



Early History of Gauge Theories and Weak Interactions¹

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1 Introduction

It took decades until physicists understood that all known fundamental interactions can be described in terms of gauge theories. My historical account begins with Einstein's general theory of relativity (GR), which is a non-Abelian gauge theory of a special type (see Secs. 3,4). The other gauge theories emerged in a slow and complicated process gradually from GR, and their common geometrical structure — best expressed in terms of connections of fiber bundles — is now widely recognized. Thus, also in this respect, H. Weyl was right when he wrote in the preface to the first edition of *Space – Time – Matter* (RZM) early in 1918: “Wider expanses and greater depths are now exposed to the searching eye of knowledge, regions of which we had not even a presentiment. It has brought us much nearer to grasping the plan that underlies all physical happening” [1].

It was Weyl himself who made in 1918 the first attempt to extend GR in order to describe gravitation and electromagnetism within a unifying geometrical framework [2]. This brilliant proposal contains all mathematical aspects of a non-Abelian gauge theory, as I will make clear in §2. The words gauge (Eich-) transformation and gauge invariance appear the first time in this paper, but in the everyday meaning of change of length or change of calibration.

Einstein admired Weyl's theory as “a coup of genius of the first rate . . .”, but immediately realized that it was physically untenable: “Although your idea is so beautiful, I have to declare frankly that, in my opinion, it is impossible that the theory corresponds to nature.” This led to an intense exchange of letters between Einstein (in Berlin) and Weyl (at the ETH

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in Zürich), which will hopefully soon be published in *The Collected Papers* of Einstein. (In my article [3] I gave an account of this correspondence which is preserved in the Archives of the ETH.) No agreement was reached, but Einstein's intuition proved to be right.

Although Weyl's attempt was a failure as a physical theory it paved the way for the correct understanding of gauge invariance. Weyl himself re-interpreted his original theory after the advent of quantum theory in a seminal paper [4] which I will discuss at length in §3. Parallel developments by other workers and interconnections are indicated in Fig.1.

At the time Weyl's contributions to theoretical physics were not appreciated very much, since they did not really add new physics. The attitude of the leading theoreticians is expressed in familiar distinctness in a letter by Pauli to Weyl from July 1, 1929, after he had seen a preliminary account of Weyl's work:

Before me lies the April edition of the Proc.Nat.Acad. (US). Not only does it contain an article from you under "Physics" but shows that you are now in a 'Physical Laboratory': from what I hear you have even been given a chair in 'Physics' in America. I admire your courage; since the conclusion is inevitable that you wish to be judged, not for success in pure mathematics, but for your true but unhappy love for physics [5].

Weyl's reinterpretation of his earlier speculative proposal had actually been suggested before by London, but it was Weyl who emphasized the role of gauge invariance as a *symmetry principle* from which electromagnetism can be *derived*. It took several decades until the importance of this symmetry principle — in its generalized form to non-Abelian gauge groups developed by Yang, Mills, and others — became also fruitful for a description of the weak and strong interactions. The mathematics of the non-Abelian generalization of Weyl's 1929 paper would have been an easy task for a mathematician of his rank, but at the time there was no motivation for this from the physics side. The known properties of the weak and strong nuclear interactions, in particular their short range, did not point to a gauge theoretical description. We all know that the gauge symmetries of the Standard Model are very hidden and it is, therefore, not astonishing that progress was very slow indeed.

Today, the younger generation, who learned the Standard Model from polished textbook presentations, complains with good reasons about many of its imperfections. It is one of the aims of this talk to make it obvious that it was extremely difficult to reach our present understanding of the fundamental interactions. The Standard Model, with all its success, is a great achievement, and one should not be too discouraged when major further progress is not coming rapidly.

Because of limitations of time and personal knowledge, I will discuss in the rest of my talk mainly the two important papers by Weyl from 1918 and 1929. The latter contains also his two-component theory of massless spin 1/2 fermions. In this context I will make in §5 a few remarks about the developments which led in 1958 to the phenomenological $V - A$ current-current Lagrangian for the weak interactions. My historical account of the non-Abelian generalizations by Klein, Pauli and others, culminating in the paper by Yang

and Mills, will also be much abbreviated. This is not too bad, since there will soon be a book by Lochlain O’Raifeartaigh that is devoted entirely to the early history of gauge theories [6]. Those who do not know German will find there also English translations of the most important papers of the first period (1918–1929). The book contains in addition the astonishing paper by Klein (1938) [7], Pauli’s letters to Pais on non-Abelian Kaluza-Klein reductions [8], parts of Shaw’s dissertation, in which he develops a non-Abelian $SU(2)$ gauge theory [9], and Utiyama’s generalization of Yang-Mills theory to arbitrary gauge groups [10]. These works are behind the diagram in Fig.1.

This talk covers mostly material contained in the papers [3], [11], and [12], which I have published some time ago in German, partly because all early publications and letters related to our subject are written in this language.

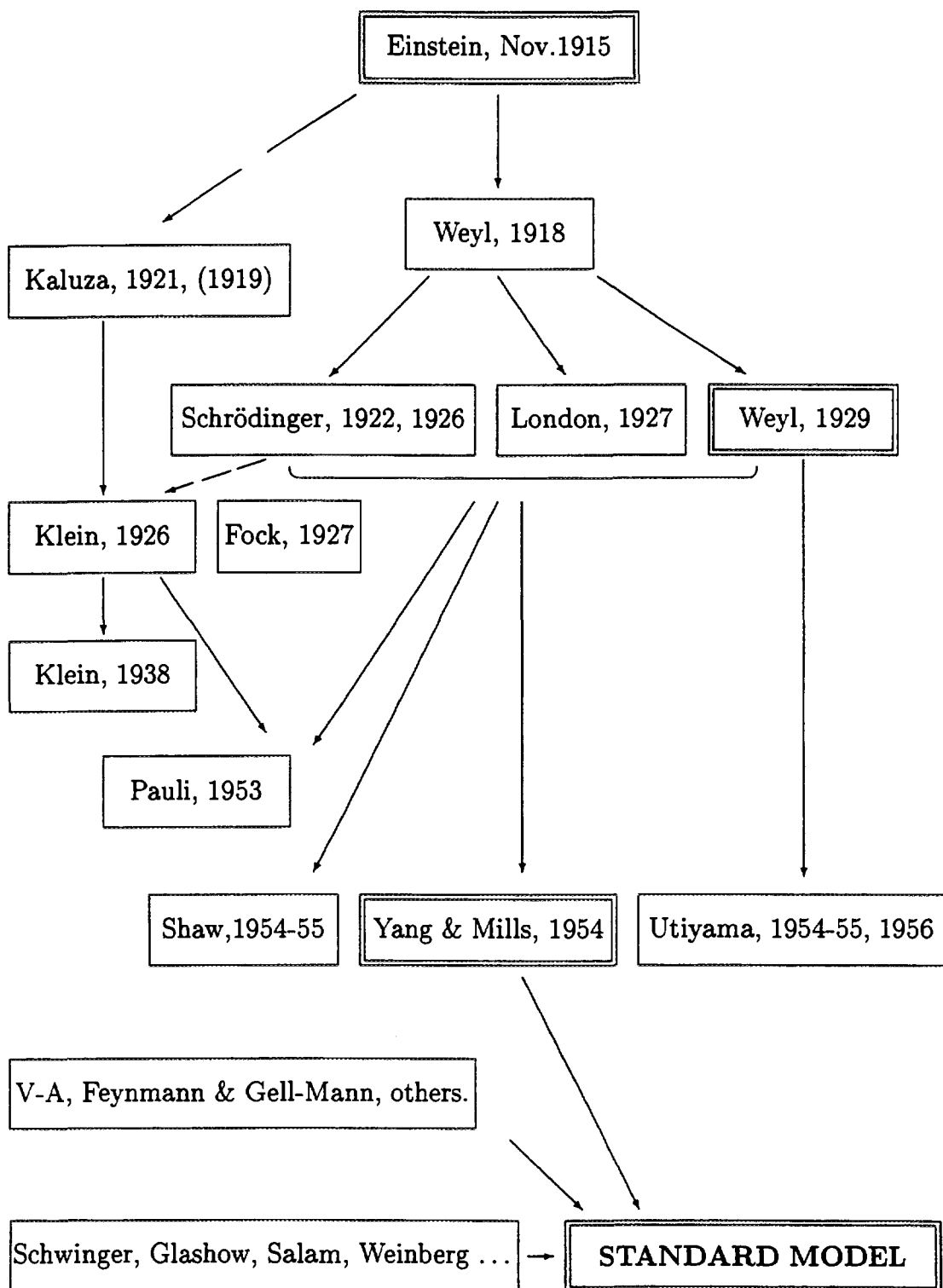


Figure 1: Key papers in the development of gauge theories.

