Hermann Weyl's analysis of the "problem of space" and the origin of gauge structures

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The Argument

H. Weyl, one of the early contributors to the mathematics of general relativity, proposed a generalization of Riemann's differential geometry as it was used by Einstein in his theory of gravitation. In Weyl's generalization an idea of point dependent "gauge", motivated by an intriguing interplay of philosophical, physical and mathematical considerations, was crucial. At first (1918 to 1920) he believed that one might be able to derive the matter structures observed in atomic physics in this gauge geometric framework, but gave up this idea under the criticism of physicists, rising with the knowledge in quantum mechanics. Weyl then turned to a conceptual analysis of generalized congruence structures and its necessary ingredients, which he called the "mathematical analysis of the problem of space" (1921 to 1923). After physicists started to adapt his earlier gauge idea of length measurement to the phase of wave functions in the rising quantum physics of the 1920s, Weyl contributed to such an adaptation from his own conceptual perspective. In 1929 he found a convincing way to build the modified gauge idea into the construction of a general relativistic framework for Dirac's theory of the (at first only special) relativistic electron.

This article argues that in this step Weyl abolished and preserved his highly philosophical and mathematical investigations of the analysis of the space problem in a modified form (they were *aufgehoben* in both aspects of the word, as one would say in German). The preservation aspect refers to the central idea of gauge as a "purely infinitesimal" aspect of (internal) symmetries in a group extension schema. With respect to methodology, however, Weyl gave up his earlier preferences for relatively a-priori arguments and tried to incorporate as much empiricism, as he could. For him, this signified a clearly expressed *empirical turn*. Moreover, he emphasized now that the mathematical objects constituted for the representation of matter structures stood at the center of the construction, rather than interaction fields which, in the early 1920s, he had considered as more or less derivable from geometrico-philosophical considerations.

Thus, with respect to methodology, at the turn to the 1930s Weyl not only proclaimed an empirical turn away from, and even against the still flowering activities in unified field theories, but enriched it by a "materialist" one with respect to the ontological orientation underlying the mathematical construction. His mature gauge approach from 1929 stood in close connection to contemporary developments proposed by physicists, most closely among them V. Fock. It was transmitted to the next generation of physicists via an extract that was incorporated into the mainstream quantum physics by W. Pauli. It may be useful to know that Pauli's extract served as an incentive and starting point for the gauge field theories of the second half of the 20th century (C.N. Yang, R. Mills, R. Utiyama and others). This is, however, part of a different story, not discussed in this article.

Introduction

The invention, maturation and rise of so-called gauge structures is one of the characteristic features of 20th century mathematical physics. The story of this development linking modern physics with geometry started in 1918 when Hermann Weyl introduced the idea of a point-dependent measuring device, a "gauge" metric, and proposed it as a mathematical frame for a unification of the field theories of gravitation and electromagnetism. In the first half of the 1920s, Weyl's at the outset rather classical, i.e. purely metrical gauge idea was modified by physicists (E. Schrödinger, O. Klein, F. London, V. Fock) in different attempts to adapt it to the context of the rising quantum mechanics. At the end of the 1920s these first, still quite unconclusive trials took a surprising and convincing turn, when V. Fock and H. Weyl related the gauge concept to the Dirac field of the relativistic electron. This shift from gauge of length by a real valued factor to a gauge of phase by a complex factor of absolute value 1 of the Dirac or Schrödinger (respectively Klein-Gordon) fields, has attracted the attention of historians of physics and historically oriented philosophers of science quite a bit (Vizgin 1994, Pais 1982, Mehra/Rechenberg 2000), (Cao 1997, Morrison 1995).

It went nearly unnoticed, however, that for Weyl there was a much deeper conceptual bridge between the early and the later gauge ideas, than just substituting real number factors by complex ones and changing the differential geometric tools accordingly. From such a point of view the change would consist essentially in a replacement of a "wrong" gauge group $(\mathbb{R}^+, \cdot)$ by the "right one" $U(1)$, as late 20th century mathematical physicists might be tempted to describe the change. Of course, the substitution of one group by another one was not essential as such,[1] but became important only through a changing methodology and physical context. Moreover, it was important for Weyl, how in both approaches the gauge group extended another more basic one, the Lorentz group ($SO^+(1,3)$ or its complex version, the special linear group in two variables $SL_2(\mathbb{C})$). For him, the context of the extension was essential for the meaning of the gauge idea which otherwise might appear as a purely formal gadget. He had already insisted on a comparable physically meaningful contextualization in his discussions with F. Klein in 1918 on the role of the group underlying relativity theory. There he reminded his senior correspondent that one should not just consider all possible cases of relativity theories with respect to any group, but concentrate on the one characterizing "reality" among all "logical possibilites" that exist in the conceptual framework (of an extended "Erlanger" program) (Sigurdsson 1991,

[1] Infinitesimally the groups were even indistinguishable, i.e. no change of the Lie-algebras was involved.

143ff.).

An extension of groups was also essential in Weyl's contributions to the analysis of the problem of space. Here he dealt with an enlargement of an underlying (abstract) group of "congruences" to a slightly larger group of "similarities". For Weyl this stood, of course, in close relation to his gauge geometric perspective, but it has been considered by mathematicians and philosophers of science nearly separated from Weyl's involvement in unified field theories, or at most indirectly linked to it (Scheibe 1988, Coleman/Korté 2001, Hawkins 2000).

When between 1921 and 1923 Weyl generalized the late 19th century's analysis of the problem of space in the sense of Helmholtz, Lie and Klein and adapted it to the context of differential geometry and to the new general theory of relativity, he made quite clear that this generalized analysis aimed at a deeper conceptual foundation for his earlier length gauge structure. After E. Cartan's important contribution to the problem of space, rephrasing Weyl's analysis, most of the later mathematical contributions following Cartan lost sight of this link to Weyl's gauge geometry. Nevertheless the link was upheld in philosophy and history of science, essentially due to the contributions of E. Scheibe.

There is more to say, however, about the interrelation between Weyl's analysis of the space problem and his work on gauge structures. There is another, less well known mathematical and conceptual link to his contributions to the relativistic Dirac field in the year 1929. Mathematically this link is slightly more hidden than the one to his early gauge geometry of 1918; and from the point of view of methodology and philosophy the step from Weyl's analysis of the problem of space to his work on the Dirac equation contains a deeper change of perspective than anything earlier in his thought. These differences reflected the changing situation in mathematical physics with the rise of the "new" quantum mechanics in the middle of the 1920s and contributed to it.

In this paper I outline the basic ideas of Weyl's analysis of the problem of space and discuss how they relate to Weyl's later modification of gauge structures in his study of the Dirac equation.

Weyl's 1918 gauge geometry and unfied field theory

To make this paper essentially self-contained, let us start with a short resumée of Weyl's original (1918) gauge idea, which has been discussed in the literature on the history of mathematics and physics in some detail from different perspectives.[2]

Weyl started from a critical evaluation of the fact that in Riemann's differential geometry the length of vectors at different points x and y of a manifold M can be compared directly without further specifications. He considered this feature as inconsistent with the principles of *close by* or *purely infinitesimal* geometry ("Nahegeometrie", "reine Infinitesimalgeometrie") and therefore as physically, conceptually and philosophically unsatisfactory. At that time, Weyl had close intellectual and personal relations to his Zürich colleague Fritz Medicus who taught philosophy and pedagogy at the ETH, was an expert in German idealist philosophy, and had edited J.G. Fichte's works. That enriched his philosophical interests which in his later Göttingen years (1910 to 1912) had come under the tension of conflicting orientations at "positivism" (in the style of H. Poincaré and E. Mach) and his awakening interest in Husserl's phenomenolgy. At Zürich, Weyl's philosophical interests shifted towards Fichte's philosophy, in particular the *Wissenschaftslehre*. Weyl looked for the latter's proposals for dialectical concept formation, in particular Fichte's approach to the concept of continuum and space. For some years, between about 1918 and 1921, Weyl's active interest in Husserl's phenomenology was shifted to the background, but as he later remembered, it continued to serve as a critical counter-balance against Fichte's highly speculative flights of thought.[3]

Fichte had sketched a programme to build space from *spheres of activity* ("Sphären der Wirksamkeit") of forces assumed "with necessity", in his words, as the formative causes of appearances. Although these spheres of activity were *not* yet to be understood spatially, Fichte had proposed to consider the concept of space to be posited ("gesezt" (sic!)) as the "extended, connected, infinitely divisible extensive continuum" giving a *common sphere*

[2]See (Pais 1982), (Vizgin 1994, chap. 3), (Sigurdsson 2001), (Coleman/Korté 2001), (Hawkins 2000, chap. 11.2), (Scholz 1995).

[3]In Weyl's own description from hindsight:

"In Zürich gerieten meine Fau und ich durch die Vermittlung von Medicus, dessen Seminar meine Frau besuchte, an die Fichtesche Wissenschaftslehre. Hier fand der metaphysische Idealismus, zu dem die Husserlsche Phänomenologie sich damals schüchtern hinzutasten begonnen hatte, den unverhohlensten und kräftigsten Ausdruck. Er packte mich, auch wenn ich meiner Frau, der die sorgfältige Mehodik Husserls mehr lag als Fichtes Draufgängertum, zugestehen mußte, daß Fichte durch seine natur- und tatsachenblinde Hartnäckigkeit im Verfolgen einer Idee zu immer abstruseren Konstruktionen sich fortreißen ließ." (Weyl 1954, 637).

For a more detailed discussion of Fichte's construction of the concept of space and Weyl's infinitesimal geomtry, see (Scholz 2001*b*). Much stronger weight on Husserl's phenomenology is given by (van Atten e.a. 2002) in their interpretation of Weyl's foundational concept of the continuum. For a historically detailed presentation of Weyl's discussion on geometry with a representative of the phenomenological "school" see (Mancosu/Ryckman 2002).

to all these non-spatial spheres of activity. Thus these spheres of activity acquired spatial positions, which ought not to be understood as pointlike elements, but as a result of an infinite division of space, as *infinitely smallest parts of space* (Fichte 1795, 198–200). Moreover, they were identical with domains of forces; and the forces again were thought to constitue matter, in the sense of *dynamistic* natural philosophy.

