A. Einstein and H. Weyl: Intertwining paths and mutual influences

Erhard Scholz, Wuppertal

Presicce, April 22, 2005

Introduction

The first intense intellectual exchange between the two protagonists of our conference, Albert Einstein and Hermann Weyl, took place during 1918, the last year of the Great War which put such an abrupt and final end to 19th-century European culture.

Of course they had known each other before. Einstein's fame had risen high in Hilbert's and Minkowski's Göttingen shortly after his invention of special relativity. About the time when H. Minkowski presented special relativity as a 4-dimensional unification of time and space and invited mathematicians to contribute to it, Weyl was just finishing his doctoral dissertation (1908). He became known, at first in the mathematical community, by his first book *Die Idee der Riemannschen Fläche / The Concept of Riemann Surface* in 1912. Already a year later he got a call to the *Eidgenössische* Polytechnical School at Zürich, now *Eidgenössische Technische Hochschule,* the *ETH* as I will call it. He started to teach there in October 1913. Einstein left Zürich some months later, in spring 1914, for his new appointment at the Berlin *Akademie der Wissenschaften.*

Einstein's intellectual influence on Weyl grew even stronger after his formulation of general relativity in late 1915. During the year 1915 Weyl was drawn to the German army, and the army service drastically interrupted the continuity of his former research. In summer semester 1916, he came back to Zürich to teach at the ETH. Now he started with a "blank mind as any veteran's" (as he later said)[1] and started intense studies of the general theory of relativity and, on the other hand, of the foundations of analysis. He was immediately attracted by the beauty and challenge of Einstein's new theory, and joined Hilbert's program of a unification of gravity and electromagnetism in order to understand the basic matter structures in a generalized Mie approach. In summer semester 1917 he gave a lecture course on general relativity and its differential geometric basis. Early next year, he prepared his lecture notes for publication by Springer in revised form as *Raum - Zeit - Materie (Space, Time, Matter)*, the first of his great books on mathematical physics. During the lecture course Weyl had already started to refine his understanding of infinitesimal geometry. In early 1918 he formulated his now famous gauge geometric generalization of Riemannian geometry. This was the mathematical starting point for his proposal of a geometrically unified theory of gravitation and electromagnetism, and an attempt to explain the basic matter structures from a purely field theoretic basis, in the line of the Mie-Hilbert program.

The central idea of Weyl's *gauge geometry* was to weaken Riemannian geometry by accepting at first only a *conformal metric* which allowed to measure angles, but made the comparison of lengths of vectors meaningful only if they were attached to the same point of the manifold. He added a structure which he called a *length connection*, given by a differential form φ and transforming properly under changes of the representative of the conformal metric (later to be called a *gauge transformation*). The length connection allowed to transfer a metric chosen at a point P to a different one, Q, by integration along a path. By such an integration procedure it became possible to compare lengths of vectors at different points P, Q at least indirectly. Transfer and comparison were, however, in general path dependent. That made the geometry look a bit strange from a classical metrical point of

---

[1] (Sigurdsson 1991)

view. On the other hand, conformal metric and length connection together uniquely specified a *parallel tranfer* in the manifold (an *affine connection*), which Weyl considered to be a crucial ingredient of any kind of proper infinitesimal geometries. Moreover, there were good reasons to interpret φ as a candidate for the potential of the electromagnetic field, and Weyl did so.

Having arrived at this point, Weyl took up new direct contact with Einstein. On March 1, 1918, he wrote to Einstein that Springer would send proof copies of his book (*RZM*) to his Berlin colleague. In addition he announced his new findings of a generalization of Riemannian geometry and his hope to formulate a unifying action principle for gravity and electromagnetism in its frame [472].[2]

The Einstein - Weyl discussion 1918

Albert Einstein responded warmly. Already a week later he answered, after having received the first proof sheet of *RZM*. The book was a great pleasure for him:

> It reads like a master symphony. Each word has its relation to the whole and the plan of the work is grandiose. [476]

Even to Weyl's unification idea of gravity and electromagnetism by his gauge geometry he responded very friendly — at first. In March 1918 Weyl visited him in Berlin and the two scientists discussed general relativity and Weyl's gauge extension of it. Apparently Einstein arose first doubts as to the physical feasibility of Weyl's proposal of a path dependent transfer of length measure. But they agreed that Weyl ought to send a manuscript explaining his idea for publication in the Berlin Academy *Sitzungsberichte*.[3] After receiving the manuscript, Einstein responded (April 4, 1918) :

> Your paper has arrived. *It is a stroke of genius of first rank.*

---

[2] Correspondence in 1918 between A. Einstein and H. Weyl published in (Einstein 1987). Square bracket numbers [XX] refer to document codes of this volume.

