9

The Path to Renormalizability

MARTINUS VELTMAN

Born Waalwijk, The Netherlands, 1931; Ph.D., 1963 (physics), University of Utrecht; Professor of Physics at the University of Michigan; high-energy physics (theory).

This is the history of the proof of renormalizability of gauge theories as I perceive it. It is a personal account.

The importance of the proof of renormalizability is well known to all. Personally I have always felt that the proof was much more important than the actual construction of a model, the Standard Model. I felt that, once you knew the recipe, the road to a realistic description of Nature would be a matter of time and experiment. There some may disagree with me; I think, however, that a careful study of the recent history of high-energy physics will lead to this conclusion. Seldom has there been such a clear watershed. Old models, truly "dormant" (as Steven Weinberg put it), became credible and popular. Quantum chromodynamics came into being almost overnight. The proof of renormalizability also provided detailed technical methods such as, for example, suitable regularization methods, next to indispensable for any practical application of the theory. In longer perspective, the developments in supersymmetry and supergravity have been stimulated and enhanced by the renewed respectability of renormalizable field theory (including the absence of anomalies). If anything "turned the wheel," as SLAC people have put it, it is this proof of renormalizability. Of course, the theory needs experimental verification, and whether people were convinced after the discovery of neutral currents, or after the discovery of charm, or W and Z, is another matter.

Whatever one may argue, there can be little disagreement on the importance of renormalizability with respect to quantitative understanding. Radiative corrections such as, for example, the vector boson mass shifts can and have been computed and measured.[1] The stunning agreement between theory and experiment reinforces the belief in our theoretical insights and prepares the way for further progress. Thus very importantly, the detailed quantitative understanding of the theory has influenced the direction of present-day research; for example, the realization that radiative corrections to W-pair production may provide essential information on the Higgs sector has pushed the design energy of LEP to well over the W-pair threshold. The design and construction of very-high-energy hadron colliders owes much to the concept of a second threshold, related to the very existence of the Higgs sector. Progress comes from a full and detailed understanding, and for that renormalizability is essential.

Technical introduction

A very abbreviated technical introduction may be helpful, and if nothing else, serve as a background for the discussion to come.

In present-day field theory the starting point is a Lagrangian. This Lagrangian, somehow, defines an S-matrix. The square of the absolute values of the S-matrix elements are the link to physical reality: they are transition probabilities. A transition probability specifies the chances of observing a certain configuration at time plus infinity given a certain configuration at minus infinity. These configurations are systems of particles, supposedly so far removed from each other that they can be taken to be free particles. In other words, S-matrix elements, when squared, describe the transition probability of a system of free particles to collide and emerge as another system of free particles.

The key word here is "free." A free particle is one whose energy is kinetic only; that is, it can be computed if the momentum is known: $E^2 = \vec{p}^{\,2} + m^2$. A particle for which energy and momentum satisfy this relationship is said to be "on the mass shell." The S-matrix refers to initial and final states whose particles are all on the mass shell.

Very roughly speaking, Green's functions are S-matrix elements extrapolated to off-mass-shell values of energy and momentum for the incoming and final particles. By themselves, Green's functions have no special physical significance. All physics is contained in the (on-mass shell) S-matrix. However, the renormalization procedure needs Green's functions with certain requirements of smoothness with respect to the extrapolation mentioned.

Because the S-matrix really contains the relevant physics, it must satisfy a number of basic requirements. It must be Lorentz invariant, and it must conserve probability, to name two important requirements.

In practice, and in any case in the context of this overview, the important property is unitarity, implying conservation of probability.

The relation between Lagrangian and S-matrix is somewhat nebulous, to say the least. There are two formalisms that span the bridge from Lagrangian to S-matrix. Certain desirable properties of the S-matrix may be guaranteed by the formalism. These two formalisms are the canonical operator formalism and the path-integral formalism.

The derivation using the operator formalism guarantees unitarity of the S-matrix, but there are difficulties with Lorentz invariance. The path-integral formalism has its own troubles, one of them being that the interpretation and relation with physics is somewhat mystical. The most important shortcoming is that the formalism has no guarantee with respect to unitarity. Therefore, within that formalism, the proof of unitarity must be specified separately, something that in any case was taken seriously by Feynman, the inventor of the path-integral method.

Both formalisms need completely ad hoc modifications in the case of gauge theories, that is, if the Lagrangian obeys a gauge invariance. The path-integral formalism can be modified in a rather elegant way, but the operator formalism remains rather ugly.

The result of these derivations is in terms of Feynman rules. These rules allow one to calculate S-matrix elements. As a matter of fact, these rules define Green's functions as well. Green's functions generally contain infinities, to be removed by means of some subtraction procedure (renormalization). This requires some scheme for handling those infinities (regularization), and today most physicists use the scheme of dimensional regularization. It is invoked at the very last stage of perturbation theory. Interestingly, that scheme cannot be formulated within the operator or path-integral formalisms. That makes the formalisms appear even more artificial.

As it happens, for a given Lagrangian, the S-matrix is generally unique (insisting on unitarity, etc.), but the extrapolation to Green's functions is not. In other words, for a given Lagrangian there may be different sets of Feynman rules, defining different Green's functions but always the same (unitary) S-matrix. Since the renormalization procedure needs Green's functions, one may be able to carry the renormalization procedure through in some sets of Feynman rules, but not in others. Unitarity remains an important constraint; today's prescription for gauge choosing is such that unitarity is satisfied.

If one is not aware of the possibility of different Green's functions for a given Lagrangian, then it is quite possible to "prove" that some theory is nonrenormalizable if one happens to work with an unfortunate set of Green's functions.

The "proof of renormalizability" amounts then to the following:

- Understanding fully the relationship between Lagrangian and Feynman rules, or rather possible sets of Feynman rules. These rules must be such that the resulting S-matrix is unitary.
- Identifying those Lagrangians that have at least one set of renormalizable Feynman rules.

Certain theories, seemingly renormalizable, turn out not to be, owing to anomalies. Such theories are to be avoided, as indeed Nature seems to do. The Standard Model has no anomalies, or so we think. Knowing about anomalies is important when constructing models. A crucial instrument here is the regulator method. The dimensional regularization method provides us with knowledge as to which theories have anomalies and where they might be. As such, one might consider the construction of a regulator method as part of the proof of renormalizability.

Lacking a good regulator method, one needs Ward identities and must try to renormalize such that the symmetries are not broken in the process, that is, such that the Ward identities remain valid. That is one reason why Ward identities played a much larger role in the beginning. Renormalization is then very complicated, especially if anomalies may occur.

Modern physicists are usually not overly concerned about the apparent lack of any complete and consistent formalism leading from Lagrangian to the S-matrix. As long as the Feynman rules are known, and the resulting S-matrix can be shown to be satisfactory, one tends to say, with Alfred E. Neuman: "What, me worry?"

Old views

It is necessary to recall some of the notions of field theory and renormalizability as understood in the fifties.

Field theory itself was understood in terms of canonical field theory involving operators, interaction Hamiltonians, and the like. Thus, unitarity was something that you understood from the formal expression for the S-matrix in terms of that interaction Hamiltonian. Studying field theory meant manipulating operators and applying subsidiary conditions to state vectors. Of course, no one understood fully the question of gauge invariance in quantum electrodynamics. The Gupta–Bleuler method is a very partial solution. The Stueckelberg formalism enjoyed some popularity in studying massive electrodynamics, and there were some attempts to generalize it to the non-Abelian case.[2] There existed some quaint concepts, such as the path-integral formalism, and there were things such as functional methods; these were known to some students of Schwinger and a very small set of mathematically oriented people. Gauge invariance was not understood within these frameworks either. Mostly physicists thought that gauge invariance meant that you could arbitrarily change the longitudinal part of the vector boson propagator.

The first thing is that people were generally not that well aware that renormalizability required Green's functions rather than the S-matrix, and that there is quite some arbitrariness in Green's functions for a given theory. That has nothing to do with gauge invariance; a simple canonical transformation of fields may turn a perfectly reasonable set of Feynman rules into an unrenormalizable mess. Let me emphasize: unrenormalizable. An example of that is a gauge theory in the physical (or unitary) gauge. That is an unrenormalizable theory. Even if you subtract the known (that is, known from the renormalizable version) infinities, you do not wind up with a finite theory. Green's functions have infinities all over the place. Only when you pass to the S-matrix do these infinities go away, assuming that your regularization method is quite perfect.

That arbitrariness was not generally understood. Canonical transformations were not part of the game yet. It was thought that once you demonstrated some bad infinity in some Green's function, then that constituted proof of nonrenormalizability. I will quote proof of this statement, by considering published work by Komar and Salam, and Glashow and Iliopoulos.[3] In fact, since 1948 the prevalent opinion was that charged massive vector-boson theories were hopelessly divergent.

Second, there was in people's minds a one-to-one relationship between infinities and physical quantities. Thus, a certain infinity related to the electron mass, another to the electric charge of the electron. That notion, in a subtle way, is not true in gauge theories. The relation is more abstract, and must be formulated differently. A theory has a certain number of free parameters, and an equal number of data points is needed to fix these parameters (to generally infinite values). The relation between infinities and parameters is more complex. A good example is the weak mixing angle. There is no clear-cut infinity related to that. The old concept of "bare" mixing angle versus "dressed" mixing angle is not appropriate. This point is not that relevant for the question of renormalizability, but I just take the opportunity to mention it. Infinities have lost their "physical" meaning. Not everybody has realized this. But you see, if you believe that infinities have something physical, then you need to find only one somewhere to demonstrate that a theory is bad. Certainly, you cannot gauge them away.