2 Weyl's attempt to unify gravitation and electromagnetism

On the 1st of March 1918 Weyl writes in a letter to Einstein: "These days I succeeded, as I believe, to derive electricity and gravitation from a common source ...". Einstein's prompt reaction by postcard indicates already a physical objection which he explained in detail shortly afterwards. Before I come to this I have to describe Weyl's theory of 1918.

2.1 Weyl's generalization of Riemannian geometry

Weyl's starting point was purely mathematical. He felt a certain uneasiness about Riemannian geometry, as is clearly expressed by the following sentences early in his paper²:

But in Riemannian geometry described above there is contained a last element of geometry "at a distance" (ferngeometrisches Element) — with no good reason, as far as I can see; it is due only to the accidental development of Riemannian geometry from Euclidean geometry. The metric allows the two magnitudes of two vectors to be compared, not only at the same point, but at any arbitrarily separated points. A true infinitesimal geometry should, however, recognize only a principle for transferring the magnitude of a vector to an infinitesimally close point and then, on transfer to an arbitrary distant point, the integrability of the magnitude of a vector is no more to be expected than the integrability of its direction.

After these remarks Weyl turns to physical speculation and continues as follows:

On the removal of this inconsistency there appears a geometry that, surprisingly, when applied to the world, explains not only the gravitational phenomena but also the electrical. According to the resultant theory both spring from the same source, indeed in general one cannot separate gravitation and electromagnetism in a unique manner. In this theory all physical quantities have a world geometrical meaning; the action appears from the beginning as a pure number. It leads to an essentially unique universal law; it even allows us to understand in a certain sense why the world is four-dimensional.

In brief, Weyl's geometry can be described as follows. First, the spacetime manifold M is equipped with a conformal structure, i.e., with a class $[g]$ of conformally equivalent Lorentz metrics g (and not a definite metric as in GR). This corresponds to the requirement that it should only be possible to compare lengths at one and the same world point. Second, it is

²I am using here and at other places the English translation of L. O'Riada [6].

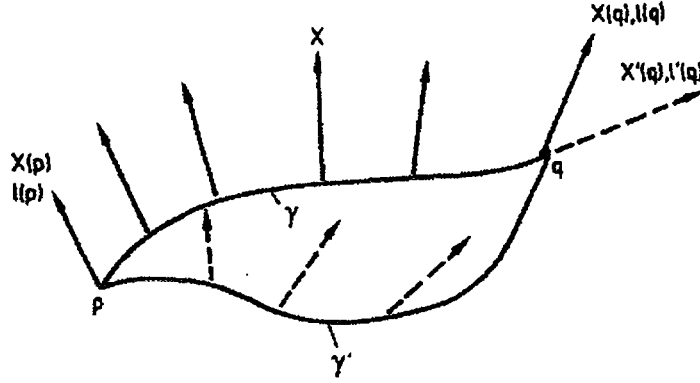


Figure 2: Path dependence of parallel displacement and transport of length in Weyl space.

assumed, as in Riemannian geometry, that there is an affine (linear) torsion-free connection which defines a covariant derivative ∇ , and respects the conformal structure. Differentially this means that for any $g \in [g]$ the covariant derivative ∇g should be proportional to g :

$$\nabla g = -2A \otimes g \quad (\nabla_\lambda g_{\mu\nu} = -2A_\lambda g_{\mu\nu}), \quad (2.1)$$

where $A = A_\mu dx^\mu$ is a differential 1-form.

Consider now a curve $\gamma : [0, 1] \rightarrow M$ and a parallel-transported vector field X along γ . If l is the length of X , measured with a representative $g \in [g]$, we obtain from (2.1) the following relation between $l(p)$ for the initial point $p = \gamma(0)$ and $l(q)$ for the end point $q = \gamma(1)$:

$$l(q) = \exp\left(-\int_\gamma A\right) l(p). \quad (2.2)$$

Thus, the ratio of lengths in q and p (measured with $g \in [g]$) depends in general on the connecting path γ (see Fig.2). The length is only independent of γ if the curl of A ,

$$F = dA \quad (F_{\mu\nu} = \partial_\mu A_\nu - \partial_\nu A_\mu), \quad (2.3)$$

vanishes.

The compatibility requirement (2.1) leads to the following expression for the Christoffel symbols in Weyl's geometry:

$$\Gamma_{\nu\lambda}^\mu = \frac{1}{2}g^{\mu\sigma}(g_{\lambda\sigma,\nu} + g_{\sigma\nu,\lambda} - g_{\nu\lambda,\sigma}) + g^{\mu\sigma}(g_{\lambda\sigma}A_\nu + g_{\sigma\nu}A_\lambda - g_{\nu\lambda}A_\sigma). \quad (2.4)$$

The second A -dependent term is a characteristic new piece in Weyl's geometry which has to be added to the Christoffel symbols of Riemannian geometry.

Until now we have chosen a fixed, but arbitrary metric in the conformal class $[g]$. This corresponds to a choice of calibration (or gauge). Passing to another calibration with metric \bar{g} , related to g by

$$\bar{g} = e^{2\lambda}g, \quad (2.5)$$

the potential A in (2.1) will also change to \bar{A} , say. Since the covariant derivative has an absolute meaning, \bar{A} can easily be worked out: On the one hand we have by definition

$$\nabla \bar{g} = -2\bar{A} \otimes \bar{g}, \quad (2.6)$$

and on the other hand we find for the left side with (2.1)

$$\nabla \bar{g} = \nabla(e^{2\lambda}g) = 2d\lambda \otimes \bar{g} + e^{2\lambda}\nabla g = 2d\lambda \otimes \bar{g} - 2A \otimes \bar{g}. \quad (2.7)$$

Thus

$$\bar{A} = A - d\lambda \quad (\bar{A}_\mu = A_\mu - \partial_\mu \lambda). \quad (2.8)$$

This shows that a change of calibration of the metric induces a “*gauge transformation*” for A :

$$g \rightarrow e^{2\lambda}g, \quad A \rightarrow A - d\lambda. \quad (2.9)$$

Only gauge classes have an absolute meaning. (The Weyl connection is, however, gauge-invariant.)

2.2 Electromagnetism and Gravitation

Turning to physics, Weyl assumes that his “purely infinitesimal geometry” describes the structure of spacetime and consequently he requires that physical laws should satisfy a double-invariance: 1. They must be invariant with respect to arbitrary smooth coordinate transformations. 2. They must be *gauge invariant*, i.e., invariant with respect to substitutions (2.9) for an arbitrary smooth function λ .

Nothing is more natural to Weyl, than identifying A_μ with the vector potential and $F_{\mu\nu}$ in eq.(2.3) with the field strength of electromagnetism. In the absence of electromagnetic fields ($F_{\mu\nu} = 0$) the scale factor $\exp(-\int_\gamma A)$ in (2.2) for length transport becomes path independent (integrable) and one can find a gauge such that A_μ vanishes. In this special case one is in the same situation as in GR.