[3] The later paper (Weyl 1918).

> I have not been able, however, to dissolve my measuring rod objection. [498, emphasis in original]

Two days later he added his admiration of the "beautiful consequence (wunderbare Geschlossenheit)" of Weyl's thought, but closed the postcard by a praise which contained a strong clause of distantation, stated in very polite form:

> *Apart from its agreement with reality* it is, in any case, a grandiose achievement of thought. [499, emphasis E.S.]

Weyl must have felt uneasy by the latter kind of acceptance. He did not respond directly to Einstein's reservation with respect to the transfer of measuring rods, but must have started to think how to cope with Einstein's objection. He admired Einstein as an ingenious, mathematically highly versed physicist. He knew well that, at least at that time of their careers, Einstein stood "in a more intimate relationship to reality" than he himself, as he wrote a little later (May 19, 1918, [544]).

Already at the beginning of their encounter in 1918, Einstein and Weyl had clear differences in the evaluation of the physical feasibility of Weyl's unifying gauge geometry, although they had great respect for each other as scientists and as contributors to mathematical physics. Einstein's first and main objection against Weyl's theory is well known and has often been cited in the literature. It went like this:[4]

> (i) If transfer of length measurements is path dependent, clocks ought to change their frequency during their life, dependent on their "history". In particular no stability and well defined equivalence of atomic clocks of the same spectral type could be obtained.

---

[4] It was apparently already mentioned in the meeting between Weyl and Einstein in March 1918 (compare [498]). A first written explicit statement can be found in a letter to Weyl from 15. April 1918 [507], which was publicly documented in the annex to Weyl's paper (Weyl 1918).

Weyl did not consider this argument as a conclusive objection. He argued that the representative of the conformal metric was no basic concept of his theory and ought not be considered as directly observable. If his unified field theory was right, so he argued, one had in any case to develop its consequences for measurement processes by material bodies first and from the basic principles of the theory, before one could compare the results with observation. He guessed that a process of "calibration by adjustment" of material bodies to local field conditions might ensure the independence of observed frequencies of atomic (or other) clocks from their history. He even argued that a mathematically very natural calibration might be obtained by reference to the scalar curvature ("Weyl gauge").[5]

Einstein seems to be struck that his argument did not convince Weyl and specified it by a methodological postulate:

> (i') The line element ds of the metric in general relativity should be directly observable.

Probably he realized that such a difference in methodological questions was a question of conviction and would never be accepted by Weyl who had just stated a methodologically different approach. This may explain why he did not write such an argument in response to Weyl, but only to other correspondents.[6] Maybe he feared that a direct exchange with Weyl on such a level would harden their contraposition and might even appear dogmatic to the latter. To Weyl he added other, more physically appearing, objections in the course of the year 1918. The most important among them were the following:[7]

(ii) If Weyl's length connection φ was interpreted as the poten-

---

[5] First statement of this argument in a letter from Weyl to Einstein, 28. 4. 1819 [526], published version in the appendix to (Weyl 1918). More extended discussion in later editions of RZM. Weyl gauge is first mentioned in Weyl's letter from 18. 9. 1918 [619].

[6] Letter to W. Dällenbach, 15. 6. 1918 [565].

[7] We cannot go here into all details. More points can be found in Einstein's letter [661] from November, 29 1918.

> tial of the Maxwell field, uncharged particles cannot move along geodesics. The deviation of the fall line of *uncharged* particles from geodetical trajectories would depend on the potential of the electromagnetic field, which appears paradoxical. [579, 3. July 1918]
>
> (iii) If Weyl proposed to weaken the possibility of direct comparison of lengths to vectors at the same point of the manifold, allowing for changes in transfer from one point to another, some "Weyl II" might come along some day and propose another weakening of classical Riemannian geometry with the same justification as Weyl I: One ought to accept only angle comparisons at the same point, while the parallel transfer of vectors might change the angles. [551, 31. 6. 1918]
>
> (iv) In any case, Weyl emphasized the concept of gauge invariance too strongly, for Einstein's taste. From the latter's view, it was not at all as important and surprising as claimed by Weyl.

