gested to some Indian zoologists that crabs, as well as other types of animals, may be sensitive to temperature, pressure, or geomagnetic changes that arise from the eclipse. J. V. R. Rao, from the Department of Zoology of Osmania University, organized experiments on the behavior of deer, fish, bats, crabs, and rabbits at ten locations on and near the eclipse path in order to further investigate this idea. Three eminent Indian neurosurgeons were interested in the possible effects of geomagnetic disturbances on the human brain. They measured electrical brain activity, cardiac response, skin resistance, and biochemical changes in a number of human subjects throughout the eclipse; one of the subjects was in a meditative state throughout totality. Inmates of the mental hospital of Hyderabad, including schizophrenics, epileptics, and retarded children, were the subjects of further electrical and biochemical tests. According to newspaper accounts, none of the human subjects in the experiments showed any marked reaction to the eclipse. Rao reported that a control group of rabbits at Rangapur, isolated in a lighted but windowless room, became quiescent or even inert during the totality phase.

Conclusion

As this survey indicates, scientific interest in total solar eclipses continues unabated. The next eclipses with reasonable chances for good weather and accessible sites will occur in July 1981 (in the Soviet Union and China), June 1983 (in Sumatra), and November 1984 (in New Guinea). I have no doubt that they will be well attended and that interesting experiments in a variety of disciplines will be performed.

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Toward a Unified Theory: Threads in a Tapestry

Sheldon Lee Glashow

In 1956, when I began doing theoretical physics, the study of elementary particles was like a patchwork quilt. Electrodynamics, weak interactions, and strong interactions were clearly separate disciplines, separately taught and separately studied. There was no coherent theory that described them all. Developments such as the observation of parity violation, the successes of quantum electrodynamics, the discovery of hadron resonances, and the appearance of strangeness were well-defined parts of the picture, but they could not be easily fitted together.

Things have changed. Today we have what has been called a standard theory of elementary particle physics in which strong, weak, and electromagnetic interactions all arise from a local symmetry principle. It is, in a sense, a complete and apparently correct theory, offering a qualitative description of all particle phenomena and precise quantitative predictions in many instances. There are no experimental data that contradict the theory. In principle, if not yet in practice, all experimental data can be expressed in terms of a small number of "fundamental" masses and coupling constants. The theory we now have is an integral work of art: the patchwork quilt has become a tapestry.

Tapestries are made by many artisans working together. The contributions of separate workers cannot be discerned in the completed work, and the loose and false threads have been covered over. So it is in our picture of particle physics. Part of the picture is the unification of weak and electromagnetic interactions and the prediction of neutral currents, now being celebrated by the award of the Nobel Prize. Another part concerns the reasoned evolution of the quark hypothesis from mere whimsy to established dogma. Yet another is the development of quantum chromodynamics into a plausible, powerful, and predictive theory of strong interactions. All are woven together in the tapestry; one part makes

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little sense without the other. Even the development of the electroweak theory was not as simple and straightforward as it might have been. It did not arise full-blown in the mind of one physicist, nor even of three. It, too, is the result of the collective endeavor of many scientists, both experimenters and theorists.

Let me stress that I do not believe that the standard theory will long survive as a correct and complete picture of physics. All interactions may be gauge interactions, but surely they must lie within a unifying group. This would imply the existence of a new and very weak interaction which mediates the decay of protons. All matter is thus inherently unstable, and can be observed to decay. Such a synthesis of weak, strong, and electromagnetic interactions has been called a grand unified theory, but a theory is neither grand nor unified unless it includes a description of gravitational phenomena. We are still far from Einstein's truly grand design.

Physics of the past century has been characterized by frequent great but unanticipated experimental discoveries. If the standard theory is correct, this age has come to an end. Only a few important particles remain to be discovered, and many of their properties are alleged to be known in advance. Surely this is not the way things will be, for nature must still have some surprises in store for us.

