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Keywords: Reichenbach • Unified Field Theory • General Relativity • Geometrization

coordination ... geometrization... unification.

Introduction

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1 Coordination. Reichenbach - Einstein Encounter and Weyl-Theory

1.1 Reichnebach's as Einstein's Lecture and his Early Critique of Field theory

Nach seinem Einsatz im Ersten Weltkrieg hörte Reichenbach in Berlin Einsteins Vorlesungen zur Speziellen und Allgemeinen Relativitätstheorie. Seine Mitschriften dieser Veranstaltungen sind erhalten geblieben: Klassische und Statistische Mechanik (SS 1918, HR-028-01-02), Allgemeine Relativitätstheorie (Teil 1 HR-018-0104, Teil 2 HR-028-01-03, Teil 3 HR-028-01-01, alle Tile undatiert). In Teil 1 der Mitschrift zur Allgemeinen Relativitätstheorie, behan-
1917–1918 and 1918-1919

Reichenbach first encounter with relativity happened in Winter term 1918-1919 when he followed Einstein's lectures in Berlin. The notes taken by Reichenbach are extant and are very similar to Einstein's own notes and in some points complementary.[1] These lectures essentially follow the corresponding sections of Einstein 1914o (Vol. 6, Doe. 9) and Einstein 1916e (Vol. 6, Doe. 30).

[1]Further information about Einstein as an academic teacher, see Vol. 3, the editorial note, "Einstein's Lecture Notes,"pp. 3-10, and for a survey of Einstein's academic courses, see Vol. 3, Appendix B..

The introduction of the Riemann curvature tensor given in these two papers can be found in these lecture notes as well (see [p. 11]), but it is clear from entries on [p. 10] and [p. 25] and from Reichenbach's notes (the last part of the second notebook) that. From the metric, Christoffel symbos, and Riemann tensor is flat.

After this standard presentation, in his lecture notes, Reichenbach also presented the new interpretation of the curvature in terms of the parallel displacement given by Tullio Levi-Civita and Hermann Weyl (who are mentioned explicitly on [p.IO]) and in Reichenbach's note on page. In Reichenbach's notes, the curvature tensor is introduced at a later point, just before the discussion of gravitational field equations (see [p. 14]). At that point, one finds the derivation that Einstein began but did not finish at the foot of [p. 10], along with a similar derivation that can be found on [p. 25]. On [p. 10], Two vectors are parallel if they have the same components. However, this is not the case in the general case, using non-linear coordinates. In order to to reinstate the connection, one has to

In Euclidean geometry, it is always possible to introduce a Cartesian coordinate system in which two vectors are equal and parallel when they have the same components. However, this relation does not hold if we introduce curvilinear coordinates, e.g., polar coordinates. Consequently, vectors at different points can no longer be directly compared. If one displaces a vector to a neighboring point dx_ν, one does not know whether the vector has remained the 'same' by simply examining its components. The 'connection' (*Zusammenhang*) from a point to another is lost. Because the affine geometry is the study of parallel lines, Weyl (1918c) used to speak of the necessity of establishing an 'affine connection' (*affiner Zusammenhang*). However, because it is a relation of 'sameness' rather than parallelism that is relevant in this context, others, such as Reichenbach, prefer to speak of the operation of 'displacement' (*Verschiebung*), where the latter indicates the small coordinate difference dx_ν along which the vector is transferred.

To reinstate the 'connection' one requires to introduce a rule for comparing vectors at infinitesimally separated points. Given a vector A^τ at x_ν in an arbitrary coordinate system, we need to determine the components of the vector $A^{*\tau}$ at $x_\nu + dx_\nu$ that is to be considered the 'same vector' as the given vector A^τ. The vector A^τ at the point $P\,(x^\nu)$ and the vector $A^\tau + dA^\tau$ at the point $P^*\,(x^\nu + dx^\nu)$ are the 'same vector,' if they satisfy the condition:

$$dA^\tau = \Gamma^\tau_{\mu\nu} A^\mu dx_\nu \,. \qquad [1]$$

The quantity $\Gamma^\tau_{\mu\nu}$ is known as the affine connection or displacement. It has three indices, i.e., entails τ possible combinations of $\mu \times \nu$ coefficients, which can vary arbitrarily from point to point, i.e., in the general case, are functions of x_ν. Because in general $\Gamma^\tau_{\mu\nu} \neq \Gamma^\tau_{\nu\mu}$, the $\Gamma^\tau_{\mu\nu}$ has $n \times n^2$ coefficients. If a vector A^τ is given at the point P with coordinates x_ν, eq. [1] yields the unknown components of the vector $A^{*\tau}$ at P^* with coordinates $x_\nu + dx_\nu$. Continuing this process $x_\nu + dx_\nu + d^*x_\nu + d^{**}x_\nu \ldots$, we can parallel displace a vector from any given point to any other distant point. As is well known, the most characteristic feature of the operation of displacement is that if one parallel displaces A^{τ} along different paths, one gets, in general, a different vector $A^{*\tau}$ at a distant point. The transfer of a vector A^k from P to the distant point Q along a given curve C is, of course, unique and reversible; that is, on transferring back along the same curve you get back the initial vector at P. But the transfer along some other curve C' would lead to another vector at Q. Altermatively, if you transfer A^k from P to Q along C, then back to P transfer A^k from P to Q along C, then back to P along C', you obtain at P a vector different in direction and length from the original A^k. you associate with any contravariant vector A^k its invariant $g_{ik}A^iA^k$ ('square of the length'). The required correspondence between the vectors A^k at P and $A'^k \left(= A^k + \delta A^k\right)$ at the neighbouring point Q can then be set up by adopting the following four rules. The $g_{\mu\nu}$ are determine the connection.

However, soon If one starts with a symmetric metric $g_{\mu\nu}$, the Christoffell symbols are, so to speak, the only possible choice, that is one is lead to. However, if one defines the operation of displacement independently from the metric, the Riemannian connection appears only as a special case that is achieved by introducing a series of conditions. Therefore, Weyl realized formalism opened a vast array of possibilities that physicists hoped to exploit to accommodate the electromagnetic field in the geometrical structure of spacetime. Weyl considered natural to maintain that the is symmetric, and exploited the possibility of considering to abandon the condition, so se it to the particular value. Indeed, Riemannian geometry is near-geometrical however that fact that we compare the at distance.

Weyl that is symmetric, but abandoned the assumption that was covariant however, that only the ratio of the $g_{\mu\nu}$ by parallel transport, that is the length of vectors. The underlying idea was that one ought not to admit the physical possibility of comparing 'lengths' at distant world points, as Einstein's theory did. Weyl's modification automatically introduces, in addition to the 'metrical tensor' g_{ik}, a 'metrical vector' φ_k of equally fundamental standing with g_{ik}, and capable of being interpreted as the electromagnetic potential. For the Γ'' you now no longer get just the $\left\{ \begin{array}{c} k \\ ml \end{array} \right\}$, but

$$\left\{ \begin{matrix} \mu\nu \\ \tau \end{matrix} \right\}$$

The restrictive Hamiltonian principle. A connected manifold of any type is not yet a field theory. To make it that, you have to impose on its basic geometrical field-quantities-the g_{ik} or the g_{ik} and φk or the Γ_{ml^k}, as the case may becertain restrictions, differential equations. One wishes them to flow from some general principle which leaves as little arbitrariness as possible. Now almost every kind of restriction contemplated hitherto has turned out to be equivalent to a Hamiltonian principle, variant density $\mathcal{E}$, taken over any fixed region, be stationary: $\delta \int \mathcal{L} dx_1 dx_2 dx_3 dx_4 = 0$. This very convenient way of searching for the 'right' field-equations (you just have to search for 'the right f ') has been widely adopted, and there are general reasons for believing that it is justified. (If the field equations amount to a Hamiltonian principle, the conservation laws are an automatic consequence of the general invariance.). two curavutres

1.2 Reichenbach's Habilitation and his critique of Weyl Theory

By 1919, Einstein argued repeatedly that Weyl's theory runs into difficulties if one assumes, as Einstein thought one should, first, that clocks and rods directly measure the line element ds^2 (see, for instance, Doc. 512). This famous obectios. that vectors have length, are measured by clocks. Iinded, bu. Howver, Einstein aslo poitend the trajectories of free uncharged particles are not geodesics (see, for instance, Doc. 579) in the theory becolse of the presence of in the definition of the connecto. If one makes the first assumption in Weyl's theory, Einstein argued, the rate of a clock will be dependent on its prehistory; if one makes the second, uncharged particles will be affected by the electromagnetic four-vector potential. In Doc. 661, Einstein reiterated both charges, that of the fourth-order and not of the second order. Einstein held the opinion that the world-lines of an uncharged particle should be described by a geodesic equation that does not explicitly depend on the electromagnetic four-potential, as it does in Weyl's theory.

Nevertheless, Einstein had started to interest in the unified field theory program, after his correspondence with Kaluza. Reichenbach discussed Einstein's first attempt to develop a relativistic theory of matter, "Auch in der ncucn Einstr insclwn Auffansung, bei der innerhbalb des Elcktroms wicder die nicllt-cuklid. Geometric gilt" (HR-028-01-04, Randbemerkung zu Blatt 18.). Thus, check at that time, Reichenbach was not only proficiency as no other philosopher, but also was up to date with first attempts at unified field theory. With this background, in Februar or April 1920 Reichenbach decided to write his habilitation. 'Im Februar (oder März) 1920 beschloß ich, meine Habilitationsschrift zu schreiben. Ich hatte in den Monaten vorher Relth. gearbeitet, auch nach Weyl; den Grund hatte ich schon 1917-1918 in Vorlesungen bei Einstein gelegt, aus check welchen meine Kenntnis der Th. herrührt. Der Kapp-Putsch gab mir 8 Tage Freiheit. Ich brauchte nicht in die Fabrik Huth und konnte un- unterbrochen schreiben. Nachts stand ich auf u. schrieb weiter. Wegeu des Streiks versagte die elektr. Beleuelıtuugç ich schrieb deshalb bei Azetylen (Falırı'ad1ateı'1ıe). Die Schrift ist in etwa 10 Tagen niedergesclırieben.' Since he mentions it in his habilitation in Stuttgart, which probably finished around March 1920.

In the book Reichenbach alludes briefly how "Weyl's generalization of the theory of relativity which abandons altogether the concept of a definite length for an infinitesimal measuring rod". Weyl's theory represents a possible generalization of Einstein's conception of space which, "although not yet confined empirically, is by no means impossible". "According to Weyl the frequency of a clock is dependent upon its previous history"; which seems to be false. However, one can also that the influences that an "these influences compensate each other on the average, then the experiences until now" "the frequency of a spectral line under otherwise equal conditions is the same on all celestial bodies, can he interpreted as approximations, rather that an expression of a true geometry of space-time. The apparent would be non exact, and the real could be non-Riemannian". The justification of the choice of such geometry was according to Reichenbach ultimately. In Weyl geometry vector moving close loop which would same length but different direction in Riemannian geometry, different length and different direction in Weyl's geometry, epistemological, was a purely infinitesimal geometry eliminated the last remanet of distance of Riemannian geometry. Reichenbach, indeed he already surmised that this generalization can be continued. would be to assume that the vector changes its length upon turning around itself. "The next step in the generalization would be to assume that the vector changes its length upon turning around itself. There is no 'most general' geometry" . Thus, to compare even not at the same point, that to consider as intrinsically more satisfying that Riemannian geometry. The claim that there is serve against the ambition 'epistemological superiority' that Weyl attributed to his purely infinitesimal geometry, which made necessarily the best candidate to be the geometry valid in reality. This critique against Weyl's theory emerges in another passage:

check which geometry would be this?