Weyl proceeds to find an action which is generally invariant as well as gauge invariant and which would give the coupled field equations for g and A . I do not want to enter into this, except for the following remark. In his first paper [2] Weyl proposes what we call nowadays the Yang-Mills action

$$S(g, A) = -\frac{1}{4} \int \text{Tr}(\Omega \wedge * \Omega). \quad (2.10)$$

Here Ω denotes the curvature form and $*\Omega$ its Hodge dual³. Note that the latter is gauge invariant, i.e., independent of the choice of $g \in [g]$. In Weyl’s geometry the curvature form splits as $\Omega = \hat{\Omega} + F$, where $\hat{\Omega}$ is the metric piece [13]. Correspondingly, the action also splits,

$$\text{Tr}(\Omega \wedge * \Omega) = \text{Tr}(\hat{\Omega} \wedge * \hat{\Omega}) + F \wedge * F. \quad (2.11)$$

³The integrand in (2.10) is in local coordinates indeed just the expression $R_{\alpha\beta\gamma\delta}R^{\alpha\beta\gamma\delta}\sqrt{-g}dx^0 \wedge \dots \wedge dx^3$ which is used by Weyl ($R_{\alpha\beta\gamma\delta}$ = curvature tensor of the Weyl connection).

The second term is just the Maxwell action. Weyl's theory thus contains formally all aspects of a non-Abelian gauge theory.

Weyl emphasizes, of course, that the Einstein-Hilbert action is not gauge invariant. Later work by Pauli [14] and by Weyl himself [1, 2] led soon to the conclusion that the action (2.10) could not be the correct one, and other possibilities were investigated (see the later editions of RZM).

Independent of the precise form of the action Weyl shows that in his theory gauge invariance implies the *conservation of electric charge* in much the same way as general coordinate invariance leads to the conservation of energy and momentum⁴. This beautiful connection pleased him particularly: "... [it] seems to me to be the strongest general argument in favour of the present theory — insofar as it is permissible to talk of justification in the context of pure speculation." The invariance principles imply five 'Bianchi type' identities. Correspondingly, the five conservation laws follow in two independent ways from the coupled field equations and may be "termed the eliminants" of the latter. These structural connections hold also in modern gauge theories.

2.3 Einstein's objection and reactions of other physicists

After this sketch of Weyl's theory I come to Einstein's striking counterargument which he first communicated to Weyl by postcard (see Fig.3). The problem is that if the idea of a nonintegrable length connection (scale factor) is correct, then the behavior of clocks would depend on their history. Consider two identical atomic clocks in adjacent world points and bring them along different world trajectories which meet again in adjacent world points. According to (2.2) their frequencies would then generally differ. This is in clear contradiction with empirical evidence, in particular with the existence of stable atomic spectra. Einstein therefore concludes (see [3]):

... (if) one drops the connection of the ds to the measurement of distance and time, then relativity loses all its empirical basis.

Nernst shared Einstein's objection and demanded on behalf of the Berlin Academy that it should be printed in a short amendment to Weyl's article, and Weyl had to cope with it. I have described the intense and instructive subsequent correspondence between Weyl and Einstein elsewhere [3]. As an example, let me quote from one of the last letters of Weyl to Einstein:

This [insistence] irritates me of course, because experience has proven that one can rely on your intuition; so little convincing your counterarguments seem to me, as I have to admit ...

⁴I adopt here the somewhat naive interpretation of energy-momentum conservation for generally invariant theories of the older literature.

By the way, you should not believe that I was driven to introduce the linear differential form in addition to the quadratic one by physical reasons. I wanted, just to the contrary, to get rid of this 'methodological inconsistency (Inkonsequenz)' which has been a stone of contention to me already much earlier. And then, to my surprise, I realized that it looks as if it might explain electricity. You clap your hands above your head and shout: But physics is not made this way ! (Weyl to Einstein 10.12.1918).

Weyl's reply to Einstein's criticism was, generally speaking, this: The real behavior of measuring rods and clocks (atoms and atomic systems) in arbitrary electromagnetic and gravitational fields can be deduced only from a dynamical theory of matter.

Not all leading physicists reacted negatively. Einstein transmitted a very positive first reaction by Planck, and Sommerfeld wrote enthusiastically to Weyl that there was "... hardly doubt, that you are on the correct path and not on the wrong one."

In his encyclopedia article on relativity [15] Pauli gave a lucid and precise presentation of Weyl's theory, but commented Weyl's point of view very critically. At the end he states:

... Resuming one may say that Weyl's theory has not yet contributed to get closer to the solution of the problem of matter.

Also Eddington's reaction was first very positive but he changed his mind soon and denied the physical relevance of Weyl's geometry.

The situation was later appropriately summarized by F.London in his 1927 paper [16] as follows:

In the face of such elementary experimental evidence, it must have been an unusually strong metaphysical conviction that prevented Weyl from abandoning the idea that Nature would have to make use of the beautiful geometrical possibility that was offered. He stuck to his conviction and evaded discussion of the above-mentioned contradictions through a rather unclear re-interpretation of the concept of "real state", which, however, robbed his theory of its immediate physical meaning and attraction.

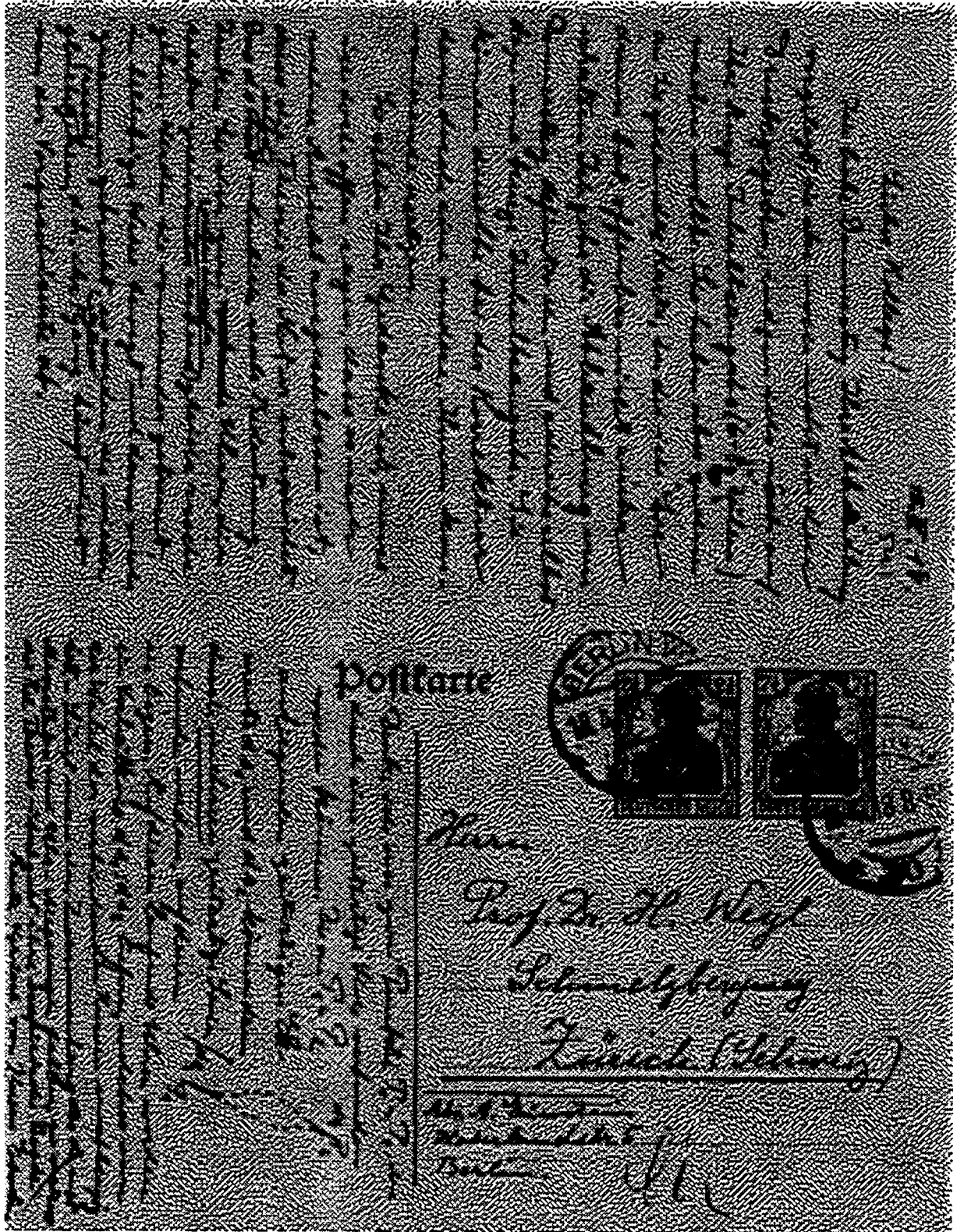


Figure 3: Postcard of Einstein to Weyl 15.4.1918 (Archives of ETH).

