Essay: Half a Century of the Standard Model*,†

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50 Years of the Standard Model[1]

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Abstract: The Standard Model is a quantum field theory that successfully accounts for the strong, weak, and electromagnetic interactions of the known elementary particles. In this essay I reminisce about the forerunners of the Standard Model, the beginnings of the model half a century ago, and its development and confirmation from then to the present.

The theoretical physicists of the late 1960s worked in the shadow of a great advance that had been made two decades earlier. Feynman, Schwinger, Tomonaga, and Dyson in the 1940s had figured out how to do calculations in quantum electrodynamics while keeping manifest the Lorentz invariance of the theory. In this way, they had been able to solve the old problem of dealing with infinities in their calculations, by absorbing infinite radiative corrections into a renormalization of the parameters and fields of quantum electrodynamics. They had thereby been able to get results for quantities like the Lamb shift and the magnetic moment of the electron with an accuracy that was unprecedented in all of science.

More than that, the theorists of the 1940s had discovered a rationale for the simplest version of quantum electrodynamics. The symmetries of electrodynamics, Lorentz and gauge invariance, by themselves would not take you very far. For instance, you could add terms to the Lagrangian that would make the magnetic moment of the electron anything you like. But then renormalization would not work. For the theory to be renormalizable, the Lagrangian had to be very simple, and it was in just that simple theory that you could calculate specific results, and get stunning agreement with observation.

This aspect of renormalization theory was not universally appreciated. After all, physicists long before renormalization theory had always tried to choose simple theories. I recall Oppenheimer grumbling that renormalization was just a way of sweeping infinities under the rug. But even where a simple theory is confirmed by observation, simplicity like everything else needs to be explained, and the requirement of renormalizability offered such an explanation. This turned out also to be important in the development of the Standard Model.

We wondered in the 1950s and 1960s if we could proceed like our predecessors. Why not add some more elementary particles to our theories, beyond the electron and photon of quantum electrodynamics, impose some new symmetries and the condition of renormalizabilty, and get a theory that would encompass everything being discovered by our experimental colleagues?

It wasn't going to be easy. Since 1934 we had a field theory of weak interactions, Fermi's theory,[2] with an interaction Lagrangian given by a product of vector currents. With axial vector currents added later by Marshak and Sudarshan[3] and by Feynman and Gell-Mann,[4] this theory worked perfectly well in describing beta decay in the lowest order of perturbation theory. But it was not renormalizable, and so could not be used to get finite results in higher orders.

On the other hand, there was no problem in thinking of any number of renormalizable field theories of the strong interactions. But there was no rationale for any of them, and no way to confirm any of them experimentally, because the strong interactions are strong, and so any power series in the coupling constant given by perturbation theory would be useless. Meanwhile, so many new strongly interacting particles were being discovered at accelerators like the Bevatron that we had to give up on identifying which of them were elementary particles, whose fields would appear in the

Lagrangian, and we even began to doubt if we knew what was meant by a particle being elementary.

These problems led to a widespread disenchantment with quantum field theory. Some theorists adopted an extreme form of positivism, allowing themselves to work only with things that could be measured, in particular with S-matrix elements, relying only on their general properties, such as Lorentz invariance, unitarity and analyticity. This aim was in a sense achieved much later in effective field theories, but it could never be implemented in the way that was being tried in the 1950s. Complex analysis with many complex variables is just too hard.

One thing on which we could all agree was the importance of symmetries. Symmetry principles could be used to make predictions even if we knew nothing else about the underlying theory, and these principles would constrain any possible future theories.

All sorts of symmetries were being discovered, but they presented us with a new puzzle, for they were only approximate or partial symmetries. There was isotopic spin symmetry, which was known since the discovery[5] in 1936 that, in a given spin state, the proton-proton nuclear force is the same as the neutron-proton force. This was clearly only a symmetry of strong interactions, not of electromagnetic or weak interactions. The eightfold way SU(3) symmetry of Gell-Mann[6] and Ne'eman[7] was not even an exact symmetry of strong interactions. Strangeness conservation[8] was known from the beginning to be respected by the strong and electromagnetic interactions, but not by the weak interactions, and Lee and Yang[9] realized that the same is true of parity conservation and charge conjugation invariance. If symmetry principles are fundamental truths about nature, how can they be approximate or apply only to some interactions and not others, and if they are not fundamental truths, what are they?