> Yet it is incorrect to conclude, like Weyl and Haas, that mathematics and physics are but one discipline. The question concerning the validity of the axioms for the physical world must be distinguished from that concerning possible axiomatic systems. It is the merit of the theory of relativity that it renowned the question of the truth of gometry from mathematics and relegated it to physics. If now, from a general geometry, theorems are derived and asserted to be a necessary foundation of physics, the old mistake is repeated. This objection myst be made to Weyl's generalization of the theory of relativity [...] Such a generalization is possible, but whether it is compatible with reality does not depend on its significance for a general local geometry. Therefore, Weyl's generalization must be investigated from the viewpoint of a physical theory, and only experience can be used for a critical analysis. Physics is not a 'geometrical necessity'; whoever asserts this returns to the pre-Kantian point of view where it was a necessity given by reason.

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This critique contains the backbone of Reichenbach's criticism of the unified field theory-project int years. In spite of his Kantian framework, Reichenbach It is characteristic of modern physics to represent all processes in terms of mathematical equations. "But the close connection between the two sciences must not blur their essential difference". The truth of mathematical propositions depends upon internal relations among their terms; the truth of physical propositions, on the other hand, depends on relations to something external, on a connection with experience. "This distinction is due to the difference in the objects of knowledge of the two sciences". Weyl's attempt had to blur the between the sciences. In the context of his etherodox Kantianism, Reichenbach defended an empirical stance towards the question of which geometry is valid in reality. Similar critique was also formulated by Erwin Freundlich. In this way, not only pre-Kantian rationalism, it is also the very spirit of relativity theory theory.

Indeed, that even geometry, that was to be ultimately Euclidean, was inded to be left to physics.

The *axioms of connection* are the empirical laws of physics, the fundamental equations of a theory. The *axioms of coordination* determine the rules of the application of the axioms of connection to reality, that is, they determine the rules of the connection. Between implicit definitions and the which coordinating principles, could play the first the second would never be abandoned by Reichenbach. However, as we shall see it will be abandoed by Einstein. Einstein received possibly 24. May 1920. The copy in Einstein's library contains some marginal notes, e.g., 'sehr gut' on p. 74 to Reichenbach's contention that it is impossible to infer *a priori* principles. Einstein praised Reichenbach's *Habilitationschrift* in Einstein to Moritz Schlick, 19 April 1920 (Vol. 9, Doc. 378).

On 30. June Reihenbach asked for a dedictioon. "Philosophen eine Ahnung davon haben, dass mit Ihrer Theorie eine philosophische Tat getan ist, und dass in Ihren physikalischen Begriffsbildungen mehr Philosophie enthalten ist, als in allen vielbändigen Werken der Epigonen des grossen Kant." "die tiefe Einsicht der Kantischen Philosophie von ihrem zeitgenössischen Beiwerk zu befreien" Der Wert der Rel.Th. für die Philosophie scheint mir der zu sein, dass sie die Zweifelhaftigkeit gewisser Begriffe dargethan hat, die auch in der Philosophie als Scheidemünzen anerkannt waren.

Ich freue mich wirklich sehr darüber, dass Sie mir Ihre ausgezeichnete Broschüre widmen wollen, noch mehr aber darüber, dass Sie mir als Dozent und Grübler ein so gutes Zeugnis ausstellen. Der Wert der Rel. Th. für die Philosophie scheint mir der zu sein, das sie die Zweifelhaftigkeit gewisser Begriffe dargethan hat, die auch in der Philosophie als Scheidemünze anerkannt waren. Begriffe sind eben leer, wenn sie aufhören, mit Erlebnissen fest verkettet zu sein. Sie gleichen Emporkömmlingen, die sich ihrer Abstammung schämen und sie verleugnen wollen. Einstein made a similar claims by writing to Cassirer in the very same days.

1.3 the Bad Nauheim meeting of September 1920

Reichenbach might have met Weyl for the first time at Bad Nauheim in public at the 86th Assembly of the Association of German Scientists and Physicians (Versammlung der Gesellschaft Deutscher Naturforscher und Ärzte) in Bad Nauheim in September 1920. Weyl (1920b) introduced the distinction between *Einstellung* and *Beharrung* to explain away the discrepancy between the non-Riemannian behavior of the 'ideal' time-like vectors implied by his theory and the Riemannian behavior of the 'real' clocks that are actually observed. He suggested that atomic clocks might not *preserve* their Bohr radius if transported, but *adjust* it every time to some constant field quantity, , which he could identify with the constant radius of the spherical curvature of every three-dimensional slice of the world, furnishing a natural unit of length.[2]

[2]He suggested that atomic clocks might not *preserve* their Bohr radius if transported, but *adjust* it every time to some constant field quantity. Weyl suggested that the atoms we use as clocks might not preserve their size if transported, but adjust it every time to

In the discussion followed, Commenting on Weyl's talk, he pointed out once again that the "arrangement of [his] conceptual system," "it has become decisive [*massgebend*] to bring elementary experiences into the language of signs [*Zeichensprache*]" (Einstein et al., 1920, 650). For Einstein, "temporal-spatial intervals are physically defined with the help of measuring rods and clocks", under the assumption that "their equality is empirically independent of their prehistory" (Einstein et al., 1920, 650). Einstein insisted that precisely upon this assumption rests "the possibility of coordinating [*zuzuordnen*] a number ds to two neighboring world points"; if this were impossible, general relativity would be robbed of "its most solid empirical support and possibilities of confirmation" (Einstein et al., 1920, 650).

However, Einstein showed a more flexible attitude replying to Pauli's remarks during the same discussion. Pauli reiterated his objection based on his 'observability' criterion. Just as the field strength in the interior of the electron is meaningless because there is no smaller test particle than the electron, "one could claim something similar concerning spatial measurements, *since there are no infinitely small measuring-rods*" (Einstein et al., 1920, 650). The time or space intervals have a physical basis only if there is some actual or possible physical process that has a length or a duration shorter or equal to the space or time interval in question. A distance smaller than the electron would be physically meaningless since there is no physical process that could realize such an interval. The attempt to define the electromagnetic field or gravitational field in the interior of elementary particle to account for their stability should be rejected on epistemological grounds.

Einstein replied to Pauli that "with the increasing refinement of the system of scientific concepts, the manner and procedure of associating the concepts with experiences becomes increasingly more complicated" (Einstein et al., 1920, 650). In particular, he recognized that in cases such as that of the continuum theories, "one finds that a definite experience cannot be associated any longer with a concept" (Einstein et al., 1920, 650). According to Einstein, there is an alternative: one can abandon 'continuum theories' for the sake of Pauli's observability criterion, or replace such a "system of associating concepts [with experiences] with a more complicated one" (Einstein et al., 1920, 650). A decision as to which alternative is more suitable, Einstein pointed out, can only be given on the basis of pragmatic reasons (Einstein et al., 1920, 650). How-

some constant field quantity, which he could identify with the constant radius of the spherical curvature of every three-dimensional slice of the world, furnishing a natural unit of length. The geometry read off from the behavior of material bodies would appear different from the actual geometry of space-time, because of the 'distortion' due to the mechanism of the adjustment. Two identical 'classic' atomic systems with different prehistories would probably differ in some small detail due to their interaction with the environment, and their spectral lines would be slightly shifted, so that classically, a spiraling charge should emit light of all colors. Emerging quantum theory had already made clear that the spectral identity of atoms revealed by experience cannot be explained in this framework. The fact that all atoms of the same type are exactly identical clearly cannot depend on an initial agreement established in the past, which has been 'preserved' since then, even though the atoms had encountered very different physical circumstances. It was more plausible to argue that they 'adjust' anew each time to a certain equilibrium value.

ever, it would soon become clear that Einstein intended to opt for the second choice. One should probably view the epistemological considerations that he made in the following years as an attempt to publicly justify this choice. One can already glimpse the main lines of Einstein's in his contributions to the the discussion which followed Max von Laue's (1920)'s Bad Nauheim paper on the gravitational redshift.[3] Replying to some concerns expressed by the mathematician Georg Hamel (cf. also Hamel, 1921), Einstein pointed out that, "[s]ince the emitting atoms are to be regarded as a clock in the sense of the theory", that is, as identically constructed clocks, "then the redshift is one of the safest results of the theory" (Einstein's reply to Laue, 1920). One can 'verify' that the relation $d\tau^2 = g_{44}d\vartheta^2$ established by general relativity actually holds in reality.[5] Einstein, however, in the very same sentence, did not hesitate to admit that "[it] is a logical shortcoming of the theory of relativity in its present form to be forced to introduce measuring rods and clocks *separately instead of being able to construct them as solutions to differential equations*" (Einstein's reply to Laue, 1920, 662; my emphasis). Thus, Einstein now openly admitted that it would have been logically or epistemologically preferable if the field equations of the theory had suitable solutions corresponding to particles, from which in principle the stability of a more complicated, bulky configuration of matter could be reconstructed, including rod- and clock-like structures. In this way the necessity of coordinating the geometrical/kinematical structure of the theory separately from the rest in terms of rods-and-clocks behavior would fall and with it also Pauli's objection that such definition is impossible within elementary particles.

However, this point seems to have been after Bad Nauheim Moritz Schlick was at Bad Nauheim who was at that time the leading philosophical authority in relativity theory wrote to Einstein about Reichenbach's book complaining about his critique of conventionalism. The question of the importance of conventionalism, that is that it was possible to choice among Riemannian geometrie.

1.4 The Reichenbach Weyl's Correspondence

At the beguignngin of 1921 Weyl presented in more details. At about the same time, Eddington, a new theory based on a general affiene connection. In spite of the polemical remarks of Weyl's theories, Reichenbach sent a copy of his *Relativitätstheorie und Erkenntnis Apriori* (Reichenbach 1920). After some weeks Weyl replied appreciate the sincerity of Reichenbach's remarks. He pointed out that he believed that due their divergent basic orientations in philosophy an agreement would have been difficult; however, and decided to answer "two remarks, which concern less the philosophical than the physical".

[3] Laue showed that the coordinate interval $d\vartheta$ measured by an atom on the sun is transmitted unchanged by light signals (at least in a static gravitational field[4]), so that the redshift emerges by confronting the frequency of such signals with those of an atom of the same type at rest measuring the proper time $d\tau$.

[5] During the same session of the conference, a young Bonn spectroscopist named Leonhard Grebe had communicated very favorable results (obtained in collaboration with Albert Bachem), measuring the shift of the cyanide band of the sun spectrum (Grebe, 1920, cf. Hentschel, 1992, for more details).

In particular for our goals it is interesting to consider the following remark.