3 Weyl's 1929 Classic: "Electron and Gravitation"

Shortly before his death late in 1955, Weyl wrote for his *Selecta* [17] a postscript to his early attempt in 1918 to construct a 'unified field theory'. There he expressed his deep attachment to the gauge idea and adds (p.192):

Later the quantum-theory introduced the Schrödinger-Dirac potential ψ of the electron-positron field; it carried with it an experimentally-based principle of gauge-invariance which guaranteed the conservation of charge, and connected the ψ with the electromagnetic potentials ϕ_i in the same way that my speculative theory had connected the gravitational potentials g_{ik} with the ϕ_i , and measured the ϕ_i in known atomic, rather than unknown cosmological units. I have no doubt but that the correct context for the principle of gauge-invariance is here and not, as I believed in 1918, in the intertwining of electromagnetism and gravity.

This re-interpretation was developed by Weyl in one of the great papers of this century [4]. Weyl's classic does not only give a very clear formulation of the gauge principle, but contains, in addition, several other important concepts and results — in particular his two-component theory. The richness of the paper is clearly visible from the following table of contents:

Introduction. Relationship of General Relativity to the quantum-theoretical field equations of the spinning electron: mass, gauge-invariance, distant-parallelism. Expected modifications of the Dirac theory. -I. Two-component theory: the wave function ψ has only two components. -§1. Connection between the transformation of the ψ and the transformation of a normal tetrad in four-dimensional space. Asymmetry of past and future, of left and right. -§2. In General Relativity the metric at a given point is determined by a normal tetrad. Components of vectors relative to the tetrad and coordinates. Covariant differentiation of ψ . -§3. Generally invariant form of the Dirac action, characteristic for the wave-field of matter. -§4. The differential conservation law of energy and momentum and the symmetry of the energy-momentum tensor as a consequence of the double-invariance (1) with respect to coordinate transformations (2) with respect to rotation of the tetrad. Momentum and moment of momentum for matter. -§5. Einstein's classical theory of gravitation in the new analytic formulation. Gravitational energy. -§6. The electromagnetic field. From the arbitrariness of the gauge-factor in ψ appears the necessity of introducing the electromagnetic potential. Gauge invariance and charge conservation. The space-integral of charge. The introduction of mass. Discussion and rejection of another possibility in which electromagnetism appears, not as an accompanying phenomenon of matter, but of gravitation.

The modern version of the gauge principle is already spelled out in the introduction:

The Dirac field-equations for ψ together with the Maxwell equations for the four potentials f_p of the electromagnetic field have an invariance property which is formally similar to the one which I called gauge-invariance in my 1918 theory of gravitation and electromagnetism; the equations remain invariant when one makes the simultaneous substitutions

$$\psi \text{ by } e^{i\lambda}\psi \quad \text{and} \quad f_p \text{ by } f_p - \frac{\partial\lambda}{\partial x^p},$$

where λ is understood to be an arbitrary function of position in four-space. Here the factor $\frac{e}{c\hbar}$, where $-e$ is the charge of the electron, c is the speed of light, and $\frac{\hbar}{2\pi}$ is the quantum of action, has been absorbed in f_p . The connection of this "gauge invariance" to the conservation of electric charge remains untouched. But a fundamental difference, which is important to obtain agreement with observation, is that the exponent of the factor multiplying ψ is not real but pure imaginary. ψ now plays the role that Einstein's ds played before. It seems to me that this new principle of gauge-invariance, which follows not from speculation but from experiment, tells us that the electromagnetic field is a necessary accompanying phenomenon, not of gravitation, but of the material wave-field represented by ψ . Since gauge-invariance involves an arbitrary function λ it has the character of "general" relativity and can naturally only be understood in that context.

We shall soon enter into Weyl's justification which is, not surprisingly, strongly associated with general relativity. Before this I have to describe his incorporation of the Dirac theory into GR which he achieved with the help of the tetrad formalism.

One of the reasons for adapting the Dirac theory of the spinning electron to gravitation had to do with Einstein's recent unified theory which invoked a distant parallelism with torsion. E. Wigner [18] and others had noticed a connection of this theory and the spin theory of the electron. Weyl did not like this and wanted to dispense with teleparallelism. In the introduction he says:

I prefer not to believe in distant parallelism for a number of reasons. First my mathematical intuition objects to accepting such an artificial geometry; I find it difficult to understand the force that would keep the local tetrads at different points and in rotated positions in a rigid relationship. There are, I believe, two important physical reasons as well. The loosening of the rigid relationship between the tetrads at different points converts the gauge-factor $e^{i\lambda}$, which remains arbitrary with respect to ψ , from a constant to an arbitrary function of space-time. In other words, only through the loosening the rigidity does the established gauge-invariance become understandable.

This thought is carried out in detail after Weyl has set up his two-component theory in special relativity, including a discussion of P and T invariance. He emphasizes thereby that the two-component theory excludes a linear implementation of parity and remarks: "It

is only the fact that the left-right symmetry actually appears in Nature that forces us to introduce a second pair of ψ -components.” To Weyl the mass-problem is thus not relevant for this. Indeed he says: “Mass, however, is a gravitational effect; thus there is hope of finding a substitute in the theory of gravitation that would produce the required corrections.”

We shall return to the two-component theory in §5 in connection with parity violation and the $V - A$ interaction.

3.1 Tetrad formalism

The method of Weyl for incorporating his two-component spinors into general relativity makes use of local tetrads (Vierbeins).

In the tetrad formalism the metric is described by an arbitrary basis of orthonormal vector fields $\{e_\alpha(x); \alpha = 0, 1, 2, 3\}$. If $\{e^\alpha(x)\}$ denotes the dual basis of 1-forms, the metric is given by

$$g = \eta_{\mu\nu} e^\nu(x) \otimes e^\mu(x), \quad (\eta_{\mu\nu}) = \text{diag}(1, -1, -1, -1). \quad (3.1)$$

Weyl emphasizes, of course, that only a class of such local tetrads is determined by the metric: the metric is not changed if the tetrad fields are subject to spacetime-dependent Lorentz transformations:

$$e^\alpha(x) \rightarrow \Lambda^\alpha_\beta(x) e^\beta(x). \quad (3.2)$$

With respect to a tetrad, the connection forms⁵ $\omega = (\omega^\alpha_\beta)$ have values in the Lie algebra of the homogeneous Lorentz group:

$$\omega_{\alpha\beta} + \omega_{\beta\alpha} = 0. \quad (3.3)$$

(Indices are raised and lowered with $\eta^{\alpha\beta}$ and $\eta_{\alpha\beta}$, respectively.) They are determined (in terms of the tetrad) by the first structure equation of Cartan:

$$de^\alpha + \omega^\alpha_\beta \wedge e^\beta = 0. \quad (3.4)$$

Under local Lorentz transformations (3.2) the connection forms transform in the same way as the gauge potential of a non-Abelian gauge theory:

$$\omega(x) \rightarrow \Lambda(x)\omega(x)\Lambda^{-1}(x) - d\Lambda(x)\Lambda^{-1}(x). \quad (3.5)$$

The curvature forms $\Omega = (\Omega^\mu_\nu)$ are obtained from ω in exactly the same way as the Yang-Mills field strength from the gauge potential:

$$\Omega = d\omega + \omega \wedge \omega \quad (3.6)$$

(second structure equation).

