Local spinor structures in V. Fock's and H. Weyl's work on the Dirac equation (1929)

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Abstract

In early 1929, V. Fock (initially in collaboration with D. Iwanenko) and H. Weyl developed independently from each other a general relativistic generalization of the Dirac equation. In the core, they arrived at the same theory by the introduction of a local (topologically trivial) spinor structures and a lifting of the Levi-Civita connection of underlying spacetime. They both observed, in slightly different settings, a characteristic underdetermination of the spin connection by a complex phase factor, which gave the symbolical possibility for a reformulation of Weyl's old (1918) idea to characterize the electromagnetic potential by a differential form transforming as a gauge field. Weyl and Fock realized the common mathematical core of their respective approaches in summer 1929, but insisted on differences in perspective. An interesting difference was discussed by Weyl in his Rouse Ball lecture in 1930,. He contrasted the new type of unification strongly to the earlier geometrically unified field theories (including his own). He was quite explicit that he now considered his earlier ideas on geometrization of "all of physics" as premature and declared that the new, more empirically based approach would have to go a long way before it could be considered as a true "geometrization" of matter structures.

Introduction

In the early 20th century the most important impact of mathematical physics on geometry came from relativity theory. Historical and philosophical questions of this interplay have been discussed at various occasions.[1] The rise of quantum physics brought about a second shift, philosophically, technically and conceptually much deeper, for the relationship of geometry to physics. It started in the late 1920s, gained momentum in the second half of the past century and began to dominate the image of knowledge for the deeper levels of physical geometry during its last two decades.[2] Other contributions to these conference proceedings are evidence for the actuality of this recent and ongoing shift in our understanding of physical geometry, which is far from completed and continues to be an open-ended and controversial project.[3]

An important turn in the relationship between relativity, quantum mechanics and field theory, which also sheds light on the nature and role of geometry in this conceptual complex, was initiated by Hermann Weyl and Vladimir Fock in early 1929. They both started to investigate (generalized) Dirac fields in the context of general relativity by the introduction of local spinor structures on Lorentz manifolds. This topic was taken up anew in the 1960s from a global point of view.[4]

[1] Among them (Boi 1992) (Gray 1999).

[2] For a first historical exploration see (Cao 1999, section V), in particular J. Stachel's introductory remarks.

[3] Cf. contributions of M. Atiyah and A. Connes to this volume.

[4] Tthe role of the Dirac operator for the interplay between differential geometry and topology in the last third of the century is being discussed in J.-P. Bourgignon's contribution to this volume.

Up to the end of the 1920s mathematical physicists had essentially two symbolic tools for the represention of physical fields at their disposal: vectors/tensors (including differential forms) and linear connections (mostly but not always affine), most important among them, of course, the Levi-Civita connection of general relativity (GRT). After 1918 H. Weyl tried to convince physicists and mathematicians for some time to use another type of connection (*length connection*) in combination with a conformal (class of) Lorentz metric in his first, strictly metrical gauge geometry.[5] Most physicists who considered Weyl's length connection at all referred to it as just another differential 1-form $\varphi = \sum \varphi_i dx^i$ with a peculiar, perhaps even strange, transformation behaviour.

In the early 1920s A. S. Eddington started to build his attempts towards a unified field theory of electromagnetism, gravitation and matter using general affine connections (not necessarily derived from a metric); and Einstein joined him for a while from 1923 onward. These activities were part of a broader move towards unified field theories (UFT's) with a first high tide in the 20s of the last century, which has been studied historically, among others, by Vladimir Vizigin (Vizgin 1994) and, more recently and in a different methodological approach, by Catherine Goldstein and Jim Ritter (Goldstein/Ritter 2000).[6] V. Vizgin presents the relationship of UFT and quantum physics (QP) as one of competing research programs mutually influencing each other. The introduction of local spinor structures by Fock and Weyl in 1929 is a beautiful example for his case. Both, Weyl and Fock, were struck by the early successes of the Dirac equation for the explanation of the motion of the electron and attempted an integration of GRT and the Dirac field. In such an attempt they were not alone. Other authors, like Wiener and Vallarta, attempted a similar integration along different lines, building upon Einstein's recent theory of "distant parallelism". They attempted to adapt the Dirac field to a framework of classical UFT's that soon turned out to be too restrictive.

Weyl and Fock, the latter after an initial phase of sympathizing with distant parallelism, pursued an approach of a covariant differentation of spinor fields derived from the underlying Levi-Civita connection, in contrast to the distant parallelism program. Both realized that, in doing so, an underdetermination of the ensuing spinor connection led naturally to an additional $U(1)$-symmetry. They used the latter for a representation of the electromagnetic field comparable to, although slightly different from, Weyl's earlier approach using a length connection. Thus they arrived at a geometric-analytical structure in which the actual knowledge of gravitation, electromagnetism and the basics of the quantum theory of the moving electron could be represented in an integrated form.[7] Both authors posed the question how geometry might be brought into agreement with quantum physical knowledge of their time. They arrived at strongly diverging evaluations as to what they had achieved in this respect and what geometrization of quantum physics might mean at all (last section).

Before I discuss Weyl's and Fock's respective approaches and differences with

[5] This approach is discussed, from a more recent point of view, by P. Cartier's in his contribution to this volume.

[6] Another high tide, in a different historical/scientific context and with changed conceptual/symbolical approaches, started in the 1970s. It has not yet found the detailed and critical historical investigation it deserves, although work has started (Cao 1997), (Morrison 1995), (Galison 1995), (O'Raifeartaigh/Straumann 2000).

[7] For a discussion of Weyl's 1929 work on gravitation and the electron see also (Straumann 2001).

respect to "quantum geometry", I want to sketch the background of common knowledge from which they started and outline their 1929 work.

Setting the stage in the later 1920's for Weyl and Fock

During the 1920s the constitutive conditions for the mathematization of geometry and matter changed deeply. In the middle of the decade (1925/26) the "new" quantum mechanics took shape, with its different versions, in central aspects compatible, although at least historically and conceptually not completely equivalent, put forward by Heisenberg/Born/Pauli, Schrödinger and Dirac.[8] Continuing this turn in late 1926, W. Heisenberg started to investigate the symmetry of atomic electrons using surprisingly old-fashioned mathematics, Serret's *Algèbre supérieure* from 1879. But already in the following year the two young Hungarians, E. Wigner and J. von Neumann, working in Berlin and Göttingen, applied group representation methods for this goal, as did H. Weyl in a lecture course devoted to this subject in the winter semester 1927/28 at the ETH Zürich.[9]

Still in 1926, W. Pauli attempted to characterize the new hypothetical electron "spin" in terms of quantum mechanical symbolism and introduced a pair of "wave" functions $(\psi_1(x), \psi_2(x))$, $x \in \mathbb{R}^3$, and Hermitian matrices, which later were given his name,

$$\sigma_1 = \begin{pmatrix} & 1 \\ 1 & \end{pmatrix}, \quad \sigma_2 = \begin{pmatrix} & -i \\ i & \end{pmatrix}, \quad \sigma_3 = \begin{pmatrix} 1 & \\ & -1 \end{pmatrix}.$$

Pauli proposed to represent the electron spin by the three component operator

$$\sigma = \frac{1}{2}\hbar(\sigma_1, \sigma_2, \sigma_3), \quad \hbar = \frac{h}{2\pi}.$$