Nevertheless, the standard theory will prove useful for years to come. The confusion of the past is now replaced by a simple and elegant synthesis. The standard theory may survive as a part of the ultimate theory, or it may turn out to be fundamentally wrong. In either case, it will have been an important way station, and the next theory will have to be better.

In this talk, I shall not attempt to describe the tapestry as a whole, nor even that portion which is the electroweak synthesis and its empirical triumph. Rather, I shall describe several old threads, mostly overwoven, which are closely related to my own researches. My purpose is not so much to explain who did what when, but to approach the more difficult question of why things went as they did. I shall also follow several new threads which may suggest the future development of the tapestry.

Early Models

In the 1920's, it was still believed that there were only two fundamental forces: gravity and electromagnetism. In attempting to unify them, Einstein might have hoped to formulate a universal theory of physics. However, the study of the atomic nucleus soon revealed the need for two additional forces: the strong force to hold the nucleus together and the weak force to enable it to decay. Yukawa asked whether there might be a deep analogy between these new forces and electromagnetism. All forces, he said, were to result from the exchange of mesons. His conjectured mesons were originally intended to mediate both the strong and the weak interactions: they were strongly coupled to nucleons and weakly coupled to leptons. This first attempt to unify strong and weak interactions was fully 40 years premature. Not only this, but Yukawa could have predicted the existence of neutral currents. His neutral meson, essential to provide the charge independence of nuclear forces, was also weakly coupled to pairs of leptons.

Not only is electromagnetism mediated by photons, but it arises from the requirement of local gauge invariance. This concept was generalized in 1954 to apply to non-Abelian local symmetry groups (*1*). It soon became clear that a more far-reaching analogy might exist between electromagnetism and the other forces. They, too, might emerge from a gauge principle.

A bit of a problem arises at this point. All gauge mesons must be massless, yet the photon is the only massless meson. How do the other gauge bosons get their masses? There was no good answer to this question until the work of Weinberg and Salam (*2*) as proven by 't Hooft (*3*) (for spontaneously broken gauge theories) and of Gross, Wilczek, and Politzer (*4*) (for unbroken gauge theories). Until this work was done, gauge meson masses had simply to be put in ad hoc.

Sakurai suggested in 1960 that strong interactions should arise from a gauge principle (*5*). Applying the Yang-Mills construct to the isospin-hypercharge symmetry group, he predicted the existence of the vector mesons ρ and ω. This was the first phenomenological SU(2) $\times$ U(1) gauge theory. It was extended to local SU(3) by Gell-Mann and Ne'eman in 1961 (*6*). Yet, these early attempts to formulate a gauge theory of strong interactions were doomed to fail. In today's jargon, they used "flavor" as the relevant dynamical variable, rather than the hidden and then unknown variable "color." Nevertheless, this work prepared the way for the emergence of quantum chromodynamics a decade later.

Early work in nuclear beta decay seemed to show that the relevant interaction was a mixture of S, T, and P. Only after the discovery of parity violation, and the undoing of several wrong experiments, did it become clear that the weak interactions were in reality V-A. The synthesis of Feynman and Gell-Mann and of Marshak and Sudarshan was a necessary precursor to the notion of a gauge theory of weak interactions (*7*). Bludman formulated the first SU(2) gauge theory of weak interactions in 1958 (*8*). No attempt was made to include electromagnetism. The model included the conventional charged current interactions and, in addition, a set of neutral current couplings. These are of the same strength and form as those of today's theory in the limit in which the weak mixing angle vanishes. Of course, a gauge theory of weak interactions alone cannot be made renormalizable. For this, the weak and electromagnetic interactions must be unified.