For a vector field V , with components V^α relative to $\{e_\alpha\}$, the covariant derivative DV is given by

$$DV^\alpha = dV^\alpha + \omega^\alpha_\beta V^\beta. \quad (3.7)$$

⁵I am using more modern notations; for details see [18].

Weyl generalizes this in a unique manner to spinor fields ψ :

$$D\psi = d\psi + \frac{1}{4}\omega_{\alpha\beta}\sigma^{\alpha\beta}\psi. \quad (3.8)$$

Here, the $\sigma^{\alpha\beta}$ describe infinitesimal Lorentz transformations (in the representation of ψ). For a Dirac field these are the familiar matrices

$$\sigma^{\alpha\beta} = \frac{1}{2}[\gamma^\alpha, \gamma^\beta]. \quad (3.9)$$

(For 2-component Weyl fields one has similar expressions in terms of the Pauli matrices.)

With these tools the action principle for the coupled Einstein-Dirac system can be set up. In the massless case the Lagrangian is

$$\mathcal{L} = \frac{1}{16\pi G}R - i\bar{\psi}\gamma^\mu D_\mu\psi, \quad (3.10)$$

where the first term is just the Einstein-Hilbert Lagrangian (which is linear in Ω). Weyl discusses, of course, immediately the consequences of the following two symmetries:

- (i) local Lorentz invariance,
- (ii) general coordinate invariance.

3.2 The new form of the gauge-principle

All this is kind of a preparation for the final section of Weyl's paper, which has the title "electric field". Weyl says:

We come now to the critical part of the theory. In my opinion the origin and necessity for the electromagnetic field is in the following. The components $\psi_1 \psi_2$ are, in fact, not uniquely determined by the tetrad but only to the extent that they can still be multiplied by an arbitrary "gauge-factor" $e^{i\lambda}$. The transformation of the ψ induced by a rotation of the tetrad is determined only up to such a factor. In special relativity one must regard this gauge-factor as a constant because here we have only a single point-independent tetrad. Not so in general relativity; every point has its own tetrad and hence its own arbitrary gauge-factor; because by the removal of the rigid connection between tetrads at different points the gauge-factor necessarily becomes an arbitrary function of position.

In this manner Weyl arrives at the gauge-principle in its modern form and emphasizes: "From the arbitrariness of the gauge-factor in ψ appears the necessity of introducing the electromagnetic potential." The first term $d\psi$ in (3.8) has now to be replaced by the covariant

gauge derivative $(d - ieA)\psi$ and the nonintegrable scale factor (2.1) of the old theory is now replaced by a phase factor:

$$\exp\left(-\int_{\gamma} A\right) \rightarrow \exp\left(-i\int_{\gamma} A\right),$$

which corresponds to the replacement of the original gauge group \mathbf{R} by the compact group $U(1)$. Accordingly, the original Gedankenexperiment of Einstein translates now to the Aharonov-Bohm effect. The close connection between gauge invariance and conservation of charge is again uncovered. The current conservation follows, as in the original theory, in two independent ways: On the one hand it is a consequence of the field equations for matter plus gauge invariance, at the same time, however, also of the field equations for the electromagnetic field plus gauge invariance. This corresponds to an identity in the coupled system of field equations which has to exist as a result of gauge invariance. All this is nowadays familiar to students of physics and needs not to be explained in more detail.

Much of Weyl's paper penetrated also into his classic book "The Theory of Groups and Quantum Mechanics" [19]. There he mentions also the transformation of his early gauge-theoretic ideas: "This principle of gauge invariance is quite analogous to that previously set up by the author, on speculative grounds, in order to arrive at a unified theory of gravitation and electricity. But I now believe that this gauge invariance does not tie together electricity and gravitation, but rather electricity and matter."

When Pauli saw the full version of Weyl's paper he became more friendly and wrote [20]:

In contrast to the nasty things I said, the essential part of my last letter has since been overtaken, particularly by your paper in Z. f. Physik. For this reason I have afterward even regretted that I wrote to you. After studying your paper I believe that I have really understood what you wanted to do (this was not the case in respect of the little note in the Proc.Nat.Acad.). First let me emphasize that side of the matter concerning which I am in full agreement with you: your incorporation of spinor theory into gravitational theory. I am as dissatisfied as you are with distant parallelism and your proposal to let the tetrads rotate independently at different space-points is a true solution.

In brackets Pauli adds:

Here I must admit your ability in Physics. Your earlier theory with $g'_{ik} = \lambda g_{ik}$ was pure mathematics and unphysical. Einstein was justified in criticizing and scolding. Now the hour of your revenge has arrived.

Then he remarks in connection with the mass-problem:

Your method is valid even for the massive [Dirac] case. I thereby come to the other side of the matter, namely the unsolved difficulties of the Dirac theory

(two signs of m_0) and the question of the 2-component theory. In my opinion these problems will not be solved by gravitation . . . the gravitational effects will always be much too small.

Many years later, Weyl summarized this early tortuous history of gauge theory in an instructive letter to the Swiss writer and Einstein biographer C.Seelig, which I reproduce in the German original [21].

Aus dem Jahre 1918 datiert der von mir unternommene erste Versuch, eine einheitliche Feldtheorie von Gravitation und Elektromagnetismus zu entwickeln, und zwar auf Grund des Prinzips der Eichinvarianz, das ich neben dasjenige der Koordinaten-Invarianz stellte. Ich habe diese Theorie selber längst aufgegeben, nachdem ihr richtiger Kern: die Eichinvarianz, in die Quantentheorie herübergerettet ist als ein Prinzip, das nicht die Gravitation, sondern das Wellenfeld des Elektrons mit dem elektromagnetischen verknüpft. — Einstein war von Anfang dagegen, und das gab zu mancher Diskussion Anlass. Seinen konkreten Einwänden glaubte ich begegnen zu können. Schliesslich sagte er dann: “Na, Weyl, lassen wir das! So — das heisst auf so spekulative Weise, ohne ein leitendes, anschauliches physikalisches Prinzip — macht man keine Physik!” Heute haben wir in dieser Hinsicht unsere Standpunkte wohl vertauscht. Einstein glaubt, dass auf diesem Gebiet die Kluft zwischen Idee und Erfahrung so gross ist, dass nur der Weg der mathematischen Spekulation, deren Konsequenzen natürlich entwickelt und mit den Tatsachen konfrontiert werden müssen, Aussicht auf Erfolg hat, während mein Vertrauen in die reine Spekulation gesunken ist und mir ein engerer Anschluss an die quanten-physikalischen Erfahrungen geboten scheint, zumal es nach meiner Ansicht nicht genug ist, Gravitation und Elektromagnetismus zu einer Einheit zu verschmelzen. Die Wellenfelder des Elektrons und was es sonst noch an unreduzierbaren Elementarteilchen geben mag, müssen mit eingeschlossen werden.

4 Yang-Mills Theory

In his Hermann Weyl Centenary Lecture at the ETH [22], C.N. Yang commented on Weyl’s remark “The principle of gauge-invariance has the character of general relativity since it contains an arbitrary function λ , and can certainly only be understood in terms of it” [23] as follows:

The quote above from Weyl’s paper also contains something which is very revealing, namely, his strong association of gauge invariance with general relativity. That was, of course, natural since the idea had originated in the first place with Weyl’s attempt in 1918 to unify electromagnetism with gravity. Twenty years later, when Mills and I worked on non-Abelian gauge fields, our motivation

was completely divorced from general relativity and we did not appreciate that gauge fields and general relativity are somehow related. Only in the late 1960's did I recognize the structural similarity mathematically of non-Abelian gauge fields with general relativity and understand that they both were connections mathematically.