Weyl was apparently highly interested in a mathematical approach to the continuum and to differential geometry, that would take into account such a perspective of what he decided to call "purely infinitesimal geometry". The emphasis of "purely" was Weyl's and indicated the intention to construct the basic concepts of differential geometry more consequentially from infinitely smallest parts of space than before.[4] Moreover he saw good reasons that such an approach was in perfect agreement with the tendencies of modern natural science to understand the processes of nature from the small, rather than from relationships assumed in the large. That stood in perfect agreement with the broader Göttingen perspective on the relationship between mathematics and physics and radicalized the view that Riemann had tried to achieve for geomtry, what Faraday and Maxwell had done for physics.[5] Weyl went just one step further. In his view, Riemann's metric was still imbued with the quasi-Euclidean tradition inherited from Gauss' theory of surfaces, which of course admitted direct comparison of lengths of vectors at different positions. From Weyl's perspective this feature was a hangover from Euclidean "distant geometry" and had to be considered a defect rather than an achievement.

As a remedy he proposed to consider the metric in M in the first instance given only up to *conformal* equivalence. This means, a Riemannian or Lorentz metrics

$$ds^2 = \sum g_{ij}(x) dx_i dx_j \tag{1}$$

could just as well be represented by a metric of the same form, but with metrical coefficients multiplied by a strictly positive real valued function $\lambda(x)$

$$g'_{ij}(x) = \lambda(x) g_{ij}(x) \ , \quad \lambda(x) > 0. \tag{2}$$

The semantical reason for this generalization of Riemann's metric was Weyl's conviction, that no measuring rod (or procedure) ought to be presupposed as globally available for a sufficiently general geometry built up according to the purely infinitesimal credo. Only some *arbitrary choice* of a measuring device in all points, a *gauge*, could be accepted which would be described in differential geometric symbolism by the choice of a Riemannian metric

[4]Cf. (Scholz 1995, Scholz 2001*b*), but compare also from a more Husserlian and foundational point of view (van Atten e.a. 2002).

[5]Compare (Sigurdsson 2001, 38ff.) on Klein's and Hilbert's respective views.

$(g_{ij}(x))$ from a conformal class as in equation (1).[6] A different choice of gauge would be described by multiplying lengths by a "scale factor" λ as described in (2). Only then, in a second step, a length comparison between different points became possible by an additional procedure which would allow to define length relations between infinitesimally close points or, after choosing a connecting path, between two general (non-infinitesimally close) points. Mathematically such a device could be given, as Weyl argued, by a differential form φ,

$$\varphi = \sum_i \varphi_i dx^i \ , \quad \text{in short notation} \quad \varphi = (\varphi_i)\,. \tag{3}$$

Weyl called a device of this type a *length connection*. Its symbolical representation by a differential form φ as above was dependent on the choice of gauge (mathematically spoken, the choice of the metric in the conformal class). A change of the gauge by a factor λ had to be accompanied by a change of the representing differential form φ to, let us say, φ'. Mathematically this change could be expressed in the form:

$$\varphi' = (\varphi'_i) = \varphi - d\log\lambda \qquad \text{with} \quad \varphi'_i = \varphi_i - \frac{1}{\lambda}\frac{\partial\lambda}{\partial x^i}, \tag{4}$$

i.e. by the addition of a complete differential dependent on the scaling factor λ. Weyl called this, at first glance strange looking but by consistency reasons completely compelling, transformation rule a *gauge transformation of the length connection.* He showed how to use such a length connection for the comparison of lengths of vectors at different points of the manifold.

Such a comparison now presupposed some integration along a connecting path between points x and y to which the vectors were attached. It was modeled upon parallel displacement of vectors in Riemannian geometry as introduced shortly before by Levi-Civita. That means, if one "transported" a length measure along a closed path, one arrived at the end of a loop with a transported measuring unit different from the one at the beginning and dependent on the path. In this sense calibration of measuring units became "localized", as physicists would later say (in Weyl's terminology it became "purely infinitesimal"). No universal "office of standards" (*Eichamt*) had any longer to be assumed a priori, as Weyl formulated in his correspondence with Einstein in late 1918, nor was its existence any more inbuilt into the conceptual structure of differential geomtry. On the other hand, once a path

[6] In case of relativity theory, a Lorentz metric had to be considered, of course. In the following I will (like Weyl) allow a Lorentz metric even where I use the terminology "Riemannian" metric in order to simplify language.

was fixed, the comparison between mesasurements at x and at y became independent of the gauge. Only in special cases of an "integrable" length transfer the comparison became independent of the choice of gauge and of the connecting path between x and y. In these cases the Weylian gauge geometry reduced to Riemannian geometry.[7]

Such an approach was in perfect agreement with Weyl's purely infinitesimal philosophy, notwithstanding the additional mathematical complications of the structure. He was fascinated by the idea that his gauge structure might be just the right way to characterize the potential of the electromagnetic field in general relativity, und thus to find a dynamical interpretation of his newly introduced *gauge potential* φ.

Moreover Weyl hoped that it might be possible to solve the riddle of the constitution and stability of the basic matter particles known at the time, the electron and the hydogen nucleus (little later known as the proton[8]) on the basis of his unification of geometry, gravitation and electrodynamics. In this attempt he joined the line of the Hilbert-Mie programme of unified field theory and hoped to be able to contribute essentially to it.[9] All this seems to have been inspired by the close kinship of his view to Fichte's radicalization of Kant's and other dynamistic philosophers' idea to "derive" matter structures from (hypothetical) force systems constituting them. Like Fichte had gone beyond Kant's dynamism through the proposal to found already the *geometrical concept* of space on the changing underlying forces, which in Kant was still thought to be fixed a priori, Weyl hoped that the new gauge geometry might allow to radicalize the Mie-Hilbert programme and lead to the solution of its core problem, the mathematical explanation of basic matter structures. He believed so, though, only for a period of about two years.

How Weyl proposed to identify the length connection φ with the electromagnetic potential and hoped to arrive at a geometrically unified field theory of gravitation and electromagnetism has been described at different places.[10] Weyl invented this programme shortly after he finished the manuscript for the first edition of his well-known book *Raum - Zeit - Materie* (RZM) (Space - Time - Matter) in the year 1918 and published it originally in two distinct papers in a highly respected scientific journal addressing the physical com-

[7]Technically the criterion of "integrable" length connection could be formulated as the vanishing of its "curvature", comparable to the reduction of Riemannian geometry to Euclidean geometry if the curvature tensor of the Riemannian metric vanishes.

[8]In 1920 Rutherford introduced the term "proton" in the public scientific discourse (Pais 1986, 296).

[9]Cf. (Vizgin 1994, Corry 1999, Sauer 1999, Kohl 2002).

[10]Most detailed in (Vizgin 1994, chap. 3), also (Cao 1997).

munity directly and in a a mathematical jounal (Weyl 1918*b*, Weyl 1918*c*). He integrated the idea into the 3rd edition of RZM one year later.[11]

This early unification programme and its reception in the community of theoretical physicists is part of an interesting story of early 20th century physics for its own.[12] For our purpose, it may suffice to indicate that already in late summer 1920 Weyl lost faith in his earlier conviction that a classical field theory might be appropriate to solve what by now he called the *problem of matter*. This rising scepticism included explicitly his own proposals of 1918.[13] This did not mean, however, that Weyl gave up his gauge geometric idea as such. He rather started to distinguish more clearly between mathematics and the physical interpretation of his new geometrical approach. And he turned to a more basic investigation of the conceptual foundations of purely infinitesimal geometry in 1921 and the following years. In these investigations he continued and elaborated a methodological idea he had sketched already two years earlier in side remarks on Riemann's inaugural lecture.

The comments upon Riemann

In 1919 Weyl added an intriguing conjectural remark to his commentaries to the reedition of Riemann's *Habilitationsvortrag* from 1854 on the hypotheses underlying geometry.[14] Riemann had mentioned that for the introduction of a differential geometric metric it was not necessary to use (the square root of) a quadratic differential form as in equation (1) above. One could, in principle, use any "strictly positive homogeneous" function, i.e. a function f of the coordinate differentials dx_i, satisfying

$$f(\lambda dx_1, \ldots, \lambda dx_n) = |\lambda| f(dx_1, \ldots, dx_n) \ , \quad \text{where} \quad f \geq 0 \ . \qquad (5)$$

The metric arising from the use of such a function for the measurement of the length of vectors and infinitesimal curve segments was investigated in 1918 by Paul Finsler in his dissertation in some detail. It has been accordingly called a *Finsler metric*; and I will use this terminology, although it was not yet adopted by Weyl (nor, obviously, by Riemann).

Riemann, on his side, had not bothered to study such a generalized "Finsler" metric, but gave a rather pragmatic argument why it was reasonable to specialize it to the square root of a positive definite quadratic differential form for f like in equation (1), the "Riemannian" metric. Relativity theory had generalized this approach to the case where f was a non-

[11] (Weyl 1918*a*, 3rd edition, 109ff.).
[12] Cf. (Vizgin 1994, chaps. 3, 4, 6), (Goldstein/Ritter 2000).
[13] (Sigurdsson 1991), (Scholz 2001*c*).
[14] (Riemann 1867), (Riemann 1919).

degenerate quadratic form of the signature of Minkowski space-time, later called a "Lorentz metric". Both cases, Riemannian and Lorentz, were each considered by Weyl as a "class of Pythagorean metrics" (distinguished by signature).[15] In 1919 he asked for more conceptual reasons to specify just these "Pythagorean classes" among all possible Finsler metrics, i.e. metrics with measuring function as in (5).[16]

Weyl conjectured that the "Pythagorean" classes were singled out, among all possible classes of Finsler metrics, by the condition that to any such metric there exists a uniquely determined compatible "affine connection", i.e. essentially a parallel displacement of vectors in the manifold. In fact, two years earlier Levi-Civita had proved that each Riemannian manifold admits exactly one parallel displacement compatible with the metric, i.e. preserving lengths of vectors and angles between them.[17] Weyl himself had distilled from Levi-Civita's construction a symbolic definition of an abstract "affine connection" which allowed to introduce a kind of parallel displacement in any differentiable manifold and a kind of differentiation adapted to this geometrical constellation, called a "covariant derivative". So the concepts of "affine connection" and "parallel displacement" were so closely related that for our purpose they can be considered as more or less interchangeable. The reader ought to know, however, that the affine connection was given by a point-dependent system of quantities $\Gamma = (\Gamma^i_{jk})$ transforming not as tensors but like the so-called Christoffel symbols of Riemannian geometry. Thus it was (and is) a rather technical symbolical device to enrich the structure of differential geometry. Parallel displacement of vectors, on the other hand, can be perceived in an intuitive way, comparable to parallel transfer in classical geometry, but now generalized to differentiable manifolds (and therefore definable only with a technical symbolical gadget like the affine connection).