Unitarity

My first enterprise in field theory was the work for my predoctoral thesis (in Dutch, "scriptie"). The subject was the Lorentz condition, Gupta–Bleuler method, and so on. I do remember thinking that the choice of gauge seemed so much more limited in quantum theory as compared to classical theory. The work itself did not contain anything original, it was a review, as is generally the case with this type of thing.

After that, and military service, I started work for a doctoral thesis under the guidance of Leon Van Hove, then professor of theoretical physics in Utrecht. I remember that the question of renormalizability of weak interactions came to the forefront as a consequence of the V–A theory. That theory revived the intermediate vector-boson hypothesis, and many theorists at that time tried to prove renormalizability, without success. A published example is an article by Glashow, claiming renormalizability.[4]

At a Scottish summer school in 1960,[5] without fully realizing it, I learned something very important from people such as Chew and Jackson: you do not have to know a Hamiltonian to do S-matrix theory. It is quite possible to study the S-matrix all by itself, and then the important thing is that you establish causality and unitarity (but how?). At that school, inspired by Chew's lecture, and after discussions with Derek Robinson, I decided that I wanted to investigate unitarity in a theory containing unstable particles. I remember, in fact, that I had the mistaken idea that something in Chew's vision was wrong there. Often, you start from something wrong. I discussed this with Van Hove, who thought that this could be an appropriate subject for a thesis.

In the end (1961), the work on unitarity of the S-matrix and unstable particles worked out very well.[6] There was already some work by Cutkosky, and that was usually quoted in this context.[7] My proof of unitarity was, I think, simple and elegant. I learned a lot from it. First, I did not really worry about interaction Hamiltonians, but started essentially directly from the S-matrix, defined in terms of diagrams. Then I proved unitarity, and also some form of causality (more or less following the formulation of Bogoliubov) for this S-matrix. Unitarity and causality of the S-matrix as functions of the Feynman rules became transparent to me. Bye-bye, operator formalism.

I want to pause for a moment here to emphasize these points, as I consider them crucial. One may consider a theory from the point of view of the canonical formalism, or from the point of view of path integrals, or, as I have done since then, just from the point of view of diagrams. For gauge theories it is very difficult to derive Feynman rules using the canonical formalism; in the path-integral formulation one is unsure about unitarity, not to speak of difficulties with fermions. To treat unstable particles correctly one must make a partial summation of perturbation theory, and that cannot be done easily in the canonical or path-integral formalism. In my world, looking directly at diagrams, these problems simply did not exist. Mind you, certain things are easier in the canonical formalism or with path integrals. To me, however, they are merely convenient heuristic tools to guess properties of diagrams.

Feynman, in his famous talk in Poland, also studied unitarity, trying to understand it directly from the Feynman rules.[8] He succeeded up to one loop. I did not see that paper till 1968; more about it later.

Going to CERN in 1961, I completed my thesis with a study of Coulomb corrections to W-production (a Dutch thesis tends to be a weighty affair).[9]

From that moment on till 1966 I involved myself mostly with phenomenological things. The only field-theoretical work that I would like to quote from that time are the works of Lee and Yang, and Lee on Feynman rules for vector bosons and the ξ-limiting formalism.[10] Anyone interested in understanding the state of affairs concerning Feynman rules for vector bosons should consult these papers. They were quite complicated. The first version of my algebraic program Schoonschip (December 1963) was actually aimed at extending the work of Lee on charged vector-boson interactions with photons. I computed symbolically the triangle diagram, with as free parameter the vector-boson magnetic moment, which is, unlike the electron magnetic moment, not fixed by the principle of minimal electromagnetic interactions (replacement of ∂_μ by $D_\mu = \partial_\mu - ieA_\mu$).

At the end, toying around with this, I established for which value of the magnetic moment the divergences would be minimal. The outcome was the value suggested by a Yang–Mills theory; that is, the W-photon vertex became the very familiar Yang–Mills vertex.[11] I did not know

Yang–Mills theory at the time, but I certainly remembered that vertex, and recognizing that was a factor in my later decision to study renormalization of Yang–Mills theories. Nothing of that work was published, but Schoonschip became an important tool.

Divergence conditions

In 1966 Gell-Mann's current algebra and Schwinger terms were the hot topic of the day, and I decided to try to understand the issue.[12] I succeeded in that, at least as far as Schwinger terms were concerned. The result of this work was a set of equations, simple extensions of the CVC (conserved vector current) and PCAC equations.[13] The equations were extended to include higher order weak and electromagnetic effects by means of the replacements ∂_μ by $\partial_\mu - W_\mu \times$ in those equations. The equations came about in a two-step process: first the minimal electromagnetic replacement, and then the extension to include vector bosons along the lines of the CVC hypothesis itself. I did not really understand what I was doing, but at least I could derive the Adler–Weisberger relation from the divergence equations without Schwinger-term ambiguities.[14] I felt quite happy about that.

Then I went to the 1966 high-energy physics conference in Berkeley and was told that Feynman had worked on the issue of Schwinger terms. Naturally I looked up Feynman and we spent an amusing hour or so discussing this point. He opened his notebook, and it turned out that he had done much the same thing as I had. My work was in fact already published, or at least on the way; Feynman never published his results. I think that he was busy working further toward Yang–Mills theories, but I am not sure, and I forgot to ask about that later.

In my view, my divergence equations resolved the issue of Schwinger terms. It was never much recognized as such; other people eventually did more or less the same and were more successful in advocating their views. I do not hold that to be very important at this point. The main issue, as far as I was concerned, is that these Schwinger terms were the result of fancy operator manipulations, not really of much physical interest. More importantly, the divergence equations, as I called the extensions of CVC and PCAC, were things that you could apply directly to diagrams. You did not have to know about operators.

As an amusing anecdote I may mention that some time later, in Utrecht, Ward visited me in my office. He sternly told me that my divergence equations were really Ward identities, and then he left again, leaving me bewildered in my office. He was right, of course, but I did not know what he was talking about.

At that time I left CERN for Utrecht. Preparing for a talk at the Royal Society in London, November 1966, I tried to make it transparent how divergence conditions could be used to derive interesting results.[15] In the process I discovered that neutral pion decay into photons was forbidden, and that was reported in London. I thought the proof entirely trivial and did not report details. I actually tried to change PCAC so as to repair this forbiddenness, and at first introduced the same term as known now, deducing its coefficient from the observed decay rate. I remember being astonished at it being a remarkable multiple of the fine-structure constant. I rejected the term, because it did not cure η-decay in three pions, also forbidden, as demonstrated before by Sutherland.[16] I cooked up some other term and reported that in London. I gave one seminar about the above, at SLAC, but most likely nobody remembers. I do remember discussing things with Treiman at that occasion.

John Bell, in the audience in London, picked up this remark on pion decay, and going back to CERN he tried to understand it. I will not go into details here; he thought that I was wrong, we had a correspondence and several telephone conversations, but somehow he did not accept my argument. Instead, with the help of Sutherland, a proof using current commutators came about.[17] As you may know, this led ultimately to the discovery of the anomaly.[18] Adler followed some other road, also discovering the anomaly.[19] My argument, incidentally, is the same as that found in Adler's paper.

Adler–Weisberger relations and gauge invariance

In 1966, as a reaction to my paper on divergence conditions, John Bell presented a more formal derivation of the divergence equations.[20] Gauge transformations were the starting point of his considerations. In a very transparent way this made it clear to me that the successes of current algebra, notably the Adler–Weisberger relation, must be considered a consequence of gauge invariance. In the spring of 1968, while spending a month at Rockefeller University, I tried to think it through. Because of the success of the Adler–Weisberger relation, I considered it an experimentally proven fact that currents satisfy divergence conditions (or current-commutation rules, if you prefer). Divergence conditions follow from gauge invariance. Why would Nature choose its currents so that they satisfied divergence conditions (or current-commutation relations)? Since I firmly believed in vector bosons, the question then was: why would Nature couple vector bosons according to the rules of a gauge symmetry? In light of the old vector-boson magnetic moment question, the (to me) obvious answer was: because that makes the theory renormalizable. In other words, I interpreted the Adler–Weisberger relation as experimental evidence for the renormalizability of a vector-boson theory of weak interactions. I have put this argument several times in print.[21]

Actually, Gell-Mann's commutation rules were, as far as I understand, constructed according to the recipe: start with a Yang–Mills theory of vector mesons, then take away the vector bosons. What is left are currents satisfying certain current-commutation rules. One could say that I thus reinstated the vector bosons, after experiment verified the correctness of the commutation rules.

This was my physics reason for entering the field of Yang–Mills theories. I repeated that argument to myself an untold number of times, with invariably the same conclusion: weak interactions must be some renormalizable Yang–Mills theory. It has to be.

Massive Yang–Mills

Thus I started to study vector bosons interacting with fermions, with in addition the vector boson self-couplings as specified by a Yang–Mills type of theory. More specifically, I started out with electrons and neutrinos. Things became very complicated very quickly and in no time I collected large amounts of diagrams, with an untransparent number of canceling divergences. Thus I started simplifying, and as a first step I threw away the fermions, considering just the vector-boson interactions. Moreover, I decided not to worry about which gauge symmetry would be appropriate, but just took the simplest one, namely SU(2). I kept, however, a finite vector boson mass, even if the mass term violated the Yang–Mills symmetry. Certainly, the vector bosons were not massless, but my main motivation was that massless vector bosons had other unrelated problems, namely infrared divergences.

At that time I became a specialist in shelving problems: shelve fermions, shelve the symmetry group, shelve the neutral-current problem, in fact shelve all attempts at phenomenology. Renormalizability is the problem. If that is understood, the rest will follow. Later on I also shelved the anomaly problem, which I took as a great danger with respect to renormalizability.