From todays point of view the last argument seems most surprising, therefore I add the direct quote here:

> You insist too strongly on the beautiful invariance theorems gained by gauge invariance ("Aich-Invarianz"). It is not so surprising that the form of the Maxwell equations results from it, because it is known a priori that the Maxwell equations satisfy the gauge invariance anyhow. [661, November 29, 1918]

The last phrase referred to Wiechert who had shown in 1900 that the potential of the Maxwell field can be changed by a gradient of any differential function.[8]

The former part of the argument would let physicists of the end of the 20th century (or of ours) stumble, as in the 1970s and 80s gauge structures

---

[8] See footnote [7] of th editors in Einstein [661].

and invariance principles were transformed into crucial features of field theory. Now they even seem to lie at the heart of the most important theories of basic matter structures, i.e. of "elementary particle" physics. Of course, we have to take into account that the relationship between Weyl's gauge geometry of 1918 and the gauge field theories of late modernity are only indirectly linked, but linked they are. Following and extending proposals by quantum phyiscists of the 1920s (E. Schrödinger, V. Fock, F. London), Weyl transformed his metrical gauge idea into a phase gauge of quantum mechanics in 1929. His results were only indirectly transmitted to the next generation of physicists, C. N. Yang and others who prepared the path to Yang-Mills fields in the 1950s, not ot talk of all the obstacles which had to be surmounted before physically successful applications could be gained from the late 1960s onward. Nevertheless, Weyl's conviction that gauge ideas may be a crucial structure for the understanding of physical fields, in particular the electromagnetic one, and his mathematical elaboration of the basic principles of this view turned out to highly fruitful and consequential for 20th century field theory, contrary to Einstein's judgement.[9]

Einstein's first two points of criticism, the clock argument and the non-geodesicity of trajectories of uncharged particles, gave Weyl reason to delve deeper into the consequences of his gauge geometric approach. He showed that under reasonable assumptions (conservation principles derivable in his approach, a specific choice of gauge, and the assumption of the existence of particle like solutions of the unified field equations) the trajectory of uncharged particles are, in fact, different from geodesics and follow the equations of standard general relativity.[10] For the first point it remained a question of taste and of practicability, whether one would follow Einstein or Weyl in the strategical choice to bind the metric ds to direct observation or to leave it to a theory of observation to link it to empirically observable quantities.

---

[9] (Cao 1997)

[10] Letter to Einstein from November 16, 1918 [657], editors' footnote [3], and (Weyl 1919).

Einstein gave his evaluation of this undecisive result of the discussion with Weyl. In a letter to M. Besso he wrote in December, 4, 1918:

> I am completely convinced that Weyl's gauge invariance does not hold in nature and I have recently given him reasons for my doubt. But I also know that nobody who stays to be in love with an idea more than half a year, can be freed from its spell, at least not by others. [669]

Weyl drew a complementary resumé of the discussion in a letter to the great physicist himself, written December 12, 1918. He regretted that Einstein continued to reject his theory. And he added that he was particularly sad ("betrübt")

> ... because it has been proved by experience that one can rely on your intuition; even though I have to admit that your objections presented up to now are not at all conclusive for me. [669]

Such different appreciations of the potentials of Weyl's gauge geometry and the unified field theory built upon it were closely related to different research goals of our two protagonists. While Einstein was mainly interested in exploring the new perspectives of his gravitation theory, Weyl considered the latter as just one constitutive element of an overarching theory of interaction fields (gravity and electromagnetism at that time). His main goal in mathematical physics was, like Hilbert's, to construct (or better, to mathematically reconstruct) the basic structures of matter, at the time essentially the electron and the hydrogen nucleus, little later called the proton. In such a research context, a mathematical frame in which the trajectories of neutral particles would not be the geometrically most simple lines of fall (geodesics) would not be a *decisive* disadvantage, *if it led* to the desired result of a mathematical understanding of the structure of matter. The latter was the core subject of Hilbert's, Minkowski's and later Born's common long term seminar on mathematical physics at Göttingen, which Weyl had participated in as a research student and later joined again in the early 1930s during his short Göttingen intermezzo as Hilbert's successor. As long as there seemed to be a chance for progress in this direction, Einstein's objections could not carry strong evidence for him.

For Einstein, on the other hand, the question of a field theoretic explanation of matter was still a subordinate one in 1918, pursued mainly by the Göttingen mathematical physics group.[11] From his perspective, the whole Mie-Hilbert-Weyl approach to field and matter theory appeared as an idea which had no great chances for success — correctly, but by other reasons than he could imagine at the time. (It soon turned out that quantum structures, which could not be dealt with in the Mie-Hilbert-Weyl approach, were of crucial importance.) Thus Weyl's insistance to go on with his research program appeared Einstein as a result of being "in love with an idea".

Apparently Einstein knew well by own experience what he was talking about. Notice the clause "nobody who stays to be in love with an idea more than half a year ...". Einstein tried to keep just below this threshold in his own love affairs with ideas. He did not always succeed, sometimes for "good" (general relativity) sometimes for "bad" (unified field theories of the 1920s ff.). So he had good reasons to be not too harsh to Weyl, besides his great admiration for Weyl as a mathematician and as a superb writer and speaker.