Like Heisenberg, Pauli did not think in terms of group representations at that time; he constructed his two-valued wave functions from the Klein-Sommerfeld theory of the spinning top and the complex representation of rotations by Cayley-angles. That was an ingenious and mathematically momentous move towards what little later turned into (Euclidean or relativistic) spinors, although Pauli's hopes to come to a direct explanation of the fine structure of the hydrogen spectrum were not fullfilled at the time.[10] Even the first attempts in 1926 and 1927 to take relativistic effects into account, spinless (Klein-Gordon) or with spin (Darwin), were no more successful in this respect.[11] The situation changed completely in January and February 1928 when Dirac proposed to use 4-component complex-valued "wave" functions $\psi(x) = (\psi_1(x), \psi_2(x), \psi_3(x), \psi_4(x))$ (x in Minkowski-space $\mathbb{M}$) in two successive publications [12]. The ψ- function had to obey the (*Dirac*) equation

$$i\hbar \sum_{\alpha=0}^{3} \gamma^\alpha \frac{\partial}{\partial x^\alpha}\psi = m_0 c \psi \qquad (1)$$

[8] For a general picture see (Rechenberg 1995), (Pais 1986) and (Hendry 1984).
[9] (Mehra/Rechenberg 2000, 488ff.).
[10] (Pais 1986, 289ff.).
[11] (Kragh 1981, 44ff.), (Mehra/Rechenberg 2000, 280ff.).
[12] (Dirac 1928).

with (Dirac) matrices γ^μ satisfying the relations $\gamma^j\gamma^k + \gamma^k\gamma^j = \delta_{jk}$ and expressible, e.g., in the form

$$\gamma^0 = \begin{pmatrix} \mathbb{I} & \\ & -\mathbb{I} \end{pmatrix}, \quad \gamma^j = \begin{pmatrix} & \sigma_j \\ -\sigma_j & \end{pmatrix}, \quad 1 \leq j \leq 3,$$

with (2×2)-unity matrix $\mathbb{I}$ and Pauli matrices σ_j.[13]

Thus things looked quite different for Weyl in the late 1920s from what they had been at the end of his first phase of activity in mathematical physics early in the decade. Already in late 1920 he had lost confidence in theories of matter by unification of classical fields according to the Hilbert/Mie approach, including his own one built upon the length gauge.[14] While expecting new insights from the rising quantum mechanics, he concentrated on more conceptual or purely mathematical research fields: the analysis of the space problem about 1922/23 and representation theory of Lie groups during the years 1924 to 1926.[15] Weyl kept well informed on the ongoing development during the crucial years for quantum mechanics in the middle of the decade, drawing upon his close scientific relationship with Pauli (1924 – 1928 at Hamburg university), dating from their cooperation on unified geometrical field theories in the early 1920s. Moreover he had contacts with E. Schrödinger who taught at the university in Zürich between 1921 and 1927. He appararently felt challenged to contribute to the conceptual and mathematical clarification of the framework of the "new" quantum mechanics, in particular from the point of view of *unitary geometry* (Weyl's title for the first part of his lecture in 1927/28) and the use of *representation theory* of (Euclidean) rotations and permutation for atomic line spectra, Pauli's non-relativistic spin, and mechanism of molecular binding forces.

In winter 1927/28 Weyl had a chance to take up the challenge. Both theoretical physicists working at Zürich had accepted outside calls and had left: P. Debye changed from the ETH to the university Leipzig and E. Schrödinger from the local university to Berlin. Weyl decided to change the subject of a lecture course initially planned and announced on (pure) group theory to one on *Gruppentheorie und Quantenmechanik (Theory of Groups and Quantum Mechanics.* Notes were taken by his assistant F. Bohnenblust and published, after revision and extension, in August 1928 as a book (Weyl 1928), which in the sequel will be abbreviated as *GQM.* In this second book on mathematical physics, Weyl was more cautious than he was in *Raum - Zeit - Materie* (Weyl 1918) in his expectations of how his contributions might be received by the workers in the field. In the preface to the new book, he remarked:

> It is the second time that I dare to turn up with a book which belongs only partly to my own speciality, mathematics, and partly to physics. ...I just cannot avoid to play the role of a messenger (often undesired, as I have experienced sufficiently clearly) in this drama of mathematics and physics - fertilizing each other in the dark, although from face to face preferring not to recognize and even renouncing each other. (Weyl 1928, Vf., my translation, E.S.)[16]

[13] Dirac used a slightly different presentation of the matrices than the one given in the text. For a detailed investigation of Dirac's work see (Kragh 1981) or (Kragh 1990).

[14] See (Sigurdsson 1991, chap. V) or (Scholz 2001*a*).

[15] (Hawkins 2000, Part IV).

[16] Not translated in the English edition by H.P.Robertson.

Weyl was not alone in this "role of a messenger" as he realized during the preparation of the lecture notes for publication. Other authors started in 1927 and 1928 to use group representations in quantum mechanics, among them, most importantly from the mathematical point of view, J. von Neumann and E. Wigner. Also on the physical side, things changed rapidly. Dirac published his papers on the relativistic theory of the electron at the end of the winter semester, in January and February 1928. The impact was enormous and were sufficient reason for Weyl to add to his book a whole new passage on Dirac's equation (Weyl 1928, 1st ed., §§39–41).

Another remark in his lectures of 1927/28 leads directly to our geometrical topic.[17] Weyl's gauge idea from 1918, originally linked to a length calibration and " infinitesimal length transport" characterized by a 1-form $\varphi = \sum \varphi_i dx^i$ was rephrased in a quantum mechanical setting by E. Schrödinger, still in a length calibration interpretation (Schrödinger 1922), and after the rise of the "new" quantum mechanics by V. Fock and F. London in the context of Kaluza-Klein theory of quantum mechanics (Fock 1926, London 1927). The core of their respective arguments dealt with "gauging" a wave function $\psi(x)$ by a point-dependent phase factor $e^{i\lambda(x)}$ (with $\lambda \in \mathbb{R}$) to $\tilde{\psi}(x) = e^{i\lambda(x)}\psi(x)$. The differential of the purely imaginary phase factor, used in Weyl's 1918 theory to "gauge-transform" length connections, could now be used to transform electromagnetic potentials φ_j a little more convincingly

Weyl endorsed this recontextualization of his original gauge idea when he discussed the Schrödinger equation in 1927/28. Probably he had read only the papers by Schrödinger and London, which he cited, not Fock's; but London was aware of and built upon (Fock 1926).[18] He remarked that the Schrödinger equation

$$i\hbar \frac{\partial \psi}{\partial t} = H\psi \,, \tag{2}$$

containing the Hamilton operator

$$H = \frac{1}{2m} \sum p_j^2 + V(x) \tag{3}$$

with potential V and momentum operator $p_j = \frac{\hbar}{i}\frac{\partial}{\partial x^j}$ for a chargeless particle, is adequately modified by using the covariant derivative ∂_φ with respect to a potential connection $\varphi = (\varphi_j)$, if a charged particle in field of potential φ is considered. Then the momentum operator becomes

$$p_j = \frac{\hbar}{i}\left(\frac{\partial}{\partial x^j} + \frac{ie}{\hbar}\varphi_j\right), \quad i = \sqrt{-1}\,, \tag{4}$$

and the Hamiltonian of the Schrödinger theory for the motion of a particle of charge e in an electromagnetic field of potential φ results. Weyl observed that now:

> The field laws satisfied by the potentials ψ and φ of the material and the electromagnetic waves are invariant under simultaneous substitution of
>
> $$\psi \text{ by } e^{i\lambda}\psi, \quad \varphi_\alpha \text{ by } \varphi_\alpha - \frac{\hbar}{e}\frac{\partial \lambda}{\partial x_\alpha}$$

[17] This passage was published only in the first edition of (Weyl 1928), no longer in the second edition of 1931 and the English translation.