The theory, at least one-loop diagrams, became more or less manageable. There were a lot of cancellations of infinities. I noted that there were many more cancellations if the external lines were kept on the mass shell. Trying to understand the cancellations in general became again hopelessly confusing. I therefore decided that somehow I had to transform the theory, changing the Feynman rules in such a way that the new rules incorporated the cancellations. Clearly, gauge invariance had a lot to do with the cancellations. But how to translate gauge invariance? It occurred to me that a change of gauge would perhaps be translatable into a change of Feynman rules. But how? I just did not know how to do that. Nobody knew.

Then I had an idea. Introduce a free scalar field, not interacting with the vector bosons. Now replace the vector field with some combination of vector field and scalar field; at the same time add vertices such that the scalar field remains a free field. Surely then the physics remains the same. But the Feynman rules for the new theory were different: the propagator for the W-field was replaced by the propagator for the combination, and that combination could be chosen so as to lead to less divergent Feynman rules. The price to be paid were the new vertices, and the new particle entered as a ghost (remember that it was a free particle). That is how ghosts entered into my scheme. I called the technique the free-field technique, and the transformation was named the Bell–Treiman transformation. Neither Bell nor Treiman was responsible.

It was a very crude beginning. But the idea was there, and the technique worked. Let me emphasize what I think was the main idea. I changed the Feynman rules before attempting to prove renormalizability. That was really the new thing. Second, I found a technique for changing the rules, that is, for changing gauge. Needless to say, I was not fully aware of the fact that I had done something new. To be clear, that I had done something new was obvious, because of the results. But I did not care too much about what precisely happened. After all, the new S-matrix was unitary and causal, so who cares where it comes from?

In 1968 I presented these first attempts at a Danish summer school. These notes were not published.[22]

In the autumn of 1968, tickled by the French "revolution," I took a sabbatical at Orsay, returning to Utrecht in August 1969. In Orsay I worked the argument to some more detail, profiting from discussions with Bouchiat, Boulware, and Mandelstam, and published the results.[23] The results were quite astonishing: Yang–Mills theory with an explicit mass term turned out to be one-loop renormalizable.

Perhaps the most important point of this paper is that it destroyed a myth, namely that charged massive vector boson theory is hopelessly divergent. To me, and some others, that stimulated further work and it just made me feel very sure that I was on the right track. You see, if you make some sophisticated reasoning and then things fall into place, there is after that nothing that will stop you. This is a very personal feeling; it is the moment of discovery.

The technical virtues of this paper that made this discovery possible will be discussed further below. They were also essential for the subsequent development of the theory. A barrier had been overcome.

Technical discussion

The Bell–Treiman transformation can briefly be described as follows. A canonical transformation is a change of variables, and physics (i.e., the S-matrix) is invariant for such a transformation (provided the transformation is local). Now let there be given an invariance, or partial invariance. Then make a canonical transformation corresponding to that invariance. Physics (but not Green's functions) must remain the same. That statement allows then derivation of identities among diagrams, and these are of course precisely the identities related to the symmetry of the theory.

Again, the invariance of the S-matrix is a consequence of the fact that it is a canonical transformation. It is a change of variables. That has nothing to do with gauge invariance. Then make a smart choice for this change of variables, thus exploiting gauge invariance.

Thus, a Bell–Treiman transformation is a canonical transformation that looks like a gauge transformation with the gauge parameter replaced by a field. I used in the above-described paper the finite form of the gauge transformations. As is well known from group theory, everything follows from the infinitesimal, and that is what I used in subsequent work.

The transformation is thus also useful if there are symmetry-breaking terms in the Lagrangian. They may be the mass terms, as above, or the gauge-breaking term. In particular, to derive generalized Ward identities (as I called them) it is advantageous to include source terms in the Lagrangian. Such terms are not invariant under the gauge symmetry.

If the field used in the Bell–Treiman transformation was a free field, that is what it remains. That is the free-field technique.

Fradkin and Tyutin extended the formalism to nonlocal transformations (local means depending only on fields located at the same point in space-time as the field being transformed).[24] That is useful if one wants to directly connect different gauges without using Ward identities.

Bell–Treiman transformations of one kind or another are now tools to derive the Ward identities of the theory. Those may be used for various purposes. The BRS transformation is the ultimate sophisticated example, the usage by 't Hooft is on the most elementary level.[25] A direct and ingenious example is the use by Slavnov in deriving the Slavnov–Taylor identities for the massless Yang–Mills theory.[26] His field is not a free field, but satisfies an equation such that also the Faddeev–Popov part of the Lagrangian is invariant. That has some analogy to classical electrodynamics: you chose a subsidiary condition, such as the Lorentz condition, and after that there remains a gauge invariance, namely invariance under gauge transformations with a function whose d'Alembertian is zero.

No one uses the name Bell–Treiman transformations these days. It is one of those things: when you do not know them you are stuck (as demonstrated by many), but once you have them you say "of course" (as also demonstrated by many). People usually call them simply gauge transformations, obscuring the fact that they are really canonical transformations, changes of variables, involving fields. Fields are replaced by combinations of fields.

The meaning of the statement "one-loop renormalizable" needs further clarification. To properly renormalize a theory with a symmetry, one needs to regularize in accordance with that symmetry. Here I had several problems. First, the theory did not have, strictly speaking, a symmetry. The mass term broke the Yang–Mills symmetry. Second, I did not have a reasonable regularization scheme. That I found particularly troublesome; I was not sure of the infinities. Now, the infinities that I found were those of a renormalizable theory with respect to power counting – that is, up to quadratic for the self-energies, linear for the three-point vertex, logarithmic for the four-point function. However, the divergences did not, at least in the crude way that I found them, obey the Yang–Mills symmetry. In that somewhat stricter sense the theory was not renormalizable.

I did not really worry very much about that point. Much more troublesome was the fact that I could not obtain similar results for two-loop diagrams. You might think, why should that be possible? The reason is that I had, or so I thought, an excellent argument. Here I have to digress a moment, to massless Yang–Mills theory.

Massless theory

Rather early in this development someone (I truly do not remember who; it was a one-day visitor to Rockefeller University) mentioned to me that Feynman had done something in this field.[27] Eventually I found out about this work, that is, the Polish lecture, and in addition discovered other relevant work on the subject. In Feynman's case, the subject was formally gravitation, but in actual fact the article contained also a discussion on Yang–Mills theory. Feynman's paper is not understandable if you do not already know the answer, but at least he made a clear statement: ghost loops were needed. He could do it only for one-loop diagrams. That was partly due to his way of understanding unitarity, and partly, I think, because he really did not study the massless case but rather the massive case in the limit of zero mass. I find it hard to tell whether Feynman obtained the correct one-loop rules for either gravity or massless Yang–Mills fields. There are not enough details. But I think he obtained those of the massive case.

The main consequence of Feynman's article is that it inspired a few physicists to study the question more precisely. Bryce DeWitt made a monumental effort and established the correct rules for gravitation in the Feynman gauge (no momenta in the numerator of the propagator).[28] To be frank, I am not entirely sure, because his papers are very complicated and I have not really digested them. But the rules seem to be there, with the correct ghost. The correct ghost for this case is a ghost with an arrow. The arrow is not mentioned by Feynman.

Faddeev and Popov, starting with whatever they saw in Feynman's work, found the ghost rules using the now-familiar argument involving path integrals.[29] They found it for one particular gauge, the Landau gauge, and their argument leaves the question of unitarity unanswered. Since the ghost is really there for reasons of unitarity, that is an important question. The Faddeev–Popov procedure amounts to gauge breaking in a certain way, and it is nontrivial that the way chosen provides for a unitary result. Other methods are needed to establish that.

I was editor of *Physics Letters* (1966–1968) when the Faddeev–Popov article arrived. Of course, it was totally incomprehensible to me, being about path integrals. But I felt that Faddeev was man enough to be responsible for his own work, and I accepted it without further ado.

Another physicist working his way through the massless Yang–Mills theory was Mandelstam.[30] He used his own formalism, and his results agreed with those of DeWitt and Faddeev and Popov. In other words, at this time (June 1968) the rules for the massless case were known, at least for some specified choices of gauge. The rules were those of a renormalizable theory, at least by power counting. Whether the infinities obeyed the symmetry was not clear. On top of this there were of course the infrared problems.

Now here is the "excellent" argument mentioned above. The non-renormalizability of the massive Yang–Mills theory relative to the massless theory is a direct consequence of the form of the vector-boson propagator. In the massive case there is the extra term $p_\mu p_\nu / M^2$ in the numerator of the propagator. This term causes all the problems. It is simply absent in the massless case. Now, let us assume that the limit of zero mass exists for the massive case. After all, it is only one term in the Lagrangian, not appearing very menacing. If the limit of zero mass exists, then obviously the extra term in the propagator must behave reasonably. This means that the product of the two momenta must be equivalent to something that behaves as M^2.

Now why could I not get through for the two-loop diagrams? I could not understand that. There were terms blowing up in the limit of zero mass. But how could that be? I assumed that it might have something to do with the perturbation expansion, and I mentioned that in an appendix.

Reactions

There were two immediate reactions to my article. The first was from Salam, who stated that "somewhat similar work" had been done by Komar and himself.[31] I looked it up and found that they had proven that massive Yang–Mills theory was not renormalizable. The proof was a calculation of the three vertex, in the one-loop approximation, in the unitary gauge. Indeed, that is nonrenormalizable. Let me, however, mention that Salam was perhaps the only one who realized the fact that I first transformed the rules and only then considered renormalizability. He appreciated this progress, and he has stated that to me at some occasion. In fact, if you read his Nobel Symposium article, you see that he and Ward (and presumably before him Higgs and Kibble) were on the way.[32] As he put it, he did not have the dictionary to go from one gauge to the other. The missing part is the idea that Green's functions may change, as long as the S-matrix remains the same. The dictionary works only on mass shell. That is what he always told me: "Ah, Veltman, the mass shell." I usually felt hollow after that remark. I did not read this lecture until much later. But frankly, who had read this before 1971? The proceedings of the Nobel Symposium is not a particularly popular channel of communication.