A crossing over of research trajectories in the early 1920s

As indicated, Einstein changed his mind with respect to unified field theories aiming at the explanation of basic matter structures in the early 1920s. His U-turn was triggered by A. Eddington's contributions to affine field theory. Eddington took up Weyl's general concept of an affine connection ("parallel discplacement") in a differentiable manifold without any a priori link to an underlying metric. But different from Weyl, he transferred the generaliza-

---

[11] That would change soon. See below, or more in detail (Pais 1982, Stachel 1986/2002, Goenner 2004).

tion directly to physics and looked for an understanding of the metric as a secondary structure, *derived* from an affine connection as the basic physical concept. So he proposed a "generalisation of Weyl's theory of the electromagnetic field and gravitational fields" (title of (Eddington 1921)).

At first Einstein reacted with the same combination of interest and scepticism, which we know already from his comments of Weyl's original theory. In a letter to Weyl from June 6, 1922, he wrote:

> With Eddington's work ... I feel the same way as with Mie's theory; it is a beautiful framework, about which one absolutely does not see how it has to be filled in. (Quoted from (Stachel 1986/2002, 466))

But he soon fell "under the spell of Eddington's idea", to borrow J. Stachel's words.[12] He wrote to Born that he had "finally understood the connection between electricity and gravitation", following the footsteps of Eddington. Now he started to publish own research along this line, even though under the pretext that it was only because "Eddington's train of thought must necessarily thought through to the end" (Einstein to Born, January 11, 1923).

Now it was Weyl's turn to react with mixed intellectual "feelings". Although he felt somehow rehabilitated that Einstein himself was now taking a similarly speculative line of investigation, which the latter had rejected five years earlier, he gave detailed criticisms of Eddington's and Einstein's approach to field theory and geometry. Weyl had developed strong conceptual and mathematical reasons not to rely on affine connections only, as a basis for physical geometry. But these reasons could only be made explicit by rather involved group theoretical considerations in differential geometry (Weyl's "analysis of the problem of space", see below).

Einstein was not impressed. He continued to be in love with his and Eddington's idea, i.e. to pursue this line of research, longer than "half a

---

[12] Further quotes without specification in the following section are from (Stachel 1986/2002).

year". He only gave up the affine program in early 1926, after he had tried all specifications of the research program he could imagine and had run into one impasse after the other. But now he had joined the belief, that a classical field theoretic explanation of matter structure was possible, contrary to most of the younger physicists of the Göttingen milieu and its Sommerfeld (Munich) complement, who had given up this perspective at the turn to the 1920s under the impact of Bohr's and Sommerfeld's theory of the hydrogen atom. To them, it no longer seemed plausible that the energy levels in the atom could be derived by a purely classical methods, even if in a tricky field theory of any kind.

Weyl changed sides already in 1920. His own experiences with the difficulties of his gauge geometric modification of the Mie-Hilbert approach had been enriched by metaphysical speculations on the one side and detailed criticism of young physicists who were, at the outset, more sympathetic to his approach than Einstein, most importantly Wolfgang Pauli. In the year 1919 Weyl gave a talk to the Swiss *Naturforschende Gesellschaft* on the relationship between the causal to the statistical view of physics which was published a year later (Weyl 1920).[13] For Weyl the topic of his talk contained a challenging combination of questions in the conceptual foundations of contemporary physics, including the rising "clouds" of quantum phenomena, with the question of how modern natural science can be made compatible with metaphysical considerations of the existential experience of the openness of evolving life processes and of the freedom of personal actions.

In September 1920, during the discussions of the Bad Nauheim meeting of the German *Naturforscher Versammlung* and from a draft manuscript of Pauli's contribution on relativity to the *Enzyklopädie der Wissenschaften*, Weyl got to know content and reason of Pauli's critical evaluation of his

---

[13] This paper has been strongly critcized by P. Forman in his otherwise very stimulating article on Weimar culture and its influence on the discourse among physicists (Forman 1971) as a document of an "antirational" kind of "conversion to acausality". This specification is more than doubtful; see comments and critique in (Hendry 1984, Sigurdsson 1991, Stöltzner 2002) and also the modifications in (Forman 1980).

modified version of the Mie theory of matter. This conjunction of detailed scientific criticism, coming from a personally close, young expert in the field, with his own most recent conceptual and metaphysical speculations, undermining the classically deterministic field structures anyhow, led Weyl to give up the belief in his program of a geometrically unified field theoretical derivation of matter structures. At the end of the year, in a letter to Felix Klein, in which he reported on his recent advances on mathematical and physical questions (included or not into the just finished fourth edition of *RZM*), he reported among others:

> (. . .) I thoroughly distached myself from Mie's theory and came to a different position with respect to the problem of matter. I no longer accept field physics as the key to reality. The field, the ether, appears to me only as a *transmitter* of effects, which is completely feeble by itself; while matter is a reality lying beyond the field and causing its states . . . . (Weyl to F. Klein, December 28, 1920 1920)

Similar phrases are to be found close to the end of the fourth edition of *RZM*. In addition Weyl indicated that he expected the solution of what he now called the *problem of matter* from new insights of the rising quantum physics which was still in its infancy.