> It is certainly not true, as you say on p. 73, that, for me, mathematics (!!, e.g. theory of the ζ-function?) and physics are growing together into a single discipline. I have claimed only that the concepts in geometry and field physics have come to coincide [...] Was nıeine erweiterte Relativitätstlıeorie betrifft. so kann ich nicht zugeben, daß da erkenntnislogisch die Sache irgendwie anders liegt wie bei Einstein. [...] Der Erfahrung wird durch die Annahme jener allgemeiner Metrik in keiner Weise vorgegriffen; dass die Naturgesetze, an welche die Wirkungsausbreituug in Äther gebunden ist, können ja von solcher Art sein, daß sie keine Streıfkenkrümmung zulassen. [...] Wofür ich allein eintrete, ist dies: Die Integrabilität der Strekkenübertraguug (wenn sie besteht, ich glaubs uielıt. denn ich sehe nicht den geriugsteu zwiugeudeu Grund dafür) liegt uielıt im Wesen des metrischen Mediumsm, sondern kann nur auf einem besonderen Wirkungsgesetz berulıen. Ware die historische Entwieklung anders verlaufien, so seheint nıir wåire uienıand darauf verfallen. von vorn- herein gerade nur den Rieuıauuselıeu Fall iu Erwåígımg zu zielıeu. - *Nas die berüchtigte "Abhšingigkeit von der Vorgeschichte" betrifft, so habe ieh darüber wohl nıeine Ansicht deutlieh gemıg in Naulıeinı ausgesprochen. An der 4. Aufl. wird Sie wahrscheinlich vor al- lenı nıeine veränderte Stellımgnalnue zum Problem der Materie i.

.

Weyl explained to Reichenbach in details his, Der Relativität der Bewegung entspricht die Relativität der Größe; der Einheit von Trägheit (= Kraft, welche eine Richtung infinitesimal bei Verpflanzung erhält) und Gravitation entspricht bei mir die Einheit jener Kraft, welche Längen infinitesimal erhält (und die man bisher nicht als Kraft anerkannt hat, sondern in die Geometrie verlegte) und des Elektromagnetismus. Der Erfahrung wird durch die Annahme jener allgemeineren Metrik in keiner Weise vorgegriffen; [118] denn die Naturgesetze, an welche die Wirkungsausbreitung im Äther gebunden ist, können ja von solcher Art sein, daß sie keine Streckenkrümmung zulassen. Diese Möglichkeit liegt sogar nicht einmal ferne. Nehmen Sie als Wirkungsgröße das mittels der von mir in Nauheim [119] benutzten "Normaleichung" $F =$ const. gemessene Volumen (Bezeichnungen nach Raum Zeit Materie, 3. oder 4. Aufl.!), so liefert das Wirkungsprinzip.

> Wofür ich allein eintrete, ist dies: Die Integrabilität der Strekkenübertragung (wenn sie besteht, ich glaubs nicht, denn ich sehe nicht den geringsten zwingenden Grund dafür) liegt nicht im Wesen des metrischen Mediums, sondern kann nur auf einem besonderen Wirkungsgesetz beruhen. Wäre die historische Entwicklung anders verlaufen, SO scheint mir, wäre niemand darauf verfallen, von vornherein gerade nur den Riemannschen Fall in Erwägung zu ziehen. 99 Was die berüchtigte "Abhängigkeit von der Vorgeschichte" betrifft, so habe ich darüber wohl meine Ansicht deutlich genug in Nauheim ausgesprochen. An der 4. Aufl. wird Sie wahrscheinlich vor allem meine veränderte Stellungnahme zum Problem der Materie interessieren; von der Universalität der Feldphysik bin ich gründlich zurückgekommen.

Weyl's reaction seems to manifest that in spite of the attacks his theory was receivn frm this diferent sides, his attitude believed he still had some cards to play. had his how Weyl had introduced 'doubling the geometry.' As it turned out there was two different ways to proceed, to different way to save the the new geometry, and in general to to a more general affine connection. A few days later on February 17 Weyl published the English description on Nature, that is that real ether non-Riemannian geometry was to the measuring rod geometry, that appears in reality. This fundamental imbalance, between in which however, the instrument that serve to measure lengths, differently, in which k_σ is exactly Riemannian. Thus the rods and clocks do not conform, that the real geometry of s non-Riemannian. A second approach, was pursued Arthur Stanley Eddington, that preferred to that only on the affine connection, without any metric. Is exactly Riemannian, in which k_σ, is exactly Riemannian, is only a graphical representation, comparable to the representation of with, to adaptation to the radius of curvature of the world, that enter into the dynamic equations determining the behavior of rods and clocks. Introduce, a tensor split into two parts, and fiannly impose. Probably, unaware of both approaches Einstein presented in March, to avoid the rods and clocks at all. Indeed, from one can define the Riemann, tensor contracted into the Ricci tensor, into two tensor, third option, that is a theory that completely renounced to the very existence of transportable measuring rods, on 17th March. In this way there was no concern about doubling the geometry, which seems to feel as unpleasent. In two published in May (Weyl, 1921c) and July (Weyl, 1921d) return for different audiences. What is most important is that in the July paper Weyl also address in public Weyl and Erwin Freundlich criticisms[6]

> From different sides, [7] it has been argued against my theory, that it would attempt to demonstrate in a purely speculative way something *a priori* about matters on which only experience can actually decide. This is a misunderstanding. Of course from the epistemological principle [aus dem erkenntnistheoretischen Prinzip] of the relativity of magnitude does not follow that the "tract" displacement [Streckenübertragung] through "congruent displacement" [durch kongruente Verpflanzung] is not integrable; from the principle that no *fact* can be derived. The principle only teaches that the integrability *per se* must not be retained, but, if it is realized, it must be understood as the *outflow* [Ausfluß]*of a law of nature* (Weyl, 1921a, 475; last emphasis mine).

At the end of the theory, e.g. electrons withc a certain cine. This can be used. However, that this hoeve this beaavior does not imply. However, this solution was unstatiscy having conpratioan, that real rods and clocks, contradict the real tranport of lenght inaccesble to especoe.

In September 1921, Pauli's encyclopedia article on relativity theory (which was finished in December, but underwent some improvements in April and May)

[6]The reference is to Reichenbach, 1920 and Freundlich, 1920. The latter however does not mention Weyl.

[7]The reference is to Reichenbach, 1920 and Freundlich, 1920.

was finally published, as part of the fifth volume of the *Enzyklopädie der Mathematischen Wissenschaften*, and later as a book with an introduction by Pauli's mentor, Arnold Sommerfeld (Pauli, 1921). Pauli of two theories. The first one in which ... Paragraph 65 amounts to a devastating epistemological 'deconstruction' of Weyl's theory in particular, a critique that was hard to dismiss. In this layed town a fundamental objections. If Weyl's theory seeks to make predictions that are closely linked with the behavior of measuring rods and clocks, just like Einstein's theory, then the theory is clearly wrong. If one renounces this interpretation, as Weyl later suggests, then the theory loses its 'convincing power,' becoming just a mathematical scheme that furnishes only 'formal, and not physical, evidence for a connection between [the] world metric and electricity' (Pauli, 1921, 763; tr., 1958, 196). Indeed, soon Pauli would embrace Eddington in which the true geometry of spacetime, prefer to goemetrical represntatio, nd that ral geometru of]rac.

1.5 The Weyl-Reichenbach Appeasement

Im Sept. 1921 trug ich schon den ersten Bericht über die Axiomatik auf dem Physikertag in .lena vor.3 Ich hatte damals gro§en Erfolg; aber niemand ist damals auf den Gedanken gekommen, mich in eine angemessene Stelle zu berufen. Ich blieb in Stuttgart sitzen. Niederschrift und Ausbau im Winter 1921 /22. However the relationship were still excellent. In the meantime on 24 March 1922 Erwin Freundluch sent to Einstein "die Druckbogen einer kritischen Untersuchung von Reichenbach auf dessen Wunsch". Coudl not take into account just after having presented also. The long Review on Relativity, was read and commented by Einstein, presentation of a very balanced of Weyl theory. Reichenahc Genauere Behandlung der allg. Th. inõ Aug.-Spt. 1922. Vortrag darüber iu. Leipzig, Sept. 1922. Frühjahr u. Sommer 1922 entstanden Logos-Aufsatz u. Aufsatz iu revue philoslophiquel de la France, and Logos Aufsatz für Logos. This was a long details. A more balanced. Weyl took exception to the fact, that Einstein has simply condoned [einfach hingenommen] the univocal transportability of natural measuring-rods [eindeutigen Uebertragbarkeit natürlicher Maßstäbe]. He does not want to dispute the axiom of the Riemannian class for natural measuring rods; he wants only to urge that the validity of this axiom, being not logically necessary, *"is understood as an outflow* [Ausfluß] *of a law of nature*". That this was a fundamental contribution Admittedly, Weyl was able to explain the univocal transportability of natural measuring-rods only in a very incomplete way. But the only fact that he had tried to follow this path, regardless of its empirical correctness, was a genial advance [genialer Vorstoß] in the philosophical foundation of physics (Reichenbach, 1922, 367f.).

Reichenbach now distinguishes two ways to approach Weyl's theory, a more moderate, however, he also fundamentally incorporate Pauli's two theory interpretation. One way was to are measured by rods and clocks, thus that change by transport. This is however, not true. The second approach that was true the do not follow the Weyl connection, but behave in a Riemannian way. "er definiert einen ideellen Prozeß der Maßstabs-Uebertragung, der jedoch mit dem

Verhalten realer Maßstäbe nichts zu tun hat".

(a) Damit verliert jedoch die Theorie ihren überzeugenden Charakter und kommt einem mathematischen Formalismus bedenklich nahe, der um eleganter mathematischer Prinzipien willen die Physik unnötig kompliziert; und aus diesem Gedanken heraus wird die Weylsche Theorie von Physikern (besonders auch von Einstein) sehr zurückhaltend betrachtet. The is very similar to Pauli statement which very probably. Althoug, that Pauli was a measure influece on Reichenbach.

(b) Weyl had then to explain

> Seine Erklärung, nach der die eindeutige Uebertragbarkeit durch Einstellung der Maßstäbe auf den Krümmungsradius der Welt« erfolgt, ist im wesentlichen nur ein andrer sprachlicher Ausdruck für den vorliegenden Tatbestand, keine Zurückführung auf ein allgemeineres Gesetz. Insbesondere hat diese 'Einstellung' nichts zu tun mit seiner kongruenten Verpflanzung, so dass diese physikalisch leer bleibt.

. However, appreciate the very distinction, between will play a central role. Indeed, the that need an explanation why have the same lenght. HOwever, Wel explaintion is more.

Thus, to indeed, would, however, there is possibly,

> Die phi 1 o so phi s c h e Bedeutung der Weylschen Entdeckung besteht deshalb darin, daß sie bewiesen hat, daß ein Abschluß des Raumproblems auch mit dem Riemannschen Raumbegriff nicht gegeben ist. Wollte also die Erkenntnistheorie heutP- die Behauptung der transzendentalen Aesthetik Kants dahin erweitern, daß die Geometrie der Erfahrung auf jeden Fall wenigstens von Riemannscher Struktur sein muß, so wird sie durch die Weylsche Theorie daran zurückgehalten. Denn daß der Weylsche Raum wenigstens für die Wirklichkeit m ö g 1 i c h ist, läßt sich nicht bestreiten. Man darf nicht einmal glauben, daß mit der Weylschen Theorie nun die höchste Stufe der Allgemeinheit erklommen sei. Einstein hat gezeigt (14), daß man die Weylsche Forderung der Relativität der Größe auch befriedigen kann, ohne von dem Weylschen Meßverfahren Gebrauch zu machen. Danach wurde von Eddington (15) wieder eine Verallgemeinerung entwickelt, von der die Weylsche Raumklasse nur ein Spezialfall ist, und die Eddingtonsche Raumklasse ist wieder als Spezialfall in eine allgemeinere eingegangen, die von Schouten (63) gefunden wurde. Der Vorzug der Schoutenschen Theorie besteht darin, daß hier die Bedingungen angegeben werden, unter welchen die entwickelte Raumklasse die allgemeinste ist; es sind sehr allgemeine Bedingungen, wie Differenzierbarkeit und ähnliches. Aber eine schlechthin allgemeinste Raumklasse gibt es natürlich nicht; und die Geschichte des mathematischen Raumproblems mag der Erkenntnistheorie eine Lehre sein, niemals schlechthin allgemeine Behauptungen aufzustellen. Es gibt keine allgemeinsten Begriffe.