The other reaction was a letter from David Boulware. He succeeded in summing the series that I mentioned in my appendix, with the result that it was still singular in the limit of zero mass. He added a comment stating that he did not know how serious this was. Neither did I.

Subsequently Boulware, working within the path-integral formalism, confirmed my results up to one loop.[33] His formulation was general, not restricted to one loop; his result for two or more loops was that the theory was not renormalizable. The essential technique is the path-integral version of what I called the free-field technique. I looked upon this with some suspicion, largely because of my unfamiliarity with the path-integral formalism, and also because within that formalism unitarity is not obvious; this suspicion was here unjustified. It was a good paper that very probably influenced Russian authors.

The zero-mass limit

I now set out to look for gaps in the argument. There was the question of the Feynman rules themselves, more precisely the relation between the Lagrangian and Feynman rules in the canonical formalism. This rather old problem concerns the handling of derivatives in the Lagrangian; vector boson Lagrangians have many derivatives. It was first tackled in connection with the case of pion–nucleon interactions, axial-vector coupling, by Matthews.[34] As cited above, Lee and Yang also considered this problem.[35] Actually, the difficulties encountered when following the usual canonical procedure are rather similar to Schwinger-term problems. We (this work was done with J. Reiff, then a graduate student) discovered a very simple way of settling this problem, leaving no doubt as to the correctness of the Feynman rules in the unitary gauge.[36]

Next, we started on the two-loop problem. We investigated the two-loop self-energy diagrams, using the rules that were valid at the one-loop level. We established that the result definitely violated unitarity. In other words, it really seemed that the limit of zero mass did not result in the massless theory. This was reported first at a conference at CERN, in January 1969.[37] It was also clear that an extra vertex had to be added at the two-loop level; this vertex was of a nonrenormalizable type. It was now definitely clear that the limit of zero mass was not the massless theory, as the extra term contained a factor $1/M$. It left me confused for quite some time. Clearly, I needed more understanding of the unitarity problem and the rules at the two-loop level.

In 1969 a visitor and old friend of mine, H. Van Dam from North Carolina, lectured on Schwinger's source formalism. In addition he imported a certain amount of enthusiasm for gravity. Both were crucial to the next development, as will become clear. Let me first, however, describe what happened with respect to the zero-mass limit problem.

By this time the Russians were entering the arena. I will enlarge upon that shortly, but for now I would like to mention the work of Slavnov and Faddeev, following up on the work of Boulware; their presentation of the essentials made the work more transparent, at least to me.[38] They refer to an unpublished article by Slavnov (of which I have a preprint) written in response to mine. It contains a treatment of the massive case, but the conclusion, namely that the massive Yang–Mills theory was renormalizable, is wrong.

Treating massive and massless theories on the one-loop level, Faddeev and Slavnov noted that there was a factor of 2 difference between the contribution of a ghost one-loop diagram in the massive case as compared with the massless case. I do remember discussions on that issue in Orsay, involving Boulware and Mandelstam. But I think none of us noted this factor. To me the observation came through a discussion with Bruno Zumino, quoting Faddeev and Slavnov.

Thus there was now also an explicit difference between the massive and massless case at the one-loop level. The factor of 2 relates to the fact that the ghost of the massive case has no arrow, while the massless ghost does. Proper symmetrization requires a factor of $\frac{1}{2}$ for the massive case.

For the experts: the rules for the massive case were much as we have them today. There are now two ghosts, the Faddeev–Popov ghost with a factor -1, and a Higgs ghost with a factor $+\frac{1}{2}$. They combine to what I had for the massive case, a diagram with a factor $-\frac{1}{2}$. To some extent that reasoning is in the Slavnov–Faddeev article.

At the one-loop level, by this time, things were transparent. It now was a matter of carefully analyzing the situation, and it became clear that indeed, the massless case is not the limit of the massive case, simply because the longitudinal polarization of the massive vector boson is not decoupling in the limit of zero mass. It sounds trivial at this time, but it really was a shocking thing. How shocking was clearly demonstrated in a paper that Van Dam and I published.[39] It so happens that a similar phenomenon occurs for gravitation, and we demonstrated that the limit of zero-mass gravity is not the same as zero-mass gravity. The difference is non-zero, already at the tree level. From the observed deflection of light by the sun and the perihelion movement of Mercury, one can then deduce that gravitation is strictly zero mass. No mass, however small, is allowed. Gravitation is not limited in range, not even at distances on the galactic scale or beyond. Many people refused to accept this result; a learned colleague in the Netherlands put it in Latin: *"Natura non facit saltum."*

This insight cheered me up. While there was clearly trouble at the two-loop level, at least the situation had become clear. And mind you, having already one-loop renormalizability is not bad. In fact, this is all you need to cover the experimental situation of today. But it was the principle that I was after.

Ward identities

The problem of unitarity of a Yang–Mills theory is more complex than in non-gauge theories. In a renormalizable gauge the Feynman rules are not manifestly unitary; there are ghosts and one must show that the contributions of the ghosts vanish. This requires the use of Ward identities. The problems that presented themselves at this stage were these:

- What are the precise rules for two or more loops?
- What are the Ward identities?

For quite some time I did not know how to handle that. Then I started to use sources, undoubtedly inspired by Van Dam's lectures on Schwinger's formalism, although I thought that I did it by myself. It often goes that way. This turned out to be a really big step forward. The derivation of Ward identities became easy; these Ward identities for the non-Abelian off-mass-shell case, later called Slavnov–Taylor identities, could then be used to work out two-loop diagrams.[40] (To avoid misunderstanding: Slavnov and Taylor derived these identities for the massless theory, which is not what I was dealing with at that time.) This removed all doubts concerning unitarity and renormalizability. The paper in which these results were published was actually written before

I understood the zero-mass limit problem.[41] In August 1970 I fully understood the massless and massive case. What now?

In the autumn of 1970 I was pondering these results. I developed the concept of a cancelable divergence: a divergence that can be canceled by a physical particle with legitimate interactions, as compared to a non-cancelable divergence that needs a particle of indefinite metric, or wrong statistics, and so on. Then I decided to somehow subtract the massless case (but with a mass inserted in the denominator of the propagator) from the massive case and see if the resulting divergences could be canceled by a physical interaction. This of course is not legitimate history, since I cannot prove that I was drifting along these lines. It is possible that I would have discovered the Higgs particle this way. It is also possible that I would have remembered Glashow's remark (see below) and tried spontaneous symmetry breakdown. All that is irrelevant. At this time 't Hooft entered in the field, and things resolved themselves in another way.

Other work

It is first necessary to paint a picture of mainstream physics in the period 1967–1971. The Adler–Weisberger relations marked the last successes of field theory; weak interactions appeared more nonrenormalizable than ever; and people turned to other methods. Most physicists considered the anomaly only as a modification of PCAC. Effective Lagrangians, low-energy theorems, Regge poles were at the center of interest. Popular opinion was that charged vector-boson theories were nonrenormalizable, and the very idea of working on such theories marked you as halfway to insanity. Thus working on Yang–Mills theories was considered far out, and many a remark in that sense came to me. Some persisted in field theory, and their contributions survived and are well known today, but they were few. Not the least among them were those that kept on doing calculations in QED, thereby fortifying the idea of renormalizability.

In the period mentioned I was a regular visitor to the Orsay group (now the theory group at the École Normale in Paris). Every summer they organized a summer institute; Bouchiat and Meyer, and later also Iliopoulos were the hosts. Regular visitors included Coleman and Glashow, and I attended every summer. Every year I reported the latest on the subject of Yang–Mills theories. After a few years Coleman once expressed his doubts: "Tini, you are just sweeping an odd corner of weak interactions."

However, not all reacted this way. I do remember positive comments from Glashow. First, he apparently read my Copenhagen lectures, notably the part involving Cabibbo matrices and neutral strangeness-changing currents; he said something like "I see that you have also been working on this problem." Also, somewhere in 1969 or 1970 he said to me, "You should try to get masses by means of spontaneous symmetry breaking," to which I answered, "I am not yet ready for that." While his remark kept on spooking through my head, I somehow never did it. Psychologists may see something here.

I firmly believe that my work of 1968 was the stimulus to the work of Glashow, Iliopoulos, and Maiani.[42] There is even a reference to this effect, although that same reference tries to weaken the case. In footnote 12, after referring to various papers, mine among them, they write: "Note however, that none of these references consider the far more difficult case of vector mesons coupled to non-conserved currents."

A few months after the GIM papers, Glashow and Iliopoulos decided to clear up the massive Yang–Mills case.[43] I quote here their footnote 4, referring to my papers (the second reference should have been to *Nucl. Phys. B21*):

> For an extensive discussion of the problem of divergences in massive Yang–Mills theory, see M. Veltman, *Nucl. Phys. B7*, 637 (1968); *B20*, 288 (1970). In the last reference a set of on-mass-shell Ward identities has been obtained which are used to analyze Feynman diagrams. The cancellations of divergences found by M. Veltman go beyond the theorem proven in this paper, but they only apply to on-mass-shell amplitudes and they depend on the assumption that the W's are coupled to conserved currents. For alternative discussions see D. Boulware, *Ann. Phys.* (N.Y.) *56*, 140 (1970), and S. K. Wong, this issue, *Phys. Rev. D3*, 945 (1971).