[18] (Vizgin 1994, 293).

. . . (Weyl 1928, 1st ed. 87f.)

He commented that this "principle of gauge invariance" was quite analogous to the one he had postulated in 1918 "by speculative reasons to gain a unified theory of gravitation and electromagnetism" and continued:

> . . . But now I believe that the gauge invariance does not couple electricity and gravitation, but rather *electricity and matter* in the mode presented here. How gravitation according to the general theory of relativity can be included is still uncertain. (Weyl 1928, 1st ed. 88)

Thus Weyl proposed more than a technical adaptation of his old gauge idea to the new framework of QP. In classical UFT the goal was to unify force fields as such in a coherently geometrized, often highly speculative, "a priori" manner, and to derive matter structures from them; here Weyl indicated a new paradigm centering around the search for conceptual and mathematical structures which link forces to matter fields, without reduction of one to the other and with strong input from experimental evidence.

Classical UFT was, of course, still quite alive at that time. In 1928 A. Einstein turned towards "distant parallelism" for his latest approach to unification. He assumed or postulated, that, in addition to the Levi-Civita connection of the Lorentz metric, an integrable, curvature free, orthogonal connection ${\Delta^i}_{jk}$ with torsion (${\Delta^i}_{jk} = -{\Delta^i}_{kj}$) is given, which he usually described by a globally parallel system of orthogonal frames. With respect to such an additional structure it was meaningful to consider constant, i.e. point independent, rotations. Although Einstein did not intend so, his additional structure allowed a formulation of the Dirac equation in the framework of GRT with distant parallelism and stimulated other physicists to do so.

V. Fock and his Leningrad colleague D. Ivanenko started to explore such an approach in a joint paper submitted to *Zeitschrift für Physik* in March 1929.[19] They hoped to find some "bridge" between gravitation and quantum theory.[20] They started with a formal construct of a linear expression in the Dirac matrices, $ds = \sum_j \gamma_j dx^j$, which they tried to interpret as a matrix valued metric form of some new "linear quantum geometry". From that point of view they hoped to find a kinship between Einstein's field of distant parallelism and the new "linear geometry" (Fock/Ivanenko 1929*c*, 801). During the following months Ivanenko and Fock realized that the linear structure of the new geometry could better be understood as a covariant derivative of the 4-component complex wave functions which they called "semi-vectors", the later spinors.[21] Still they called the geometry they were heading for "géométrie quantique linéaire" (Fock/Ivanenko 1929*b*, Fock 1929*a*).[22] V. Fock continued to explore the terrain and realized soon that the new covariant derivation of spinors had a much closer kinship with a Weylian phase gauge than with Einstein's distant parallelism. He presented his findings in two articles (no longer co-authored by Ivanenko) to *Physikalische Zeitschrift* and *Comptes Rendus* (Fock 1929*a*, Fock 1929*b*).[23]

[19] March 25, 1929.

[20] For the group of young relativists in Leningrad see (Gorelik/Vizgin 1987), for the early involvement in QP (Frenkel/Gorelik 1994). More on Fock in (Gorelik 1993).

[21] The terminology of "semi-vectors" was proposed by L. Landau.

[22] (Fock/Ivanenko 1929*b*) was submitted May 22, 1929.

[23] (Fock 1929*a*) dated June 24, (Fock 1929*b*) July 5, 1929.

He thus arrived at a theory combining gravitation, Dirac field, and electromagnetism, which overlapped in large parts with what Weyl achieved in early 1929 when he continued research along the lines indicated in *GQM*.

Weyl's and Fock's local spinor structure

Weyl left Zürich in September 1928 for Bologna (ICM) and Princeton where he spent a year as reseach professor in mathematical physics.[24] There he could continue, among other things, his research on the Dirac equation in general relativity. The approach of distant parallelism did not appear at all convincing to him. He considered it to be a completely "artificial" device and looked for a combined structure of GR and the Dirac equation from the point of view of "purely infinitesimal" geometry, which now had to be refined and extended in the light of new physical knowledge. In February 1929 Weyl submitted a first sketch of methods and results under the title *Gravitation and the electron* to the *Proceedings of the National Academy of Sciences* (Weyl 1929*a*). Three months later he delivered a more extended exposition to *Physikalische Zeitschrift* (Weyl 1929*b*).[25] At that time he could not know of Fock's parallel work, nor did he know of it when he wrote his third paper on the topic in early summer (Weyl 1929*c*).

Fock, on the other hand, got to know of Weyl's new researches (Weyl 1929*a*) only after he finished his own article for *Physikalische Zeitschrift.* He accepted the common mathematical core of their respective approaches, but emphasized the differences from the physical point of view in a postscript (Fock 1929*b*, 276f.). Weyl apparently got to know Fock's work in summer 1929 and was so fond of the common features of their work that he considered it as establishing essentially one and the same theory. He thus referred to it in the preface to the second edition of *GQM* as the "general relativistic formulation of the quantum laws, which have been developped by Mr. V. Fock and the author [Weyl himself]" (Weyl 1928, vii, 2nd edition 1930).[26]

Fock and Weyl applied the method of (pseudo-) orthogonal moving frames in Lorentzian space-time M, i.e. they supposed an

$$\text{orthonormal frame of tangent vectors (ONF):} \quad e(\alpha, x), \;\; 0 \leq \alpha \leq 3,$$

in each point $P \in M$ with coordinates $x = (x^0, \ldots, x^3)$ (depending differentiably on the point). Tangent vectors v at $x \in M$ could thus be represented in components referring to the coordinate basis (ξ^j), or in components with respect to the ONF ($\xi(\alpha)$ in Weyl's notation):

$$v = \sum_{j=0}^{3} \xi^j \frac{\partial}{\partial x^j} = \sum_{\alpha=0}^{3} \xi(\alpha) e(\alpha, x)\,. \tag{5}$$

Besides (differentiable) change of coordinates, changes of the ONF from $e(\alpha, x)$ to $e'(\beta, x)$ $(0 \leq \alpha, \beta \leq 3)$ had also to be taken into account. The latter were

[24] (Frei 1992, 107ff.).

[25] Submitted, May 8, 1929.