Around 1961 a new idea was brought into particle physics from condensed matter physics, by people like Goldstone[10] and Nambu[11] who had experience with both. Maybe there are more symmetries than we knew about, but some are spontaneously broken – that is, there are exact symmetries of the underlying equations that are not respected by the solutions of the equations, the physical phenomena. I was at first enthusiastic about this idea, but almost right away it seemed to be killed off by a 1962 theorem[12] of Goldstone, Salam, and me: The theorem seemed to say that for each spontaneously broken exact continuous symmetry of a theory, the physical spectrum must contain a massless spin zero particle, having the quantum numbers of the current associated with that symmetry. Massless particles of this sort had already been encountered in models studied by Goldstone and Nambu. But of course, there was no experimental sign of such particles.

A couple of years later an exception to this no-go theorem was found. Englert and Brout,[13] Higgs,[14] and Guralnik, Hagen and Kibble[15] independently showed in 1964 that a word was missing in the statement of the 1962 theorem: it should have been that there is a massless spin zero particle for each spontaneously broken exact continuous *global* symmetry.

There were already speculations about symmetries that are local rather than global, in the sense that, like gauge invariance in electrodynamics, the symmetry transformations can vary from one place in spacetime to another. Yang and Mills[16] in 1954 had studied a local version of isotopic spin symmetry. It had a beauty like that of general relativity: the force-carrying particles carried isospin, and therefore had to interact with each other in a way that was governed by the symmetry, just as gravitons interact with each other because they carry energy.

This idea had not gotten anywhere, because it seemed that for each exact local symmetry there had to be a massless spin-one particle, like the photon, and of course, aside from the photon, no such particles were known. But now in 1964 it was found that for each spontaneously broken exact continuous *local* symmetry, what would have been a massless spin zero particle instead becomes the helicity zero component of what would have been a massless spin one particle, which thereby gets a mass. This is what became known as the Higgs mechanism.

Incidentally, the Higgs mechanism had actually been encountered before 1964, in the 1957 theory of superconductivity of Bardeen, Cooper, and Schrieffer.[17] Although broken symmetry is nowhere mentioned in the BCS paper, in their theory the photon gets a mass, responsible for the Meissner effect, from the spontaneous breaking of electromagnetic gauge invariance. That's what a superconductor is — it is a place where electromagnetic gauge invariance is spontaneously broken. In fact, Anderson, who understood this interpretation of the Meissner effect very well, cited it in 1963[18] in a criticism of the paper by Goldstone, Salam, and me. We didn't take his criticism seriously at the time, because our theorem made essential use of Lorentz invariance, and the BCS theory was not relativistic.,

In retrospect it is surprising that neither the authors who discovered the Higgs mechanism in 1964 nor at first anyone else tried to use it to work out a realistic theory This may have been because at just that time theorists were beginning to have real success in applying an *approximate global* spontaneously broken symmetry to the known strong interactions. The symmetry was chiral SU(2) x SU(2), a symmetry under isotopic spin transformations acting independently on the right- and left-handed parts of the nucleon fields (or, as we understand today, of the quark fields). Its spontaneous breakdown to ordinary isospin symmetry led to a Nambu-Goldstone triplet, the pions, not massless because the symmetry is not exact, but very light compared with other hadrons.

Knowledge of this symmetry emerged from current algebra, the study of the vector and axial-vector currents of the contemporary current-current theory of weak interactions, which had led to successful results like the Goldberger-Treiman formula[19] for the pion decay amplitude and the Adler-Weisberger sum rule[20] for the axial vector coupling constant. But it was soon realized that the chiral symmetry could stand alone, as a property of the strong interactions that would be important even if there were no weak interactions.[21] In the mid 1960s we learned how to use this symmetry to calculate all sorts of things about the strong interactions of pions at low energy, including the pion-pion and pion-nucleon scattering lengths,[22] in good agreement with experiment. This was a big factor in killing off the S-matrix program of strong interaction calculations.

Then, in 1967, I thought of looking into the possibility of promoting this chiral symmetry to a local symmetry. I was trying to understand some of the consequences of sum rules for the spectral functions of the weak currents.[23] (These are Adler-Weisberger sum rules, but with the vacuum state replacing the one-nucleon state.) It was widely assumed that these spectral functions would be dominated by isotopic spin triplets of one-particle states, the familiar spin one odd parity meson, the ρ, dominating the vector current, and a spin one even parity meson called the a_1, dominating the axial vector current. The spectral function sum rules predicted that the a_1 mass was larger than the ρ mass by a factor $\sqrt{2}$. As was inevitable in making the chiral symmetry local, I encountered the Higgs mechanism: The a_1 meson indeed picked up a mass, splitting it from the ρ. But in accord with a general remark of Kibble,[24] the ρ meson which is associated with the unbroken local isospin symmetry, remained massless. Not good.