He did *not* withdraw, however, from the conviction that his gauge geometry was a conceptually very well founded generalization of Riemannian geometry and might therefore be useful in a coming physical generalizations of geometry. During late 1920 and 1923, he embarked upon what he now called the *analysis of the problem of space* (Weyl 1921, Weyl 1923). In a philosophical and mathematical investigation of the basic features of any kind of geometry, which worked with congruence geometry in the infinitesimal, he came to a beautiful group theoretical characterization of infinitesimal geometry.[14] Here he worked with an idea of a group extension from some group of abstract "congruences" to a larger group of "similarities" and general connections with values in the infinitesimal groups (the corresponding Lie algebras), which today would be expressed in terms of fibre bundles.

The result of his analysis gave very general conceptual reasons that the groups which might play a role in any decent geometry with some concept of infinitesimal congruences were just the (generalized) orthogonal groups which played the crucial role in (semi-) Riemannian geometry and his own gauge geometrical generalization of 1918. That gave new conceptual grounds on which one had to consider his gauge geometry as the proper frame for a rather general "purely infinitesimal geometry" with any kind of congruence concept in the infinitesimal. Indirectly, his analysis of the problem of space gave an answer to Einsteins objection, that some day a "Weyl II" might come and do away with angle conservation in infinitesimal parallel transfer with the same legitimation as Weyl in 1918. Weyl (I) had now shown that the attempt of such a "Weyl II" would contradict basic principles of a conceptually satisfying geometry.

But Einstein's former objection did no longer play any role in the discourse among the two. Under Eddington's influence, Einstein had turned towards general affine connections without a specification of infinitesimal congruence groups (to which the affine connection could be reduced, in more modern terms) as a basis for physical geometry. In doing so he converted, knowingly or unknowingly, into what he had called a "Weyl II" five years earlier. But Einstein/Weyl II did not listen much to the objections raised by the original Weyl, as we have already seen above.

Weyl could only draw one satisfaction from Einstein's turn, as he openly admitted with a some background smile in a letter to Einstein, written May 18, 1923:

> On the whole I am delighted naturally, that I am meeting you

---

[14] (Scholz 2004 *a*)

> on the same purely speculative paths, against which you always protested before.[15]

General relativistic Dirac spinors versus *Fernparallelismus*

About the middle of the 1920s. Einstein and Weyl went completely different paths and had no occasion for new encounters. That was the time, when W. Heisenberg, M. Born, P. Jordan, and E. Schrödinger invented the new quantum mechanis, a little later joined by P.A.M. Dirac and others. Einstein continued to work mainly with different attempts of unified field theories of electromagnetism and gravity. Weyl was strongly occupied by his involvement with the representation theory of Lie groups (1924/25) and, a year later (1926), with his studies for this book on the philosophy of science and mathematics.

The situation changed in the late 1920s, when Einstein and Weyl embarked on new investigations in general relativistic physics with a small segment of overlap, although in otherwise strongly diverging perspectives. In June 1928 Einstein started to investigate the possibilities opened for the unification program by a geometrical structure of *distant parallelism*, defined by a connection with vanishing curvature but nonvanishing torsion. He pursued this program until late 1931, before he came to the conclusion that this approach was inappropriate for his physical goals.[16] Weyl came back to publish in mathematical physics after he gave a lecture at the ETH on *Quantum Mechanics and Group Theory* in winter semester 1927/28. Again he prepared a book which bore the same title from the lecture notes. It appeared in summer 1928.[17] That gave him the occasion to react to proposals by E. Schrödinger, F. London and others to transfer his gauge concept from scaling the metric of general relativity to rotating the phase of quantum mechanical wave functions. In early 1929 he extended the idea to the context of

---

[15] Quoted from (Stachel 1986/2002)

[16] (Sauer 2003, Goenner 2004)

[17] (Mehra 2000, Coleman/Korté 2001, Scholz 2004 *b*)

Dirac's electron theory with its 4-component wave functions (shortly later to be called Dirac spinors), which he generalized to a general relativistic framwork. A similar approach was pursued by V. Fock at about the same time. Both scientists got to know of each others investigations in early summer 1929 and realized that they had developed nearly the same theory.[18]