[26] Weyl saw no chance to give an exposition of this theory in the book *GQM.* In the second edition he rephrased, however, his discussion of the representation of the Lorentz group and of the special relativistic Dirac equation, in particular the decomposition of the 4-dimensional spinors into irreducible 2-dimensional representations.

given by point-dependent Lorentz-rotations $\vartheta(x)$ represented by matrices (as the ONF's were given in components with respect to a local coordinate system):

$$\vartheta(x) = (\vartheta^{\alpha}_{\beta}) \in SO(1,3) \, .$$

The parallel transport of a frame by the Levi-Civita connection $\Gamma^{i}{}_{jk}$ could be expressed in terms of "infinitesimal rotations" o depending linearly on infinitesimal displacements $dx = (dx^j)$ in space-time

$$o^{\alpha}_{\beta} = \sum_{k} \omega^{\alpha}_{\beta k} dx^k \, . \tag{6}$$

In more recent terminology: By means of the ONF's Fock and Weyl reduced the group of the affine connection $\Gamma^{i}{}_{jk}$ to the orthogonal group, and characterized parallel transport in M by the resulting orthogonal connection $\omega^{\alpha}_{\beta k}$.

In the late 1920s this was standard knowledge. The idea of ONFs had already been introduced by Ricci and Levi-Civita in 1900; it had been worked out by differential geometers in the 1920s, most prominent among them E. Cartan (in lectures from 1926/27 published as (Cartan 1928)), J.A. Schouten, R. Weitzenböck, L.P. Eisenhart (in monographs 1926 and 1927). Moreover, orthonormal frames played a central role in Einstein's theory of "distant parallelism", from which Fock (and Ivanenko) took the idea.[27] Fock (still in his cooperation with Ivanenko) and Weyl realized that reduction of the Levi-Civita connection to the orthogonal group by the ONF method allowed one to introduce covariant differentiation of spinors.[28] Weyl explained clearly that the orthogonal reduction of the connection was necessary in this context, because "Dirac's quantity" ψ

> ...corresponds to a representation of the orthogonal group which cannot be extended to the group of all linear transformations. The tensor calculus is consequently an unusuable instrument for considerations involving ψ. (Weyl 1929*a*, 219)

For Weyl, this group-theoretic consideration was of great importance. In the early 1920s he had analyzed the role of tensors from the point of view of group represensions and found out that all irreducible representations of $GL(n, \mathbb{R})$ with a specified permutation symmetry can be characterized by tensors over $\mathbb{R}^n$.[29] In a language closer to physicists he explained more in detail:

> Vectors and [tensors] are so constructed that the law which defines the transformation of their components from one Cartesian set of axes [ONF] to another can be extended to the most general linear transformation, to an affine set of axes. That is not the case for [the] quantity ψ, however; this kind of quantity belongs to a representation of the rotation group which cannot be extended to the affine group. (Weyl 1929*a*, 234)

He admitted that the ONF method used by him resembled Einstein's latest appproach in formal aspects, but insisted that this was only a superficial coincidence.

[27] In his main article Fock referred, however, also to (Eisenhart 1926) (Fock 1929*b*, 263, footnote).

[28] For simplicity, I will no longer always add in the sequel Ivanenko to Fock, even in case that concepts appeared already in their joint work.

[29] See (Hawkins 2000, 440ff.).

But here there is no talk of "distant parallelism"; there is no indication that Nature has availed herself of such an artificial geometry. I am convinced that if there is a physical content in Einstein's latest formal development it must come to light in the present connection.

And he added a reason that went beyond purely mathematical considerations:

It seems to me that it is now hopeless to seek a unification of gravitation and electricity without taking material waves into account. (Weyl 1929*a*, 219)

Dirac had shown that the *equation* of the free electron expressed in ψ is invariant under Lorentz transformations without asking for the underlying reprentation of the Lorentz group,[30] but other authors did so immediately later. F. Möglich calculated the complex 4×4-matrices for the "Dirac-quantity" corresponding to a given Lorentz transformation (Möglich 1928), and J. von Neumann discussed the resulting relation

$$\begin{array}{rrcl} \Lambda: & SO^+(1,3) & \longrightarrow & GL(4,\mathbb{C}) \\ & o & \longmapsto & \Lambda(o) \end{array}$$

as a "(multivalued!) 4-dimensional representation of the Lorentz group" (von Neumann 1929, 867). Von Neumann emphasized, very much like Weyl, that something essentially new was introduced into mathematical physics:

The case of a quantity of 4 components which is no 4-vector has never occurred in relativity theory, the Dirac ψ-vector is the first example of this kind. (ibid.)[31]

Thus, immediately after Dirac's publications on the "spinning" electron, theoretically minded authors realized that the new "Dirac quantity" (Weyl), the "ψ-vector" (von Neumann), or the "semi-vector" (Fock, Landau e.a.) was more than just another technical device, but led to a *conceptual innovation* for mathematical physics. Change of reference systems in special relativity ("Cartesian systems of axes" as Weyl would say) by a Lorentz transformation had to be represented by $\Lambda(o)$ in the ψ-space in a way that could not be extended to general linear transformations and thus could not, in a straight-forward manner, be transferred to general relativity.

At the time when Fock and Weyl approached the problem of a general relativistic formulation of the Dirac equation, the young algebraist B.L. van der Waerden established an algebraic calculus for all possible quantities appearing in any representation of the Lorentz group. His contribution was meant as a sort of service to the physicists, stimulated by a question of P. Ehrenfest who had posed the question to design such an algebraic calculus. Van der Waerden picked up the terminology"spinor" from Ehrenfest and gave him a broad audience (van der Waerden 1929, 100). In this work he built upon Weyl's exposition of the representation theory of the Lorentz group in *GQM*.

Distinct from other work about 1929, Fock and Weyl admitted point-dependent (Lorentz-) rotations of ONF in space-time, $o(x) \in SO^+(1,3)$, differentiably depending on x, inducing point-dependent transformations $\Lambda(o(x))$ of the spinor

[30] (Dirac 1928, 310ff.), discussed in (Kragh 1981, 57f.).

[31] Translation E.S.

space. While Fock immediately headed for the covariant derivation of a spinor ("semi-vector"), Weyl made the underlying invariance idea explicit. He stated for the "laws" that would be characterized by an action principle and by differential equations derived from it:

> The laws shall remain invariant when the axes in the various points P are subjected to arbitrary and independent rotations. (Weyl 1929*a*, 219)

Variational equations were thus required to be invariant under simultaneous transformations

— of vectors/tensors by Lorentz rotations $o(x)$

— and of the spinors under $\Lambda(o(x))$.

In this way, Weyl and Fock introduced and started to study a *local spinor structure* on the underlying space-time manifold M. Both authors *used* local change of coordinates in the spinor space $\Lambda(o(x))$ (the change of trivialization in later language) accompanying a change of ONF's $o(x)$, and Weyl *discussed* its conceptual role quite clearly, although of course not yet applying the terminology of local bundles trivialization.

Weyl did not mention, however, that for a globalization of the procedure the topology of the M might play a role. Such questions of global existence of an ONF (presupposing parallelizability of M), were posed and answered only in the 1930s by the young generation of topologists (E. Stiefel, H. Whitney), apparently stimulated by Einstein's use of (local) "distant parallelism", not by local spinor structures of Fock and Weyl. Global questions for spinor structures were taken up still another generation later and became a research topic only in the 1960s.[32] Weyl, in his 1929 articles, did not even indicate that there might be an open and challenging question in the relationship between spinor structures on M and its topology.

Of immediate interest, for our authors, was the introduction of an "infinitesimal displacement of semi-vectors" (Fock) or the "invariant change $\delta\psi$ on going from the point P to a neighbouring point P'" (Weyl 1929*a*, 221), i.e. in modern terminology the introduction of a connection and parallel transport in a local spinor structure, lifted from the Levi-Civita connection in the underlying Lorentz manifold. On this point the two authors applied slightly different approaches; Weyl's approach was, as one may expect, more conceptual and Fock's more calculational.