This footnote shows that they had not understood the fundamental point: you first change gauge, then consider renormalizablity. I quote this mainly to show that this was not a trivial thing, but that it truly prevented people from discovering renormalizability.

Let me comment on, again, this reference to nonconserved currents. That problem is not and never was any more serious than the nonvanishing of the W-mass. If they had applied my technique to the fermion–W coupling they would have seen that; it is quite trivial, and I knew it. They never asked me about it. Today we use the same Higgs to cure both problems. In the fermion sector, in the limit of large Higgs mass, at the one-loop level there is a term logarithmic in the Higgs mass. That is what they would have found had they used my technique: a logarithmic divergence. Precisely like the four-point W vertex. I have used that information later to locate effects that go to infinity as the Higgs mass becomes large, as such effects might be used to deduce the Higgs mass from experiment.[44] The "screening theorem" reflects the fact that all such effects are logarithmic in the Higgs mass.

This is perhaps the moment to cite Weinberg's unpublished attempts at renormalizing his model. This was in his "dormant" period. I quote from his Nobel lecture:[45]

> The next question now was renormalizability. The Feynman rules for Yang–Mills theories ... had been worked out by deWitt, Faddeev and Popov and others, and it was known that such theories are renormalizable. But in 1967 I did not know how to prove that this renormalizability was not spoiled by the spontaneous symmetry breaking. I worked on the problem on and off for several years, partly in collaboration with students, but I made little progress. With hindsight, my main difficulty was that in quantizing the vector fields I adopted a gauge now known as the unitary gauge: this gauge has several wonderful advantages, it exhibits the true particle spectrum of the theory, but it has the disadvantage of making renormalizability totally obscure.

Russian work

Russian authors, notably Fradkin and Tyutin, contributed in an important way to the subject. They, obviously well informed on the subject of path integrals, introduced and extended my techniques in that context. Eventually, Fradkin and Tyutin established a procedure by which one obtains the Feynman rules for a general gauge. The ghost part of the Lagrangian was still written in terms of a determinant, as in the work of Faddeev and Popov.[46] Initially Fradkin and Tyutin thought that massive Yang–Mills theories were renormalizable; I have not tried to trace the mistake.[47] I think that they bought my argument and Slavnov's work on the limit to zero mass. In 1970 Fradkin and Tyutin published a paper applying to massless Yang–Mills theories and gravitation (i.e., theories without any symmetry breaking).[48] Let me quote from their introduction: "The basic idea of the method proposed is to choose the Lagrange multiplier in such a way that one is led to free equations of motion for the additional field." With this work one could write Feynman rules for different choices of gauge. They did it for a number of gauges.

Utrecht

In 1966 I began teaching in Utrecht, and in the period until 1971 several students (J. Reiff, P. Van Nieuwenhuizen, G. 't Hooft, and B. de Wit) started graduate work under my supervision. High-energy physics, at that time, was not a popular subject in the Netherlands, traditionally strong in the field of statistical mechanics. The disadvantage was a certain isolation, the advantage a certain isolation. Starting on Yang–Mills theories in 1968, I found it extremely pleasant that I did not have to defend my aberrant views.

As a general rule I avoided dragging students into the field of Yang–Mills theories. It was too risky. Armed solely with knowledge on that subject, they were at a disadvantage in finding proper employment afterwards, or so I thought. For at least part of their thesis work, I insisted on more phenomenologically oriented work.

Another subject very popular in Utrecht was the sigma model. I always felt that this model, due to Schwinger and employed in the article of Gell-Mann and Levy in relation to PCAC, was of fundamental importance.[49]

In the beginning of 1969 a student, Gerhard 't Hooft, was assigned to me for his predoctoral thesis. For a good understanding, we (my colleagues and I) shared this type of task, and students were more or less distributed among the professors. A student could, however, express his preferences, and when expressing a preference for high-energy physics, as 't Hooft did, they tended to wind up with me. The work that I asked him to consider was the sigma model, axial currents, and anomalies. His predoctoral thesis was completed in 1969.[50] It contained a discussion about PCAC, the sigma model, the anomaly, and renormalization of the sigma model.

In October 1969 he was offered a position at the Institute, in order to enable him to complete a doctoral thesis. He expressed his interest to work under my supervision, in high-energy physics, rather than the alternative, statistical mechanics with his uncle, N. G. Van Kampen.

At that time I felt an urgent need to understand path integrals. The best way to learn is to lecture on the subject, and so I did, first in Orsay, in 1968–1969. Ben Lee was in the audience. I felt that I still needed further education and proposed a course in Utrecht in 1969–1970 on the same subject, in collaboration with Van Kampen, who agreed. We thus lectured on the use of path integrals in statistical mechanics and high-energy physics. 't Hooft was assigned the task of writing the lecture notes. I remember being quite happy about it; at times he improved the derivations considerably.

At some point I had to specify a thesis subject. I do not remember precisely at what time, but I assume it was in the autumn of 1969. As explained before, I was not particularly happy about students going into Yang–Mills theories; I therefore mentioned the "hot" topic of the day, the $A2$ resonance splitting.

Somewhat later he expressed his disdain, rightly so, for the subject of $A2$ splitting. Actually, I certainly had not much sympathy for the subject either. The point then was, what now? We discussed this together in the presence of Van Kampen, and I gave in: if he so wanted, let him have a try at Yang–Mills theory. More specifically, I suggested to him the problem of finding a good regulator method to be used with Yang–Mills theories. That was something for which I felt a real need, and which seemed just the type of thing for a proper initiation into the subject.

In the summer of 1970, like many other European students, 't Hooft went to a summer school. In his case that school was in Cargese. Ben Lee lectured there, on the renormalization of the sigma model, in particular focusing on what happens to infrared problems when spontaneous symmetry breaking occurs.[51] According to 't Hooft, he found there the inspiration to introduce spontaneous symmetry breaking into the pure Yang–Mills theory.[52] However, he first worked on the unbroken theory, and somewhere near the end of 1970 his first article came to my desk: "Renormalization of Massless Yang–Mills Fields."[53]

I do remember quite clearly a number of discussions, but I will not elaborate on them here. Mainly I remember the moment when I, alone in my office, pondered whether this should be published; that is, I tried to weigh what was really new in the paper. In my view these were the truly new elements: a new cutoff method that, however, worked up to one loop only, and also a quite elegant extension of the work by Fradkin and Tyutin on Feynman rules in an arbitrary gauge.[54] The ghost determinant was replaced by explicit ghost Feynman rules.

Actually, I did not know the work of Fradkin and Tyutin in detail at that time. I just knew that the rules were known (also to myself) for a few gauges in the massless case. I liked 't Hooft's derivation a lot. There could be no doubt that this was a nice article.

The cutoff method was a precursor to dimensional regularization. For one loop he introduced a fifth dimension. Dimensional regularization came from the idea that somehow this fifth dimension should be distributed over the loops. I once said to 't Hooft that once you collaborate you forget about who invented what. I am not going to violate my own rules here.

Somewhere in the autumn or winter of 1970–1971 we walked together from one building of the Institute to another. I complained about theories of charged vector bosons. I said something like "All this stuff about massless theories is very nice, but if we only had one renormalizable theory of massive charged vector bosons, no matter how far removed from reality. In any case all possible models exist already." He answered "I can do that." This moment is grafted in my brain, as I almost ran into a tree. I said "What?!" He repeated his statement. I said, "Write it down, we will see." And he did, and we saw.[55]

The moment his second article came under my eyes, I knew that this was it. In fact, I think that he was very surprised at my immediate acceptance. He expected, I think, a lot of arguments about the Higgs mechanism, and we did argue some about it. The fact is that I did, indeed, not like it very much, not then and not now, but at that time I was only interested in the result and could not care less how it came about. Actually, we did not know that it was the Higgs mechanism; to us it was the spontaneous symmetry breaking of the sigma model, as in the articles of Schwinger and Gell-Mann and Levy.[56] In my opinion spontaneous symmetry breaking, at least in this context, owes nothing to the work by Anderson in superconductivity and subsequent developments.[57] This is different for Weinberg's paper.[58]

As a testimony to our ignorance, I remember sitting with 't Hooft and musing that this probably had something to do with Goldstone's theorem. Since neither of us really knew precisely what that meant, and since the theory was obviously correct, we decided to forget about it. My unease with the Higgs mechanism remained throughout, and eventually I realized why I felt that way: it is the problem of the cosmological constant.[59]

At the end I said to 't Hooft: "Now the time has come to construct a realistic model." Then I took the article with me to CERN and verified the whole lot, in particular two-loop unitarity, with the help of Schoonschip. As Jacques Prentki put it: "If it is wrong, you will get the blame; if it is correct, he will get the credit." Furthermore, I asked Zumino to read it, and to provide me with references that he thought to be relevant. He was always my infallible guide to the literature. And so it came to pass that references to Kibble and Weinberg were included.[60] Well, this time Zumino was not so infallible, or else he should have mentioned the work of Englert and Brout, which slightly predates the work of Higgs.[61] These are evidently independent pieces of work. Actually Englert and Brout saw clearly the connection with renormalizability, and they advised Weinberg of that, at the Solvay Conference of 1967.[62] Students of the history of the Standard Model may want to check this little-known reference.

I am reasonably sure that 't Hooft deduced the Weinberg model by himself. The vector-boson part was already in his work when I took it to Geneva. We discussed the lepton part on the telephone. When I informed him of Weinberg's article, his first reaction was that it was wrong; a few days later he said that it was the same as his version. As I have stated before: once you know the rules it is easy. The number of models that were cranked out in the ensuing years testifies to this fact.