To sum up, the innovation in 1917 and 1918 had been to create the concepts of parallel displacement and connection in the sense of differential geometry and to relate it to the older concept of covariant derivative of Rieman-

[15] By an appropriate choice of basis for the differentials a quadratic form can be brought into diagonal form with entries ± 1. The signature (p, q) specifies the number of positive entries p and of negative entries q in the diagonal. Thus the signature of the Lorentz case is (1,3) – or the other way round – and for the Riemanian of the same dimension it is (4,0).

[16] Weyl chose the terminology "Pythagorean classes" for Finsler metrics satisfying the generalized Pythagorean theorem. That is, for right-angled triangles in the infinitesimal neighbourhood of a point, or right-angled $(n+1)$-gons in the n-dimensional case, with short sides of length $a_1, \ldots, a_n$ and long side a, one finds for the length $|a|$ of the latter: $|a|^2 = \sum_i (\pm) a_i^2$. In later terminology a "Pythagorean class" corresponds to the class of all semi-Riemannian metrics with a given signature (p, q).

[17] (Reich 1992, Bottazzini 1999)

nian geometry and Ricci calculus (Levi-Civita). In a second step, all three had been liberated from their original links to a presupposed Riemannian metric (Weyl). Now Weyl went one step further and stated the conjecture, that it were exactly the "Pythagorean metrical classes" (the *semi-Riemannian* metrics in later terminology) which were related to such affine connections and parallel displacements in the manifold. If this was true, the existence and unique determination of an affine connection would give some "a priori" distinction of the semi-Riemannian metrics among all Finsler ones.[18]

Thus already in 1919, at a time when his hopes for a geometrical unification by his gauge geometry of 1918 were still fresh and high, Weyl started to look for criteria which types of infinitesimal geometries could be considered as conceptually more convincing than others. And he considered the symbolic structures of an affine connection and its accompanying parallel displacement as essential ingredient for this analysis and its (conjectured) result. I will use the attribute "metageometrical" for such studies which aim at a conceptual analysis of the principles on which the axioms and definitions of a certain type of geometry are founded and how different types of geometry compare with each other, rather than on the derivation of consequences of the definitions and axioms themselves.[19] Already a year after writing his comments on Riemann, Weyl had much stronger reason than he might have expected before to scrutinize his own geometry from such a "metageometric" point of view .

Analysis of the problem of space, 1920 – 1923

In late summer 1920 Weyl lost the conviction that his geometrically unified field theory was a semantically sound application of his gauge geometry and, even more, contained the clue to the solution of the question of the "problem of matter", i.e. the characterization of constitutive conditions for the basic matter structures. The reasons for this loss of faith and shift of research orientation were manifold.

They seem to have started from internal difficulties of the Weylian version of the Hilbert-Mie research programme. Although these difficulties were not finally decisive, W. Pauli's critical evaluation of the status of the programme in his article on relativity for the *Enzyklopädie der Mathematischen Wissenschaften*) was of particular importance (Pauli 1921). Pauli's article was published in 1921, but already in summer 1920 Weyl had a manuscript

[18] In fact, Weyl's conjecture was proved by D. Laugwitz in the year 1958.

[19] Klein's *Erlanger Programm* is a well known earlier example for a metageometrical investigation in this sense.

of it and could start to reevaluate the physical perspectives of his unified theory of 1918.[20]

More generally, in the Sommerfeld school rose and stabilized doubts as to whether quantum phenomena might be represented at all in the framework of classical field theories. Such doubts were expressed by Sommerfeld in his 3rd edition of *Atombau und Spektrallinien* (1919),[21] and, perhaps with larger public effect, by Pauli in the discussion of the Bad Nauheim meeting of natural scientists in September 1920 (Pauli 1920).

Finally, Weyl and other scientists sensed a philosophical uneasiness with classical determinism in a fundamental theory of nature as in a unified theory based on classical field theories. Weyl, in particular, turned towards the search for a mathematical representation of the basic principles of nature, that allow an open interplay of binding laws and "freedom" as expressed by him in his article (Weyl 1920). This article took up the discussion on the relation between statistical "laws" and classical determinism, that was central for the Wien tradition (Boltzmann, Exner, von Mises e.a.) and gave it a specific "Weylian turn".[22]

Notwithstanding the loss of direct semantical grounds for his gauge geometry, Weyl continued to be convinced that it had important conceptual advantages in comparison with more special (Riemannian) or other (Finsler) infinitesimal geometries. In order to put critical substantation to this conviction or, perhaps, to refute it, Weyl embarked on an analysis of the *mathematical problem of space* (in the sequel abbreviated as PoS), in analogy and continuation to what had already been discussed in the late 19th century by Helmholtz, Lie, and Klein under different circumstances. Following an empirically twisted Kantian question, H. Helmholtz had analyzed in the later 1860s conditions that allowed to compare geometrical quantities (lengths, angles, ...) at different points of space and in different positions. He characterized these conditions by "free mobility" of a rigid body serving as a measuring device. S. Lie recasted Helmholtz' conditions of free mobility into conditions for a transformation group acting on space like the motions of rigid bodies and representing the congruences of the space. Helmholtz' and Lie's analysis led to a characterization of the congruence groups of three different

[20] See the discussion in (Hendry 1984, chap. 2).

[21] I owe Jim Ritter this hint.

[22] P. Forman has correctly emphasized the role of what I call here the "philosophical uneasiness" with classical determinism in the wake of World War I (Forman 1971, Forman 1980), although in a rather misleading interpretation of scientific and cultural motivations of the criticized authors (not only Weyl). The reader might like to compare the critical remarks in (Hendry 1984, Sigurdsson 1991, Hochkirchen 1999, von Mayenn 1994*a*) and more recently (Stöltzner 2002).

types of geometries, Euclidean, non-Euclidean (in the sense of Bolyai and Lobachevsky) and spherical or, topologically simplified, "elliptical" geometry (in Klein's terminology). These were exactly the most simple exemplars, i.e. simply connected, spaces of constant curvature (0, +1 or −1).[23] F. Klein indicated that one could ask, moreover, for the different topological space forms in each of the three main (curvature) types. At the end of the 19th century, the Helmholtz-Lie-Klein space forms were widely accepted as constituting the adequate theoretical ("a-priori") frame for a critical discussion of the empirical nature of the physical and/or astronomical space.

Now the group theoretical analysis had to be reconsidered, according to Weyl, under the new conditions presented by general relativity and the progress in infinitesimal geometry. In this respect Weyl's approach to the problem differed completely from Schlick's discussion under the same headline (space problem) in (Schlick 1919). Schlick's subject was the discussion of philosophical questions relating to the combined treatment of time and space in general relativity; he *did not even pose* the question of how to adapt or modify the group theoretical analysis that was given at the end of the 19th century to the new conditions.

Weyl realized that, for the time being, he could approach the problem only on an intermediate level, leaving aside the "huge intuitive or conceptual difficulties contained in the characterization of a continuum of dimension 3 or 4" and the global questions of a topological nature (Weyl 1921, 213). That is, he wisely excluded from his investigation the controversial foundational question of the "nature" of the continuum and the difficult topic of the interrelation between purely continous and the differential topology of manifolds.[24] Here he wanted to address only the "problem of the structure of space (Raumstruktur) as it poses itself to the mathematician" on a local level, informed by differential geometry and group theory and living up to the knowledge standards established by general relativity. Moreover, he left unconsidered what he called the *philosophical problem of space* which would have to deal, in his opinion, with the mutual relationships between the "extensive medium of the external world", i.e. space-time, its metrical

[23]Riemann had already indicated the relationship between free mobility of a measuring device and constant sectional curvature in his inaugural lecture of 1854, although, of course, without establishing any relationship to transformation groups (which were appeared only 15 years later in the work of C. Jordan, S. Lie and F. Klein.).

[24]Even restricted to the mainstream continuum concept of classical modernity, based on general topology and ZFC set theory, the topology of 3- and 4-manifolds has given rise to great surprises in the 1980s (differential structures on 4-manifolds) and may still be open to new surprise. The topology of (3- and 4-) manifolds based on non-classical continuum concepts has still today to be considered largely as *terra incognita.*

structure, and "its material content, its *quale* (Latin in the German original, E.S.) changing from place to place" (Weyl 1921, 212), (Weyl 1923, 1).

In a short overview in (Weyl 1921) and a more extended one in (Weyl 1923) he looked at different approaches mathematicians and scientists of the past had chosen with respect to the PoS:

— at first the approach leading from classical Euclidean geometry to a group theoretical characterization by projective transformations in the sense of Klein's Erlanger programme, culminating finally in Hilbert's researches on the foundations of geometry (Weyl 1923, lecture 1);

— a second approach based on differential geometry, leading from Riemann to Levi-Civita (Weyl 1923, lecture 2);

— and as third one, the Helmholtz-Lie characterization of spaces of constant curvature by the conditions of free mobility of bodies represented here as a condition of homogeneity and (maximal) isotropy (Weyl 1923, lecture 5).[25] Weyl was particularly fond of this approach because of its achievement to derive, at least in this special context, the Riemannian nature from more general Finsler metrics in "a most satisfying way" (Weyl 1921, 221).