Later, in connection with Weyl's strong emphasis of the relation between gauge invariance and conservation of electric charge, Yang continues with the following instructive remarks:

Weyl's reason, it turns out, was also one of the melodies of gauge theory that had very much appealed to me when as a graduate student I studied field theory by reading Pauli's articles. I made a number of unsuccessful attempts to generalize gauge theory beyond electromagnetism, leading finally in 1954 to a collaboration with Mills in which we developed a non-Abelian gauge theory. In [...] we stated our motivation as follows:

The conservation of isotopic spin points to the existence of a fundamental invariance law similar to the conservation of electric charge. In the latter case, the electric charge serves as a source of electromagnetic field; an important concept in this case is gauge invariance which is closely connected with (1) the equation of motion of the electro-magnetic field, (2) the existence of a current density, and (3) the possible interactions between a charged field and the electromagnetic field. We have tried to generalize this concept of gauge invariance to apply to isotopic spin conservation. It turns out that a very natural generalization is possible.

Item (2) is the melody referred to above. The other two melodies, (1) and (3), where what had become pressing in the early 1950's when so many new particles had been discovered and physicists had to understand now they interact which each other.

I had met Weyl in 1949 when I went to the Institute for Advanced Study in Princeton as a young "member". I saw him from time to time in the next years, 1949-1955. He was very approachable, but I don't remember having discussed physics or mathematics with him at any time. His continued interest in the idea of gauge fields was not known among the physicists. Neither Oppenheimer nor Pauli ever mentioned it. I suspect they also did not tell Weyl of the 1954 papers of Mills' and mine. Had they done that, or had Weyl somehow come across our paper, I imagine he would have been pleased and excited, for we had put together two things that were very close to his heart: gauge invariance and non-Abelian Lie groups.

It is indeed astonishing that during those late years Pauli never talked with Weyl on non-Abelian generalizations of gauge-invariance, since he himself had worked on this — even before the work of Yang and Mills. During a discussion following a talk by Pais at the 1953 Lorentz Conference [24] in Leiden, Pauli said:

...I would like to ask in this connection whether the transformation group [isospin] with constant phases can be amplified in a way analogous to the gauge

group for electromagnetic potentials in such a way that the meson-nucleon interaction is connected with the amplified group . . .

Stimulated by this discussion, Pauli worked on this problem and drafted a manuscript to Pais that begins with [8]:

Written down July 22-25, 1953, in order to see how it looks. Meson-Nucleon Interaction and Differential Geometry.

Unaware of Klein's earlier contribution [7], Pauli generalizes in this manuscript the Kaluza-Klein theory to a sixdimensional space, and arrives through dimensional reduction at the essentials of an $SU(2)$ gauge theory. The extra-dimensions are two-spheres with space-time dependent metrics on which $SU(2)$ operates in a spacetime dependent manner. Pauli develops first in "local language" the geometry of what we now call a fiber bundle with a homogeneous space as typical fiber (in his case $S^2 \cong SU(2)/U(1)$). Studying the curvature of the higher dimensional space, Pauli automatically finds for the first time the correct expression for the non-Abelian field strengths. Afterwards, Pauli sets up the 6-dimensional Dirac equation and writes it out in an explicit manner which is adapted to the fibration. Later, in December 1953, he sends a "Mathematical Appendix" to Pais and determines — among other things — the mass spectrum implied by this equation. The final sentence reads: "So this leads to some rather unphysical 'shadow particles'." Pauli did not write down a Lagrangian for the gauge fields, but as we shall see shortly, it was clear to him that the gauge bosons had to be massless. This, beside the curious fermion spectrum, must have been the reason why he did not publish anything.

With this background, the following story of spring 1954 becomes more understandable. In late February, Yang was invited by Oppenheimer to return to Princeton for a few days and to give a seminar on his joint work with Mills. Here, Yang's report [25]:

Pauli was spending the year in Princeton, and was deeply interested in symmetries and interactions. (He had written in German a rough outline of some thoughts, which he had sent to A. Pais. Years later F.J. Dyson translated this outline into English. It started with the remark, "Written down July 22-25, 1953, in order to see how it looks," and had the title "Meson-Nucleon Interaction and Differential Geometry.") Soon after my seminar began, when I had written down on the blackboard,

$$(\partial_\mu - i\epsilon B_\mu)\psi,$$

Pauli asked, "What is the mass of this field B_μ ?" I said we did not know. Then I resumed my presentation, but soon Pauli asked the same question again. I said something to the effect that that was a very complicated problem, we had worked on it and had come to no definite conclusions. I still remember his repartee: "That is not sufficient excuse." I was so taken aback that I decided, after a few moments' hesitation to sit down. There was general embarrassment.

Finally Oppenheimer said, "We should let Frank proceed." I then resumed, and Pauli did not ask any more questions during the seminar.

I don't remember what happened at the end of the seminar. But the next day I found the following message:

February 24, Dear Yang, I regret that you made it almost impossible for me to talk with you after the seminar. All good wishes. Sincerely yours, W.Pauli.

I went to talk to Pauli. He said I should look up a paper by E. Schrödinger, in which there were similar mathematics⁶. After I went back to Brookhaven, I looked for the paper and finally obtained a copy. It was a discussion of space-time-dependent representations of the γ_μ matrices for a Dirac electron in a gravitational field. Equations in it were, on the one hand, related to equations in Riemannian geometry and, on the other, similar to the equations that Mills and I were working on. But it was many years later when I understood that these were all different cases of the mathematical theory of connections on fiber bundles.

Later Yang adds:

I often wondered what he [Pauli] would say about the subject if he had lived into the sixties and seventies.

At another occasion [22] he remarked:

I venture to say that if Weyl were to come back today, he would find that amidst the very exciting, complicated and detailed developments in both physics and mathematics, there are fundamental things that he would feel very much at home with. He had helped to create them.

Having quoted earlier letters from Pauli to Weyl, I add what Weyl said about Pauli in 1946 [26]:

The mathematicians feel near to Pauli since he is distinguished among physicists by his highly developed organ for mathematics. Even so, he is a physicist; for he has to a high degree what makes the physicist; the genuine interest in the experimental facts in all their puzzling complexity. His accurate, instructive estimate of the relative weight of relevant experimental facts has been an unfailing guide for him in his theoretical investigations. Pauli combines in an exemplary way physical insight and mathematical skill.

To conclude this section, let me emphasize the main differences of GR and Yang-Mills theories. Mathematically, the $so(1, 3)$ -valued connection forms ω in §3.1 and the Liealgebra-valued gauge potential A are on the same footing; they are both representatives of connections in (principle) fiber bundles over the spacetime manifold. Eq.(3.6) translates into the

⁶E. Schrödinger, Sitzungsberichte der Preussischen (Akademie der Wissenschaften, 1932), p. 105.

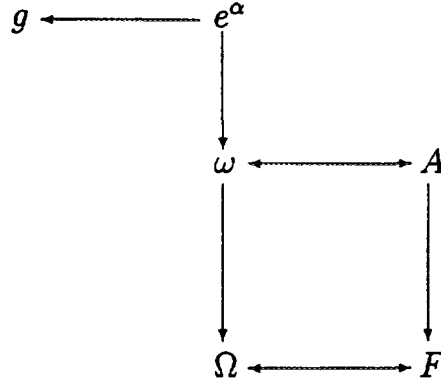


Figure 4: General Relativity versus Yang-Mills theory.

formula for the Yang-Mills field strength F ,

$$F = dA + A \wedge A. \quad (4.1)$$

In GR one has, however, additional geometrical structure, since the connection derives from a metric, or the tetrad fields $e^\alpha(x)$, through the first structure equation (3.4). Schematically, we have:

(In bundle theoretical language one can express this as follows: The principle bundle of GR, i.e., the orthonormal frame bundle, is constructed from the base manifold and its metric, and has therefore additional structure, implying in particular the existence of a canonical 1-form (soldering form), whose local representative are the tetrad fields; see, e.g. [38].)