I then set out to promote 't Hooft's work, starting at the Amsterdam Conference of 1971.[63] I organized a session, inviting Salam and T. D. Lee to present their views on finiteness of field theory (these were nonpolynomial Lagrangians and unstable particles with negative metric, respectively). After that 't Hooft presented his work.

Admittedly, at that time this gave me some pleasure of a dubious kind. Being chairman, I was up on the podium. Salam was sitting in the first row, looking glassily. Coleman, about ten rows back on the left, looked at least as intelligent. I did not see Glashow; he probably was not interested in all this field-theory stuff. As far as I know, he has never mentioned his presence in Amsterdam. Ben Lee was perhaps the only one who understood what was going on; we talked about it afterwards.

Little did I realize the contributions of Glashow and Salam at that time! Students of the history of the Standard Model may want to check the references that Salam added to his own paper in the proceedings in relation to 't Hooft's talk.

If somebody would have told me then about the 1979 Nobel prize, I would have laughed. Later I got used to the idea. Such is life.

Mopping up

There were a number of loose ends. First, there still was no regularization scheme. Furthermore, renormalization, Ward identities, and the like had to be established on a rigorous base.

The idea of dimensional regularization was first hinted at publicly in the middle of 1971. In the autumn of 1971 I spent most of my time on this subject; 't Hooft, stimulated by Symanzik, became very interested in asymptotic freedom and the massless theory as a model for strong interactions. In fact, he established asymptotic freedom and reported that at the Marseille Conference in June 1972 – a full ten months before the publications of Politzer, Gross, and Wilczek.[64] This is actually quite well known although not published. The communication was in the form of a comment after a lecture by Symanzik. However, the connection with observed physics was not mentioned, and certainly I did not understand the relevance of the affair. Nor do I know the complete history; it appears that there are even earlier Russian papers containing all or parts of the calculation.

There is not much to say about the paper on dimensional regularization, ready and submitted by the end of February 1972.[65] It did take a lot of effort, but it was essentially straightforward. The effort concerns the formulation of the method beyond the one-loop level, and given that there was already a working one-loop method, it was felt that this was essential. It may perhaps be mentioned that dimensional regularization cannot be formulated within the path-integral formalism nor the conventional operator formalism. Therefore a general formulation of the method beyond one loop is not entirely trivial.

I would like to comment here on the subject of competing papers. A number of other papers came out containing the same idea. The idea of continuation in the number of dimensions was known to us since somewhere in the begining of 1971, and we made no particular secret of it.[66] It was most explicitly mentioned at the Orsay conference of January 1972. I did not recognize, and have not in general recognized, papers dated after February, in particular if they limited their treatment to one-loop diagrams. There is one exception: the paper by Bollini and Giambiagi, received 8 February 1972.[67] I received a letter from them (9 March 1972) while correcting some misprints in our article, and I added a reference to their work; they added similarly to theirs. In their article they referred to a preprint that I have never seen and that they did not send me; though showing a received date of October 1971, it was finally published almost a year later (August 1972).[68] Evidently they had a lot of trouble getting the paper accepted. That paper explicitly mentions the idea of dimensional regularization and a few one-loop diagrams are worked out. The motivation was certainly very different from ours, and the essential advantage of the method, that is, the respecting of non-Abelian gauge invariance, was not mentioned (QED gauge invariance was mentioned in their second paper).[69] They did not consider the extension to more than one loop. Their work is clearly independent, even if it is almost unbelievable that an outlandish idea such as dimensional regularization would happen simultaneously in two unrelated instances. Let me add, though, that they had worked in this field before: I believe that they are among the inventors of analytic regularization.[70] For more information see Speer.[71]

At the Orsay Conference 't Hooft and I presented various subjects such as a clear exhibition of general gauge fixing, ghost generation, and dimensional regularization. The matter of anomalies was thrashed out there; 't Hooft thought he had an argument showing that anomalies were harmless in the Weinberg model.[72] Bardeen argued against it, with success; the main argument centered around a diagram with two triangle anomalies, showing clearly that the Weinberg model contains anomalies and is as such nonrenormalizable. The paper of Bouchiat, Iliopoulos, and Meyer shows how to avoid anomalies, by including quarks.[73] It was, I believe, inspired by the arguments at this conference.

The final work, as far as I am concerned, is a paper by 't Hooft and myself containing a formal combinatorial derivation of the Ward identities and a proof of renormalizability.[74] A preliminary version was presented at the Marseille Conference; Bell pointed out that there was a difficulty with respect to external-line renormalization.[75] He referred to an important piece of work by Bialynicki-Birula, which I am happy to acknowledge.[76] It was quite serious criticism, and took some time to correct. At the conference we also presented a very explicit example [pure SU(2) case], with a two-parameter choice of gauge and illustrating the content of the more formal paper.[77] There is a delicate sentence in ref 74, just above section 3.

This, from my perspective, was the road to the proof of renormalizabilty. After this 't Hooft and I collaborated on a few more papers, among them an investigation of the divergencies in gravitation.[78] I believe that that paper as well as the related lectures in Les Houches have had their impact, but that is another history.[79] Later I interested myself in the Higgs mechanism and radiative corrections. After all, measuring and comparing radiative corrections with the predictions of the theory is in my view an indispensable part of the proof of renormalizability. But this is again another chapter of history.

Other authors, notably Ben Lee and Jean Zinn-Justin, have published work after July 1971 that differs from ours in the fact that heavy use is made of path-integral methods.[80] That may have helped acceptance of the formalism; apparently formal path-integral methods are more readily accepted than combinatorial arguments relating to diagrams. Our attempt at popularization is a CERN yellow report entitled "Diagrammar."[81]

The contribution of Becchi, Rouet, and Stora concerning Ward identities should perhaps be mentioned as the final part of the formalism.[82] What remained and partly remains after that are some technical questions concerning the handling of spinors and γ^5.

Assessment

At this moment, after 20 years of experimentation, the Standard Model has been verified, including radiative corrections. That is, effects needing renormalization for their calculation have been verified. The agreement so far is excellent.

An interesting point may now be raised. To what extent has the theory been tested in all its glory and renormalizability? My first paper established one-loop renormalizability; 't Hooft's paper specified fully renormalizable models including at least one extra particle, the Higgs particle. On the phenomenological level, Glashow's paper as compared with that of Weinberg did have masses put by hand rather than generated by the Higgs mechanism.[83] If full renormalizability has indeed been tested, then we ought to have a statement on the Higgs mass.

Well, there is no statement on the Higgs mass. We have no clue to its magnitude from experiment. That is, experiment has verified Glashow's model, using my one-loop renormalizability result. Quantum chromodynamics is a pure Yang–Mills theory. The final word on the renormalizability of that one was established with the advent of dimensional regularization, undoubtedly an indispensable tool in present-day theory. I do not know to how many loops this theory has been established with certainty, but I would say well beyond two loops. That is where we stand. To be complete, another fact that some might interpret as a tie to the Higgs system is that the ρ parameter turns out to be close to 1, experimentally.[84]

The problem of the cosmological constant has further aggravated the Higgs problem.[85] It has motivated me to investigate the theory without a Higgs – that is, essentially the same thing that I started with.[86] At this time there will be few physicists who would bank on actually finding the Higgs particle. Many of us feel that the world is more complicated than that. Even so, the fully renormalizable theories with a Higgs sector provide the framework for parametrizing the present-day situation.

So, all told, the word is still out. However, the psychological effect of a complete proof of renormalizability has been immense. This then is the important point. The proof of renormalizability gives certain theories a certain internal strength that makes them credible. People (at least most) did not go into Yang–Mills theories after Glashow's, Weinberg's, or my paper. The proof of renormalizability provided the necessary psychological impact.

Notes

The date on which the article was received is indicated here in square brackets.

1 F. Antonelli, M. Consoli, and G. Corbó, "One-Loop Correction to Vector Boson Masses in the Glashow–Weinberg–Salam Model of Electromagnetic and Weak Interactions," *Phys. Lett. B91* (1980), pp. 90–4 [11 Jan 1980]. M. Veltman, "Radiative Corrections to Vector Boson Masses," *Phys. Lett. B91* (1980), pp. 95–8 [11 Jan 1980].

2 Selected references to the Stueckelberg formalism: E. C. G. Stueckelberg, "Interaction Forces in Electrodynamics and in the Field Theory of Nuclear Forces," *Helv. Phys. Acta 11* (1938), pp. 299–328 (in German); W. Pauli, "Relativistic Field Theories of Elementary Particles," *Rev. Mod. Phys. 13* (1941), pp. 203–32; Y. Miyamoto, "On the Interaction of the Meson and Nucleon Field in the Super-Many-Time Theory," *Progr. Theor. Phys. 3* (1948), pp. 124–40; F. J. Belinfante, "Quantum Electrodynamics" *Phys. Rev. 75* (1949), p. 1321; F. Coester, "Quantum Electrodynamics with Nonvanishing Photon Mass," *Phys. Rev. 83* (1951), pp. 798–800; R. J. Glauber, "On the Gauge Invariance of the Neutral Vector Meson Theory," *Prog. Theor. Phys. 9* (1953), pp. 295–8; H. Umezawa, *Quantum Field Theory* (Amsterdam: North-Holland, 1956), pp. 113 and 204; E. C. G. Stueckelberg, "Theory of the Radiation of Photons of Small Arbitrary Mass," *Helv. Phys. Acta 30* (1957), pp. 209–35 (in French); A. Fujii, "On the Analogy Between Strong Interaction and Electromagnetic Interaction," *Prog. Theor. Phys. 21* (1959), pp. 232–40; H. Umezawa and S. Kamefuchi, "Equivalence Theorems and Renormalization Problem in Vector Field Theory (The Yang–Mills Field with Non-Vanishing Masses)," *Nucl. Phys. 23* (1961), pp. 399–429; D. Boulware and W. Gilbert, "Connection between Gauge Invariance and Mass," *Phys. Rev. 126* (1962), pp. 1563–7; S. Bonometto, "On Gauge Invariance for a Neutral Massive Vector Field," *Nuovo Cimento 28* (1963), pp. 309–19; J. A. Young and S. A. Bludman, "Electromagnetic Properties of a Charged Vector Meson," *Phys. Rev. 131* (1963), pp. 2326–34; A. Fujii and S. Kamefuchi, "A Generalization of the Stueckelberg Formalism of Vector Meson Fields," *Nuovo Cimento 33* (1964), pp. 1639–56; A. Slavnov, "Renormalization of Gauge Invariant Theories," *Sov. J. Part. and Nucl. 5* (1975), pp. 303–17.