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That Weyl did not agree, with Some technical details, however concering that did not in the attacks

> Den Plan, starre Maßstäbe mit meiner Verpflanzımg zu identifizieren, lıabe ich aufgegeben. weil ich ilm nie gehabt lıabe: sondern ich war überrascht, als ich salı. daß Plıysiker das in mıeine Worte hineininterpretiert hatten. Genau, wie Einstein zeigen muß, daß aus der Dynamik des starren Körpers lıeraus ein solelıes Verlıalteu folgt, daß der Maßstab iumıer dieselbe Länge hat, gemessen iu seinenı (Is, so muß ich zeigen. daß er inmıer das gleiche durch $R = const.$ normierte ds lıat. Wir etwa Einstein so gut als ich das zu maclıen hätten, habe ich am Schluß meiner Arbeit " Feld und Materie". Ann. d. Plıysik angedeutet..

. Thus, neither, in Einstein nor in the identification is legitimate.

Two critique, one that epistemological, better than other geometries (there is no reaso, eliminat the asumm), that thus that one should give ... however the theory, formalism, that iintrisc superiority of Weyl geometry is not even true, an there are 27 different connections among which one can chose. A second aspect is that Weyl requires an, there is than as at least a good overview of some differential geometry, not Weyl but also Eddington and even Schouten. is even more surprising the work of Schouten 1921 classifies 18 possible linear affine connections ('Übertragungen') numbered as I,..., VI a?c. In Schouten 1922 he consider by he improved he classification trhee tensor 27 possible connection. All the further less general cases of linear connections are obtained by introducing restrictions to such quantities (for more details see Vizgin 1994, 184, Goenner 2004). This classification was Schouten point of view not only Schouten that Weyl was not but one could think in which. The third tensor which never plaed the role, the pwer can be. In which the tensor of asymmetry vanish symmetric, but the tensor of non metricity does not vanish. That more general even non-symmetrical connections (IVc). In which the tensor of non metricity vanish, but the tensor of asymetry does not, which will be the geometry used by Reichenbach That even Schouten most general linera, lieanr But ieven the condition that a connection is linear was not necessary. Start from a general affine connection, and then restrict the possibilities. However, there was no particular.

1922 French article, that was empirical fact that rods and clocks behave in a Riemannian way. Thus this resctriv that to Riemannian geometry. However, which Riemannian geomtry remains a question of choince. Idneed, one can imm 'Darrigol classes.' However, was on a fact, that in nature there are. The choice between geometry is conventional; indeed there might be didferent. By the there are no differential forces.

2 Geometrization: Reichenbach's Correspondence with Einstein

By the end of the 1922, Einstein had to exploit that Eddington's theory was more. Semi-metrical theory, that is was the reason why Einstein. Indeed, was semi-metrical and indeed, it was in general more effective to go in where the affine connection has not physical meaning at all 1923. That the theory, the goal was to construct an action principle. And indeed, found in first approximation. The manuscript consists of five pages. The first two pages were written on the back of a fragment of an unidentified typescript. The third, fourth, and fifth pages were written on the back of the beginning of a typescript of Reichenbach's contribution to the meeting of German physicists in Jena, 18-24 September 1921 (Reichenbach 1921). The reason. Weyl's theory was a semi-metrical theory. Vectors have length at one point, but by transportation. In Eddington's theory was purely affine, letting the connection copletly indetermined. That the latter was indeed, without any physical meaining. Was more appropriate. That one serve search for the field equations. To search for the indeed, and variation with respect to

Indeed, will make Pauli's famous reaction. In a latter to in 1923. Semte er. Eddington's attitude was in his 1923 to Pauli, As again Pauli pointed out to 'graphische Darstellung,' but not a natural geometry like that of rods and clocks. That soon appear in the 1925 German translation of Pauli's review indeed, presented the same argument.

In a long letter to Eddington, why this was dangerous. The second si that even if we can define, that this particular geometrical structure. There was not start which is undefined, that the graphical presentation was precisely the reason for rejecting the theory. Again Reichenbach's critique would follow Pauli's ideas. From Hans Reichenbach Stuttgart, 19 April 1923 Asks for help in finding a publisher for Reichenbach 1924. ALS. [20 079]. 50. Ilse Einstein to Hans Reichenbach Berlin, 12 May 1923 Informs him that AE has not seen Reichenbach's letter since he already left for the Netherlands. She no longer forwards AE's mail since he brings it all back unopened. She assures Reichenbach that she will give his letter to AE upon his return. AKS. [87 944]. 122. From Hans Reichenbach Stuttgart, 10 July 1923 Thanks AE for Abs. 89. Is sorry that the Academy did not agree to print his manuscript. Asks whether this was due to financial or other reasons. Springer cannot accept his suggestion that the *Notgemeinschaft* cover part of the printing cost. Verlag Witwer, on the other hand, has agreed to publish the work with support from the *Notgemeinschaft*. Enclosed sends the request to the *Notgemeinschaft* and asks to forward it to Fritz Haber and to put in a good word for him personally. Asks to mail back the manuscript. ALS. [20 082]. During 1924 Reichenbach finally managed to published, his Axiomatic in which. the negative review finally convinced him of the debacle, Weyl. In 1925

At about the same time Reichenbach started to work. Of course Einstein had by that time lost this was reason of the critique that required, and only a the theory as whole could serve as a that in 1925, getting back increasingly rationalistic. That Einstein was in 1925, that was already became skeptical again was to 1925 non-symmetric affine connection. In 1924 publishing a review of ... was clear that his past position was wrong. And the appear on several. The publication of Eddington 1925 was probably the starting point. However, Reichenbach also borrowed betweeh graphical representation and natural geometry. A proper geometrical interpreation.

During those same months, Reichenbach, despite the support of Max Planck, was struggling to obtain his *Umhabilitation*[8] from Stuttgart to Berlin in order

[8] The process of obtaining the *venia legendi* at another university.

to be appointed to a chair of natural philosophy that had been created there (Hecht and Hoffmann, 1982). On March 16, 1926, Reichenbach sent a letter to Einstein in which, after discussing his academic misadventures, he remarked on the new 'metric-affine' theory (Einstein, 1925b):

> I have read your last work on the extended Rel. Th[9] more closely, but I still can't get rid of a sense of artificiality which characterizes all these attempts since Weyl. The idea, in itself very deep, to ground the affine connection independently of the metric on the Γ^i_{kl} alone, serves only as a calculation crutch here in order to obtain differential equations for the g_{ik} and the φ_{ik} and the modifications of the Maxwell equations which allow the electron as a solution. If it worked, it would of course be a great success; have you achieved something along these lines with Grommer? However, the whole thing does not have the beautiful convincing power [*Ueberzeugungskraft*] of the connection between gravitation and the metric based on the equivalence principle of the previous theory (Reichenbach to Einstein, Mar. 16, 1926 EA, 20-83).

Reichenbach expressed skepticism early on towards Weyl's theory (Reichenbach, 1920, 73). Even if he partly retracted some of his concerns (Reichenbach, 1922, 367–368), he still felt that the theory did not have the same 'convincing power' (*Überzeugungskraft*) of general relativity (Reichenbach, 1922, 367), in which the identification of the g_{ik} with the gravitational potentials was solidly anchored in the principle of equivalence. Perhaps it is not a coincidence that Reichenbach uses the very same turn of phrase in this letter. Einstein's theory introduces the affine connection independently of the metric. However, it does not attribute any physical meaning to the former; the separate variation of the metric and connection was nothing more than a 'calculation device' to find the desired field equations. Reichenbach, however, was ready to revise his negative judgment if Einstein's theory delivered the 'electron.' At the end of the paper (Einstein, 1925b), Einstein had in fact claimed that he was working with his assistant Jakob Grommer on the problem of establishing whether the theory allows for "the existence of singularity-free, centrally symmetric electric masses" (Einstein, 1925b, 419). For Einstein this was a fundamental criterion for the viability of a unified field theory (cf. e.g., Einstein and Grommer, 1923).

On March 20, 1926 Einstein replied that he warm-heartedly agreed with Reichenbach's 'Γ-Kritik': "I have absolutely lost hope of going any further using these formal ways"; "without some real new thought"it simply does not work[Ohne einen wirklich neuen Gedanken geht es nicht][Einstein to Reichenbach, Mar. 20, 1926][20-115][EA]. Einstein's reaction reflects his disillusion with the attempts to achieve the sought-for unification of gravitational and electromagnetic field via some generalization of Riemannian geometry. He would have probably been less ready to embrace Reichenbach's critique if he had knew what the latter exactly had in mind (see next section). However, Reichenbach was of course pleased by Einstein's endorsement. On March 31, 1926 he revealed

[9]Einstein, 1925b.

that his remarks were not extemporary, but were the fruit of a more thorough consideration of the topic that he had jotted down at the time:

> I'm of course very glad that you agree with my Γ-critique. I have now made a few reflections on the topic, which seem to me to prove that the Weylean thought, although good mathematically, does not bring about anything new physically. The geometrical interpretation of electricity is only a visualization, which in itself still does not say anything, and can also be realized in the original relativity theory. I have attached the note and would be grateful if you could give it a look Reichenbach to Einstein, Mar. 24, 26][20-085][EA].

Reichenbach attached to this letter a typewritten note. As we shall see, far more was at stake in it than a critique of Weyl's theory (which was generally considered a dead horse at the time). Reichenbach intended to call into question the very idea that, since general relativity has 'geometrized' the gravitational field, the obvious next move should be to try to 'geometrize' the electromagnetic field.

The importance of this formalism for general relativity was to which was the key content of the theory. Let's conisder the four-velocity of a particle $u^\tau = dx_\nu/d\lambda$. By parallel-displacing a vector u^τ indicating the direction of a curve $x_\nu(\lambda)$ at any of its points, one can define a special class of curves, the straightest lines.

$$\frac{du^\tau}{d\lambda} - \Gamma^\tau_{\mu\nu} u^\mu u^\nu = \underbrace{\frac{\rho}{\mu} f^\mu_\tau \frac{dx^\nu}{d\tau}}_{\hookrightarrow \text{ force term}}$$

The force term indicates that the force experienced by charged particles is directly proportional to the charge and inversely proportional to the mass. On the contrary, since the gravitational charge is the same to all particles, the mass coupling factor can be eliminated from the geodesic equation. Thus grav- check itation appears to be geometrized, but not electromagnetism. To define the affine connection as which is general non symmetric. Every is the sum of and a non-symmetric tensor:

$$\Gamma^\tau_{\mu\nu} = \left\{ \begin{matrix} \mu\nu \\ \tau \end{matrix} \right\} + \frac{\rho}{\mu} g_{\mu\sigma} f^\mu_\tau$$

The term is introduced to lower the indeces. As one can see simply the force term in the definition of the of the affine connection. In this way, is also geometrized. That uncharged particles travel and charged particles on the shortest line, that a different affine connection for every particle.

Einstein must have immediately read or at least glanced at Reichenbach's attempt at providing a unified field theory, and he replied a few days later on March 31, 26. His initial reaction: Reichenbach's equations of motion can be valid only for a certain charge-density-to-mass-density ratio ρ/μ (or, in the case of particles, a certain charge-to-mass ratio e/m). In a given displacement, there is only one straightest line passing through a point in a given direction, but different test particles with different charge-to-mass ratios accelerate differently in the same electric field. Thus they cannot all travel on the same straightest line (see below in **??**). After all, this is the precise difference between gravitational and non-gravitational forces. Reichenbach was not very impressed by the objection, which actually quite.