Considering two (infinitesimally) "neighbouring" points P, P' with coordinates $x = (x^0, \ldots, x^3)$ and $x' = (x'^0, \ldots, x'^3)$ differing by an "infinitesimal displacement" $dx = (dx^0, \ldots, dx^3)$ Weyl argued that parallel displacement of a frame $\{e(\alpha, P)\}$ from P to P' leads to an infinitesimally rotated frame $\{e'(\alpha, P')\}$ described by an infinitesimal rotation $o = \omega(dx)$ with respect to the ONF-system $\{e(\alpha, P')\}$ in P', in slightly metaphorical notation

$$\{e'(\alpha, P')\} - \{e(\alpha, P)\} = \omega \cdot \{e(\alpha, P')\} \tag{7}$$

(compare equation (6)). The representation Λ induces an infinitesimal tranformation dE (Weyl's notation) in $gl(n, \mathbb{C})$, which depends linearly on dx

$$dE = \Lambda(o) = \Lambda\omega(dx)$$

[32] See P. Bourgignon's contribution, this volume.

The "differential $\psi(P') - \psi(P)$", i.e. $d\psi = \sum_j \frac{\partial\psi}{\partial x^j} dx^j$, had to be modified accordingly to give the *covariant differential* $\delta\psi$ of ψ (Weyl 1929*a*, 221) (Weyl 1929*b*, 253f):

$$\delta\psi = d\psi + dE \cdot \psi \,. \tag{8}$$

This conceptually clear description of the covariant differential, had the advantage that in Weyl's discussion Λ could stand for *any representation of the Lorentz group*, not just Dirac's original 4-dimensional one.

Weyl realized of course, as did von Neumann in 1928, that Dirac's representation can be decomposed into two irreducible representations ρ and ρ^+ (which generate all finite dimensional representations of $SL(2,\mathbb{C})$ by tensor products and direct sums). He gave a beautiful geometrical description of the 2-valued inverse of the covering map[33]

$$SL(2,\mathbb{C}) \longrightarrow SO^+(1,3)$$

and took ρ as the identical representation of $SL(2,\mathbb{C})$ and $\rho^+ = {}^t\bar{\rho}$ its adjoint. Then he could write Dirac's representation (up to a permutation of ψ-coordinates) as

$$\Lambda \cong \rho \oplus \rho^+ , \tag{9}$$

and wrote the 4-spinors (after a linear transformation) as $(\psi_1^+, \psi_2^+, \psi_1^-, \psi_2^-)$.

Fock analyzed the condition (incorporated by Dirac into his new symbolic game) that the ψ-functions get their physical meaning from the condition that the evaluation map

$$\psi \longmapsto (a^0, \ldots, a^3) \quad \text{with} \quad a^j = <\gamma^j\psi, \psi> \,, \quad 0 \leq j \leq 3,$$

leads to a vector (a^j). Therefore it was natural to postulate that "changes of a semi-vector ψ under an infinitesimal parallel displacement" are compatible with parallel displacement of vectors. This allowed him to compute matrices $C_l \in GL(4,\mathbb{C})$ which describe such compatible "infinitesimal changes of semi-vectors" (the parallel displacement in the local spinor structure). In his own representation $\tilde{\gamma}^j$ of the Dirac matrices Fock derived the condition

$$C_l = \frac{1}{4} \sum_{j,k,l} \tilde{\gamma}_j \tilde{\gamma}^k \omega^j{}_{kl} + i\phi_l \,, \quad \text{with} \quad \tilde{\gamma}_j = \sum_k \epsilon_{jk} \tilde{\gamma}^k , \tag{10}$$

$\epsilon = \mathrm{diag}(1,-1,-1,-1)$ the signature diagonal matrix, ω the orthogonally reduced Levi-Civita connection, and ϕ_l any matrix "proportional" to unity

$$\phi_l = f_l \mathbb{1} \qquad \text{with } f_l \text{ real-valued function} \tag{11}$$

(Fock 1929*b*, 264f.). Fock thus arrrived at an explicit form of Weyl's infinitesimal spinor transformation dE, at least for the case of the (original) Dirac representation,

$$dE \cdot \psi = \sum_l C_l dx^l \psi \,.$$

On that basis Fock easily expressed covariant differentiation of a spinor with respect to a vector direction of a the frame $\{e(\alpha)\}$

$$D'_\alpha \psi = \frac{\partial}{\partial e(\alpha)} \psi - C_\alpha \psi \tag{12}$$

[33] (Weyl 1929*b*, 247f.).

or a coordinate direction x^j

$$D_j\psi = \frac{\partial}{\partial x^j}\psi - \tilde{C}_j\psi \tag{13}$$

where $\tilde{C}_j$ are slightly different matrices calculated from the C_j's. For Weyl, both versions of covariant differentiation could be derived from his "covariant differential" $\delta\psi$ of equation (8).

An additional $U(1)$-gauge

Up to this point I omitted an important observation made by both authors, which led back to Weyl's gauge idea. The "lifting" of the Levi-Civita connection to the spinor structure was not uniquely determined, even if we neglect the double valuedness of the $SL(2,\mathbb{C})$ covering of the Lorentz group.

Fock's calculation of the the matrices (equation (10)) showed that the compatibility condition determines the C_l only up to addition of purely imaginary matrices $if_l\mathbb{1}$. Covariant differentiation of spinors (equations (12), (13)) is then affected by an additive term $-if_\alpha\psi$. In a kind of *déja vu* Fock realized that the additional term could be perceived as derived from a phase-gauge factor of the ψ-field:

> The appearance of the Weylian differential form in the law of parallel displacement stands in close relation to the fact remarked by the author [Fock] and also by Weyl (...) that the addition of a gradient to the 4-potential corresponds to a multiplication of the ψ-function by a factor of absolute value 1. (Fock 1929*b*, 266)

On that basis, Fock formulated the Dirac equation for the general relativistic electron by covariant derivation in his local spinor structure, including a Weylian $U(1)$-gauge term as an integrated part of the covariant derivation (13) (ibid.)

$$F\psi = 0 \quad \text{with} \quad F = i\hbar\sum_{j=0}^{3}\gamma^j D_j + mc\gamma_4\,. \tag{14}$$

Weyl discussed the question similarly, although slightly more general. He argued that any semantically relevant information derived from a spinor field had to be invariant under $U(1)$-symmetries of the spinor representation, because the $SO^+(1,3)$-covariants used to represent physical quantities were given by Hermitian forms $<\psi, A\psi>$ and thus were invariant under multiplication by a phase factor $e^{i\lambda}$ of ψ. Therefore the spinor connection ("the infinitesimal linear transformation dE of the ψ") is determined by the "infinitesimal rotations" ω of the reduced Levi-Civita connection only up to "a purely imaginary multiple $i\cdot df$ of the unit matrix". In other words, with dE

$$dE' = dE + idf\,\mathbb{1}$$

is also compatible with the underlying metric of GRT. Weyl concluded:

> For the unique determination of the covariant differential $\delta\psi$ of ψ such a df for each line element $\vec{PP'} = (dx)$ starting from P is needed. (Weyl 1929*b*, 263)

The selection among the spinor connections compatible with the Levi-Civita connection could justly be considered as a "gauge", in strong analogy to the length gauge of 1918. Morover Weyl used, just like Fock, the possibility to express the Dirac equation of the electron in an electromagnetic field by means of covariant differentiation of spinors *including* a $U(1)$-gauge potential ("such a *df*").