In order to establish a general relativistic setting for the Dirac equation, one had to cope with the geometrical consequences of a purely group theoretical fact. The representation of the Lorentz group implicit in Dirac's characterization of the 4-wave functions could *not be extended* to the full linear group $GL_4(\mathbb{R})$. That made it impossible to implement Dirac's theory straight forward and without further considerations into the framework of general relativity, as long as the latter was formulated in terms of the Ricci calculus. To rely on Ricci calculus meant that *general covariance* was understood in the sense of the full group of general linear transformations on the tangent spaces (structure group $GL_4(\mathbb{R})$ in more recent terms). Fock and Weyl realized that it was easily possible to restrict the transformations of tangent spaces to the Lorentz group and still to be able to keep the essential content of the principle of general covariance, if one choose a representation of vectors in an accompanying *orthogonal 4-frame*, a *tetrad* as it is called. In more recent terms this artifice allowed to reduce the group of general covariance, and in particular of the Levi-Civita connection, from general linearities to the Lorentz group.

Now there was a superficial agreement with Einstein's latest theory of distant parallelism, as also there a system of orthogonal tetrads was chosen in every point of the space-time manifold. In Einstein's approach the tetrads were, in addition, bound together by the additional structure of distant parallelism, foreign to general relativity. This was *not* the case in the Fock/Weyl theory of the general relativistic Dirac equation. Here the tetrads were chosen independently in every point as an orthogonal reference frame

---

[18] (Vizgin 1994, Scholz 2001*a*)

for the local description of physics. They could freely be changed by point dependent Lorentz transformations. The principle of general covariance in the sense of independent choices of reference systems at different points of the manifold, was not given up like in Einstein's latest approach, but only rephrased in a mathematically equivalent form.[19] Moreover, it turned out that in this context there arose a very natural place for the introduction of a phase gauge in order to represent the electromagnetic potential. This gave Weyl's gauge idea a form which was much better adapted to physical theory building than it had been the case in his gauge geometry of 1918.

Weyl was well aware of these conceptual differences between the two approaches, although he would express them in slightly different terms, developed in his studies of the analysis of the problem of space. He criticized Einstein's turn towards distant parallelism as an incomprehensible move in which the clue of general relativity was given up. From his point of view, the principle of general covariance was naturally related to "purely infinitesimal geometry", the core idea of which was to build up geometrical structures from the infinitesimal neighbourhoods of the manifold, without recourse to any kind of "distant geometry".[20]

For the first time Weyl argued strongly *against* Einstein in scientific publications (and even on the level of the science discourse for a broader public). I give just one example from a paper of 1930:

> For two years Einstein has stubbornly pursued a new trace (...). In addition to the Riemannian metric he postulates a *distant parallelism* of vectors. ...Einstein breaks with the infinitesimal point of view. In consequence nearly everything, which seemed to be permanently gained by the transition from special to general

---

[19] The choice of a metric permits the restriction of the group to the Lorentz group, without a change of structure.

[20] Therefore Weyl's term "purely infinitesimal geometry" cannot be modernized as "differential geometry" without further specifications. The latter allows to consider "distant structures" from a differential point of view and is thus a wider term.

> relativity, is given up again. At the moment, the loss is not compensated by any concrete gain. (Weyl 1931, 343)

He gave arguments on different levels against Einstein's new theory: Mathematically distant geometry seemed very unnatural. Moreover, he could not "imagine which force has frozen the local tetrads in their rotated position", and did not expect any gain for the comprehension of electromagentism. As he had done in 1918, he now restated with new optimism that the gauge characterization of the electromagnetic potential should be more realistic, because it leads so naturally to the invariance of charge and to the Maxwell equations (at least its first set) .

Above all, Weyl had changed his mind about the role of a priori speculations in mathematical physics. While in 1918 this role appeared him to be quite important, the experiences of the following decade led him to the conclusion that the mathematizations ought to follow more closely the experimental insights of physics. Einstein had taken the inverse path. From a very detailed evaluation of experimental results in his studies leading to special and general relativity, he had now come to a methodology of investigating the consequences of mathematical hypotheses which seemed attractive to him by one reason or other, even *before* a connection to observable phenomena was established. He now tried to find a link to observable phenomena only in a second step.