Another important difference is that the gravitational Lagrangian $*R = \frac{1}{2}\Omega_{\alpha\beta} \wedge *(e^\alpha \wedge e^\beta)$ is linear in the field strengths, whereas the Yang-Mills Lagrangian $F \wedge *F$ is quadratic.

5 Parity Violation and 2-Component Neutrino

The two-component spinor theory was only briefly mentioned in my discussion of Weyl's great 1929 paper. Since this massless spin 1/2 equation became very important after the discovery of parity violation I would now like to add a few remarks.

Due to the fact that there exist two inequivalent irreducible (projective) representations of the one-component of the homogeneous Lorentz group, L_+^\uparrow (with $SL(2, C)$ as universal covering group), there are two types of fundamental Weyl spinors, ϕ_α and χ^β , for which the free Weyl equations read as follows:

$$\hat{\sigma}^\mu \partial_\mu \phi = 0, \quad \sigma^\mu \partial_\mu \chi = 0. \quad (5.1)$$

Here, $(\sigma^\mu) = (\mathbb{1}, -\vec{\sigma})$, $(\hat{\sigma}^\mu) = (\mathbb{1}, \vec{\sigma})$ ($\vec{\sigma}$: Pauli matrices). In spinor calculus these equations

become

$$\partial^{\alpha\dot{\beta}}\phi_{\alpha} = 0, \quad \partial_{\alpha\dot{\beta}}\chi^{\dot{\beta}} = 0. \quad (5.2)$$

In his “New Testament” from 1933 [27], Pauli rejected these equations: “Indessen sind diese Wellengleichungen, wie ja aus ihrer Herleitung hervorgeht, nicht invariant gegenüber Spiegelungen (Vertauschung von links und recht) und infolgedessen sind sie auf die physikalische Wirklichkeit nicht anwendbar.”

However, as long as no interactions are taken into account, this statement is not correct. To make this evident one only has to note that both equations in (5.1) are equivalent to the Majorana formulation: Consider, for instance, the ϕ -field and set

$$\psi = \begin{pmatrix} \phi \\ \varepsilon\phi^* \end{pmatrix}, \quad \varepsilon = \begin{pmatrix} 0 & 1 \\ -1 & 0 \end{pmatrix}, \quad (5.3)$$

then the first equation in (5.1) is equivalent to the massless Dirac equation,

$$\gamma^{\mu}\partial_{\mu}\psi = 0. \quad (5.4)$$

Furthermore, ψ is self-conjugate: A general Dirac spinor $\begin{pmatrix} \phi_{\alpha} \\ \chi^{\dot{\beta}} \end{pmatrix}$ transforms under charge conjugation C according to

$$C : \begin{pmatrix} \phi \\ \chi \end{pmatrix} \rightarrow \begin{pmatrix} -\varepsilon\chi^* \\ \varepsilon\phi^* \end{pmatrix} \quad (5.5)$$

and, for (5.3), this reduces to the Majorana condition $C : \psi \rightarrow \psi$. Nobody would say that the Majorana theory is not reflection invariant.

Note in this connection also the following: A Dirac field transforms under P as

$$P : \psi \rightarrow \psi'(x) = \gamma^0\psi(Px). \quad (5.6)$$

For the Majorana field (5.3) this translates into an antilinear transformation for ϕ ,

$$P : \phi \rightarrow \phi'(x) = \varepsilon\phi^*(Px), \quad (5.7)$$

which leaves the Weyl equation invariant. Usually this operation is interpreted as CP , but without interactions this is a matter of semantics.

Before I will return to history, let me also remind you of the formulation of Lee and Yang [28]. These authors introduce in the Weyl representation of the γ -matrices the Dirac spinor $\psi = \begin{pmatrix} \phi \\ 0 \end{pmatrix}$, whence $(1 - \gamma^5)\psi = 0$. The first Weyl equation in (5.1) is then again equivalent to the massless Dirac equation (5.4). In the Lee-Yang formulation one thus has

$$\gamma^{\mu}\partial_{\mu}\psi = 0, \quad (1 - \gamma^5)\psi = 0. \quad (5.8)$$

These equations are, of course, independent of the representation of the γ -algebra.

Thus, the three formulations of Weyl, Majorana, and Lee-Yang are entirely equivalent. This was noticed by several authors [28] shortly after the discovery of parity violation, but had been worked out by J.Serpe [29] already in 1952. Today, because of the chiral nature of the fundamental fermions, the use of Weyl spinors has become common practice.

The discovery of parity violation early in 1957 in several experiments suggested by Lee and Yang [30] was one of the most exciting events in the fifties. Its impact was enormous, as is illustrated by the following letter from Pauli to Weisskopf [31]:

Dear Weisskopf,

Now the first shock is over and I begin to collect myself again (as one says in Munich).

Yes, it was very dramatic. On Monday 21st at 8:15 p.m. I was supposed to give a talk about "past and recent history of the neutrino". At 5 p.m. the mail brought me three experimental papers: C.S. Wu, Lederman and Telegdi; the latter was so kind to send them to me. The same morning I received two theoretical papers, one by Yang, Lee and Oehme, the second by Yang and Lee about the two-component spinor theory. The latter was essentially identical with the paper by Salam, which I received as a preprint already six to eight weeks ago and to which I referred in my last short letter to you. (Was this paper known in the USA?) (At the same time came a letter from Geneva by Villars with the New York Times article.)

Now, where shall I start? It is good that I did not make a bet. I would have resulted in a heavy loss of money (which I cannot afford); I did make a fool of myself, however (which I think I can afford to do)— incidentally, only in letters or orally and not in anything that was printed. But the others now have the right to laugh at me.

What shocks me is not the fact that "God is just left-handed" but the fact that in spite of this He exhibits Himself as left/right symmetric when He expresses Himself strongly. In short, the real problem now is why the strong interaction are left/right symmetric. How can the strength of an interaction produce or create symmetry groups, invariances or conservation laws? This question prompted me to my premature and wrong prognosis. I don't know any good answer to that question but one should consider that already there exists a precedent: the rotation group in isotopic spin-space, which is not valid for the electromagnetic field. One does not understand why it is valid at all. It seems that there is a certain analogy here!

In my lecture I described how Bohr (Faraday lecture, 1932, Solvay Conference, 1932), as my main opponent in regard to the neutrino, considered plausible the violation of the energy law in the beta-decay (what one calls today "weak interaction"), how his opposition then became weaker and how he said in a more general way (1933) that one must be "prepared for surprises" not anywhere but specifically with the beta-decay. Then I said spontaneously (on the spur of the moment) that at the end of my talk I would come back to the surprises which Professor Bohr had foreseen here . . .

Many questions, no answers !

Let me say a bit more about the paper of Salam which is mentioned in Pauli's letter. In September 1956 Salam had heard Yang's talk at the Seattle Conference on his and Lee's famous solution of the $\vartheta - \tau$ puzzle by abandoning left/right symmetry in weak interactions. In his Nobel Price lecture Salam recollects [32]:

I remember travelling back to London on an American Air Force (MATS) transport flight. Although I had been granted, for that night, the status of a Brigadier or a Field Marshal — I don't quite remember which — the plane was very uncomfortable, full of crying servicemen's children — that is, the children were crying, not the servicemen. I could not sleep. I kept reflecting on why Nature should violate left/right symmetry in weak interactions. Now the hallmark of most weak interactions was the involvement in radioactivity phenomena of Pauli's neutrino. While crossing over the Atlantic came back to me a deeply perceptive question about the neutrino which Professor Rudolf Peierls had asked when he was examining me for a Ph.D. a few years before. Peierls' question was: "The photon mass is zero because of Maxwell's principle of a gauge symmetry for electromagnetism; tell me, why is the neutrino mass zero?"