In early 1921, and in fact already in the second half of 1920, Weyl expressed clearly that he expected a deep link between the problem of space (PoS) and the "problem of matter" (PoM). That was, of course, a well known topic in general relativity, and could even be traced back deep into the 19th century,[26] but Weyl now indicated a definite methodologically determining direction: *Matter determines the structure of space, not the other way round,* as one might easily read in several passages in Weyl's earlier papers of 1918 and 1919.

After 1920 this insight became a stable conviction of the mature Weyl. He expressed it at different occasions, sometimes in beautiful phrasing like the following of 1923:

> Reality does not move into space like into a right-angled uniform tenement house on which all its changing play of forces glides past

[25]Weyl characterized maximal isotropy in the sense of simply transitivity on "flags" (in later terminology), i.e. chains of 1-, 2- and 3-dimensional subspaces. Neither here nor in lecture 1 did he mention Hilbert's deeper level of foundations by axioms about topological transformation groups developed in his papers of 1902 and published as appendix IV of the 2nd edition of (Hilbert 1898) in 1903. See for them (Strantzalos 2002).

[26]Weyl referred for such a physicalizing tendency in geometry to the authority of Riemann, in a true Göttingen tradition, although he could just as well have quoted Lobachevsky or other authors (Weyl 1921, 223).

without leaving any trace; but rather like the snail does matter itself build and shape this house of its own.[27]

Weyl concluded that the structure of space should be as flexible, and adaptable to matter as possible, although some conceptually well-defined geometrical principle had to be imposed as the *nature of space*. At that time in the early 1920s, Weyl still thought (in distinction to 1929) that such a "nature of space" could be specified by some properly designed a-priori consideration. The old "division of labour" between a-priori, here as determination of possible nature(s) of space, and a-posteriori, the specification of one structure among all those possible among a "nature of space" on the basis of a specific matter constellation, was still part of Weyl's analysis, although now modified in comparison with pre-relativistic times.

For the a-priori characterization of "purely" infinitesimal geometry in a manifold M (the "extensive medium of the external world" in the quote above) Weyl concluded from "conceptual analysis" that the following methodological tools are necessary:

— *Point congruences* operating only in the infinitesimal neighbourhood of each point $p \in M$ (slightly modernized, in each tangent space T_pM), leaving p fixed. They were given by point-dependent subgroups of the linear group in the tangent space (see below, equation (6)) and were understood to define generalized "metrical" relations *inside every infinitesimal neighbourhood*. Their role was comparable to rotations about points in classical geometry and in fact Weyl spoke of them as "rotations" (in the infinitesimal region).

— Infinitesimal *congruence transfer* relating *different*, although infinitesimally close, point-neighbourhoods. Their role was comparable to the introduction of congruence relations between figures at different places in classical geometry; thus they "connected" the metrical relations between different infinitesimal neighbourhoods. Weyl represented them by linear connections given by point-dependent number systems $\Lambda = (\Lambda^i_{jk})$ transforming like affine connections (not like tensors).[28] A general congruence transfer was more general than an affine connection

[27] "Das Wirkliche zieht in den Raum nicht ein wie in eine rechtwinklig-gleichförmige Mietskasern, an welcher all sein wechselvolles Kräftespiel spurlos vorüber geht, sondern wie die Schnecke baut und gestaltet die Materie sich ihr Haus" (Weyl 1923, 44). English translation due to Skúli Sigurdsson.

[28] "Linear connection" means that Λ has values $\Lambda^i_{.k}$ in the $n \times n$-matrices, the Lie algebra of the general linear group $GL_n(\mathbb{R})$. Weyl spoke of the linear connection in this context as a (generalized) "metrical connection" (Weyl 1921, 225). This term has to be read with care, however, as the "metrical" connotation is rather indirect and makes sense only in

(parallel transfer) in that it was not necessarily symmetric in the lower indices (in later mathematical terminology: "with torsion") (Weyl 1923, 49).

— In this context, two linear connections Λ^i_{jk} and $\tilde{\Lambda}^i_{jk}$ would characterize essentially the same infinitesimal congruence structure, if they differed only by (point-dependent) "infinitesimal rotations".[29] In this sense an *equivalence* of linear connections was naturally established (ibid.).

— Finally among all linear connections, equivalent in the sense indicated above, affine connections Γ continued to play the role of "parallel transfer" and achieved a specific role in Weyl's analysis. Formally they were specified by the symmetry condition in the lower indices ($\Gamma^i_{jk} = \Gamma^i_{kj}$, in later terminology the condition for a "torsion free" connection). From the point of view of geometric content, they can be compared with parallel displacements (translations) in classical geometry, which play a distinguished role among all congruence transformations between different points (Weyl 1923, 48f.).

To be a bit more specific with respect to the infinitesimal "rotations", Weyl considered the point congruences in p as forming a group G_p depending on the point, but postulated that all these groups are isomorphic to one and the same (connected) Lie subgroup G of all linear transformations of the infinitesimal neighbourhood, i.e. the linear group $GL_n\mathbb{R}$ (or even $SL_n\mathbb{R}$). Moreover, he considered the point dependence of the groups as given by "conjugation" in the sense of group theory, i.e.

$$G_p = h_p^{-1} G h_p \tag{6}$$

with point-dependent elements h_p (Weyl 1923, 48). He spoke of G as (a standard version of the) *rotation group* which specifies the *nature of the metric*. As a result of the further development (consequences derived from the "synthetical" principles discussed below), Weyl arrived at a representation of the structure which shows that, after proper choice of "coordinates" in the infinitesimal neighbourhoods h_p can be reduced to the normalizer H of G in the group of all linear transformations.[30] H played the role of *similarity group* related to G (Weyl 1923, 51f.).

the context of specified point congruences. A possibility to reduce the group (to H) was inbuilt in Weyl's analysis; see below about footnote(29).

[29]More precisely: The connections Λ and $\tilde{\Lambda}$ are equivalent in this sense, iff $\Lambda - \tilde{\Lambda}$ is a differential form with values in the Lie algebra of the group G of "rotations".

[30]In more recent terminology one would speak of an appropriate trivialization of the tangent bundle, rather than "coordinate choice" in the infinitesimal neighbourhoods. The

The manifold M and the "nature" of the space, i.e. the rotation group G were the building blocks, or base data, for what Weyl called in this context a (generalized) "concept of metric" ("Begriff der Metrik") (Weyl 1923, 47). As there have already been discussed so many different ways to generalize the concept of metric, I prefer to call this specific type of generalization a (Weylian) "infinitesimal congruence structure".

Definition 1 *A* Weylian infinitesimal congruence structure *of "nature" G on a n-dimensional manifold M is given by a group G (connected Lie subgroup of the linear group in dimension n, $GL_n(\mathbb{R})$), together with*

(1) an operation of G on the (tangent) vectors that can be described by point-dependent conjugation $G_p = h_p^{-1} G h_p$ with $h_p \in GL_n(\mathbb{R})$,[31]

(2) and a linear connection $\Lambda = (\Lambda^i_{jk})$, or more precisely, its equivalence class as indicated above.[32]

(1) characterizes infinitesimal rotations and (2) infinitesimal congruence transfer.

These Weylian infinitesimal congruence structures were conceptually intricate objects. Weyl described them rather intuitively, with the effect that different interpretations have been proposed by later readers.[33]

He considered their characterization to be the result of a purely conceptual analysis of what a generalized concept of metric ought to consist of.

congruence transfer Λ will then be reduced to H too, i.e. takes values in the Lie algebra of H. — "Normalizer" means that H is the largest subgroup of $GL_n\mathbb{R}$ containing G as a normal subgroup.

[31] If the nature of the space satisfies additional "synthetic" postulates discussed below, the conjugation in (1) can be reduced to operation with h_p in the similarity group $H :=$ normalizer (G).

[32] More formally: equivalence up to differential forms with values in the Lie algebra of G.

[33] Talking in fibre bundle language, it is important to notice that Weyl's "infinitesimal rotations" *cannot* adequately be represented in a principal bundle with group G. They rather live in an adjoint sub-bundle of a principle bundle with group H and typical fibre G. Therefore Weyl's infinitesimal congruence structures cannot be characterized simply as a G-structure in the terminology of (Kobayashi/Nomizu 1963), as E. Scheibe rightly noticed at different places, e.g. in (Scheibe 1988). The interpretation given here owes much to communications from J. Ehlers and has been outlined in (Scholz 2001*c*). Another interpretation of R. Coleman and H. Korté avoids fibre bundle language and analyzes Weyl's generalized "metrical structure" in terms of Finsler geometry (Coleman/Korté 2001). I disagree with their claim, however, that their interpretation is close to Weyl's texts and intentions of the early 1920s. Moreover, the relationship between Weyl's analysis of PoS and his later work on the Dirac equation becomes completely lost from their point of view.

Therefore he called them *analytic* in the Kantian sense. Moreover he insisted on the necessity to add some *synthetical* principles before one would arrive at a concept that was in agreement with the "essence of space". Such a synthetical principle would lead to some additional a-priori restriction on the group G acceptable for the description of the "nature of space". On the level of epistemology this was again an open reference to Kantian philosophy. In the sequel it will become clearer, however, that Weyl's "synthesis" did not take place in *pure intuition* but was nurtured by a rather more complex (cultural) medium.