Pauli as an ironic "referee"

Of course, both approaches are legitimate in mathematical physics. In place of a methodological discussion or a lengthy resumé of the development, I want to finish by quoting from a letter of Wolfgang Pauli to Hermann Weyl. It was written shortly after Pauli had read Weyl's paper on the general relativistic Dirac equation in *Zeitschrift für Physik* (Weyl 1929). Pauli welcomed Weyl's short but clear criticism of the theory of distant parallelism given at the beginning and at the end of this paper:

> Just like you, I am opposed to the theory of distant parallelism, and it is a true salvation that, in your approach, the tetrads can be rotated freely against each other at different points. (And here I have to do justice to your earlier activities in physics. Once, when you fabricated the theory with $g'_{ik} = \lambda g_{ik}$, that was pure mathematics and unphysical. Einstein was right in criticizing and grumbling. Now the time of revenge has come for you; now Einstein has committed the blunder of distant parallelism (hat den Bock des Fernparallelismus geschossen) which also is nothing but pure mathematics and has nothing to do with physics, and you can grumble.)(Pauli 1929)[21]

Pauli's irony was drastic, as always; but basically his observation was right. At the end of the 1920, Weyl and Einstein had exchanged the positions they had taken in 1918, with respect to the role of mathematics in theoretical physics. We can enjoy this paradoxy of history without necessarily sharing Pauli's value judgement about where the "true" physics lies.

References

Cao, Tian Yu. 1997. *Conceptual Developments of 20th Century Field Theories.* Cambridge: University Press.

Coleman, Robert; Korté, Herbert. 2001. Hermann Weyl: Mathematician, physicist, philosopher. In *(Scholz 2001*b*).* pp. 161–388.

Eddington, Arthur. 1921. "A generalisation of Weyl's theory of electromagnetism and gravitational fields." *Proceedings Royal Society London, Ser. A* 99:104–122.

Einstein, Albert. 1915*a*. "Die Feldgleichungen der Gravitation." *Sitzungsberichte der Königlich Preußischen Akademie der Wissenschaften zu Berlin* pp. 844–847.

Einstein, Albert. 1915*b*. "Erklärung der Perihelbewegung des Merkur aus der allgemeinen Relativitätstheorie." *Sitzungsberichte der Königlich Preußischen Akademie der Wissenschaften zu Berlin* pp. 831–839.

Einstein, Albert. 1915*c*. "Zur allgemeinen Relativitätstheorie." *Sitzungsberichte der Königlich Preußischen Akademie der Wissenschaften zu Berlin* pp. 778–786.

Einstein, Albert. 1916. "Die Grundlagen der allgemeinen Relativitätstheorie." *Mathematische Annalen* 49:769–822.

Einstein, Albert. 1923. "Zur affinen Feldtheorie." *Sitzungsberichte der Königlich Preußischen Akademie der Wissenschaften zu Berlin* pp. 137–140.

---

[21] See also (Straumann 1987, Straumann 2001)

Einstein, Albert. 1928. “Riemann-Geometrie mit Aufrechterhaltung des Begriffs des Fernparallelismus.” *Sitzungsberichte der Königlich Preußischen Akademie der Wissenschaften zu Berlin* 17:217–221.

Einstein, Albert. 1930. “Auf die Riemann-Metrik und den Fern-Parallelismus gegründete einheitliche Feldtheorie.” *Mathematische Annalen* 102:685–697.

Einstein, Albert. 1998. *The Collected Papers of Albert Einstein. Volume 8. The Berlin Years: Correspondence, 1914–1918, Part B: 1918* , edited by R. Schulmann, A.J. Kox, M. Janssen, J. Illy, K. von Meyenn. Princeton: University Press.

Einstein, Albert. 1987ff. *The Collected Papers of Albert Einstein.* Princeton: University Press.

Forman, Paul. 1971. “Weimar culture, causality, and quantum theory, 1918–1927: Adaptation by German physicists and mathematicians to a hostile intellectual environment.” *Historical Studies in the Physical Sciences* 3:1–116.

Forman, Paul. 1980. “Kausaltät, Anschaulichkeit und Individualität, oder wie Wesen und Thesen, die der Quantenmechanik zugeschrieben, durch kulturelle Werte vorgeschrieben wurden.” *Kölner Zeitschrift für Soziologie und Sozialpsychologie, Sonderheft* 22:393–406. In (von Meyenn 1994, 181–200).

Goenner, Hubert. 2004. “On the history of unified field theories.” *Living Reviews in Relativity* . [http://relativity.livingreviews.org/Articles/lrr-2004-2, visited June 2, 2004].

Hendry, John. 1984. *The Bohr-Pauli Dialogue and the Creation of Quantum Mechanics.* Dordrecht: Reidel.

Mehra, Jagdish; Rechenberg, Helmut. 2000. *The Historical Development of Quantum Theory. Vol 6: The Completion of Quantum Mechanics 1926–1941. Part 1.* Berlin etc.: Springer.

O’Raifeartaigh, Lochlainn. 1997. *The Dawning of Gauge Theory.* Princeton: University Press.

Pais, Abraham. 1982. *“Subtle is the Lord . . . ”: The Science and Life of Albert Einstein.* Oxford: University Press.