Schwinger, as early as 1956, believed that the weak and electromagnetic interactions should be combined together into a gauge theory (*9*). The charged massive vector intermediary and the massless photon were to be the gauge mesons. As his student, I accepted this faith. In my 1958 thesis, I wrote: "It is of little value to have a potentially renormalizable theory of beta processes without the possibility of a renormalizable electrodynamics. We should care to suggest that a fully acceptable theory of these interactions may only be achieved if they are treated together" (*10*). We used the original SU(2) gauge interaction of Yang and Mills. Things had to be arranged so that the charged current, but not the neutral (electromagnetic) current, would violate parity and strangeness. Such a theory is technically possible to construct, but it is both ugly and experimentally false (*11*). We know now that neutral currents do exist and that the electroweak gauge group must be larger than SU(2).

Another electroweak synthesis without neutral currents was put forward by Salam and Ward in 1959 (*12*). Again, they failed to see how to incorporate the experimental fact of parity violation. Incidentally, in a continuation of their work in 1961, they suggested a gauge theory of strong, weak, and electromagnetic interactions based on the local symmetry group SU(2) $\times$ SU(2) (*13*). This was a remarkable portent of the SU(3) $\times$ SU(2) $\times$ U(1) model which is accepted today.

We come to my own work (*14*), done in Copenhagen in 1960, and done independently by Salam and Ward (*15*). We finally saw that a gauge group larger than

SU(2) was necessary to describe the electroweak interactions. Salam and Ward were motivated by the compelling beauty of gauge theory. I thought I saw a way to a renormalizable scheme. I was led to the group SU(2) × U(1) by analogy with the approximate isospin-hypercharge group which characterizes strong interactions. In this model there were two electrically neutral intermediaries: the massless photon and a massive neutral vector meson which I called B but which is now known as Z. The weak mixing angle determined to what linear combination of SU(2) × U(1) generators B would correspond. The precise form of the predicted neutral current interaction has been verified by recent experimental data. However, the strength of the neutral current was not prescribed, and the model was not in fact renormalizable. These glaring omissions were to be rectified by the work of Salam and Weinberg and the subsequent proof of renormalizability. Furthermore, the model was a model of leptons—it could not evidently be extended to deal with hadrons.

Renormalizability

In the late 1950's, quantum electrodynamics and pseudoscalar meson theory were known to be renormalizable, thanks in part to work of Salam. Neither of the customary models of weak interactions—charged intermediate vector bosons or direct four-fermion couplings—satisfied this essential criterion. My thesis at Harvard, under the direction of Schwinger, was to pursue my teacher's belief in a unified electroweak gauge theory. I had found some reason to believe that such a theory was less singular than its alternatives. Feinberg, working with charged intermediate vector mesons, discovered that a certain type of divergence would cancel for a special value of the meson anomalous magnetic moment (*16*). It did not correspond to a "minimal electromagnetic coupling," but to the magnetic properties demanded by a gauge theory. Tzou Kuo-Hsien examined the zero-mass limit of charged vector meson electrodynamics (*17*). Again, a sensible result is obtained only for a very special choice of the magnetic dipole moment and electric quadrupole moment, just the values assumed in a gauge theory. Was it just coincidence that the electromagnetism of a charged vector meson was least pathological in a gauge theory?

Inspired by these special properties, I wrote a notorious paper (*18*). I alleged that a softly broken gauge theory, with symmetry breaking provided by explicit mass terms, was renormalizable. It was quickly shown that this is false.

Again, in 1970, Iliopoulos and I showed that a wide class of divergences that might be expected would cancel in such a gauge theory (*19*). We showed that the naïve divergences of order $(\alpha\Lambda^4)^n$ were reduced to "merely" $(\alpha\Lambda^2)^n$, where Λ is a cutoff momentum. This is probably the most difficult theorem that Iliopoulos or I had ever proved. Yet, our labors were in vain. In the spring of 1971, Veltman informed us that his student 't Hooft had established the renormalizability of spontaneously broken gauge theory.