At some point in the autumn of 1967, I realized that I was working on the wrong problem. Maybe in a theory of weak and electromagnetic interactions, of course with a different spontaneously broken local symmetry group and different matter fields, the massive spin one particle would turn out to be not the a_1 meson, but the *W* particle that had long been supposed to transmit the weak force. And the massless spin-one particle would be not the ρ meson, but the photon, associated with unbroken electromagnetic gauge invariance.

At that time no one had clear ideas about the strong interactions. Even Gell-Mann was making skeptical remarks about the real existence of quarks. So I just considered the known leptons, and called my paper "A Model of Leptons".[25] Imposing the kind of simplicity needed for renormalizability, the symmetry group became essentially inevitable. In fact, as I learned later, the same group structure had already appeared in models of Glashow[26] and of Salam and Ward,[27] though approximately and without spontaneous symmetry breaking.

To break the local symmetry. I followed the example of Goldstone rather than Nambu, and introduced scalar fields, whose vacuum expectation values would break the symmetry. The choice of these scalars was almost inevitable: In order to give mass to the electron and muon as well as the gauge bosons in a renormalizable theory, only doublets of complex scalar fields would do. With just one doublet, three of the four real components would go to give mass to the W^+ and W^- particles and the Z^0 particle. The masses of the W and Z particles were given by the theory in terms of a single unknown angle, but whatever the value of this angle these masses turned out to be comfortably heavy, heavy enough for the W and Z to have escaped detection. The fourth real scalar component would show up as a real neutral spinless particle, with uniquely predicted interactions, which later came to be called the Higgs boson. This completed the "model of leptons." The same theory was independently proposed a little later by Salam,[28] who came up with a better name: "the electroweak theory."

Both Salam and I speculated in our papers that the theory was renormalizable, but neither of us was able to prove it. I worked on this on and off for a few years, partly with a student,[29] but got nowhere. I can't speak for Salam, but I know what my problem was. In order to derive Feynman rules in a gauge theory you have to choose a gauge. The only way I knew then to do that was to impose conditions on the field operators, respecting the unitarity of quantum mechanics and the actual particle content of the theory. This is called "unitarity gauge." The Feynman rules then lack the kind of manifest Lorentz invariance that allowed the theorists of the 1940s to control infinities. In fact, in the past 50 years no one has succeeded in using unitarity gauge to renormalize this sort of theory.

Incidentally, this is why local gauge theories provide an exception to the 1962 theorem of Goldstone, Salam, and me. In proving this theorem we had used not only the ordinary unitarity of quantum mechanics but also manifest Lorentz invariance – that is, the Lorentz invariance of every equation in our proof. These theories are both unitary and Lorentz invariant, but there is no gauge in which these properties are both manifest.

Fortunately, ‘t Hooft and Veltman were familiar with another formalism, based on Feynman path integrals, as I then was not. Faddeev and Popov[30] and de Witt[31] had shown in 1967 that path integral methods allow a Lorentz-invariant choice of gauge in Yang-Mills theories. In 1971 `t Hooft used these methods to outline a demonstration of renormalizability in the electroweak theory,[32] completed in 1972 by ‘t Hooft & Veltman,[33] and Lee & Zinn-Justin.[34]

(The introduction of path integral methods into particle physics had a future importance that went even beyond the proof of renormalizability. Many theorists subsequently used path integral methods to discover effects that vanish in any finite order of perturbation theory, but can have dramatic effects, including the violation of symmetries of the perturbative theory.[35])

Suddenly in 1971 the electroweak theory looked very interesting. Right away it was extended to quarks,[36] and I found that with four quarks there was not only a suppression of strangeness-changing effects in *WW* exchange, as had been found earlier in the old current-current theory by Glashow, Iliopoulos, and Maiani,[37] but also in Z^0 exchange in the electroweak theory. Later in 1974 the discovery of the J/ψ particle by the Ting[38] and Richter[39] groups not only confirmed the existence of the fourth quark, but did much to set minds at ease about the reality of quarks. In 1972 I carried out a study of experimental evidence regarding the new neutral current weak interaction transmitted by the *Z* particle, and found that there was at that time no real evidence against it.[40]

Experimentalists began to search for neutral current weak interactions. They were found at CERN, first in the recoil of electrons in the scattering of neutrinos,[41] and then in the deep inelastic interactions of neutrinos with nucleons,[42] all with the predicted cross sections. After some kerfuffle about optical rotation in bismuth vapor, a SLAC-Yale experiment[43] showed that neutral currents do produce a predicted parity violation in the electron-nucleon interaction. With the acceptance of the electroweak theory, we had one part of the Standard Model.