However, Einstein misunderstood the spirit of the typescript. Reichenbach makes clear that the physicists should in no way think that he had some "secret physical intention" (Reichenbach to Einstein, Apr. 4, 1926; EA, 20-086). Thus, Reichenbach recounted to Einstein why he decided to write the note. He was working on a philosophical presentation of the problem of space (see below in section 3), and of course he felt compelled to add a chapter about 'Weyl space,' or more generally about attempts to 'geometrize' the electromagnetic field by using some generalization of Riemannian geometry: "Thereby I wondered what the geometrical presentation of electricity actually means" (Reichenbach to Einstein, Apr. 4, 1926 EA, 20-086) .

One start to find a suitable geomeri structure, e.g. the $\Gamma^{\tau}_{\mu\nu}$ or the $g_{\mu\nu}$ and ϕ_{μ}, as in Weyl geometry. The identification of only at hidest to find the e.g. in Einstien's new theory it is natural. This In one class of theories, $\Gamma^{\tau}_{\mu\nu}$ without any physical meaning, however, this was precisely only a graphical only 'graphical representations' (*graphische Darstellungen*)—an expression he evidently borrowed from Eddington (1925, 294ff.). Had no physical meaning.

$\Gamma^{\tau}_{\mu\nu}$ and give to this structure to start. e.g. Weyl version of the theory. Reichenbach's geometrical interpretation, he insisted, had "the advantage over other geometrical representations in that *the operation of displacement possesses a physical realization [Realisierung]*" (Reichenbach to Einstein, Apr. 4, 1926 EA, 20-086; my emphasis) , namely, the velocity-vector of charged mass particles of unit mass. In Eddington's parlanceit is a 'natural geometry.' This is geometrization. However, Reichenac's htat such geoemtrization does not bring any physical meanign in tiself.

Reichenbach offered to send Einstein the corresponding epistemological sections of the text on which he was working (possibly §50 of the Appendix). In a letter from April 8, 1926 Einstein did not comment on this offer, but his reaction to Reichenbach took a different tone. Even if Einstein did not reply to Reichenbach's more technical remarks, Reichenbach's philosophical point clearly resonated with him:

> You are completely right. It is incorrect to believe that 'geometrization' means something essential. It is instead a mnemonic device [*Eselsbrücke*] to find numerical laws. If one combines geometrical representations [*Vorstellungen*] with a theory, it is an inessential, private issue. What is essential in Weyl is that he subjected the formulas, beyond the invariance with respect to [coordinate] transformation, to a new condition ('gauge invariance').[10] However, this advantage is neutralized again, since one has

[10]That is, invariance by the substitution of g_{ik} with λg_{ik} where λ is an arbitrary smooth

> to go to equations of the 4. order,[11] which means a significant increase of arbitrariness (Reichenbach to Einstein, Apr. 8, 1926 EA, 20-117).

For our purposes it is interesting that Einstein not only endorsed Reichenbach's claim that a 'geometrization' is not an essential achievement of general relativity, but also questioned the meaning of the notion of 'geometrization,' and for that matter the very notion of 'geometry' (Lehmkuhl, 2014). This latter step was not taken by Reichenbach, who preferred to speak of general relativity as a 'geometrical interpretation of the gravitational field,' albeit not a 'geometrization' of the latter. The point that geometrical interpreation, however was not itself of physocal reasult.

On May 26, 1926 Reichenbach might have possibly have presented this improved version of the note in Stuttgart at the Gauvereinstagung of the Deutsche Physikalische Gesellschaft (the regional meeting of the German Physical Society). The abstract of this presentation was published under the title "Die Weylsche Erweiterung des Riemannschen Raumes und die geometrische Deutung der Elektrizität" (Reichenbach, 1926). Reichenbach revealed that what he wanted to achieve was a geometrical interpretation of a physical field 'in *the same sense as gravitation*' in Einstein's theory, i.e., one that was *just as good* as that attained by general relativity. The geometrical operation of displacement has a physical interpretation in Reichenbach's toy-theory, just like the *ds* does in general relativity Thus, Reichenbach claims to have provided not just a successful 'geometrical interpretation' of the electromagnetic field, but an interpretation that was of the same 'quality' as the one general relativity provided for the gravitational field. However, this was Reichenbach's point: the theory was not a successful physical theory like general relativity. Thus, he concluded, providing a geometrical interpretation of a physical field is not in itself a physical achievement: "the geometrical interpretation is only a different parlance, which does not entail anything new physically". [][25; my emphasis][Reichenbach1926d].

3 Unification: Back to Berlin

In August 1926, Reichenbach was granted teaching privileges as an "unofficial associate professor" (*nichtbeamteter außerordentlicher Professor*) at the University of Berlin (Hecht and Hoffmann, 1982). The discussion seminar that he started to hold in October became soon the basis of the so-called 'Berlin group,' which, together with Schlick's cognate 'Vienna circle' has marked the history of 20th century 'scientific philosophy' (Danneberg, Kamlah, and Schäfer, 1994, Milkov and Peckhaus, 2013). By the end of the year, Reichenbach wrote to Schlick, keeping him up to date with the progress of a two-volume book he was

function of position (cf. Weyl, 1918b, 468). Weyl introduced the expression 'gauge invariance' (*Eichinvarianz*) in Weyl, 1919, 114.

[11] Cf. Weyl, 1918b, 477. Einstein regarded this as one of the major shortcomings of Weyl's theory; see Einstein to Besso, Aug. 20, 1918; CPAE, Vol. 8b, Doc. 604, Einstein to Hilbert, Jun. 9, 1919; CPAE, Vol. 9, Doc. 58.

writing, which was supposed to bear the title *Philosophie der exakten Naturerkenntnis*. "The first volume that deals with space and time," he wrote, "is finished" (Reichenbach to Schlick, Dec. 6, 1926; SN). Reichenbach hoped to publish the book in the forthcoming Springer series 'Schriften zur wissenschaftlichen Weltauffassung' directed by Schlick and Philipp Frank. However, Springer rejected the book as being too long. According to Reichenbach's later recollections, the manuscript of the first volume was not changed significantly after February 1927. By July Reichenbach could announce to Schlick that he had reached a publication arrangement (Reichenbach to Schlick, Jul. 2, 1927; SN). The publisher agreed to publish only the first volume under the title *Philosophie der Raum-Zeit-Lehre*. The drafts were finished in September and the preface was dated October 1927.

Einstein read the manuscript of the *Philosophie der Raum-Zeit-Lehre* while on his way to Brussels to attend the fifth Solvay Congress (Bacciagaluppi and Valentini, 2009). In a letter to his second wife Elsa after his arrival, he appeared impatient toward Reichenbach's assertive style, but possibly also about his 'positivistic' philosophical point of view: "I finished reading Reichenbach. To be so delighted with oneself must be pleasing, but less so for other people" (Einstein to Elsa Einstein, Oct. 23, 1927; CPAE, Vol. 16, Doc. 34). Some weeks later, in December, Reichenbach wrote to Einstein that Paul Hinneberg, the editor of the *Deutsche Literaturzeitung* had told him that Einstein intended to write a review of his forthcoming book, *Philosophie der Raum-Zeit-Lehre*. Reichenbach sent him the galley proofs and also added that he would send an Appendix in the coming days (Einstein to Reichenbach, Dec. 1, 1927; EA, 20-090). Einstein's review appeared in the first 1928 issue of the *Deutsche Literaturzeitung* (Einstein, 1928c). abs 295?

The review was little more than a short summary. However, Einstein emphasized two philosophically significant issues, both concerning the Appendix of the book. (1) "In the Appendix, the foundation of the Weyl-Eddington theory is treated in a clear way and in particular the delicate question of the *coordination* of these theories to reality" (Einstein, 1928c, 20; my emphasis). Reichenbach had claimed that, as in any other theory, also in unified field theory, one should give physical meaning to the variables used ($g_{\mu\nu}$, $\Gamma^{\tau}_{\mu\nu}$, etc.) from the outset, before starting to search for the field equations. Einstein did not comment further on this issue, probably because, over the years, he had come to realize that this requirement was too strict. However, Einstein was in full agreement with the second point made by Reichenbach: (2) In the Appendix, "in my opinion quite rightly—it is argued that the claim that general relativity is an attempt to *reduce physics to geometry* is unfounded" (Einstein, 1928c, 20; my emphasis). As we have mentioned, Reichenbach and Einstein had already discussed this topic in a private correspondence less than two years earlier (Giovanelli, 2016). For this reason, Einstein immediately perceived the importance of this theme in Reichenbach's book, a theme that later readers often overlooked (Giovanelli, 2021).

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The issue of 'geometrization' was indeed close to Einstein's heart at that time (Lehmkuhl, 2014). Simultaneously with Reichenbach's review, after nearly a year-long correspondence, at the end of 1927, Einstein (Einstein to Meyerson, Dec. 24, 1927; EA, 18-294) gave final authorization for the publication of another, more extensive review of *La déduction relativiste* written by the French philosopher Émile Meyerson (Meyerson, 1925). The review was published in Spring 1928 in French (Einstein, 1928a). In the book, Meyerson had regarded relativity as a central stage in the process of progressive geometrization of physics, which had started with Descartes and that promised to go on with the theories of Weyl and Eddington. In the context of an otherwise laudatory review, Einstein strongly disagreed. According to Einstein, "the term 'geometrical' used in this context is entirely *devoid of meaning*" (Einstein, 1928a, 165; my emphasis). Historical reasons aside, there was no real ground to define $g_{\mu\nu}$, the gravitational field, as a geometrical field, and, say, the $F_{\mu\nu}$, the electromagnetic field, as a non-geometrical field. The goal of the unified field theory-project was not to 'geometrize' both fields but to 'unify' them, to show that they are nothing but two aspects of a unique 'total' field of unknown structure. Without the equivalence principle, mathematical simplicity had become the only guide in the quest for the fundamental field structure. For this reason, however, Einstein very much appreciated Meyerson's insistence on 'the deductive-constructive character' of relativity theory (on this episode see Giovanelli, 2018).

In spring 1928, during a period of rest after a circulatory collapse, Einstein, as he wrote to Besso, "laid a wonderful egg in the area of general relativity" (EA, 40-69) . On June 7, 1928 he presented a note to the Prussian Academy on a 'Riemannian Geometry, Maintaining the Concept of Distant Parallelism' (Einstein, 1928d), a flat space-time that is nonetheless non-Euclidean since the connection is non-symmetrical. On June 14, 1928 he submitted a second paper in which the field equations are derived from a variational principle (Einstein, 1928b). Reichenbach wrote to Einstein with some comments on the theory on October 17, 1928:

> Dear Herr Einstein,
> I did some serious thinking on your work on the field theory and I found that the geometrical construction can be presented better in a different form. I send you the ms. enclosed. Concerning the physical application of your work, frankly speaking, it did not convince me much. *If geometrical interpretation must be, then I found my approach simply more beautiful, in which the straightest line at least means something.* Or do you have further expectations for your new work? (Reichenbach to Einstein, Oct. 17, 1928 EA, 20-92; my emphasis).