Pauli, Wolfgang. 1929. “Letter W. Pauli to H. Weyl, August 26, 1926.”. In (Pauli 1979, 518–521).

Pauli, Wolfgang. 1979. *Wissenschaftlicher Briefwechsel . . . . Scientific Correspondence with Bohr, Einstein, Heisenberg a.o.. Volume I: 1919–1929* Edited by A. Hermann, K. von Meyenn, V.F. Weisskopf. Berlin etc.: Springer.

Sauer, Tilmann. 2003. “Field equations in teleparallel spacetime: Einstein’s *Fernparallelismus* approach towards unified field theory.”. Preprint Pasadena, to appear in *Historia Mathematica.*

Scholz, Erhard. 2001*a*. “Local spinor structures in V. Fock’s and H. Weyl’s work on the Dirac equation (1929).”. Preprint Wuppertal, to appear in D. Flament, C. Houzel. (eds.) *Géométrie au XXième siècle.* Paris [http://arxiv.org/physics/0409158].

Scholz, Erhard (ed.). 2001*b*. *Hermann Weyl’s* Raum - Zeit - Materie *and a General Introduction to His Scientific Work.* Basel etc.: Birkhäuser.

Scholz, Erhard. 2004. “Hermann Weyl’s analysis of the “problem of space” and the origin of gauge structures.” *Science in Context* 17:165–197.

Scholz, Erhard. 2004. “The introduction of groups into quantum mechanics.” Preprint Wuppertal, to appear in *Historia Mathematica* .

Sigurdsson, Skúli. 1991. “Hermann Weyl, Mathematics and Physics, 1900 – 1927.” Cambridge, Mass.: PhD Dissertation, Harvard University.

Stachel, John. 1986. Eddington and Einstein. In *The Prism of Science, The Israel Colloquium of Science*, ed. E. Ullmann-Margalit. pp. 225–250. Reprint in (Stachel 2002, 453–475).

Stachel, John. 2002. *Einstein from 'B' to 'Z'.* Basel: Birkhäuser.

Stöltzner, Michael. 2002. "Vienna Indeterminsm. Causality, Realism and the Two Strands of Boltzmann's Legacy (1896–1939).". PhD Dissertation, Bielefeld University.

Straumann, Norbert. 1987. "Zum Ursprung der Eichtheorien." *Physikalische Blätter* 43:414–421.

Straumann, Norbert. 2001. Ursprünge der Eichtheorien. In *(Scholz 2001*b*).* pp. 138–160.

Vizgin, Vladimir. 1994. *Unified Field Theories in the First Third of the 20th Century.* Translated from the Russian by J. B. Barbour. Basel etc.: Birkhäuser.

von Meyenn, Karl (Hrsg.). 1994. *Quantenmechanik und Weimarer Republik.* Braunschweig: Vieweg.

Weyl, Hermann. 1918. "Gravitation und Elektrizität." *Sitzungsberichte der Königlich Preußischen Akademie der Wissenschaften zu Berlin* pp. 465–480. GA II, 29–42, [31]. In (Weyl 1968, II, 29–42) [31], English in (O'Raifeartaigh 1997, 24– 37).

Weyl, Hermann. 1919. "Eine neue Erweiterung der Relativitätstheorie." *Annalen der Physik* 59:101–133. In (Weyl 1968, II, 55–87) [34].

Weyl, Hermann. 1920. "Das Verhältnis der kausalen zur statistischen Betrachtungsweise in der Physik." *Schweizerische Medizinische Wochenschrift* 50:737–741. GA II, 113–122, [38].

Weyl, Hermann. 1921. "Das Raumproblem." *Jahresbericht DMV* 30:92ff,. In (Weyl 1968, II, 212–228) [45].

Weyl, Hermann. 1923. *Mathematische Analyse des Raumproblems.* Vorlesungen gehalten in Barcelona und Madrid. Berlin etc.: Springer. Nachdruck Darmstadt: Wissenschaftliche Buchgesellschaft 1963.

Weyl, Hermann. 1929. "Elektron und Gravitation." *Zeitschrift für Physik* 56:330–352. In (Weyl 1968, III, 245–267) [85]. English in (O'Raifeartaigh 1997, 121– 144).

Weyl, Hermann. 1931. "Geometrie und Physik." *Die Naturwissenschaften* 19:49–58. GA III, 336–345, [93].

Weyl, Hermann. 1968. *Gesammelte Abhandlungen, 4 vols.* Ed. K. Chandrasekharan. Berlin etc.: Springer.

Letter H. Weyl to F. Klein, December 28, 1920. *Nachlass F. Klein* Universitätsbibliothek Göttingen Codex Ms Klein 12:297.