3 A. Komar and A. Salam, "Renormalization Problem for Vector Meson Theories," *Nucl. Phys. 21* (1960), pp. 624–30 [22 Aug 1960]. A. Salam, "Renormalizability of Gauge Theories," *Phys. Rev. 127* (1962), pp. 331–4

[27 Nov 1961]; S. L. Glashow and J. Iliopoulos, "Divergences of Massive Yang–Mills Theories," *Phys. Rev. D3* (1971), pp. 1043–5 [15 Sep 1970].
4 S. L. Glashow, "The Renormalizability of Vector Meson Interactions," *Nucl. Phys. 10* (1959), pp. 107–17 [24 Nov 1958].
5 G. R. Screaton, ed., *Dispersion Relations: Scottish Universities Summer School, 1960* (New York: Interscience Publishers, 1961; and Edinburgh: Oliver and Boyd, 1961), pp. 186–205.
6 M. Veltman, "Unitarity and Causality in a Renormalizable Field Theory with Unstable Particles," *Physica 29* (1963), pp. 186–207 [5 Nov 1962].
7 R. E. Cutkosky, "Singularities and Discontinuities of Feynman Amplitudes," *J. Math. Phys. 1* (1960), pp. 429–33 [31 Mar 1960].
8 R. P. Feynman, "Quantum Theory of Gravitation," *Acta Phys. Pol. 24* (1963), pp. 697–722 [Talk July 1962, received 3 Jul 1963].
9 M. Veltman, "Higher Order Corrections to the Coherent Production of Vector Bosons in the Coulomb Field of a Nucleus," *Physica 29* (1963), pp. 161–85 [24 Oct 1962].
10 T. D. Lee and C. N. Yang, "Theory of Charged Vector Mesons Interacting with the Electromagnetic Field," *Phys. Rev. 128* (1962), pp. 885–98 [29 May 1962]; T. D. Lee, "Application of ξ-Limiting Process to Intermediate Bosons," *Phys. Rev. 128* (1962), pp. 899–910 [29 May 1962].
11 C. N. Yang and R. L. Mills, "Conservation of Isotopic Spin and Isotopic Gauge Invariance," *Phys. Rev. 96* (1954), pp. 191–5 [28 Jun 1954].
12 M. Gell-Mann, "The Symmetry Group of Vector and Axial Vector Currents," *Physics 1* (1964), pp. 63–75 [25 May 1964].
13 M. Veltman, "Divergence Conditions and Sum Rules," *Phys. Rev. Lett. 17* (1966), pp. 553–6 [29 Jul 1966].
14 S. L. Adler, "Calculation of the Axial-Vector Coupling Constant Renormalization of β Decay," *Phys. Rev. Lett. 14* (1965), pp. 1051–5 [17 May 1965]; W. I. Weisberger, "Renormalization of the Weak Axial-Vector Coupling Constant," *Phys. Rev. Lett. 14* (1965), pp. 1047–51 [26 May 1965].
15 M. Veltman, "Theoretical Aspects of High Energy Neutrino Interactions," *Proc. Roy. Soc. A301* (1967), pp. 107–12 [2 Nov 1966].
16 D. Sutherland, "Current Algebra and the Decay $\eta \to 3\pi$," *Phys. Lett. 23* (1966), pp. 384–5 [24 Oct 1966].
17 D. Sutherland, "Current Algebra and Some Non-Strong Mesonic Decays," *Nucl. Phys. B2* (1967), pp. 433–40 [30 May 1967].
18 J. S. Bell and R. Jackiw, "A PCAC Puzzle: $\pi^0 \to \gamma\gamma$ in the σ-Model," *Nuovo Cimento A60* (1969), pp. 47–60 [11 Sep 1968].
19 S. L. Adler, "Axial-Vector Vertex in Spinor Electrodynamics," *Phys. Rev. 177* (1969), pp. 2426–38 [24 Sep 1968].
20 J. S. Bell, "Current Algebra and Gauge Variance," *Nuovo Cimento 50A* (1967), pp. 129–34 [16 Dec 1966].
21 See, in particular, M. Veltman, ref. 23.
22 For a belated reprinting, see M. Veltman, "Relation Between the Practical Results of Current Algebra Techniques and the Originating Quark Model," in R. Akhoury, B. De Witt, P. Van Nieuwenhuizen, and H. Veltman, eds., *Gauge Theory–Past and Future* (Singapore: World Scientific, 1992), pp. 293–336.
23 M. Veltman, "Perturbation Theory of Massive Yang–Mills Fields," *Nucl. Phys. B7* (1968), pp. 637–50 [10 Sep 1968].

24 E. S. Fradkin and I. V. Tyutin, "Feynman Rules for the Massless Yang–Mills Field Renormalizability of the Theory of the Massive Yang–Mills Field," *Phys. Lett. 30B* (1969), pp. 562–3 [15 Oct 1969]; E. S. Fradkin, E. Esposito, and S. Termini, "Functional Techniques in Physics," *Rivista del Nuovo Cimento 2* (1970), pp. 498–560 [26 Sep 1970].
25 C. Becchi, A. Rouet, and R. Stora, "Renormalization of Gauge Theories," *Ann. Phys. (N.Y.) 98* (1976), pp. 287–321 [8 Dec 1975]; G. 't Hooft, "Renormalization of Massless Yang–Mills Fields," ref. 53.
26 A. A. Slavnov, "Ward Identities in Gauge Theories," *Theoretical and Mathematical Physics 10* (1972), pp. 153–61, (English translation pages 99–104) [23 Jun 1971]; J. C. Taylor, "Ward Identities and Charge Renormalization of the Yang–Mills Field," *Nucl. Phys. B33* (1971), pp. 436–44 [25 Jun 1971].
27 R. P. Feynman, "Quantum Theory of Gravitation," ref. 8.
28 B. S. DeWitt, "Theory of Radiative Corrections for Non-Abelian Gauge Fields," *Phys. Rev. Lett. 12* (1964), pp. 742–6 [12 May 1964]; "Quantum Theory of Gravity. I. The Canonical Theory," *Phys. Rev. 160* (1967), pp. 1113–48; "Quantum Theory of Gravity. II. The Manifestly Covariant Theory," *Phys. Rev. 162* (1967), pp. 1195–239.
29 L. D. Faddeev and V. N. Popov, "Feynman Diagrams for the Yang–Mills Field," *Phys. Lett. 25B* (1967), pp. 29–30 [1 Jun 1967].
30 S. Mandelstam, "Feynman Rules for Electromagnetic and Yang–Mills Fields from the Gauge-Independent Field-Theoretic Formalism," *Phys. Rev. 175* (1968), pp. 1580–1603 [17 Jun 1968].
31 A. Komar and A. Salam, "Renormalization Problem"; A. Salam, "Renormalizability," ref. 3.
32 A. Salam, "Weak and Electromagnetic Interactions," in Nils Svartholm, ed., *Elementary Particle Theory* (Stockholm: Almqvist & Wiksell, 1968), pp. 367–77.
33 D. Boulware, "Renormalizeability of Massive Non-Abelian Gauge Fields: A Functional Integral Approach," *Ann. Phys. 56* (1970), pp. 140–71 [14 May 1969].
34 P. T. Matthews, "The Application of the Tomonaga–Schwinger Theory to the Interaction of Nucleons with Neutral Scalar and Vector Mesons," *Phys. Rev. 76* (1949), pp. 1657–74 [28 Jun 1949].
35 T. D. Lee and C. N. Yang, "Theory of Charged Vector Mesons," ref. 10.
36 J. Reiff and M. Veltman, "Massive Yang–Mills Fields," *Nucl. Phys. B13* (1969), pp. 545–64 [11 Aug 1969].
37 M. Veltman, "Massive Yang–Mills Fields," in J. S. Bell, ed., *Proc. Topical Conf. on Weak Interactions* (CERN, Geneva, Switzerland, 14–17 Jan 1969), CERN yellow report No. 69–7, pp. 391–3.
38 A. A. Slavnov and L. D. Faddeev, "Massless and Massive Yang–Mills Fields," *Teoreticheskaya i Matematicheskaya Fizika* Vol. 3, No. 1 (April 1970), pp. 18–23 [4 Nov 1969]; D. Boulware, "Renormalizeability," ref. 33.
39 H. Van Dam and M. Veltman, "Massive and Massless Yang–Mills and Gravitational Fields," *Nucl. Phys. B22* (1970), pp. 397–411 [8 Jun 1970].
40 A. A. Slavnov, "Ward Identities in Gauge Theories"; J. C. Taylor, "Ward Identities and Charge Renormalization," ref. 26
41 M. Veltman, "Generalized Ward Identities and Yang–Mills Fields," *Nucl. Phys. B21* (1970), pp. 288–302 [16 Apr 1970].