There are two aspects of this passage that should be considered separately. The first part refers to the mathematical-geometrical aspect of Einstein's papers. The manuscript to which Reichenbach refers seems to have been lost. However, from Einstein's reply on October 19, 1928 one can easily infer that Reichenbach must have sent him the classification of geometries which would appear in an article Reichenbach submitted in February of 1929 (Reichenbach, 1929c, see below in this section). Einstein agreed that in principle it was possible to proceed as Reichenbach suggested, “starting with displacement law, and to specialize it on the one hand with the introduction of a metric on the other side with the introduction of integrability properties” (Einstein to Reichenbach, Oct. 19, 28 EA, 20-094) . Reichenbach in fact defines a metrical space by imposing the condition $d(l^2) = 0$ to the displacement space $\Gamma^{\tau}_{\mu\nu}$, which in general is non-symmetrical; he then obtains Einstein space by requiring that the Riemann tensor $R^{\tau}_{\mu\nu\sigma}(\Gamma)$ vanishes.[12] Einstein, in contrast, preferred the classification he had given in his paper: Weyl’s geometry allows for the comparison over finite distances neither of lengths nor of directions; Riemannian geometry allows the comparison of lengths, but not directions; and Einstein’s geometry directions but not lengths (Sauer, 2006).

This, however, was only a minor point. Reichenbach’s further remark concerning the physical application of Einstein’s geometrical setting is, from a philosophical standpoint, more interesting, even if Einstein did not comment on it. Reichenbach claims that, if one really wants to provide a geometrical interpretation of gravitation and electricity, then his own approach was better after all. Reichenbach uses his own toy-theory as a benchmark for a good ‘geometrical interpretation’ (but of course not for a good physical theory). Reichenbach’s theory provides a physical meaning to the displacement operation and thus a physical definition of a straightest line. On the contrary, Einstein’s theory did not attempt to provide a physical interpretation of the notion of displacement, nor even the field quantities; if the theory has nothing more to offer, Reichenbach claims, (i.e., if the theory does not solve the problem of the electron) it is merely a ‘graphical representation’ (cf. also Eddington, 1929 for a similar judgment).

In a note added by hand at the bottom of the typewritten letter, Einstein invited Reichenbach and his first wife Elisabeth for a cup of tea on November 5, 1928, mentioning that Erwin Schrödinger[13] would also be present (Reichenbach to Einstein, Oct. 17, 1928; EA, 20-92). It was probably on that occasion that Einstein told Reichenbach about the physical consequences of the theory he was working on. In the meantime, on November 4, 1928, an article by Paul Miller appeared in *The New York Times* with the sensational title “Einstein on Verge of Great Discovery; Resents Intrusion”. The paper triggered the curiosity of the press. In the late 1920s Reichenbach was a regular contributor to the *Vossische Zeitung*, at that time Germany’s most prestigious newspaper; not surprisingly he was asked for a comment on Einstein’s theory. With the advantage of having personally discussed the topic with Einstein, Reichenbach published a brief didactic paper on Einstein’s theory on January 25, 1929 (Reichenbach, 1929b).

Reichenbach conceded that Einstein’s theory provided a unification of gravitation and electricity which had more than just formal significance, since it made “new assertions concerning the relation between gravitation and electricity in relatively complicated fields” (Reichenbach, 1929b) . However, he maintained

[12]The Γ alludes to the fact that this condition can be defined without reference to the $g_{\mu\nu}$.

[13]Schrödinger succeeded Max Planck at the Friedrich Wilhelm University in Berlin in 1927. He held his inaugural lecture on July 4, 1929 (Schrödinger, 1929).

his skepticism by claiming that the theory was "only a first draft, lacking the persuasive powers of the original relativity theory because of the *very formal method by which it is* established" (Reichenbach, 1929b, ; my emphasis) .

Einstein insisted on the speculative nature of the new theory, which, however, he presented as a continuation of the same strategy that was successful in his search for the field theory of gravitation: individuate a suitable field structure, the $g_{\mu\nu}$, and search for simplest differential generally covariant equations that can be obeyed by the $g_{\mu\nu}$. For general relativity, the choice of the $g_{\mu\nu}$ was suggested by a physical fact, the equivalence principle. However, in the search for a more general mathematical structure that would include the electromagnetic field, Einstein continued, "the experience does not give—so it seems—any starting point" (Einstein, 1929e, 128). Thus, the only hope is to develop a theory "in a speculative way" (Einstein, 1929e, 128). To solve this problem, the physicist must venture along "a purely intellectual path" having as only motivation the deep conviction of the "formal simplicity of the structure of reality" (Einstein, 1929e, 127). The belief in the fundamental simplicity of the real is "so to speak, the religious basis of the scientific endeavor" (Einstein, 1929e, 127).

Indeed, for *Fernparallelismus*, no attempt was made to give a direct physical meaning to the fundamental field variables h_a^ν. One starts from this mathematical structure and then searches for the simplest and most natural field equations that the vierbein-field can satisfy (Einstein, 1929e, 131). The physical soundness of the field equations thus found can be confirmed only by integrating them, which was usually a very difficult task. Einstein warned his readers of the dangers of proceeding "along this speculative road" (Einstein, 1929e, 127). In a footnote, Einstein even endorsed "Meyerson's comparison with Hegel's program [*Zielsetzung*]" which "illuminates clearly the danger that one here has to fear" (Einstein, 1929e, 127).

4 Unificaiton

Reichenbach was clearly not the only one to write about Einstein's new theory in the press. On January 12, 1929—one day after Einstein submitted a third paper on distant parallelism (Einstein, 1929f) to the Academy—*The New York Times* published an article entitled 'Einstein Extends Relativity Theory.' It was amid this atmosphere that, at the end of January, Einstein wrote an angry letter to the *Vossische Zeitung* lamenting Reichenbach's "tactless behavior" in violating the academic code (Einstein to the Vossische Zeitung, Jan. 25, 1925; EA, 73-229). On January 26, 1929, the curator of the literary section, Monty Jakobs (cf. Badenhausen, 1974), defended the behavior of the newspaper and forwarded Einstein's letter to Reichenbach (Jakobs to Einstein, Jan. 26, 1925; EA, 73-230). Reichenbach wrote to Einstein the next day with feelings ranging from surprise to anger; he complained that Einstein did not write directly to him after all he had done to defend relativity theory (Hentschel, 1982), and he denied any wrongdoing (Reichenbach to Einstein, Jan. 27, 1925; EA, 20-

096). On January 30, 1928 Einstein replied that he was somewhat pleased by Reichenbach's annoyance, which was the "fair equivalent" of the annoyance he had caused by feeding the press private information (Einstein to Reichenbach, Jan. 30, 1920; EA, 20-099). However, Einstein quickly settled the dispute to Reichenbach's relief (Reichenbach to Einstein, Jan. 31, 1920; EA, 20-101).

On January 30, 1929 Einstein's paper was finally published in the proceedings of the Academy with the vague title 'On the Unified Field Theory' (Einstein, 1929f). On February 2, 1929 another semi-popular paper by Reichenbach was published in the *Zeitschrift für Angewandte Chemie* (Reichenbach, 1929a) without any reaction from Einstein. Einstein's anger at Reichenbach (which might at first seem rather exaggerated) is understandable if one keeps in mind the attention that the theory was attracting among the public; Einstein might have been upset that a colleague and friend would also contribute to the craze. At the beginning of February the *New York Herald Tribune* (February 1) printed a translation of the entire paper. Several days later *The New York Times* (February 3) and the *London Times* (February 4) published Einstein's own popular account. The 'irrational exuberance' towards the theory is well attested to by a letter Eddington sent to Einstein a few days later, recounting that Selfridges—a British chain of high-end department stores—had pasted all six pages of Einstein's papers in its window (Eddington to Einstein, Feb. 11, 1929; EA, 9-292).

The first article of the order of publication was entitled "Die neue Theorie Einsteins über die Verschmelzung von Gravitation und Elektrizität" (Reichenbach, 1929a) and would appear in February in the *Zeitschrift für Angewandte Chemie*. The second article was an extended version of the manuscript that Reichenbach had sent to Einstein in October and bore the same title "Zur Einordnung des neuen Einsteinschen Ansatzes über Gravitation und Elektrizität" (Reichenbach, 1929c). It was published only in September in the *Zeitschrift für Physik*. These articles represent Reichenbach's last important contribution to issues related to relativity theory and spacetime theories. On the one hand, Reichenbach attempted to make his previous reflections about the unified field theory-project in the Appendix to the *Philosophie der Raum-Zeit-Lehre* to bear fruit (Reichenbach, 1928, §46). On the other hand, he added new elements of clarification by clearly distinguishing the 'geometrization program' and the 'unification program.'

In the first paper for the *Zeitschrift für Angewandte Chemie*, Reichenbach introduced the history of the unified field theory in an entirely different manner than before. The brief history of the unified field theory-project appeared to him as the progressive *downfall* of the geometrization program and the concurrent *rise* of the unification one. In analogy with general relativity, Weyl introduced what, in Reichenbach's parlance, amounts to a coordinative definition of the operation of displacement of vectors. In Weyl's geometry, spacetime lengths of vectors at the same point in different directions can be compared, but the length of vectors at distant points is path-dependent. It was then natural to assume that the length of vectors could be measured by rods and clocks. Consequently, "one would surmise an influence of the electric field on transported rods and clocks " (Reichenbach, 1929a, 122). However, as it turns out, rods and clocks under the influence of the electromagnetic field does not behave as predicted by Weyl's theory. This argument is the gist of Einstein's so-called 'measuring rod objection.' The fact that the atoms that we use as clocks have sharp spectral lines, Einstein (1918) argued, disproves Weyl's theory (see Ryckman, 2005, §4.2.4).

According to Reichenbach, Weyl (1920a), rather than abandoning the theory, decided to simply forego such a coordinative definition of the process of displacement in terms of rods-and-clocks readings. The selection of Weyl geometry rather than Riemannian geometry would be justified only after the field equations are established, usually by way of an action principle (Weyl, 1921b,c). From the latter, one should have been able to deduce the behavior of the material structures that one uses as rods and clocks, which, however, would have nothing to do with the law of parallel transport of vectors that lies at the basis of the theory. In a somewhat disguised form, Weyl's strategy was ultimately adopted by physicists working on the unified field theory-project (Eddington, 1921, 1923). In this manner, however, Reichenbach concluded, the 'geometrization program' was implicitly abandoned and substituted by a new, different 'unification program.' Most physicists, including Einstein (1923, 1925) considered this strategy legitimate. It was preferable to sacrifice the geometrical interpretation—i.e., to relinquish the coordination of geometrical notion of parallel transport of vectors with the behavior rods and clocks—and then to use the geometrical variables ($\Gamma^{\tau}_{\mu\nu}$, φ_{ν} and so on) as 'calculation device' for the greater good of finding the field equations. From the field variables, one has to attempt to establish the simplest differential invariants that can be used as an action function.

In the meantime, on January 22, 1929, Reichenbach had already submitted a second and more technical paper, which only appeared in the *Zeitschrift für Physik* in September (Reichenbach, 1929c). The paper offers a readable presentation of Einstein's new theory; Reichenbach again presented his own take on the relationship between displacement and metrical space, and located Einstein space as an alternative to Riemannian space, rather than a generalization of it (Reichenbach, 1929c, 684–687).

After this semi-popular presentation of Einstein's geometry and its physical application, Reichenbach added some remarks that are interesting from a philosophical point of view. He pointed out that there are two ways to unify two different physical theories. The first is a *formal unification*, comparable to the relationship between Lagrangian and Hamiltonian formalism in classical mechanics (the first can be Legendre transformed into the other without adding any new physical knowledge); the second is an *inductive unification*, exemplified by the relationship between Kepler and Newton's laws (something new is of course added by moving from Kepler's laws to Newton's theory of gravitation).