All together, Weyl thought that such an approach was sufficiently close to the intentions of phenomenology that his analysis of the concept of space could be considered as a successful example for a the "analysis of essence (Wesensanalyse) aimed at by the phenomenological philosophy (Husserl)" (Weyl 1918*a*, ^{4}1921, 133). He made quite clear that his analysis was no direct product of an immediate act of a priori insight. Therefore his "Wesensanalyse" could not be considered as identical with what Husserl and his pupils tried to do. Weyl insisted that his conceptual analysis and the preparation of the "synthetical" principles relied strongly on the results of a long tradition of mathematical and physical research (Euclid, Galilei, Newton, Riemann, Einstein) and even warned readers, that reason "had not the power to penetrate" with one flash "what is of itself intelligible to it (das ihr aus-sich-selbst-Verständliche)". He continued:

> This reproach must be directed at the impatience of those philosophers who believe it possible to describe adequately the [essence] on the basis of a single act of typical presentation (exemplarischer Vergegenwärtigung): in principle they are right: yet from the point of view of human nature, how utterly wrong are they. (Weyl 1918*a*, ^{4}1921, 133)[34]

This "reproach" was probably directed at least as much at the address of Fichte's philosophy which Weyl had been addicted to the few years before, as at Husserl's. But he also insisted on a difference to phenomenology proper. Even where he took up questions posed by phenomenological philosophers, he was open that he gave them a turn of his own. In particular, he claimed that his analysis would take into account a certain ingredient of conventionalism in the construction of the "a priori":

> The problem of space is at the same time a very instructive example of that question of phenomenology that seems to the author

[34] Brose's translation from (Weyl 1922, 148), one word corrected by me: "essence (Wesen)", translated by Brose as "mode of existence" (!).

to be of greatest consequence, namely, in how far the delimitation of the essentialities perceptible in consciousness expresses the structure peculiar to the realm of presented objects, and in how far mere convention participates in this delimitation. (ibid.)[35]

When Weyl talked about "conventions" participating in the construction of concepts ("delimitation of essentialities") one should not misunderstand them in the sense of a purely technical question of epistemology. Apparently for Weyl, such "conventions" were placed and reflected in a wide cultural and social field, as the discussion of the "synthetical" postulates would show a little after he wrote the lines quoted here, when he gave lectures on the problem of space at Barcelona in early 1922 (published as (Weyl 1923)).

That does not mean that Weyl "derived" such postulates from social and cultural reflections. They had, of course, a strong mathematical core, which Weyl liked to bring in communication with the rest of his mind in a kind of "cultural resonance".[36] Mathematically, his conjecture of 1919 formulated in the commentaries on Riemann were at the back of Weyl's mind. Now he hoped to refine and extend this conjecture and to cast it into the form of a synthetical priniciple (or a couple of them) for a convincing new version of a priori foundation of post-relativistic "purely" infinitesimal geometry.

As a result of his considerations Weyl added two postulates to his analysis of the purely infinitesimal "concept of a metric" in the sense of an infinitesimal congruence structure (our Definition 1). He openly declared that they could not be gained by pure analysis, but were contributing something "synthetical" to his investigation (Weyl 1923, 49)

Postulate 1 ("Freedom") *The group G allows the widest conceivable range of possible congruence transfers in one point, in adaptation to matter distribution.*[37]

[35] "Das Beispiel des Raumes ist zugleich sehr lehrreich für diejenige Frage der Phänomenologie, die mir die eigentlich entscheidende zu sein scheint: inwieweit die Abgrenzung der dem Bewußtsein aufgehenden Wesenheiten eine dem Reich des Gegebenen selbst eigentümliche Struktur zum Ausdruck bringt und inwieweit an ihr bloße Konvention beteiligt ist." (Weyl 1918*a*, ^{4}1921, 133)

[36] I adopt this term from an ongoing discussion among historians of mathematics, mathematicians, and cultural scienties trying to understand the analogous problem of the relation und communication between the mathematician F. Hausdorff and the intellectual and cultural criticist P. Mongré. The "resonance" metaphor is due to E. Brieskorn.

[37] More formally: Given G and a point $p_0 \in M$, there are sufficiently many possibilities to form an infinitesimal congruence structure with congruence transfer Λ^i_{jk} that all systems $L^i_{jk} \in \mathbb{R}^{3n}$ can arise as values at one *selected* point $p_0 \in M$: $L^i_{jk} = \Lambda^i_{jk}(p_0)$.

Postulate 2 ("Coherence") *The "nature of space" (specified by the data manifold M and rotation group G) must be such that any infinitesimal congruence structure built from it allows exactly one parallel displacement.*[38]

Weyl argued that a given "nature of space" is in conformity with the "essence of space", if and only if theses postulates of freedom and coherence are satisfied. He compared Postulate 1 with Helmholtz' principle of free mobility, in a form adapted to the new situation of relativistic geometry (Weyl 1923, 46). Moreover, the importance of parallel displacement (affine connection) in the analysis of the PoS was influenced by Weyl's experience in general relativity. He had stressed earlier than the physicists, including Einstein himself, the central role of the affine connection as the *guiding field* of the inertio-gravitational structure of the world. The new stage of analysis led to such drastic changes, however, that Weyl felt it necessary to give additional supporting motivations for the acceptance of his postulates as "synthetic" principles from a philosophical point of view.

The most remarkable passage, in this respect, is contained in his Barcelona lectures where Weyl employed a strong *social metaphor* to characterize the new synthetical part of his investigation of the problem of space Here he reflected on the configuration of social activities by members of a state and compared the concept of space with the constitution of a "state". For him, the "nature of the space" was comparable to the "binding law for all citizens" of a social collective, a "state" as Weyl preferred to put it. The principles of a liberal democratic state, the widest possible freedom for the individuals to act (inside the limits of the "binding laws") was, for him, the social equivalent to the "postulate of freedom" in his analysis of space. But then an additional condition had to be presupposed, which would ensure a kind of social coherence among all free individual choices, i.e. some "common good" which had to be compatible with all the individual choices.

> The essence of the constitution of a state is not yet specified if I postulate: The constitution is a binding law for all citizens of the state, although inside the limits of the constitution each citizen has complete freedom to act. Some positive postulate like the following has still to be added: However the citizens will use their liberty, it is an essential property of our constitution that the common good is sufficiently guaranteed. (Weyl 1923, 46)

The regulative idea of "common good" in the case of the problem of social

[38]More technically: The congruence transfer Λ^i_{jk} of any infinitesimal congruence structure on M can equivalently be represented by exactly one affine connection.

constitution was now compared by Weyl with the postulate of coherence (Postulate 2) in the case of the problem of space:

> If one considers the construction of infinitesimal geometry as a whole, including its applications to reality which it finds in general relativity, then the following crucial fact will jump onto us with unescapable uniqueness: that the metrical field determines uniquely the affine connection; on this [principle], so it seems, relies the well-being of the whole in the realm of space and time (ibid.).[39]

These were strong words to justify the synthetical part of the investigation of the problem of space. They take up and elaborate a motif we find times and again in Weyl's writings, the interrelationship between freedom and being bound, often presented as a question of the self-definition of the individual (Sigurdsson 2001). In the Barcelona lectures we find, moreover, strong indications that the productive imagination of the author of these lines depended on an acceptable social environment. In this sense, Weyl's coherence postulate reflected how he understood the "social field" as a binding law on the indiviual freedom to create and to behave. It depends on the reader's perspective whether she understands this longing for interconnectedness and coherence as a hangover of "organistic" metaphors in German social philosophy or as an inkling of something beyond "modernity", as a necessity to respect conditions of conviviality.[40]

Independent of the interpretation just mentioned, this passage is indicative for some of the forces shaping the "genealogy" of the research discourse (to take up a formulation of M. Foucault) in this seemingly distant field of intellectual activity. H. Mehrtens has contributed to the broader investigation of the interrelation between the "discourse on mathematics" with all its social and cultural ingredients and the mathematical research discourse itself (Mehrtens 1990). He investigated, among others, H. Weyl's contributions to the foundations of mathematics from that point of view.[41] Weyl's discussion of the social connotations of the principles of the analysis of the problem of

[39]"Wenn man sich den Aufbau der Infinitesimalgeometrie vor Augen hält und auch die Anwendung, die sie in der allgemeinen Relativitätstheorie auf die Wirklichkeit findet, so springt einem als die entscheidende Tatsache, welche die ganze Entwicklung möglich macht, mit unentrinnbarer Eindeutigkeit diese entgegen: daß durch das metrische Feld der affine Zusammenhang bestimmt ist; darauf, scheint es also, beruht im Reiche von Raum und Zeit das 'Wohl des Ganzen' ."(Weyl 1923, 46)

[40]I refer here to the use of language of Ivan Illich.

[41]For a more recent discussion of this topic compare the interesting remarks in (Schappacher 2002).

space links directly to these observations; it might enrich the picture of the intertwining levels of discourse.

After this highly involved establishment of the principles of a space concept, incorporating the new situation arising from so different spheres of cultural reality as relativity and the crisis of modern society as it was experienced in the early Weimar Republic, came the mathematical work for the evaluation of the consequences of the postulates. It was clear that the postulates implied restrictions on the groups that could figure for the representation of the "nature of space". Apparently Weyl conjectured that these restrictions were similar to what he had presumed in his remarks upon Riemann and would lead to those rotation groups which arise as the symmetries of "Pythagorean" (semi-Riemannian) metrics, the so-called "generalized special orthogonal groups" of arbitrary signature $\mathrm{SO}(p, q, \mathbb{R})$.[42] Thus he was glad when, in several steps between 1921 and 1923, he was able to prove:

Theorem 1 *The groups satisfying Weyl's conditions for the characterization of the PoS in a manifold of dimension n (Definition 1, Postulates 1 and 2) are just the generalized special orthogonal groups of arbitrary signature (p, q) $(p + q = n)$*

$$G \cong SO(p, q, \mathbb{R}) .$$

Taking into account the definition of infinitesimal congruence structures, it was clear that Weylian manifolds (of any signature) were examples satisfying the characterization and the postulates of his analysis of the PoS. Moreover Weyl argued, although only sketchily, that also the converse was true; i.e. an infinitesimal congruence structure with group like in the theorem leads to a manifold with Weylian metric (Weyl 1923, 51f.). Thus semi-Riemannian manifolds *considered from the generalizing point of view of Weylian gauge metrics* were just the right class of mathematical objects to specify the infinitesimal congruence structures of the problem of space of the years 1921 to 1923. If one accepts Weyl's result (or reconstructs it by more recent means), a choice of orthogonal basis with respect to the Weylian metric in the tangent spaces reduces the conjugation of equation (6) to the normalizer H of G, the "similarity" group.