For action functions applying to spinor fields he felt it legitimate to postulate:

> If one (...) substitutes
>
> $$\psi \text{ by } \quad e^{i\lambda} \cdot \psi \qquad f_p \text{ by } \quad f_p - \frac{\partial \lambda}{\partial x_p}$$
>
> with λ an arbitrary function of the position, gauge invariance necessarily holds, in the sense that the action principle remains invariant. (Weyl 1929*b*, 263)

From the point of view of infinitesimal symmetries, the new gauge structure resembled in certain features Weyl's study of the *Raumproblem* early in the 1920s. In the analysis of the space problem he had characterized "congruences" by a subgroup G of $SL(n, \mathbb{R})$, contained in a larger group H of "similarities", in which G was normal (in fact, H was the normalizer of G in $GL(n, \mathbb{R})$). One of his postulates was a uniqueness condition for an affine connection equivalent (in a certain sense) to a given linear connection in the larger group. In 1929 he again dealt with a pair of groups, now given by physical considerations, the smaller one being the Lorentz group or its universal convering, $G = SL(2, \mathbb{C})$, and the larger one was $H = SL(2, \mathbb{C}) \times U(1)$ in which G was normal by construction. Again a uniqueness condition for a connection, compatible to another given one, played a crucial role for the analysis. The uniqueness condition was now formulated "bottom up", i.e. from a given (Levi-Civita) connection in the smaller group to the larger one, and uniqueness of the (spinor) connection with respect to the larger group was achieved only by adding a connection in the quotient group $U(1)$ (respectively bundle, from the later point of view). In this sense there was a structural analogy considering group extensions for infinitesimal symmetries, although the methodology had changed considerably. In 1929 Weyl no longer tried to found his approach on a priori principles, but rather analyzed symbolic forms worked out ("constructed") by mathematical physicists in close communication with experimental knowledge of the rising quantum physics.

Weyl discussed how one could arrive at physical consequences from his approach. It would lead us too far to follow this line here.[34] I just want to mention that Weyl drew impressive consequences from the postulate of invariance of the action integral under infinitesimal symmetries of different kinds:

— infinitesimal rotations of the frames leads to symmetry of the energy-momentum tensor,

— infinitesimal coordinate translations leads to "quasi"-conservation of energy and momentum and in the case of special relativity by integration to invariance of rotational momentum (Weyl 1929*b*, 256ff.),[35]

[34] Cf. (Straumann 2001).

[35] Weyl spoke of "quasi-conservation" of energy-momentum t_p^q, because of a second term in

— infinitesimal $U(1)$ gauge transformations leads to conservation of charge (ibid., 264f.).

He hoped, morover, that his general relativistic approach to the Dirac equation, together with the separation of the spinor fields into components of irreducible representations ρ and ρ^+ might lead to a solution of the problem of *negative energies* in the original Dirac equation. In late 1929 Dirac proposed a solution to this problem by some imaginative ad-hoc arguments postulating the existence of positive electrons (positrons) appearing as constitutive parts of the solution of the original Dirac equation with non-vanishing mass term, and surprising "fluctuations" between positive and negative charge contributions to it. It turned out that neither the positive charge contributions could be separated nor the resulting "fluctuations" eliminated from the solution (Kragh 1990, 90ff.).

Weyl, for his part, attempted for a short while in 1929 to avoid such fluctuations by the proposal to study solutions of a modified Dirac equation in the irreducible components of the representation ρ and ρ^+ separately (Weyl spinors). He remarked, however, that in this equation no mass term could be included without losing gauge invariance (Weyl 1929*a*, 242). As a research strategy to overcome the problem he proposed to neglect at first, on the level of the spinor equation, the mass of the electron and to reconstruct it, in a second step of theory development, as an integral invariant that couples to gravitation.

> Be bold enough to leave the term involving mass entirely out of the field equations. But the integral of the total energy density over space yields an invariant, and at the same time constant, mass; *require of it that its value be an absolute constant of nature* m which cannot vary in value from case to case. This introduction of mass is born of the idea that the inertia of matter is due to its energy content. (Weyl 1929*a*, 243)

Such an approach made sense only in a joint theory of gravitation, quantum physics (in the sense of the modified Dirac equation) and electromagnetism. In his attempt for an integrated theory Weyl now pursued the concrete goal to contribute to the solution of the mass problem of the electron.

The proposal to start from a "massless" electron was rejected by physicists immediately. In the postscript to his article for the *Physikalische Zeitschrift* Fock argued strikingly (and presumably also convincingly for Weyl)[36] that the current of the Weyl-spinor field was lying on the light-cone. Thus there remained no realistic hope for a solution of the electron's mass problem along the line indicated by Weyl (Fock 1929*b*, 276f.). Similarly Pauli rejected Weyl's proposal to circumvent the mass problem for the electron, although from a conceptual point of view he found the new integration of the gauge idea into quantum physics

the differential equation derived from invariance under infinitesimal translations:

$$\frac{\partial t_p^q}{\partial x_q} + \frac{\partial e^q(\alpha)}{\partial x_p} t_q(\alpha) = 0$$

Literal conservation of energy and momentum holds only if the respective terms of the gravitational fields are added or, in special relativity, after specialization of the ONF's (Weyl 1929*b*, 257f.).

[36] In the 2nd edition for *GQM* Weyl no longer insisted on his 1929 proposal and supported Dirac's strategy to deal with the problem (Weyl 1928, 2nd. edition, 230, 233).

most convincing. He contributed essentially to its dissemination and survival in the physics community. Moreover he revived Weyl spinors in 1956 when he looked for an adequate mathematical representation of his newest hypothetical entity, the neutrino. This is a different and historically complicated story which cannot be dealt with here.[37]

Weyl indicated that field quantization was another problem that had to be solved before one might hope for an answer to the questions raised:

> Another difficulty which stands in the way of a comparison with experience is that the field equations must first be quantized before they can be applied as a basis for the statistics of quantum transitions. But our theory is also hopeful in this respect inasmuch as the anti-symmetric Fermi statistics of the electrons, corresponding to the Pauli exclusion principle, here necessarily leads to the symmetric Bose-Einstein statistics of photons. (Weyl 1929*a*, 244)

Weyl could probably not surmise which tremendous difficulties had to be surmounted on the path indicated here. When he reworked *GQM* for the second edition he knew already more about the nature of problems arising from the infinities of field quantization. He made some striking observations with respect to symmetries in quantum electrodynamics, but did not contribute to its further development in the later 1930s and 40s.[38]

Geometry and physics: interpretations and perspectives

As we have seen, Weyl's and Fock's 1929 work contained a strong common mathematical core. They both established local spinor structures on Lorentz manifolds with an additional internal $U(1)$ symmetry and proposed to use a connection in this structure, determined by or determining gravitation and electromagnetism and governing the motion of the spinor field. But they had strong differences with respect to the question of how geometry and physics could or should be related.