The first approach was the one used by Reichenbach himself in his own 'unified field theory':

> The author [Reichenbach] has shown that the first way can be realized in the sense of a combination of gravitation and electricity to one field, which determines the geometry of an extended Riemannian space; it is

> remarkable that thereby *the operation of displacement receives an immediate geometrical interpretation, via the law of motion of electrically charged mass-points.* The straightest line is identified with the path of electrically charged mass-points, whereas the shortest line remains that of uncharged mass points. In this way one achieves *a certain parallelism to Einstein's equivalence principle.* By the way [the theory introduces] a space which is cognate to the one used by Einstein, i.e., a metrical space with non-symmetrical $\Gamma^{\tau}_{\mu\nu}$. The aim was to show that the geometrical interpretation of electricity does not mean a physical value of knowledge per se (Reichenbach, 1929c, 688; my emphasis).

Notice that, according to Reichenbach, the advantage of his own approach consists in the fact that it provides a physical realization of the displacement operation, and also (Reichenbach insists) an analogon to the equivalence principle (at least for particles of certain charge-to-mass-ratio). The disadvantage is that it is only a *unification of the representations* of two physical fields in a common geometrical setting. The second approach is the one used by Einstein, and it presented the opposite characteristics:

> On the contrary Einstein's approach of course uses the second way, since it is a matter of increasing physical knowledge; it is the goal of Einstein's new theory to find such a concatenation of gravitation and electricity, that only in first approximation it is split in the different equations of the present theory, while is in higher approximation reveals a reciprocal influence of both fields, which could possibly lead to the understanding of unsolved questions, like the quantum puzzle. However, it seems that this goal can be achieved only *if one dispences with an immediate interpretation of the displacement, and even of the field quantities themselves.* From a geometrical point of view this approach looks very unsatisfying. Its justification lies only on the fact that the above mentioned concatenation implies more physical facts that those that were needed to establish it (Reichenbach, 1929c, 688; my emphasis).

Einstein's theory was claimed to be a *unification of the dynamics* of two physical fields, i.e., a unification of the fundamental interactions. However, Reichenbach argues that Einstein could achieve this result only at the cost of dispensing with a physical interpretation of the fundamental quantities. Thus, according to Reichenbach, his own theory had the ambition of being a '*proper geometrical interpretation*' (or, one might say, to provide a 'natural geometry'), but it was physically sterile; Einstein's theory sought to be physically fruitful, but it was merely a '*graphical representation*' (see also Eddington, 1929). Clearly, for Reichenbach, only general relativity was able to combine both virtues: it was a proper geometrical interpretation (the ds, and thus the $g_{\mu\nu}$ are measured using rods and clocks) that leads to new physical results. Reichenbach did not seem to realize (or at least does not explicitly point out) that this epistemological standard had become hard to comply with in precisely the context of the field-theoretical explanation of the electron that he was calling for.

However, in Reichenbach's reconstruction, after Weyl's failure of pursuing **??**, most physicists, and in particular Einstein, opted for **??**. Einstein seemed to believe that **??** could be justified based on a different ground, assuming that nature satisfies the simplest imaginable mathematical laws. This assumption was the new *physical hypothesis* on which the strategy **??** could be based (see Reichenbach, 1928, §50). One searches for the most natural field structure, and the simplest field equations that such structure satisfies. After all, Einstein could claim, this is how physics has always been done: Maxwell's equations are nothing but the simplest laws for antisymmetric tensor field $F_{\mu\nu}$ which is derived from a vector field; Einstein's equations were the simplest generally covariant laws that govern a Riemannian metric $g_{\mu\nu}$ and so on. The only warranty of the success of this speculative groping in the chaos of mathematical possibilities was the unification power of the field equations obtained. The latter should have predicted some unknown coupling between the electromagnetic field and the gravitational field, which ultimately would have served as the basis of a theory of matter. This was indeed the case of the *Fernparallelismus*-field theory.

To Reichenbach's dismay, Einstein had abandoned the *physical heuristic* that leads him to general relativity in the name of a *mathematical heuristic* that was not different from Weyl's speculative approach that he had dismissed a decade earlier.[14] Einstein's philosophical volte-face might have appeared to Reichenbach as a sort of *trahison des clercs*, an unacceptable intellectual compromise. (a) The core of Reichenbach's philosophy was the *separation of mathematical necessity and physical reality.* Reichenbach had always perceived this separation as nothing more than a philosophical distillation of Einstein's scientific practice. (b) In the search of a unified field theory, Einstein had come implicitly to question this very distinction, ultimately pleading for a *reduction of physical reality to mathematical necessity*[15] . Einstein put it bluntly in his Stodola-*Festschrift*'s contribution—that he sent for publication toward the end of January (Einstein to Honegger, Jan. 30, 1929; CPAE, abs. 864). The ultimate goal of understanding reality is achieved when one could prove that "even God could not have established these connections otherwise than they actually are, just as little as it would have been in his power to make the number 4 a prime number" (Einstein, 1929e, 127). In this sense, Einstein's God indeed resembles Spinoza's God (Einstein, 1929b), for whom the laws of nature are necessary, and rather than, say, Leibniz's God for whom the laws of nature are contingent.

4.1 A Parting of The Ways. Positivists and Metaphysicians

On January 30, 1929, Einstein's rumored new derivation of the *Fernparallelismus*-field equations was published in the Proceedings of the Berlin Academy with the ambitious tile *Zur einheitlichen Feldtheorie* (Einstein, 1929f). Despite

[14]See Weyl to Einstein, May 18, 23; CPAE, Vol. 13, Doc. 30 and Weyl to Seelig, May 19, 1952, cit. in Seelig, 1960, 274f..

[15]Already in his habilitation, Reichenbach, although rather in passing, accused Weyl of attempting to deduce physics from geometry, by reducing physical reality to 'geometrical necessity' (Reichenbach, 1920, 73). However, the greatest achievement of general relativity, Reichenbach claimed, was to have shifted the question of the truth of geometry from mathematics to physics (Reichenbach, 1920, 73). Einstein was now committing the very same "old mistake" again (Reichenbach, 1920, 73). On Reichenbach's habilitation, see Padovani (2009).

his anger toward Reichenbach's 'leaks,' Einstein did not hesitate to feed the hopes of the general public by popularizing his new theory in the daily press. On February 2, 1929, in its section "News and Views" (1929), *Nature* reported an interview of Einstein published in the *Daily Chronicle*, on January 26, 1929, a day after the publication of Reichenbach's infamous article in the *Vossische Zeitung*. Einstein's quarrel with Reichenbach had deeper philosophical roots that went way beyond questions of academic etiquette. A few days later, Einstein wrote a popular account of the new theory (Einstein, 1929a). Its English translation was published on the first page of their Sunday supplement of the *New York Times* on February 3 and in *The Times* of London in two installments on February 4 and 5 (Einstein, 1929c,d, also published as Einstein, 1930e).

Einstein insisted on "the degree of formal speculation, the slender empirical basis, the boldness in theoretical construction, and finally the fundamental reliance on the uniformity of the secrets of natural law and their accessibility to the speculative intellect" (Einstein, 1930e, 114). This "speculative method", Einstein claimed, was the same that lead to to success of general relativity: "Which are the simplest formal structures that can be attributed to a four-dimensional continuum, and which are the simplest laws that may be conceived to govern these structures?" (Einstein, 1930e, 115). In trying to defend this epistemological stance, Einstein was not afraid to side with "Meyerson in his brilliant studies on the theory of knowledge" who had emphasized the 'Hegelian' nature of such enterprise, "without thereby implying the censure which a physicist would read into this" (Einstein, 1930e, 115).

The fact the Einstein chose to mention Meyerson rather than Reichenbach as a philosophical reference in a popular presentation of his last theory for a major newspaper cannot be underestimated. Of course, Einstein was well aware of Reichenbach's technically informed work on this very subject, having discussed it with him in the previous months. Nevertheless, as he did in the contribution for the Stodola-*Festschrift* (see above **??**), Einstein preferred to side with Meyerson's less detailed, but, in his view, a more profound philosophical outlook—endorsing even his somewhat outrageous comparison with Hegel (Giovanelli, 2018). After a decade of personal friendship and intellectual exchange that had shaped the history of 20th-century philosophy of science and, to a certain extent of 20th-century physics, a minor squabble had unwittingly revealed a nearly unbridgeable philosophical divide.

After 1919 Einstein benefited from a universal acclaim among the general public; however his positions among the physics community became progressively more isolated. Till 1925-1926 the unified field theory-project was pursued by scholars of the stature by Weyl and Eddington, but was also regarded as a viable options by leading quantum theoreticians (Vizgin, 1994, 209). Even, after the 1925-1927 rapid advance in quantum mechanics, were made of relate unified theories to quantum theory (Klein, 1926). However, most leading physicists soon started to perceive the program as obsolete. Einstein was fully aware of the marginality of his position, but, throughout 1929, continued express his confidence in *Fernparallelismus* program. In the second paper of this year finished in August—the fourth in the series in the Berlin Academy—which reflects the priority dispute with Élie Cartan (Debever, 1979), Einstein returned to the Hamiltonian principle after objections raised by his collaborators Lanczos and Müntz (Einstein, 1930a). In spite of the many doubts, Einstein was finally convinced that he had "found the simplest legitimate characterization of a Riemannian metric with distant parallelism that can occur in physics" (Einstein to Cartan, Aug. 25, 1929; Debever, 1979, Doc. V).

However, like Reichenbach, fellow physicists were not impressed, in particular given the growing success of quantum mechanics-program. Weyl, who had always been scolded by Einstein for his speculative style of doing physics could relaunch the accusation in a paper (Weyl, 1929) in which he had uncovered the gauge symmetry of the Dirac theory of the electron (Dirac, 1928a,b). "The hour of your revenge has come", Pauli wrote to Weyl in August: "Einstein has dropped the ball of distant parallelism, which is also pure mathematics and has nothing to do with physics and *you* can scold him" (Pauli to Weyl, Aug. 26, 1929; WPWB, Doc. 235). Although Einstein's papers had been discussed widely especially among mathematicians, Einstein was aware of the poor reception that his work had especially among the colleagues that he probably felt has his peers (Goldstein and Ritter, 2003). As Pauli complained, writing to Einstein's close friend Paul Ehrenfest, "God seems to have left Einstein completely!" (Pauli to Ehrenfest, Sep. 29, 1929; WPWB, Doc. 237).

Nevertheless, Einstein continued to defend the theory in public (in talks given in October and December) (Einstein, 1930b,c,d), as well as in as well in private correspondence. However, Pauli did not hesitate to describe Einstein's presentation at the Berlin Colloquium as a "terrible rubbish" (Pauli to Jordan, Nov. 30, 1929; WPWB, Doc. 238). When he received the drafts of Einstein's *Annalen* paper, he wrote only slightly more politely that he no longer believed that the quantum theory might be an argument for the distant parallelism after Weyl's work on Dirac theory had shown that Dirac's electron theory could be incorporated into a relativistic gravitation theory if the vierbein are introduced but the equations remain invariant if the vierbein at distant points are rotated in arbitrary manner. Pauli also wrote that he did not find the derivation of the field equations convincing; they show "no similarities with the usual facts confirmed by experience" (Pauli to Einstein, Dec. 19, 1929; WPWB, Doc. 239). In particular, Pauli missed the validity of the classical tests of general relativity, perihelion motion and gravitational light bending: "These results seem to be lost in your sweeping dismantling of the general theory of relativity. However, I hold on to this beautiful theory, even if it is betrayed by you!" (Pauli to Einstein, Dec. 19, 1929; WPWB, Doc. 239). When Einstein expressed caution towards the definitive validity of his equations, he, "so to speak, took the words right out of my mouth of criticism-loving physicists" (Pauli to Einstein, Dec. 19, 1929; WPWB, Doc. 239). Pauli knew that Einstein would not have changed his mind, but he was ready to "make any bet" that "after a year at the latest you will have given up all the distant parallelism, just as you had given up the affine theory before" (Pauli to Einstein, Dec. 19, 1929; WPWB, Doc. 239).