It must have been a great relief to Weyl in the early years of the 1920s that his gauge geometry had a broader and deeper conceptual justification, independent from success or failure of his early hopes from 1918 for an immediate application in unified field theory. Although these hopes now appeared as premature, gauge structures remained important geometrically. They even

[42] For a short explanation of the terms "semi-Riemanniann metrics" of "signature (p, q)" see footnote 15 and 16.

became more diversified in the infinitesimal congruence structures of his analysis of the PoS. Last but not least, Weyl's analysis led to a closer and intimate link between gauge geometry and group operations in the infinitesimal realm. Essential for Weyl's gauge aspect was now that the action of a "small" group G as infinitesimal symmetries at every point ("rotations") was complemented by a larger group H ("similarities") used for the description of transfer of symmetries from one point to another. The quotient group H/G played the role of what later became the "internal symmetries" of the gauge structure. Here, however, H arose rather naturally from the geometrical context as a particularly simple group extension of G.[43]
Weyl's analysis of the PoS had brought him to the brink of considering the gauge connection itself from a group theoretic point of view, related to the extension group $H/G = \mathbb{R}^+$ which arises from the geometrical context.[44] *But he did not yet take the step to the other side of the threshold.* Even at the end of the Barcelona lectures, Weyl presented the gauge connection as a differential form like in our equations (3) and (4) and related to the gauge of length measure (Weyl 1923, lecture 8). Apparently there was no reason for him to comment in more detail on the group theoretic content now coming into being.

It may well be that the importance of this result decreased for its author during the second half of the decade. Already in his work on the representation theory of Lie groups during the years 1924 to 1926 Weyl turned into mathematical investigation full of technical intricacies and achieved deep results in this field (Hawkins 2000, chap. 12). Directly after the establishment of his main results on Lie groups, he started to teach and to publish in the rising "new" quantum mechanics; as a consequence he distanced himself even further from his earlier tendency to build his arguments on a-priori considerations. Perhaps this was the reason that in a manuscript written in 1925 for the Lobachevsky anniversary he alotted only little space to his analysis of the PoS, although the topic of the paper was a general overview of the relationship between differential geometry and group theory, to which a broader discussion of the problem of space would have fitted beautifully (Weyl 1925/1988, 37f.).

He wrote the paper at a time when the "new" quantum mechanics was in the making. Its rise apparently induced Weyl to go on with his rearrangement of his former balance between mathematics, philosophical perspective, and physical theory building. During the year 1927 the first paper documenting

[43] H turned out to be a "direct product" of G by the commutative group of positive reals: $H = SO(p,q) \times \mathbb{R}^+$.

[44] More precisely: A connection specific for the *gauge* aspect in this context had to have values in the Lie algebra of H/G (here isomorphic to $\mathbb{R}^+$).

Weyl's thoughts on the new quantum mechanics appeared (Weyl 1927). At the beginning of the winter semester of the same year both professorships in theoretical physicists at Zürich, at the ETH and at the university, were vacant.[45] Weyl used the opportunity to integrate quantum mechanics into a planned course on group theory and turned it into a lecture course on group theory and quantum mechanics, which gave rise to his book on the topic (Weyl 1928).

Of course, this did not imply that after 1925 he would tend to neglect or even reject the mathematical achievements of the gauge principle or the analysis of the PoS. It even turned out that the form, which the gauge principle had acquired during his work on the problem of space, was sufficiently general to serve as a perfect preparation for a further transformation and fruitful adaptation to physical semantics, opened up by the new quantum mechanics.

Adaptation of gauge structures to the Dirac field

During the 1920s several physicists proposed to adapt Weyl's gauge geometry of 1918 to the quantum mechanical description of the electron, among them E. Schrödinger (1922), V. Fock (1926), and F. London (1927). They did so in quite different attempts to make sense of the rising quantum world and to relate it in some way or the other to the known symbolical representations of fields in relativity theory or its generalizations (unified field theories). Schrödinger still stuck to the original length calibration idea, but explored its use in a heuristic attempt to model an (electron) matter field of its own, which three years later turned into the Schrödinger wave field. Fock and London worked in a 5-dimensional relativistic framework, later called "Kaluza-Klein theory", although Fock did not know Kaluza's and Klein's work, before he wrote his paper, and got to know it only after its submission.[46] F. London knew both, O. Klein' s and V. Fock's work, and tried to integrate de Broglie's and Schrödinger's wave fields with relativity along these lines.[47] Fock and London realized that the gauge idea could rather naturally be applied in the quantum context, but now for the phase of the wave function rather than for length measurement. The exact formulation of a relativistic theory of the electron remained open, however, in both papers. London stated that the task was now, to accomplish for the "Weylian theory (i.e. Weyl's gauge geometry of 1918, E.S.) which now appears as obsolete ... the corresponding step which leads from de Broglie to Schrödinger" (London 1927, 386).

[45] See (Frei/Stammbach 1992, 92ff.), (Sigurdsson 1991, 235ff.).

[46] For T. Kaluza and O. Klein see (Goenner/Wünsch 2003).

[47] See (Vizgin 1994, chap 6), (Cao 1997, Morrison 1995), (Gavroglu 1995).

Weyl did not react directly to any of these proposals in public, although it is very likely that there was oral exchange between him and Schrödinger who was close by, at the University of Zürich, and possibly also with F. London who joined Schrödinger as a Rockefeller fellow between April and October 1927, before he went to Berlin as Schrödinger's assistant.[48] At the occasion of his lecture-course on group theory and quantum mechanics in the winter semester 1927/28, Weyl definitively endorsed the new variant of the gauge principle. In his second great book in mathematical physics *Gruppentheorie und Quantenmechanik* (Group Theory and Quantum Mechanics), arising from the lecture notes, he referred to F. London's (1927) version of the adaptation of the gauge idea to quantum mechanics (Weyl 1928, 87f.).[49]

Weyl gave here a streamlined presentation of the basic idea, stripped from the 5-dimensional relativistic framework which apparently appeared unconvincing to him. He emphasized the use of a calibration for the phase of a complex-valued wave function $\psi(x)$ in place of length calibration of vectors as the most important point of the modification. In fact, there was much less reason to assume a universal "Eichamt" for the phase of a wave function, i.e. a natural way for a phase calibration, than there had been for the length measurement. Whereas in the latter context Einstein had given strong physical reasons (stability of spectral lines in atoms) speaking against Weyl's claim that one ought to give up distant calibration, a reasonable physical calibration for the phase of wave functions was completely out of sight. Thus it was natural to consider wave functions ψ, $\tilde{\psi}$, related by phase transformations of the form

$$\tilde{\psi}(x) = e^{i\lambda(x)}\psi(x) \quad \text{with} \quad i = \sqrt{-1}\,, \tag{7}$$

as physically equivalent symbolic objects. This substitution looked highly plausible because of the underdetermination of the complex phase of a wave function. As Weyl put it:

> Only $\bar{\psi}\psi$ has a simple physical significance; it is therefore to be assumed that the laws which govern ψ remain invariant on replacing ψ by $e^{i\lambda} \cdot \psi$, where λ is any real function of position in space-time. (Weyl 1928, 87)

The transformation (7) was complemented by a modification rule for electromagnetic potentials $\varphi = (\varphi_i)$,

$$\varphi_i' := \varphi_i - \frac{\hbar}{e}\frac{\partial \lambda}{\partial x_i}\,, \tag{8}$$

[48](Gavroglu 1995).

[49]In the second edition discussed in §12, see English translation (Weyl 1931*a*, 100ff.)

which was easy to identify *formally* as a gauge transformation.[50] For the investigation of a relativistic particle in an electromagnetic field this transformation acquired a surprising physical content. The operator for the quantum mechanical description of linear momentum $p_i = \frac{\partial}{\partial x_i}$ took on just the form of a *covariant derivative* with respect to the gauge connection,[51] That was at the back of Weyl's mind, when he declared (here he denoted the index by α):

> *The action of an electromagnetic field on a particle of charge $-e$ can be expressed by substitution of p_α by $p_\alpha + e\varphi_\alpha$.* (Weyl 1928, 100, emphasis in original)

In this passage Weyl referred to the description of a relativistic particle by a "wave" function, later called Klein-Gordon field, and hinted at the *miraculous effect* that the Hamilton operator of a free uncharged relativistic particle was transformed into the Hamilton operator of a charged particle in an electromagnetic field with potential φ_i, just by *formally* using the covariant derivative with respect to the gauge connection.

This was such a striking coincidence that the adaptation of the gauge idea to (special) relativistic quantum mechanics promised to contain more potentialities than a superficial analogy would let one expect. Or put differently, if one considered the usefulness of the covariant derivative in the construction of the Hamilton operator as arising from a formal analogy, then it was already clear that this analogy had remarkable physical consequences, not yet directly inbuilt into its construction.[52] I consider such a symbolic surplus to be an open or latent *hyperbolic symbol potentiality* in the original sense of the Greek word.[53] Weyl, at least, had good reasons to hope to be able to endow the new use of phase gauge with a conceptual significance by relating it to the framework of general relativity and "purely infinitesimal" geometry.

Weyl accepted that this new "principle of gauge invariance" was analogous to the one he had postulated in 1918 "on speculative grounds, in order to arrive at a unified theory of gravitation and electricity". He added a

[50] If one adapts the φ_i by an additional factor $\frac{-e}{i\hbar}$ to the components of a differential form, like in our quation (3), and takes into account that the gauge function is now $e^{i\lambda(x)}$, the gauge transformation (4) leads to (8).

[51] The covariant derivative with respect to a length- or a phase-connection φ (in this case a differential form with values in $\mathbb{R}$) is given by $\frac{\partial}{\partial x_i} + \varphi_i$.

[52] This feature added a strong empirical input to a conceptual strategy that recently has been characterized as "structural metaphysics" by M. Morrison (1995).