Fock proclaimed that his goal was "the geometrization of Dirac's electron theory and its subsumption (Einordnung) in general relativity" (Fock 1929*b*, 275). This was a conceptual-methodological task, rather than one of concrete physical theory building. He hoped, however, that his investigation might "contribute to the solution of the problems" in Dirac's theory, referring apparently to the paradox of negative energies and positive probability of fluctuations between negative and positive energies, respectively charges. He thus expected that his geometrization of the Dirac operator might lead, in the long run, to progress of a physical theory in a more technical sense. Fock's main hope was, however, to contribute to what he (and Ivanenko) thought to be a challenging goal of contemporary physics, the development of a common conceptual structure for relativity and quantum physics.

V. Fock had learned relativity from A. Friedmann and participated prominently in the development of relativity theory in Russia.[39] In the later 1920s he maintained close contact to a group of young physicists in Leningrad around L.

[37] See (Pais 1986, 313ff.), (Straumann 2001).

[38] For Weyl's contribution to the symmetries in early quantum electrodynamics, see (Coleman 2001, 287ff.); for the history of quantum electrodynamics (Schweber 1994).

[39] (Gorelik/Vizgin 1987, 286ff.).

Landau, G. Gamow, and M. Bronstein, to which his early 1929 coauthor D. Ivanenko belonged. The young physicists enthusiastically supported the cultural awakening in the early Soviet Union and wanted to contribute to it through their work in relativity and quantum physics.[40] This was apparently part of the background for Fock's and Ivanenko's premature claim to have found a path towards *quantum geometry.*

In a letter to *Nature*, dated March 21, 1929, they announced their first, still very sketchy ideas on "linear geometry" as a contribution to this challenging task.[41] In the *Comptes Rendus* note of May 22, 1929, they shifted attention in their "géométrie quantique linéaire" from the "matrix valued linear metric" to parallel displacements and covariant differentiation in a local spinor structure. Once more, they claimed to have found a method to reconcile quantum physics with geometry

> Il importe de signaler un point qui distingue les idées exposées dans cette Note de celles d'Einstein et de Levi-Cività: c'est l'intervention des matrices-opérateurs dans les équations pour les quantités purement géométriques. Grâce à cela on peut bien s'imaginer un champ électromagnétique dans un espace euclidien, ce qui était impossible dans les autres théories. (Fock/Ivanenko 1929*b*, 1472)

In his later contributions Fock was more cautious and weakened the claim to the more moderate one of having pursued "the geometrization of Dirac's theory of the electron and its subsumption under the general theory of relativity" (Fock 1929*b*, 275). He admitted that the "difficulties which are inherent in Dirac's theory" had not yet been touched, but added:

> Our investigations might perhaps contribute indirectly to the solution of these difficulties, by showing what the original unchanged Dirac theory can achieve. (ibid.)

The reference to the "original unchanged Dirac theory" was probably formulated after Fock got to know Weyl's proposal and indicated a disassociation from the latter, the reasons of which were explained in the postscript. Fock thus proclaimed that the geometrization of the Dirac equation by the spinor structure with connections and covariant derivation was an important methodological achievement in itself.

On this point Weyl did not agree at all. He had lost confidence in the geometrical unification programs which he himself had contributed so effectively by his gauge unification in 1918. About the end of the 1920s he no longer expected any deeper understanding of physical reality by the still blossoming geometrical unification programs.[42] He criticized, in particular, Einstein's latest attempt at unification by an additional structure of distant parallelism as a turn towards a physically unmotivated "artificial geometry" (Weyl 1929*a*, 219 quoted

[40] (Frenkel/Gorelik 1994, 20ff.).

[41] With respect to their purely formal "linear form with matrix coefficients" $ds = \sum_k \gamma_k dx_k$ (see above) they proclaimed: "This linear ds is connected with Dirac's wave equation in the same way as the Riemannian ds^2 with the relativistic wave equation of the older theory. ...This linear geometry seems to furnish a basis on which a uniform theory of gravitation, radiation, and quantum phenomena is to be constructed" (Fock/Ivanenko 1929*a*). For more details they referred to their forthcoming paper (Fock/Ivanenko 1929*c*)

[42] On the "diversity" of these programs see (Goldstein/Ritter 2000).

above). In his later 1929 paper for *Physikalische Zeitschrift* he argued in more detail:

> I am unable to believe in distant parallelism for several reasons. Firstly, a priori, my mathematical sense (mathematisches Gefühl) opposes against accepting such an artificial geometry; for me, it is difficult to conceive of a power which would make the local systems of axes, in their twisted position in the different world-points, freeze together in rigid affiliation. Moreover, two important physical reasons have to be added. ... (Weyl 1929*b*, 246)

As "first physical reason", Weyl mentioned his gauge theory of electromagnetism. He argued that only the point-dependence of the ONF's gave rise to a variable phase factor $e^{i\lambda}$ and thus the new principle of gauge invariance. The "second physical reason" was, to Weyl, the possibility to derive symmetry of the energy-momentum tensor and the invariance of rotational momentum in special relativity from infinitesimal rotations of the ONF's or of infinitesimal translations of coordinates (see above). Thus Weyl's "physical reasons" consisted essentially of methodological arguments for the superiority of invariance properties in an infinitesimal symmetry approach, close to those which about three decades later became central in the rise to prominence of more general "gauge" theories.[43]

The 1930 Rouse Ball lecture at Cambridge university gave Weyl the opportunity to explain his view of the unification programs to a wider scientific audience. He still considered the attempts "to geometrize the whole of physics", undertaken after Einstein had so successfully geometrized gravitation, very comprehensible at its time (Weyl 1931, 338). He explained his own theory of 1918 and summarized its critical reception by physicists. He reviewed Eddington's approach to unification by affine connections and Einstein's later suppport for that subprogram, always in comparison with his own "metrical" unification of 1918, and concluded that in hindsight one could see that both theory types were "merely geometrical dressings (geometrische Einkleidungen) rather than proper geometrical theories of electricity". He ironically added that the struggle between the metrical and affine UFT's (i.e. Weyl 1918 versus Eddington/Einstein) had lost importance, as in 1930 it could no longer be the question which of the theories would "prevail in life", but only "whether the two twin brothers had to be buried in the same grave or in two different graves" (ibid., 343). He again made clear that he could not find any argument in favour of Einstein's distant parallelism approach, nor could he find good prospects for the Kaluza-Klein approach.[44] Weyl even accused Einstein's new theory of "breaking with the infinitesimal point of view. (...) The result is to give away nearly all which has been gained in the transition from special to general relativity. The loss is not compensated by any concrete gain" (Weyl 1931, 343).

Weyl perceived a nearly complete scientific devaluation of the UFT's of the 1920s, resulting from developments in the second part of the decade:

> In my opinion the whole situation has changed during the last 4 or 5 years by the detection of the matter field. All these geometrical leaps

[43] Cf. (Morrison 1995).

[44] The revival of Kaluza-Klein type theories in the 1980s happened in a completely different content of theory development. In this conference, moreover, P. Cartier argued that there are reasons which might lead to a renewed interest in the original form of Weyl's *purely infinitesimal geometry* — again in a modified physical interpretion and theory context.