Einstein complained that Pauli's remarks were superficial and asked him to return on the issue after some months (Einstein to Pauli, Dec. 19, 1929; WPWB, Doc. 140). Although the unified field theory-project was disavowed by its own initiators (Weyl, 1931), Einstein insisted in the pursuit of *Fernparallelismus* discussing with Mayer two solutions of his last field equations (Einstein and Mayer, 1930). However, Pauli would have clearly won the bet. Only a few months later Einstein and Walther Mayer presented a new approach (Einstein and Mayer, 1931) that, by generalizing the n-bein formalism to five dimensions, may have appeared more promising. This approach was ideally connected with that of Kaluza, but the shortcoming of that theory "by sticking to the four-dimensional continuum, but with vectors with five components" at each point of four-dimensional space-time (Einstein and Mayer, 1931, 377). The optimism once again faded away quickly, since the theory was unable to solve the problem of matter. In a popular talk given in Vienna towards mid-October of 1931, Einstein could only describe his field-theoretical work since general relativity as a "cemetery of buried hopes" (Einstein, 1932, 441).

field equations admitted at least one unphysical solution, namely, a static configuration of uncharged, gravitating bodies.

A few days later, Lanczos wrote to Einstein from the United States (Lanczos to Einstein, Oct. 20, 1931; EA, 15-243) where he had just taken a position at Purdue University. Lanczos told Einstein that, at Arnold Berliner's suggestion, the influential editor of the *Die Naturwissenschaften*, he had prepared a semi-popular presentation of *Fernparallelismus* approach for the *Ergebnisse der Exakten Wissenschaften*, a series sponsored by Berliner's journal (Lanczos, 1931). Lanczos had worked on the topic during his time as Einstein's assistant. The Lanczos/Einstein relation had become somehow strained (Stachel, 1994), and Lanczos was not fully convinced by Einstein's approach. However, he was confident to have found "a tone that should correspond to your conviction as well. I think that, deep down, we have something in common" (Lanczos to Einstein, Oct. 20, 1931; EA, 15-243). Lanczos presented *Fernparallelismus* as a completion rather than an generalization of Riemannian geometry; nevertheless he also recognized the correctness of Reichenbach's approach (Lanczos, 1931, 118). What is more important, he opened the paper with some general considerations which give a glimpse in the philosophical atmosphere which pervaded the physics community. Lanczos distinguished between two "spiritual attitudes" towards relativity:

1. a *positivist-subjectivist* insistence that physics has to do with observable quantities, and what cannot be observed is not part of physics. This "rigorous and therefore more intolerant form of positivism" (Lanczos, 1931, 104), defended in particular by quantum theoreticians, lead to the rejection of the unified field theory program as such. Since a field is nothing but a tool to describe the behavior of test particles, rods and clocks and so, it is vain to search for solutions of the field equations that correspond to protons and electrons. In fact, the fields inside of elementary particles "could never in their details become the object of observation" (Lanczos, 1931, 104), since there are no test particles or measuring scales smaller than the electron itself.

2. A *metaphysical-realistic* perspective, based on the conviction that physical reality exists independently of the possibility of measuring or observing it.

> If special relativity seemed to be close to the positivistic/operationalistic ideals, with Minkowski the theory underwent a "'metaphysical' turn" in favor of a "logical-constructive understanding [*Verstehen*]" (Lanczos, 1931, 103). General relativity had finally brought "the logical-deductive exploration into the depths of nature, under the presupposition of its universality and understandability, and with faith in the laws of mathematics" (Lanczos, 1931, 102).

The positivist described by Lanczos could be easily identified with Pauli, who had indeed raised similar objections against Weyl's theory early on (Pauli, 1919, see Hendry, 1984, 13). However, Pauli, by reviewing Lanczos article, did not fully recognize himself in the portrait of the 'positivist' (Pauli, 1932). Such labels, he argued, "are highly subjective and arbitrary"; it is obvious that in order to gain new scientific insights one does not only requires inductive generalizations, but also logical-constructive imagination. Pauli mocked the *Naturwissenschaften* for having published the paper in series entitled 'Results in exact sciences' (*Ergebnisse der Exakten Wissenschaften*). Indeed, Einstein published this sort of theories at rhythm of one each year and in every case he claims that it is the definitive solution: "Einstein's new field theory is dead, long live Einstein's new theory!".

However, if many readers might have easily recognized someone like Pauli in Lanczos's 'positivist,' other were baffled to find out Einstein located among the 'metaphysicians.' At the beginning of 1932 the introduction of Lanczos's 1931 paper was published at Berliner's suggestion as a *seperatum* in the *Die Naturwissenschaften* "to make it available to a larger public" (Lanczos, 1932, 113; fn. 1). It is probably this article of Lanczos that Frank read with some bewilderment, as he reports in his Einstein's biography (Frank, 1947). Frank was "quite astonished" to find the theory of relativity characterized as the expression of a realist program "since I had been accustomed to regarding it as a realization of Mach's program" (Frank, 1947, 215). However, when Frank met Einstein in Berlin at around the same time, he found out that Lanczos had indeed well characterized Einstein's point of view (Frank, 1947, 215f.). According to his recollection, Einstein complained that "[a] new fashion" had arisen in physics according to which quantities that in principle cannot be measured do not exist, and that to "to speak about them is pure metaphysics" (Frank, 1947, 216). Frank objected that this was the very same philosophical attitude that led to relativity theory. By contrast, Einstein insisted, the essential point of relativity theory is to "regard an electromagnetic or gravitational field as a physical reality, in the same sense that matter had formerly been considered so" (Frank, 1947, 216). The theory of relativity teaches us the connection between different descriptions of one and the same reality. Was not a theory about the behavior of rods and clocks, but a unification of two fields.

Lanczos' reconstruction is too broadly stroked to be fully accurate; nevertheless it undeniably grasps something of the intellectual mood (*geistige Einstellung*) of the time. Reichenbach, like Pauli, would not have been entirely pleased of having been cast among the 'positivists,' with whom he was in conflict for quite some time; however, like Frank, he would have been puzzled, if not appalled, by seeing Einstein categorized among the 'metaphysicians.'

5 Conclusion

Reichenbach invited Einstein to contribute to the newly founded journal *Erkenntnis* published by Felix Meiner and edited with Carnap (Reichenbach to Einstein, Apr. 25, 1930; EA, 73-226). However, to no avail. Nevertheless, when Hugo Dingler (1933), a few years later, launched a political attack against the journal, he mocked Reichenbach as "Einstein's self-proclaimed personal philosopher [*Leibphilosoph*]" who replaced logic with the authority of a great physicist (Dingler, 1933, VI). As we have seen, besides the deterioration of their personal relationship, Einstein's extreme rationalism in those years (Einstein, 1933a) could not be more distant from Reichenbach's inductivism (Reichenbach, 1931, see Galavotti, 2009). But Dingler did not mean to open a scholarly dispute (Howard, 2003). Reichenbach replied from his Turkish exile, insisting on the political independence of journal (Reichenbach, 1934). However, the situation rapidly deteriorated, and the seventh volume of *Erkenntnis* (1937-1938) was edited by Carnap alone.

Reichenbach's initial enthusiasm for Turkey soon waned and he tried to obtain a position in Princeton, where Einstein had settled in 1933 (Verhaegh, 2020). However, Reichenbach feared Weyl's opposition: "He is my adversary since a long time," he wrote to the American philosopher Charles W. Morris, a supporter of a form a "mathematical mysticism" that was "very much opposed to my empiricistic interpretation of relativity" (Reichenbach to Morris, Apr. 12, 1936; HR, 013-50-78). Thus, in April 1936, Reichenbach turned to Einstein to ask his support: "I surmise that Weyl's opposition persists to these days and therefore I'd be grateful if you could put a word in my favor" (Einstein to Reichenbach, May 2, 1936; EA, 20-118). By this time, it was ironically Einstein the one indulging in the sort of mathematical mysticism that Reichenbach attributed to Weyl. As Einstein famously confessed to Lanczos, his work on general relativity had made him "a believing rationalist" (Einstein to Lanczos, Jan. 24, 1938; EA, 15-268), convinced that physical truth lies in mathematical simplicity (Ryckman, 2014). However, he continued, the mathematical formulation of the laws of nature need not to be of "*geometrical* nature" (Einstein to Lanczos, Jan. 24, 1938; EA, 15-268).

Only in 1938, because of Morris' mediation, Reichenbach managed to move to the United States (Verhaegh, 2020). The American years did nothing to bridge the philosophical cleavage that had emerged during their late Berlin time. Einstein (1949a, 1949b) praised Reichenbach's (1949)'s contribution to the volume in his honor of the series *Library of Living Philosophers* edited by Paul Schilpp (1949). On those occasion, famously Einstein reestablished a dialogue with Reichenbach (Helmholtz) and Poincaré. Indeed, that ds was a ... whereas ds was of a convention. As we have, here introduced a third figure non-positivist:

That only the whole, the issue of coordination was then in a quite different way. The choice of $g_{\mu\nu}$, the choice is not justified, by the direct physical meanig. The question of identification, was only after the integration of the field equaitons. these would have ... ecc. Besso. The only guearantee was indeed mathematicla, that only semplicity, was the garantiee of realty.

However, the self-described "tamed metaphysician" had grown increasingly impatient toward any philosophy that smelled of 'positivism' (Einstein, 1950, 13). When in 1953 Schilpp asked Einstein for contributing to the volume of the same series in honor of Carnap (Schilpp to Einstein, May 11, 1953; EA, 80-539), he famously declined. After "Reichenbach's death (a few weeks ago),"[16] Schilpp wrote, Carnap was the most important exponent of logical empiricism (Schilpp to Einstein, May 11, 1953; EA, 42-534). Although Einstein agreed with this assessment, he expressed disenchantment toward that type of philosophy that Schlick, Reichenbach, and Carnap represented: "the old positivistic horse, which originally appeared so fresh and frisky, has become a pitiful skeleton" (Einstein to Schlipp, May 19, 1953; EA, 42-534; quot. and tr. in Howard, 1990, 374)

Abbreviations

CPAE	Albert Einstein (1987–). *The collected papers of Albert Einstein.* Ed. by John Stachel et al. 15 vols. Princeton: Princeton University Press, 1987–.
EA	*The Albert Einstein Archives at the Hebrew University of Jerusalem.*
HR	*Archives of Scientific Philosophy* (1891–1953). *The Hans Reichenbach Papers.* 1891–1953.
SN	*Schlick Nachlass.* Noord-Hollands Archief, Haarlem.
WPWB	Wolfgang Pauli (1979–). *Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.* Ed. by Karl von Meyenn. 4 vols. Berlin/Heidelberg: Springer, 1979–.

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[16] Reichenbach died on April 9, 1953.

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