[53] Hyperbolic in the sense of *hyperballein* — to exceed, be surplus, to surpass.

remark about what he considered to be the most important aspect of the modification:

> ... But I now believe that this gauge invariance does not tie together electricity and gravitation, but rather *electricity and matter* in the manner described above. How gravitation according to the general theory of relativity can be included, is still uncertain. (Weyl 1928, 1st ed. 88)[54]

Thus, for Weyl, the new physical contextualization of the gauge principle was of much greater importance than the purely formal side of the change from considering gauge factors no longer to be real but complex (unimodular), i.e. the substitution of the gauge group $\mathbb{R}^+$ by $U(1)$. It appeared most pleasing to him to find now the gauge field of electromagnetism (or its potential, the connection) to be tied ("coupled") to the matter field of the wave function, no more to the inertio-gravitational field like in his unified theory of 1918. But Weyl was also clear that this modification of the gauge principle appeared unsatisfactory as long as one did not understand how it fitted in with general relativity, from which his older gauge idea had originated.

A year later, in the first half of 1929, Weyl made considerable advances towards a solution of the problem of how to relate the wave field with inertio-gravitational structures in the spirit of general relativity.[55] Now the complex scalar (Klein-Gordon) wave field like the one considered in his book (Weyl 1928) was superseded by a complex 4-component field $\psi(x)$ invented by P.A.M. Dirac to develop a relativistic theory of the electron.[56] Dirac had achieved in early 1928 the first physically realistic description of the relativistic motion of an electron in a centrally symmetric, static electrical field. He introduced a "wave" function $\psi(x)$ with four components on Minkowski space as a new symbolical device, later called a *spinor field*,

$$\begin{aligned} \psi(x) &= (\psi_1(x), \psi_2(x), \psi_3(x), \psi_4(x)), \\ x &= (x_0, x_1, x_2, x_3) \text{ coordinates in Minkowski space,} \end{aligned}$$

where the $\psi_i(x)$ (for $i = 1, \ldots, 4$) were complex valued functions. Dirac's wave quantities satisfied a dynamical condition given as a first order partial differential equation, the *Dirac equation.* They encoded a lot of structural and dynamical information on the electron, among others the probability density of its location, probability current densities for its dynamical behaviour

[54] Translation of the first sentence due to H.P. Robertson in (Weyl 1931*a*, 100f.). The last phrase has been cancelled in the second edition, because in 1931 the "uncertainty" mentioned there had vanished (for Weyl); see below.

[55] (Weyl 1929*a*, Weyl 1929*b*).

[56] See (Kragh 1981) or (Kragh 1990, chap. 3).

and in particular its angular momentum. From the probability densities the empirically better accessible densities of charge and current of a moving electron could be calculated. Moreover, and most important, the total angular momentum and the energy levels of an electron in the rotationally symmetric electric field could be derived, like the one around a hydrogen nucleus. The calculated quantities were covariant with respect to Lorentz transfomations. They led to a surprising and unsurpassed agreement with spectroscopical observations, including the *fine structure* of the hydrogen spectrum. No other approach had been able to achieve a comparable result. Dirac's theory was thus justly considered as a decisive breakthrough on the difficult road towards a relativistic quantum theory.[57]

For mathematicians, the most intriguing feature of Dirac's ψ-quantities was that they no longer transformed like vectors or tensors in differential geometry, but according to different rules which were immediately identified as a *complex representation* of the Lorentz group. They could be described by the 2×2 complex matrices of determinant 1, called $SL_2(\mathbb{C})$, and their adjoints, i.e. complex conjugate and transposed matrices.[58]

Such a complex representation of the Lorentz group could not be extended to the whole linear group, as used by Einstein when he formulated the general covariance principle of general relativity in terms of the Ricci-calculus. Thus a severe obstacle appeared for any attempt to generalize and integrate Dirac's theory to general relativity. Nevertheless, different roads towards such an integration were possible. In early 1929, H. Weyl who at that time was at Princeton (giving talks also at other places in the US) and V. Fock at Leningrad developed one of these approaches, at first not knowing of the other's steps until early summer 1929. Without going into the details of the mathematical construction and its physical interpretations, we need here to consider only the following remarkable feature of Weyl's proposal to establish a common conceptual framework for gravitation, the Dirac field $\psi(x)$, and an electromagnetic field with potential $\varphi(x)$.[59]

Lorentz transformations were represented on the "Dirac quantities" (the later spinors) by transformations of the group $\mathrm{SL}_2\mathbb{C}$, as noted already above. That made it possible to transport the Dirac field into the context of gen-

[57] Compare (Kragh 1990), (Pais 1986, 290ff.), (Mehra/Rechenberg 2000, 299ff.).

[58] The Dirac field had 4 complex-valued components $\psi(x) = (\psi_1(x), \psi_2(x), \psi_3(x), \psi_4(x))$ in Dirac's original version, or 2 components $\psi(x) = (\psi_I(x), \psi_{II}(x))$ in Weyl's own simplified version using an irreducible group representation (whereas Dirac's was/is reducible). Later terminology would call them "Dirac spinors" or "Weyl spinors".

[59] For more details on Weyl's work on the Dirac equations see (Straumann 2001, O'Raifeartaigh/Straumann 2000) and for a comparison between Fock and Weyl (Vizgin 1994, 292ff.) or more recently (Scholz 2001*a*).

eral relativity by the so-called method of "orthonormal tetrads", developed by Levi-Civita, Cartan, and other differential geometers to represent vectors and tensors differently from the usual Ricci-calculus, and picked up by Einstein at about the same time for different reasons.[60] That allows to take over the affine connection of general relativity (the Levi-Civita connection) to the new context of spinors.[61] Thus the affine connection from general relativity allowed to introduce a kind of derivative adapted to this geometrical background. Weyl called it, in analogy to the situation in differential geometry, a "covariant derivative" of spinor quantities (Weyl 1929*b*, 253ff.).[62] Such a "covariant derivative" of spinors was not uniquely determined by the affine connection from general relativity, though, because the spinor function $\psi(x)$ themselves could only be specified up to a point-dependent phase factor $e^{i\lambda(x)}$.

Weyl considered this underdetermination to be a result of the general relativistic symbolic environment for the "Dirac quantities" ψ in the method of orthogonal tetrads. The underdetermination of the phase was comparable to the earlier observations of the phase of the Schrödinger or the Klein-Gordon "wave" functions, nevertheless it now appeared clear to him that phase gauge was essentially a general relativistic phenomenon or, in mathematical terms, a question of "purely infinitesimal" geometry (Weyl 1929*b*, 246, 263). In Weyl's perspective this meant that the covariance group of the spinors $\mathrm{SL}_2\mathbb{C}$ had to be extended to the larger group $\mathrm{SL}_2\mathbb{C}\times\mathrm{U}(1)$, with the unitary group $\mathrm{U}(1)$ of complex numbers of norm 1.

> Actually, the components ψ_1, ψ_2 (Weyl worked with a 2-component version of spinors, E.S.) are not uniquely determined by the tetrad, but only inasmuch as they can still be multiplied by an arbitrary "gauge factor" $e^{i\lambda}$ of norm 1. The transformation which ψ suffers under the impact of a rotation of the tetrad is determined only up to such a factor. (Weyl 1929*b*, 263)

That was quite close to how the "rotations" G were extended to the "group of similarities" H in the analysis of the PoS. But now the extension of the "rotation group" $G = SL_2(\mathbb{C})$ by $U(1)$ was no longer left implicit in the

[60]The name "orthonormal tetrads" is chosen, because in each tangent space (or infinitesimal neighbourhood) a basis of (pairwise) orthogonal vectors of length 1 is chosen ("orthonormal") and space-time is 4-dimensionial ("tetrad" for 4 base vectors).

[61]The Levi-Civita connection was "lifted" to the spinor bundle, assuming there values in the Lie algebra of $\mathrm{SL}_2\mathbb{C}$. A topological obstrucion did not appear, as Weyl worked only locally, i.e. in simply connected regions.

[62]Different from Fock, Weyl avoided to speak of "parallel displacement" of spinor quantitites, or to use other openly geometric language.

overall description. Weyl gave here a completely explicit description of the full symmetry group of the spinors $H = SL_2(\mathbb{C}) \times U(1)$. The context gave him strong reasons to do so, because the internal symmetries $H/G = U(1)$ led to the physically important *conservation of electric charge* (Weyl 1929*b*, 264).[63]

For the full characterization of a covariant derivative of Dirac quantities (spinors) a connection with values in the extended group $H = \mathrm{SL}_2\mathbb{C}\times\mathrm{U}(1)$ (or more precisely its Lie algebra) had to be given. The $\mathrm{SL}_2\mathbb{C}$ part was determined by the *guiding field* of space-time, lifted to the world of spinors. The U(1) component of the connection had to be given by an additional differential form $\varphi = \sum_i \varphi_i dx^i$ like in the original theory of 1918 and transformed like it under changes of the gauge by the complex function $\tilde{\lambda}(x) = e^{i\lambda(x)}$ (equation (8) above) (Weyl 1929*b*, 263). It could now be used to represent the *electromagnetic potential*, without the disturbing gauge effects for length measurement of the approach of 1918. Much more, it fitted beautifully to the Dirac equation of the electron in a static electromagnetic potential (modelling e.g. the hydrogen nucleus). This aspect appeared acceptable even for the otherwise highly critical W. Pauli.[64]

By the development of the basic ingredients of the same structure by V. Fock "independently" of Weyl in early 1929,[65] it became all the more clear that this new approach was strongly adapted to the context of the Dirac equation. Although Fock did not emphasize a group theoretic perspective, he considered the question of how to define "parallel transfer" and the related covariant derivative of spinor quantities as a type of differential geometry adapted to quantum mechanics.[66] In a slightly different setting he found the same underdetermination of the parallel transfer of spinor quantities as Weyl and developed the consequences for the introduction of the electromagnetic

[63]Today this conservation argument is considered as a special case of the *Noether theorem*. For the case of gauge symmetries it seems that Weyl realized the conservation principle independently; he quoted E. Noether only marginally in this context (Rowe 1999, Brading 2002).