> (geometrische Luftsprünge) have been premature, we now return to the solid ground of physical facts. (Weyl 1931, 343)

He continued to sketch the theory of spinor fields, their phase gauge and its inclusion into the framework of general relativity along the lines of the 1929 articles. Weyl emphasized that, in contrast to the principles on which the classical UFT's had been built, the new principle of phase gauge "has grown from experience and resumes a huge treasury of experimental facts from spectroscopy" (ibid. 344). He still longed for safety, just as much as at the time after the First World War, when he designed his first gauge unification. Now he no longer expected to achieve it by geometric speculation, but tried to anchor it in more solid grounds:

> By the new gauge invariance the *electromagnetic field now becomes a necessary appendix of the matter field, as it had been attached to gravitation in the old theory.* (Weyl 1931, 345, emphasis in original)

Weyl made it very clear to his readers that he had changed his perspective. He no longer saw a chance in attempts to derive matter in highly speculative approaches from mathematical structures devised to geometrize force fields; he now set out to search forms for the mathematical represention of matter, which gave expression to the enduring traces in the "huge treasury" of experimental knowledge. For him, this was reason enough to prefer the view that the electrical field "follows the ship of matter as a wake, rather than gravitation" (ibid.).

In short, *Weyl had turned from his idealist approach to matter*, pursued at the turn to the 1920s, *to a symbolic realist* one at the end of the decade. This change of perspective had consequences for his views on geometrization. With reference to Fock's interpretation of the role of geometry in the general relativistic Dirac equation Weyl continued:

> Mr. Fock calls the derivation of the new gauge invariance from general relativity, which he arrived at nearly simultaneously with me, a geometrization of Dirac's theory of the electron. In this respect I cannot agree with him. My impression is that we have abandoned geometrization by linking electricity to matter rather than to gravitation. I fear that the geometrizing tendency, which seized gravitation in full right and supported by the most intuitive arguments, was misled when it was extended to other physical entities. (Weyl 1931, 345)

Weyl did not, on the other hand, completely negate any *possibility* to find a geometrical quantum theory. He only warned that, if one wanted to continue with the geometrizing tendency, one had to invent a "natural geometry" leading to a spinor type field ψ for the characterization of its structure, in addition to the ONF. Whereas Fock claimed to have achieved this already, Weyl remained agnostic:

> One had to set out in search of a geometrization of the matter field; if one succeeds here, the electromagnetic field is added as a premium to the bargain. I have no idea what kind of geometry this might be. (ibid.)

From the perspective of late 20th century developments in differential geometry and the tremendous role of gauge field theories, Weyl's evaluation is highly surprising and even seems paradoxical: Why did he not percieve his own and Focks's invention of local spinor structures with additional $U(1)$-gauge as a sufficiently rich extension of geometry to deal with matter structures?[45]

Our own perspective has been shaped by the development of differential geometry and topology in the second half of the last century, which was deeply influenced by Elie Cartan's work, the work of his students and other researchers. In the late 1950s and 1960s bundle structures with their inbuilt transformation behaviour have become central concepts in geometry and topology. In this sense, Weyl's first desideratum of a "natural geometry" which includes spinor type field in its core structure seems to be satisfied, and it becomes difficult to grasp why Weyl, unlike Fock, did *not* accept their common contribution as a valuable step in this direction.

We may assume that Weyl over-emphasized his scepticism with respect to geometrization of physics at the turn to the 1930s, because he still wanted to correct his earlier exuberance in this respect. Moreover he wanted to disassociate himself strongly from the "old" unification programs which where still alive in the latest attempts of Einstein, or Kaluza and Klein, and wanted to counteract them in the scientific discourse as clearly as possible.

For a proper historical understanding we have to take another aspect into account. Weyl's attempts to integrate geometry with physics had, from their very beginnings after the First World War, a strong intentional reference to the quantum stochastical aspects of matter as a a "dynamical agens", even at a time when these were not understood at all. In the early 1920s Weyl had dared to speculate in wide leaps about a possible relationship between the intuitive, the mathematical and the physical understanding of the continuum, some inbuilt discrete "free-choice" structures and the end of classical determinism in natural science.[46] In 1925, in his manuscript for the Lobachevsky centenary volume (published only posthumously (Weyl 1988)), Weyl indicated that the vagueness of physical determination of space-time localization has to be taken seriously for the basic theoretical structure of geometry. This vagueness ought to be considered a principal feature for the mathematical characterization of geometry and to be dealt with, in principle, in some stochastical approach informed by "the actual state of physics", i.e. quantum physics. But then, so Weyl remarked, at a time when the "new" quantum mechanics was just being shaped, the question, how such a quantum stochastical foundation for geometry relates to the differentiable structure of classical geometry, turned into a completely open problem. He ended the passage by the honest remark:

> One has to admit that until now nearly nothing has been achieved for the question what it means to apply differential calculus to [physical] reality. (Weyl 1988, 12)

With such questions Weyl was not *completely* alone. But they were far from what most physicists or mathematicians considered useful at the time, or even later in the 1930, when Fock's young colleague M. Bronstein explored the questions of a necessary revision of time-space concepts from the point of view of

[45] I thank Jim Ritter who indicated this point to me and insisted on a closer historical perspective.

[46] Most prominent and controversially discussed in this respect is (Weyl 1920).

quantum physics (Frenkel/Gorelik 1994, 83ff.). Fock's hope of 1929 to leave classical geometry behind and to turn towards geometrical quantum structures was comparably innocent. With such a point of view he was content with an extension of differential geometry which would appear, at most, as a semi-classical enrichment.

In his 1930 talk at Cambridge (and its later publication) Weyl expressed clearly that from a proper *geometry of matter* he expected a deep break with the classical tendency of geometrization prevailing in the UFT's. He was less clear, to say the least, what should be substituted for it; but there were strong reasons for such vagueness. His own approach to the mass problem of the electron had turned out to be unsatisfactory; Dirac's alternative appeared more promising, but still had a long way to go before a technically valid solution of the quantization problem was in sight[47] — not to speak about the extensions of later quantum gauge field theories and the still unanswerable question of the mass spectrum of basic constituents of matter. Therefore Weyl's remark "I have no idea what kind of geometry this might be", was just as honest as his comment in 1925 that "nearly nothing had been achieved" for a semantically reliable relation of the differentiable structure of geometry to the "actual state of physics".

Other contributions to this conference explore the much broader and deeper mathematical knowledge at the turn to the 21st century. Notwithstanding a whole range of new open questions and desiderata, including the one for a historical evaluation of recent developments, we now see several candidate programs for a quantum geometry aiming at (or preparing) a unification of quantum field theories.[48] It is not yet clear, whether one of them (or perhaps several) will "prevail in life". Weyl's proposal to look for a "geometry of matter" informed by the treasury of experimental knowledge could still be taken as an advice for a critical discourse in and among the different research programs.[49] Perhaps future developments will show whether Weyl's guess that the geometrization of interaction and metrical fields is "added as a bonus" once a proper geometry of matter has been achieved is just another speculative dream. It still may turn out that it indicates a hint for an appropriate theory development.

[47] See (Schweber 1994).

[48] Two, at least, were presented to the conference (M. Atiyah and A. Connes), another one was planned (C. Rovelli).

[49] At the turn of the century we may add that, in addition to recent and coming results in high-energy spectroscopy, geometrical aspects of low energy EPR-type experiments constitute a valuable novel part of the "treasury" of experimental knowledge, which ought to be taken into account in a future "geometry of matter".

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