During that comfortless night he realized that Weyl's two-component equation for the neutrino would account for both parity violation and the masslessness of the neutrino. Soon afterwards he presented the idea to Peierls, who replied: "I do not believe left/right symmetry is violated in weak forces at all." After that, Salam was hoping to find more resonance at CERN. There he communicated the idea to Pauli, through Villars, who "returned the next day with a message of the Oracle: Give my regards to my friend Salam and tell him to think of something better."

Meanwhile parity violation was discovered and Salam got a kind, apologetic letter from Pauli. But this changed again soon afterwards. I quote:

Thinking that Pauli's spirit should by now be suitably crushed, I sent him two short notes (Salam, 1957b) I had written in the meantime. These contained suggestions to extend chiral symmetry to electrons and muons, assuming that their masses were a consequence of what has come to be known as dynamical spontaneous symmetry breaking. With chiral symmetry for electrons, muons, and neutrinos, the only mesons that could mediate weak decays of the muons would have to carry spin one. Reviving thus the notion of charged intermediate spin-one bosons, one could then postulate for these a type of gauge invariance which I called the "neutrino gauge". Pauli's reaction was swift and terrible. He wrote on 30 January 1957, then on 18 February and later on 11, 12 and 13 March: "I am reading (along the shores of Lake Zurich) in bright sunshine quietly your paper . . ." "I am very much startled on the title of your paper 'Universal Fermi Interaction' . . . For quite a while I have for myself the rule if a theoretician says

universal it just means pure nonsense. This holds particularly in connection with the Fermi interaction, but otherwise too, and now you too, Brutus, my son, come with this word . . .” Earlier, on 30 January, he had written: “There is a similarity between this type of gauge invariance and that which was published by Yang and Mills... In the latter, of course, no γ_5 was used in the exponent,” and he gave me the full reference of Yang and Mills’ paper [18]. I quote from this letter: “However, there are dark points in your paper regarding the vector field B_μ . If the rest mass is infinite (or very large), how can this be compatible with the gauge transformation $B_\mu \rightarrow B_\mu - \partial_\mu \Lambda$?” and he concludes his letter with the remark: “Every reader will realize that you deliberately conceal here something and will ask you the same questions.

6 Chiral Invariance and Universal $V-A$ Interaction

These recollections bring me to the last subject of my lecture. The two-component model of the neutrino paved also the way for a successful phenomenological description of weak interaction processes at low energies. In his masterly written review article “On the earlier and more recent history of the Neutrino” [33], Pauli remarks:

For some time I faced this particular model with a certain skepticism [42], since it seemed to me that the special role of the neutrino was emphasized too strongly. It turned out, however, that by further developing the ideas of Stech and Jensen (see §3 above) the model allowed an interesting generalization for the form of the interaction energy for all weak interactions.

After an inventory of the experimental situation, mentioning in particular the new recoil experiments on ${}^6\text{He}$, Pauli continues with:

Based on the Stech-Jensen transformation and the two-component model of the neutrino the following postulate suggests itself for the theoretical interpretation: The Hamiltonian of each weak 4-fermion interaction shall “universally” contain either only R or only L components of the involved fermions. Equivalent to this postulate is the formulation that in the transformation $\psi' = \gamma_5 \psi$ the density of the interaction energy for each particle separately should “universally” remain unchanged or change its sign.

At this point the classical papers [34] are quoted, followed by the statement:

The Stech-Jensen transformation referred to a pair of the particles simultaneously while the two-component model of the neutrino is equivalent to the validity of the result of the transformation for the neutrino alone. The postulate of the extended Stech-Jensen transformation now under discussion is therefore a generalization of the two-component model of the neutrino.

As we all know this postulate leads uniquely to the universal $V-A$ interaction. At the time it was disturbing that the V and A interaction strengths for nucleons in beta decay are empirically not equal. Today we know that the equality does hold on the level of the quark fields.

It is, unfortunately, not generally known that W. Theis proposed independently the parity violating $V-A$ interaction in a paper submitted on 20 December 1957 to the *Zeitschrift für Physik* [35]. Theis emphasized that in the spinor calculus a Dirac spinor can be expressed in terms of a single two-component Weyl spinor

$$\psi = \left(\begin{array}{c} \phi_\alpha \\ \frac{i}{m} \partial^{\alpha\dot{\beta}} \phi_\alpha \end{array} \right), \quad (6.1)$$

and that the Dirac equation is then equivalent to the Klein-Gordon equation for ϕ_α . Since in this representation ψ contains derivatives, the author finds Fermi's requirement of a derivative-free coupling not so convincing and requires instead a derivative-free four-Fermi interaction for the Weyl spinors. This allows for only one possibility, namely

$$p_\alpha^* n_{\dot{\beta}} e^{*\alpha} \nu^{\dot{\beta}} + \text{h.c.}, \quad (6.2)$$

which is just the $V-A$ coupling.

This formal argument is similar to the one in the classic paper by Feynman and Gell-Mann [34]. The latter goes, however, beyond the $V-A$ interaction and advocates a current-current interaction Lagrangian, containing also hypothetical self-terms. These imply processes like neutrino-electron scattering or the annihilation process $e^- + e^+ \rightarrow \nu + \bar{\nu}$, which was soon recognized to be very important in the later evolutionary stages of massive stars [36]. (We have heard a lot about this during the school.)

It may also not be known to the young generation that various experiments⁷ were in conflict with chiral invariance at the time when Feynman and Gell-Mann wrote their paper. They had the courage to question the correctness of these experiments:

These theoretical arguments seem to the authors to be strong enough to suggest that the disagreement with the ${}^6\text{He}$ recoil experiment and with some other less accurate experiments indicates that these experiments are wrong. The $\pi \rightarrow e + \bar{\nu}$ problem may have a more subtle solution.

The later verification of the prediction for the ratio $\Gamma(\pi \rightarrow e\nu)/\Gamma(\pi \rightarrow \mu\nu)$ was one of the triumphs of the universal $V-A$ interaction.

We will certainly hear more from J. Steinberger about the experimental side of the story.

⁷For a description of the classic experiments, I refer to an excellent paper by Telegdi [37].

7 Epilogue

The developments after 1958 consisted in the gradual recognition that — contrary to phenomenological appearances — Yang-Mills gauge theory can describe weak and strong interactions. This important step was again very difficult, with many hurdles to overcome.

One of them was the mass problem which was solved, perhaps in a preliminary way, through spontaneous symmetry breaking. Of critical significance was the recognition that spontaneously broken gauge theories are renormalizable. On the experimental side the discovery and intensive investigation of the neutral current was, of course, extremely crucial. For the gauge description of the strong interactions, the discovery of asymptotic freedom was decisive. That the $SU(3)$ color group should be gauged was also not at all obvious. And then there was the confinement idea which explains why quarks and gluons do not exist as free particles. All this is described in numerous modern text books and does not have to be repeated.

The next step of creating a more unified theory of the basic interactions will probably be much more difficult. All major theoretical developments of the last twenty years, such as grand unification, supergravity and supersymmetric string theory are almost completely separated from experience. There is a great danger that theoreticians get lost in pure speculations. Like in the first unification proposal of Hermann Weyl they may create beautiful and highly relevant mathematics which does, however, not describe nature. Remember what Weyl wrote to C. Seelig in his late years:

Einstein glaubt, dass auf diesem Gebiet die Kluft zwischen Idee und Erfahrung so gross ist, dass nur der Weg der mathematischen Spekulation (...) Aussicht auf Erfolg hat, während mein Vertrauen in die reine Spekulation gesunken ist ...

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