In pursuit of renormalizability, I had worked diligently but I completely missed the boat. The gauge symmetry is an exact symmetry, but it is hidden. One must not put in mass terms by hand. The key to the problem is the idea of spontaneous symmetry breakdown: the work of Goldstone as extended to gauge theories by Higgs and Kibble in 1964 (*20*). These workers never thought to apply their work on formal field theory to a phenomenologically relevant model. I had had many conversations with Goldstone and Higgs in 1960. Did I neglect to tell them about my SU(2) × U(1) model, or did they simply forget?

Both Salam and Weinberg had had considerable experience in formal field theory, and they had both collaborated with Goldstone on spontaneous symmetry breaking. In retrospect, it is not so surprising that it was they who first used the key. Their SU(2) × U(1) gauge symmetry was spontaneously broken. The masses of the W and Z and the nature of neutral current effects depend on a single measurable parameter, not two as in my unrenormalizable model. The strength of the neutral currents was correctly predicted. The daring Weinberg-Salam conjecture of renormalizability was proven in 1971. Neutral currents were discovered in 1973 (*21*), but not until 1978 was it clear that they had just the predicted properties (*22*).

The Strangeness-Changing Neutral Current

I had more or less abandoned the idea of an electroweak gauge theory during the period 1961 to 1970. Of the several reasons for this, one was the failure of my naïve foray into renormalizability. Another was the emergence of an empirically successful description of strong interactions—the SU(3) unitary symmetry scheme of Gell-Mann and Ne'eman. This theory was originally phrased as a gauge theory, with ρ, ω, and κ^* as gauge mesons. It was completely impossible to imagine how both strong and weak interactions could be gauge theories: there simply wasn't room enough for commuting structures of weak and strong currents. Who could foresee the success of the quark model, and the displacement of SU(3) from the arena of flavor to that of color? The predictions of unitary symmetry were being borne out—the predicted Ω^- was discovered in 1964. Current algebra was being successfully exploited. Strong interactions dominated the scene.

When I came upon the SU(2) × U(1) model in 1960, I had speculated on a possible extension to include hadrons. To construct a model of leptons alone seemed senseless: nuclear beta decay, after all, was the first and foremost problem. One thing seemed clear. The fact that the charged current violated strangeness would force the neutral current to violate strangeness as well. It was already well known that strangeness-changing neutral currents were either strongly suppressed or absent. I concluded that the Z^0 had to be made very much heavier than the $W^{\pm}$. This was an arbitrary but permissible act in those days: the symmetry-breaking mechanism was unknown. I had "solved" the problem of strangeness-changing neutral currents by suppressing all neutral currents: the baby was lost with the bath water.

I returned briefly to the question of gauge theories of weak interactions in a collaboration with Gell-Mann in 1961 (*23*). From the recently developing ideas of current algebra, we showed that a gauge theory of weak interactions would inevitably run into the problem of strangeness-changing neutral currents. We concluded that something essential was missing. Indeed it was. Only after quarks were invented could the idea of the fourth quark and the GIM (Glashow-Iliopoulos-Maiani) mechanism arise.

From 1961 to 1964, Sidney Coleman and I devoted ourselves to the exploitation of the unitary symmetry scheme. In the spring of 1964, I spent a short leave of absence in Copenhagen. There, Bjorken and I suggested that the Gell-Mann-Zweig system of three quarks should be extended to four (*24*). [Other workers had the same idea at the same time (*25*).] We called the fourth quark the charmed quark. Part of our motivation for introducing a fourth quark was based on our mistaken notions of hadron spectroscopy. But we also wished to enforce an analogy between the weak leptonic current and the weak hadronic current. Because there were two weak doublets of leptons, we believed there had to be two weak doublets of quarks as well.

The weak current Bjorken and I introduced in 1964 was precisely the GIM current. The associated neutral current, as we noted, conserved strangeness. Had we inserted these currents into the earlier electroweak theory, we would have solved the problem of strangeness-changing neutral currents. We did not. I had apparently quite forgotten my earlier ideas of electroweak synthesis. The problem which was explicitly posed in 1961 was solved, in principle, in 1964. No one, least of all me, knew it. Perhaps we were all befuddled by the chimera of relativistic SU(6), which arose at about this time to cloud the minds of theorists.