42 S. L. Glashow, J. Iliopoulos, and I. Maiani, "Weak Interactions with Lepton–Hadron Symmetry," *Phys. Rev. D2* (1970), pp. 1285–92 [5 Mar 1970]. See also, Yasuo Hara, "Unitary Triplets and the Eightfold Way," *Phys. Rev. B134* (1964), pp. 701–4 [23 Dec 1963]; and J. D. Bjorken and S. Glashow, "Elementary Particles and SU(4)," *Phys. Lett. 11* (1964), pp. 255–8 [19 Jun 1964].
43 S. L. Glashow and J. Iliopoulos, "Divergences of Massive Yang–Mills Theories," ref. 3.
44 M. Veltman, "Second Threshold in Weak Interactions," *Acta Phys. Pol. B8* (1977), pp. 475–92 [7 Jan 1977].
45 S. Weinberg, "Conceptual Foundations of the Unified Theory of Weak and Electromagnetic Interactions," *Rev. Mod. Phys. 52* (1980), pp. 515–23, on p. 518.
46 L. D. Faddeev and V. N. Popov, "Feynman Diagrams," ref. 29
47 E. S. Fradkin and I. V. Tyutin, "Feynman Rules"; E. S. Fradkin, E. Esposito, and S. Termini, "Functional Techniques in Physics," ref. 24.
48 E. S. Fradkin and I. V. Tyutin, "S Matrix for Yang–Mills and Gravitational Fields," *Phys. Rev. D2* (1970), pp. 2841–57 [19 Jan 1970].
49 J. Schwinger, "A Gauge Theory of Fundamental Interactions," *Ann. Phys. 2* (1957), pp. 407–35 [31 Jul 1957]. M. Gell-Mann and M. Lévy, "The Axial Vector Current in Beta Decay," *Nuovo Cimento 16* (1960), pp. 705–26 [19 Feb 1960].
50 G. 't Hooft, Utrecht scriptie, 1969 (unpublished); xerox copy in existence.
51 B. Lee, "Chiral Dynamics," *Cargese Lecture in Physics*, Vol. 5 (New York: Gordon and Breade, 1971), pp. 1–119; D. Bessis and Turchetti, "Renormalization of the σ model through Ward Identities," ibid., pp. 119–179.
52 G. 't Hooft, thesis, Utrecht, March 1972. This thesis contains essentially the papers of refs. 53 and 55, with an additional introduction, summary, and short curriculum vitae in Dutch. There is also, according to Dutch tradition, a sheet with 15 "stellingen" (propositions). They must be arguable.
53 G. 't Hooft, "Renormalization of Massless Yang–Mills Fields," *Nucl. Phys. B33* (1971), pp. 173–99 [12 Feb 1971].
54 E. S. Fradkin and I. V. Tyutin, "S Matrix for Yang–Mills," ref. 48.
55 G. 't Hooft, "Renormalizable Lagrangians for Massive Yang–Mills Fields," *Nucl. Phys. B35* (1971), pp. 167–88 [13 Jul 1971].
56 J. Schwinger, "A Gauge Theory of Fundamental Interactions"; M. Gell-Mann and M. Levy, "The Axial Vector Current," ref. 49.
57 P. W. Anderson, "Plasmas, Gauge Invariance and Mass," *Phys. Rev. 130* (1963), pp. 439–42 [8 Nov 1962].
58 S. Weinberg, "A Model of Leptons," *Phys. Rev. Lett. 19* (1967), pp. 1264–6 [17 Oct 1967].
59 M. Veltman, "Cosmology and the Higgs Mechanism," Rockefeller University preprint May 1974 (unpublished). M. Veltman, "Cosmology and the Higgs Mass," *Phys. Rev. 34* (1975), pp. 777–8 [5 Dec 1974].
60 T. W. B. Kibble, "Symmetry Breaking in Non-Abelian Gauge Theories," *Phys. Rev. 155* (1967), pp. 1554–61 [24 Oct 1966]; S. Weinberg, " A Model of Leptons," ref. 58.
61 F. Englert and R. Brout, "Broken Symmetry and the Mass of Gauge Vector Mesons," *Phys. Rev. Lett. 13* (1964), pp. 321–3 [26 Jun 1964]; P.

W. Higgs, "Broken Symmetries, Massless Particles and Gauge Fields," *Phys. Lett. 12* (1964), pp. 132–3 [27 Jul 1964].

62 *Fundamental Problems in Elementary Particle Physics*, Proceedings of the Fourteenth Conference on Physics, University of Brussels, 2–7 October 1967 (New York: John Wiley, 1968). See discussion after the lecture of H. P. Durr, page 18. Weinberg distributed there one or more copies of the handwritten manuscript of his 1967 article; the difference between that and the published version is minimal.

63 A. Tenner and M. Veltman, eds., *Proceedings of the Amsterdam International Conference on Elementary Particles*, June 30–July 6, 1971 (Amsterdam: North-Holland, 1972).

64 H. D. Politzer, "Reliable Perturbative Results for Strong Interactions," *Phys. Rev. Lett. 30* (1973), pp. 1346–8 [3 May 1973]. D. Gross and F. Wilczek, "Ultra-Violet Behavior of Non-Abelian Gauge Theories," *Phys. Rev. Lett. 30* (1973), pp. 1343–6 [27 Apr 1973].

65 G. 't Hooft and M. Veltman, "Regularization and Renormalization of Gauge Fields," *Nucl. Phys. B44* (1972), pp. 189–213 [21 Feb 1972].

66 It was alluded to in section 5 of G. 't Hooft, "Renormalization of Massless Yang-Mills Fields," ref. 53.

67 C. Bollini and J. Giambiagi, "Dimensional Renormalization: The Number of Dimensions as a Regularizing Parameter," *Nuovo Cimento 12B* (1972), pp. 20–6 [8 Feb 1972].

68 C. Bollini and J. Giambiagi, "Lowest Order Divergent Graphs," *Phys. Lett. 40B* (1972), pp. 566–70 [18 Oct 1971].

69 C. Bollini and J. Giambiagi, "Dimensional Renormalization," ref. 67.

70 C. G. Bollini, J. J. Giambiagi, and A. Gonzalez Dominguez, "Analytic Regularization and the Divergences of Quantum Field Theories," *Nuovo Cimento 31* (1964), pp. 550–61 [15 Jul 1963].

71 Eugene R. Speer, "Analytic Renormalization," *J. Math. Phys. 9* (1968), pp. 1404–10 [1 Dec 1967].

72 See comments in G. 't Hooft, "Prediction for Neutrino–Electron Cross Sections in Weinberg's Model," *Phys. Lett. 37B* (1971), pp. 195–9 [27 Oct 1971]. This paper contains in footnote 2 a statement suggesting the existence of an argument that the Weinberg model of leptons as such is renormalizable, i.e., that anomalies are harmless.

73 C. Bouchiat, J. Iliopoulos, and Ph. Meyer, "An Anomaly-Free Version of Weinberg's Model," *Phys. Lett. 38B* (1972), pp. 519–23 [11 Feb 1972].

74 G. 't Hooft and M. Veltman, "Combinatorics of Gauge Fields," *Nucl. Phys. B50* (1972), pp. 318–53 [31 Jul 1972].

75 G. 't Hooft and M. Veltman, "Example of a Gauge Field Theory," in C. P. Korthals-Altes, ed., *Renormalization of Yang–Mills Fields and Applications to Particle Physics*, Marseille Conference 19–23 June 1972 (Marseille: Centre de Physique Théorique, 1972), pp. 37–75. . Note: J. S. Bell is not mentioned in the list of participants; he was there.

76 I. Bialynicki-Birula, "Renormalization, Diagrams and Gauge Invariance," *Phys. Rev. D2* (1970), pp. 2877–86 [27 Aug 1970].

77 G. 't Hooft and M. Veltman, "Example of Gauge Field Theory," ref. 75. Actually typed at CERN in October 1972.

78 G. 't Hooft and M. Veltman, "One-loop divergencies in the theory of gravitation" *Annales de l' Institut Henri Poincaré 20* (1974), pp. 69–94 [4 Sep 1973].

79 M. Veltman, "Quantum Theory of Gravitation," in R. Balian and J. Zinn-Justin, eds., *Structural Analysis of Collision Amplitudes*, Proceedings of the Les Houches Summer School on Theoretical Physics, 2–27 June 1975 (Amsterdam: North-Holland, 1976), pp. 265–327.
80 B. Lee and J. Zinn-Justin, "Spontaneously Broken Gauge Symmetries," *Phys Rev. D5* (1972), pp. 3121–37, 3137–55, 3155–60 [10 Mar 1972]; B. Lee and J. Zinn-Justin, "Spontaneously Broken Gauge Symmetries," *Phys. Rev. D7* (1973), pp. 1049–56 [30 Oct 1972].
81 G. 't Hooft and M. Veltman, "Diagrammar," CERN yellow report No. 73-9 (Geneva, 1973).
82 C. Becchi, A. Rouet, and R. Stora, "Renormalization of Gauge Theories," ref. 25.
83 S. L. Glashow, "Partial-Symmetries of Weak Interactions," *Nucl. Phys. 22* (1961), pp. 579–88 [5 Sep 1960].
84 D. A. Ross and M. Veltman, "Neutral Currents and the Higgs Mechanism," *Nucl. Phys. B95* (1975), pp. 135–47 [11 Apr 1975].
85 M. Veltman, "Cosmology and the Higgs Mechanism" ref. 59.
86 M. Veltman, "Second Threshold in Weak Interactions," ref. 44.