The other part wasn't long in coming. The crucial step was taken in 1973 by Gross and Wilczek[44] and by Politzer,[45] who showed that in many theories with local symmetries like the Yang-Mills theory, interactions become weaker with decreasing distance, This could explain the "Bjorken scaling" found in 1968 in a MIT-SLAC experiment, and even more important, it held out the prospect for the first time of doing calculations using perturbation theory that could confirm a theory of strong interactions. (It's not so easy; one can only calculate things like operator-product coefficient functions[46] and infrared-safe scattering amplitudes,[47] that do not involve virtual gluons of low energy.) On this basis, they proposed a specific theory with a local SU(3) symmetry, now known as quantum chromodynamics. The name is reminiscent of quantum electrodynamics, because the theories are so similar, more similar in fact than was realized at first.

With the example of the electroweak theory then much on people's minds, Gross and Wilczek and Politzer in their first papers proposed that we do not see the massless gluons of the theory because a spontaneous breaking of the color gauge symmetry produces large gluon masses. Very soon, however, Gross and Wilczek[48] and I[49] instead suggested independently that the gauge symmetry of quantum chromodynamics is unbroken; so gluons are massless, and we do not see them or quarks either because color is trapped by the growth of the gauge coupling at large distances. This has not been proved, but it has become what Wightman used to call a folk theorem.

One of the things that particularly attracted me to this view is that, with no strongly interacting scalars or anything else added to allow spontaneous color symmetry breaking, and with only quarks and gluons in the theory, once one imposes the condition of renormalizability, quantum chromodynamics could explain some of the partial symmetries that had puzzled us for decades. The theory simply can't be complicated enough to violate the conservation of flavors like strangeness, even spontaneously.[50] If the up and down quark masses are small (not necessarily even approximately equal) it also automatically approximately conserves both isospin and chiral SU(2) x SU(2). If the strange quark mass is not too large one even automatically gets the approximate symmetry of the eightfold way. Quantum chromodynamics also respects charge-conjugation invariance and (aside from nonperturbative effects that need special treatment[51]) parity conservation. These symmetries are not respected by the weak interactions because there was never any reason why they should be respected – they are not fundamental principles, but only accidental consequences of the simplicity imposed by renormalizability on a theory of quarks and gluons. This was a true "Aha!" moment, when things you have known about for years are suddenly explained.

There are anomalies in the application of symmetries, that led to further constraints on the Standard Model. It goes back to the late 1960s, when calculations showed that the electromagnetic interaction does not have the chiral transformation properties expected from inspection of the Lagrangian, providing an explanation why the rate of neutral pion decay is not suppressed by a soft pion theorem.[52]

Chirality is just an accidental symmetry, so its limitation by anomalies was no great loss, but the consistency of the standard model requires that the anomalies in *local* symmetries (including general covariance) must all cancel. It is easy to see that this condition is satisfied if quarks and leptons fill out whole generations, but typically not otherwise. Thus, when the τ lepton was discovered,[53] it became necessary to complete a third generation with a bottom[54] and top[55] quark, which were later discovered.

The existence of a third generation then explained the breaking of another accidental symmetry. With just two generations (and one or two scalar doublets) there is no way that the renormalizable Standard Model could perturbatively break *CP* invariance. The weakness of the observed *CP* violation[56] is due to the weak mixing of the third generation with the first two.[57] The necessity of cancellation of anomalies in local symmetries also tightly constrained the U(1) quantum numbers (and hence the electric charges) of quarks and leptons, leaving little freedom in the Standard Model.

One question remained. Is the electroweak symmetry spontaneously broken *a la* Goldstone, by the expectation values of elementary scalar fields, as originally suggested by Salam and me, or *a la* Nambu (and BCS), by dynamical effects of new "technicolor forces," as suggested in 1979 by Susskind[58] and (hedging my bets) by me?[59] This was pretty well settled by the discovery in 2012 of a neutral particle[60] that appears to be the Higgs boson, the left-over member of the quartet of elementary scalars in the original electroweak theory, the one that does not get used up giving mass to the W^+ , W^- and Z^0 particles. So far, it seems to have just the properties predicted in 1967-8 by the electroweak theory.