Five years later, Iliopoulos, Maiani, and I returned to the question of strangeness-changing neutral currents (*26*). It seems incredible that the problem was totally ignored for so long. We argued that unobserved effects (a large K_1K_2 mass difference, decays like $K \rightarrow \pi\nu\bar{\nu}$, and so on) would be expected to arise in any of the known weak interaction models: four-fermion couplings, charged vector meson models, or the electroweak gauge theory. We worked in terms of cutoffs, since no renormalizable theory was known at the time. We showed how the unwanted effects would be eliminated with the conjectured existence of a fourth quark. After languishing for a decade, the problem of the selection rules of the neutral current was finally solved. Of course, not everyone believed in the predicted existence of charmed hadrons.

This work was done fully 3 years after the epochal work of Weinberg and Salam, and was presented in seminars at Harvard and Massachusetts Institute of Technology. Neither I, nor my co-workers, nor Weinberg sensed the connection between the two endeavors. We did not refer, nor were we asked to refer, to the Weinberg-Salam work on our paper.

The relevance became evident only a year later. Due to the work of 't Hooft, Veltman, Benjamin Lee, and Zinn-Justin, it became clear that the Weinberg-Salam *ansatz* was in fact a renormalizable theory. With GIM, it was trivially extended from a model of leptons to a theory of weak interactions. The ball was now squarely in the hands of the experimenters. Within a few years, charmed hadrons and neutral currents were discovered, and both had just the properties they were predicted to have.

From Accelerators to Mines

Pions and strange particles were discovered by passive experiments which made use of the natural flux of cosmic rays. However, in the last three decades, most discoveries in particle physics were made in the active mode, with the artificial aid of particle accelerators. Passive experimentation stagnates from a lack of funding and lack of interest. Recent developments in theoretical particle physics and in astrophysics may mark an imminent rebirth of passive experimentation. The concentration of virtually all high-energy physics endeavors at a small number of major accelerator laboratories may be a thing of the past.

This is not to say that the large accelerator is becoming extinct; it will remain an essential if not exclusive tool of high-energy physics. Do not forget that the existence of Z^0 at $\sim$ 100 GeV is an essential but quite untested prediction of the electroweak theory. There will be additional dramatic discoveries at accelerators, and these will not always have been predicted in advance by theorists. The construction of new machines like LEP and ISABELLE is mandatory.

Consider the successes of the electroweak synthesis, and the fact that the only plausible theory of strong interactions is also a gauge theory. We must believe in the ultimate synthesis of strong, weak, and electromagnetic interactions. It has been shown how the strong and electroweak gauge groups may be put into a larger but simple gauge group (*27*). Grand unification—perhaps along the lines of the original SU(5) theory of Georgi and me—must be essentially correct. This implies that the proton, and indeed all nuclear matter, must be inherently unstable. Sensitive searches for proton decay are now being launched. If the proton has a lifetime shorter than 10^{32} years, as theoretical estimates indicate, it will not be long before it is seen to decay.

Once the effect is discovered (and I am sure it will be), further experiments will have to be done to establish the precise modes of decay of nucleons. The selection rules, mixing angles, and space-time structure of a new class of effective four-fermion couplings must be established. The heroic days of the discovery of the nature of beta decay will be repeated.

The first generation of proton decay experiments is cheap, but subsequent generations will not be. Active and passive experiments will compete for the same dwindling resources.

