "explanation" of why the overwhelming majority of the democratic press just ignores the pamphlet:

> … the major press in Germany is almost exclusively in the hands of Einstein's fellow countrymen and is not able to find any fault with him. He is their protégé and darling. A public discussion is prevented in the exclusive interest of Einsteinianism.[46]

Twenty-two articles in the Gehrcke Collection refer directly to the declaration. Most of them are published by right-wing newspapers such as *Deutsche Zeitung*, *Neue Preussische (Kreuz-) Zeitung* and the *Rheinisch-Westfälische Zeitung*, which reprint the declaration without comment or with sympathizing comments. Only very few are published by democratic newspapers.

For our purposes, it is useful to distinguish here between conservative-democratic newspapers and liberal-democratic newspapers. This is because the conservative newspapers (*Frankfurter Nachrichten and Düsseldorfer Nachrichten*) reprint or paraphrase the declaration without comment while the liberal newspapers (*Berliner Tageblatt, Leipziger Volkszeitung, Frankfurter Zeitung*) express criticism. The *Leipziger Volkszeitung*, for example, ridicules the pamphlet as a

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[43] "Es hat den Anschein, als ob jeder Widerstand gegen die Theorie von vornherein gewaltsam unterdrückt werden sollte …." (Fricke 1922).

[44] "Ein Protest deutscher und auswärtiger Gelehrter gegen die Stimmungsmache zugunsten des Professors Einstein geht uns mit der Bitte um Veröffentlichung zu." "Einstein." In: *Die Wahrheit*, Sepember 23, 1922.

[45] Hirzel is a pseudonym of the Swiss amateur scientist and Einstein opponent Johann Heinrich Ziegler. Ziegler's quarrel with relativity against the background of his (amateur scientific) theory about the world will form part of my dissertation.

[46] "… die große Presse ist in Deutschland fast ausschließlich in den Händen der Volksgenossen Einsteins und lässt diesem nichts anhaben. Er ist ihr Schützling und Schoßkind. Man verhindert eine öffentliche Diskussion im ausschließlichen Interesse des Einsteinianismus" (Hirzel 1922).

document “characterizing these luminaries of the university” (*Leipziger Volkszeitung*) and the *Berliner Tageblatt* sees the declaration in the tradition of the “spirit” of the first anti-Einstein wave:

> The embittered opponents of Einstein … thus regard it “to be irreconcilable with the seriousness and dignity of German science” when an “unproven hypothesis” is put to a forum of mathematicians for discussion, but they apparently find it thoroughly dignified of German science to present this “unproven hypothesis” with all its difficult scientific issues to the layman in the Berlin Philharmonic Hall.[47]

Here, the *Berliner Tageblatt* unmasks in one sentence the dubious argumentation of Einstein’s opponents.

2.5 Summary

An examination of the sample of newspapers that Gehrcke obtained from his clipping services clearly disproves the oppression theory by Einstein’s opponents who claimed that the published opinion oppressed critical views on his theory.

On the contrary, of the more than one hundred articles in the Gehrcke collection covering the centennial of the German Society of Natural Scientists and Physicians in 1922, the majority of these articles published after the publication of the declaration referred to it, some were even sympathetic towards it. While several articles in the Leipzig Folder were written by Einstein’s opponents,[48] not a single one was written by one of his followers.

At least to some extent the articles in the collection may be considered as a representative sample as they came from clipping services. Also, as far as is known, Gehrcke did not select articles as his collection contains newspapers from the radical right as well as the *Rote Fahne*, the communist party newspaper. Thus the Gehrcke material can be used for research on a broad array of questions, in particular concerning the way in which a scientific theory can enter the public

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[47] “Die verbissenen Gegner Einsteins …betrachten es also “als unvereinbar mit dem Ernst und der Würde deutscher Wissenschaft”, wenn eine “unbewiesene Hypothese” vor einem Forum exakter Wissenschaftler zur Diskussion gestellt wird, aber sie halten es anscheinend durchaus für würdig, der deutschen Wissenschaft diese “unbewiesene Hypothese” mit all ihren schwierigen wissenschaftlichen Fragen in der Berliner Philharmonie dem Laienurteil zur Erledigung vorzusetzen.”

[48] See, for example, (Kühn 1922a, 1922b; Hirzel 1922, Fricke 1922, Lenard 1922a, 1922b).

sphere either by triggering political debates or by becoming a topos of everyday thinking as when one newspaper writes: “Only a few understand the theory of relativity, but nobody understands the new tariff law” (Heldt 1921).

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