So now we have the Standard Model. Its success is also the success of quantum field theory.

Or is it? Since the 1970s we have understood that within broad limits, *any* relativistic quantum theory will look like a quantum field theory, what is called an effective field theory, at energies E less than some fundamental scale M. (It's another folk theorem.)

In some theories symmetries do not allow any renormalizable theory, and effective field theory at energy E yields a power series in E/M, in which the leading term is given by tree graphs built from the nonrenormalizable interactions whose coupling constants are of lowest order in $1/M$, while higher order terms come from loops as well as from trees involving non-renormalizable interactions with couplings of higher order in $1/M$ that are available to cancel the infinities in the loops.[61] (As I like to put it, nonrenormalizable theories are just as renormalizable as renormalizable theories.) Since adding spacetime derivatives or factors of fields to an interaction increases the dimensionality of the interaction in units of mass, and hence increases the number of factors of $1/M$ in its coefficient, there can be only a finite number of parameters in the theory to any given order in $1/M$. These field theories then allow perfectly respectable calculations, although of course they lose all predictive power at energies approaching the fundamental scale M. It was in the case of soft pions governed by spontaneously broken chiral symmetry that all this about effective field theories was first understood.[62] In this case M is about 1200 MeV. General relativity is presumably the first term in another effective field theory, where M is the Planck scale, about 10^{18} GeV.

Of more relevance to the Standard Model is the case where symmetries do allow a renormalizable theory. Then the leading term, of zeroth order in E/M, will be a sum of all graphs built from renormalizable interactions. With hindsight, this is why the search for renormalizable theories turned out to be such a good idea. But there are also corrections of higher order in E/M, coming from the nonrenormalizable interactions. These can violate accidental symmetries, symmetries that are automatically (at least to all orders of perturbation theory) respected by any interaction that satisfies the gauge and Lorentz symmetries of the Standard Model and that is simple enough to be renormalizable.

In particular, the renormalizable Standard Model is too simple to violate baryon and lepton conservation perturbatively.

These conservation laws are violated in the Standard Model by nonperturbative effects, but these effects are negligible at ordinary temperatures. But there is no reason to expect these conservation laws to be respected at any temperature by nonrenormalizable corrections.[63] The discovery of tiny neutrino masses[64] shows that lepton number is in fact not absolutely conserved, and suggests a value of M of the order of 10^{15} GeV, similar to the energy at which the three gauge couplings of the Standard Model approach each other.[65] The universe itself suggests a tiny violation of baryon conservation, tiny in the sense that in the early universe, at temperatures above a GeV, there was about one extra quark for each 10^9 quark-antiquark pairs.

The question before us, then, is what is the theory that describes nature at very high energies? Is it a quantum field theory, maybe asymptotically safe, maybe supersymmetric, maybe a grand unified theory? Is it a theory that applies only in just one part of a multiverse? Is it a theory with three space and one time dimension, or something quite different, like a string theory? Does it even precisely obey the rules of quantum mechanics, as we know them?

The present generation of young physicists may envy those of us who had the excitement and delight of developing the Standard Model. This might be a mistake, just as it turned out that my generation would have been mistaken to envy the earlier heroes of quantum electrodynamics. Our newly minted experimentalists and theorists now have a chance to participate in making the next big step beyond the Standard Model. They may even be able to see their way clear to the very high energy scale where a final theory will be revealed.

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[1] In 2017-8 there were several meetings to celebrate the 50th anniversary of the Standard Model. The article below is based on talks I gave at some of these meetings, as follows: "Reminiscences of the Standard Model," livestreamed to the International Centre for Theoretical Physics, Trieste, Italy, to mark the 50th anniversary of the paper "A Model of Leptons," October 17, 2017;
"The Rise of the Standard Models," inaugural lecture in the Brazilian Physical Society Distinguished Colloquium Series, livestreamed to the Brazilian Physical Society, October 20, 2017;
"Conceptual Basis of the Standard Model," closing talk at a celebratory symposium "The Standard Model at 50 years," Case Western Reserve University, June 1-4 (2018);
"Origins of the Standard Model," opening talk at the SLAC Summer Institute 2018, livestreamed to the Stanford Linear Accelerator Center, July 30, 2018.
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