Other new physics may show up in elaborate passive experiments. Today's theories suggest modes of proton decay which violate both baryon number and lepton number by unity. Perhaps this $\Delta B = \Delta L = 1$ law will be satisfied. Perhaps $\Delta B = -\Delta L$ transitions will be seen. Perhaps, as Pati and Salam suggest, the proton will decay into three leptons. Perhaps two nucleons will annihilate in $\Delta B = 2$ transitions. The effects of neutrino oscillations resulting from neutrino masses of a fraction of an electron volt may be detectable. "Superheavy isotopes," which may be present in the earth's crust in small concentrations, could reveal themselves through their multi-GeV decays. Neutrino bursts arising from distant astronomical catastrophes may be seen. The list may be endless or empty. Large passive experiments of the sort now envisioned have never been done before. Who can say what results they may yield?

Premature Orthodoxy

The discovery of the J/Ψ in 1974 made it possible to believe in a system involving just four quarks and four leptons. Very quickly after this a third charged lepton (the tau) was discovered, and evidence appeared for a third $Q = -1/3$ quark (the b quark). Both discoveries were classic surprises. It became immediately fashionable to put the known fermions into families or generations:

$$\begin{bmatrix} u & \nu_e \\ d & e \end{bmatrix} \qquad \begin{bmatrix} c & \nu_\mu \\ s & \mu \end{bmatrix} \qquad \begin{bmatrix} t & \nu_\tau \\ b & \tau \end{bmatrix}$$

The existence of a third $Q = 2/3$ quark (the t quark) is predicted. The Cabibbo-GIM scheme is extended to a system of six quarks. The three-family system is the basis of a vast and daring theoretical endeavor. For example, a variety of papers have been written putting experimental constraints on the four parameters which replace the Cabibbo angle in a six-quark system. The detailed manner of decay of particles containing a single b quark has been worked out. All that is wanting is experimental confirmation. A new orthodoxy has emerged, one for which there is little evidence and one in which I have little faith.

The predicted t quark has not been found. While the upsilon mass is less than 10 GeV, the analogous $t\bar{t}$ particle, if it exists at all, must be heavier than 30 GeV. Perhaps it does not exist.

Georgi and I, and others before us, have been working on models with no t quark (*28*). We believe this unorthodox view is as attractive as its alternative. And it suggests a number of exciting experimental possibilities.

We assume that b and τ share a quantum number, like baryon number, that is essentially exactly conserved. (Of course, it may be violated to the same extent that baryon number is expected to be violated.) Thus, the b, τ system is assumed to be distinct from the lighter four quarks and four leptons. There is, in particular, no mixing between b and d or s. The original GIM structure is left intact. An additional mechanism must be invoked to mediate b decay, which is not present in the SU(3) × SU(2) × U(1) gauge theory.

One possibility is that there is an additional SU(2) gauge interaction whose effects we have not yet encountered. It could mediate such decays of b as

$$b \rightarrow \tau^+ + (e^- \text{ or } \mu^-) + (d \text{ or } s)$$

All decays of b would result in the production of a pair of leptons, including a τ^+ or its neutral partner. There are other possibilities as well, which predict equally bizarre decay schemes for b matter. How the b quark decays is not yet known, but it soon will be.

The new SU(2) gauge theory is called upon to explain CP violation as well as b decay. In order to fit experiment, three additional massive neutral vector bosons must exist, and they cannot be too heavy. One of them can be produced in e^+e^- annihilation, in addition to the expected Z^0. Our model is rife with experimental predictions: for example, a second Z^0, a heavier version of b and of τ, the production of τ b in e p collisions, and the existence of heavy neutral unstable leptons which may be produced and detected in e^+e^- or in νp collisions.

This is not the place to describe our views in detail. (Nonetheless, I must say in passing that our scheme fits neatly into a grand unified theory based on the exceptional group E_6.) The point I wish to make is simply that it is too early to convince ourselves that we know the future of particle physics. There are too many points at which the conventional picture may be wrong or incomplete. The SU(3) × SU(2) × U(1) gauge theory with three families is certainly a good beginning, not to accept but to attack, extend, and exploit. We are far from the end.

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