[64]The reception of Weyl's new proposal by Pauli, changing from sharp criticism, through partial adaptation, while rejecting certain parts of the theory, to a public endorsement and presentation in a simplified form in the *Handbuch der Physik*, turned out to be crucial for the dissemination of the modified gauge idea to the next generation of physicists. That is a different story, highly interesting in itself; for first explorations see (Straumann 2001, O'Raifeartaigh/Straumann 2000, O'Raifeartaigh 1997, Pais 1986, Mehra/Rechenberg 2000).

[65](Fock 1929); Fock collaborated initially with D. Ivanenko. See (Vizgin 1994, chap. 6).

[66]In the first enthusiasm Fock and his colleague Ivanenko even anounced their findings as the realization of some new "quantum geometry". Later Fock became more cautious and Ivanenko withdrew from the game.

potential as a connection related to a U(1)-gauge in the same sense as outlined above.

Fock's perspective as theoretical physicist was different in several respects from Weyl's. The core of his work was nevertheless so close to Weyl's that the latter had good reason to consider their work as characterizing essentially the same theory, after he got to know Fock's researches in summer 1929. There remained, however, highly interesting differences in the broader interpretational questions of the relationship between physics and geometry. Whereas Fock initially hoped to have a kind of "quantum geometry" at hand, Weyl now warned against all hopes for a rash continuation of the geometrization program of physics stimulated by general relativity or, the other way round, against the expectation of a direct continuation of the physicalization of geometry into the realms of quantum physics. Weyl expected a long road to go, before *perhaps* a "geometry of matter" in the sense of quantum physics might come into being. This is part of a different story.[67] In our context the most important aspect of this new theory lies in points shared by both of its protagonists, and in the sequel we will only deal with such aspects of the approach.

Weyl formulated the common points in a discussion in the early 1930s, looking back at the unified field theories of the past decade. Comparing his "old" (1918) with the "new" (1929) gauge principles he concluded:

> In formal respect there is greatest similarity, in factual respect, however, there exist important differences.
>
> 1. The new prinicple has grown from *experience* and it resumes a huge treasury of experience from spectroscopy.
> 2. *The gauge factor* $e^{i\lambda}$ *is not set at the side of the metrical quantities (...) but at the side of the material quantities* ψ.
> 3. The exponent is not real but purely imaginary. ...
> 4. The natural unity in which the electromagnetic potentials have to be measured is not an unknown cosmological quantity but a known atomistic one $\frac{e}{2\pi h}$.
>
> (Weyl 1931*b*, 344, emphasis in the original)

Conceptual analysis and the development of proper symbolical devices to give an adequate mathematical expression to the result was still at the heart of Weyl's contributions to the interchange between mathematics and physics;

[67] For a discussion of Weyl's differences to Fock's "quantum geometry" and the (classical) unified field theory program of the late 1920s, see (Weyl 1931*b*) and (Scholz 2001*a*).

but now *he emphasized empirical foundations and material references of the symbolic constructs* rather than their legitimation a-priori. The symbolical structure arising from this analysis was still quite close to the one of the problem of space: a group of symmetries in every point neighbourhood ($G \cong \mathrm{SL}_2(\mathbb{C})$) extended to a larger structure group H for the transfer of spinors to close by points (and the corresponding covariant derivative). While the point-symmetries in G remained properly geometric (spinor "rotations"), the extending group of internal symmetries $H/G = U(1)$ was no longer derivable from geometry, perhaps even from "a-priori" considerations, although it could be very naturally implemented in the symbolic construction of the reltivistic Dirac equation. Finally the resulting gauge group characterized the *symmetry of the matter field* and had been arrived at by the collective effort of physicists and mathematicians to resume "a huge treasury of experience from spectroscopy".

Moreover, this link between gauge structures and wave functions incorporated now the aspects of statistical physics that had intrigued Weyl so much in his early and highly speculative talk on the relationship between classical determinism, stochastic principles of physics and the geometry of the continuum (Weyl 1920). Mediated by M. Born's statistical interpretation of the wave function, Weyl could now do a first step towards a more solid connection between his geometrical methods and the Vienna tradition of indeterminism.[68]

Concluding remarks

Looking back, we can see how H. Weyl's work on the analysis of the problem of space in the early 1920s was part of a broader move to enlarge the scope of gauge geometry from a conceptual, a mathematical, and a physical point of view. While E. Schrödinger experimented in a fruitful mixture of speculation (with respect to physical content) and pragmatism (with respect to the "length" gauge structure) in quantum mechanics, Weyl attempted to find conceptually deeper ground for "purely infinitesimally" metrical structures taking up and generalizing the intentions of Klein's Erlanger Programm and the, by then classical, problem of space in the sense of Helmholtz, Lie, and Klein. In this approach the original fixation of the gauge concept to a geometrical measurement of length was already loosened and abstracted by the emphasis on group theoretic aspects of the "purely infinitesimal" congruence structures (Weyl: general concept of metric). At the outset of his analysis, Weyl had to admit a rather general symmetry group G operating

[68] Compare (Sigurdsson 1996), (Stöltzner 2002).

as point symmetries in the infinitesimal neighbourhoods, which he called "rotations" by reference to tradition and convention. In order to describe infinitesimal transfer properly, the rotational symmetries had to be extended to a larger group H of similarities, by some additional component H/G of internal symmetries of the infinitesimal congruence structure. The gauge connection had values in H/G (or, more precisely, its Lie-algebra) and was inseparably linked to the affine connection characterizing the respective "parallel transfer" (which, in modern terminology, was assumed to be reducible to H). As a result of his analysis this internal symmetry part turned out to be quite simple, $H/G \cong \mathbb{R}^+$, and therefore also the extension that had to be considered: $H \cong G \times \mathbb{R}^+$.

In his work on the analysis of the problem of space, Weyl discussed the group theoretic aspect explicitly and in great length only for the rotational part, G, of the structure. The similarities (H) and the specific contribution of the internal symmetries arising purely geometrically from similarity considerations (H/G) had been discussed by him in his earlier arguments for his approach to the original length gauge idea much more explicitly.[69] The extension of the point-symmetries G to the structure group H of infinitesimal congruences was the consequence of a complex set of arguments, given "a priori" as a result of the analysis of minimal conceptual necessities for a geometry, living up to the purely infinitesimal standards, and some additional "synthetical" postulates. At a closer look, these considerations were only *presented* in an a-priori *form*, as much as possible. In fact, they had been constituted in an intriguing interplay of mathematical, physical, and philosophical considerations, which Weyl explained quite openly.

The maturation of quantum physics and the first attempts to adapt the original length gauge to the phase of wave functions, either of Klein-Gordon (complex scalar field) or of Dirac type (spinor field), convinced Weyl that the whole context and approach to gauge concepts in physics had to be considered with a much stronger a-posteriori component than he would have liked to admit in the early 1920s. In his contributions to the modified gauge concept in quantum mechanics, he emphasized that the new approach was informed by the "huge treasury of spectroscopic experience" and was adapted to it. Now it appeared to him safer to anchor his conceptual contributions to physics in experience, rather than to base it on an a-priori argumentation. In this sense, about the middle of the 1920s we find an *empirical turn* of our author, richly nurtured by his scientific experiences, clearly reflected,

[69] This may have contributed to the later re-interpretations of Weyl's analysis of the PoS, either neglecting the proper gauge aspect in the Weylian sense by identifying the roles of G and H (Kobayashi/Nomizu 1963), or disemphasizing the group theoretical aspect by a Finsler metric type of reading (Coleman/Korté 2001), cf. footnote (32).

and beautifully expressed in his publications. In earlier years he had been so strongly immersed in the idealist tradition of German philosophical and cultural thought that, even when this turn of his philosophical outlook commenced at the beginning of the 1920s, his approach to the problem of matter still reflected a kind of "idealist materialism" as Skúli Sigurdsson has put it, reflecting Weyl's ambivalence and his difficulties of change with sympathy and some friendly irony (Sigurdsson 2001, 22).

At the end of the 1920s this "materialism" had taken on clearer shape by an empirically backed realism of the symbolic constructions involved. As a result, Weyl's style of writing changed. Whereas until the early 1920s many of his important articles combined considerations of mathematics, physics, and philosophy in his peculiar style, after 1926 we find a much clearer separation between philosophical "reflection" (*Besinnung*), as he later said, and his writings in mathematics or mathematical physics. The reader will find the reflection of this change in the course of this article, which started with highly philosophical considerations and ends more deeply inside the corpus of knowledge in mathematical physics.[70]

Weyl's move towards stronger relations with the empirical practices of the sciences turned out to be quite successful and one might be tempted to consider it as only, or at least predominantly, a question of methodology (empirical turn). For Weyl, however, the ontological aspect of his new perspective appeared at least as much important as the methodological one. In his different publications on the subject he strongly and repeatedly emphasized that the electromagnetic field was no longer tied to gravitation in the new theory, but rather to "matter" (the Dirac field). This change appeared to him of utmost importance. Thus both aspects, the ontological ("materialist") and the methodological (empiricist) one, were closely related for Weyl and complemented each other.

Weyl pursued the analysis of the problem of space in the early 1920s, at the beginning of a phase of deep reorientation in mathematical physics. At that time he addressed the question as "it poses itself to the mathematician" in a relatively a-priori manner and had to exclude the discussion of how it relates to the "problem of matter" and the "problem of continuum of 3 or 4 dimensions". These questions had to be left open for future research. At the end of the decade, he could give first hints, how the relation to the matter problem might be approached, although a solution of the problem was still far away. In any case, some central aspects of the mathematical structure elaborated in the analysis of the space problem could be saved;

[70] I owe this observation on the shift in style of this paper and its Weylian background a remark of Skúli Sigurdsson.

they reappeared in the first step towards an understanding of the problem of matter in the sense of quantum physics, in a slightly modified form.

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