UNIFICATION — THEN AND NOW

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On this multiply celebratory occasion I shall describe various incidents of travel on the road to electroweak unification. Three quotations will serve to introduce my talk. The epigram to my 1958 doctoral thesis was taken from Galileo's *Dialogo sopra i due Sistemi: "The poetic imagination takes two forms: as those who can invent fables, and as those who are inclined to believe them."* My advisor, Julian Schwinger, invented the fable of electroweak unification, but hardly anyone was disposed to believe it. When he entrusted the matter to me, as his graduate student I had little choice.

My second quotation is from Hamlet: *"And thus do we by indirections find directions out."* It introduced a section of my thesis which tried, and failed, to explain a puzzling property of strange particles. In the course of my tale, I shall mention several other false starts and bumbling blunders (mostly mine) and a few brilliant insights (mostly by others). I apologize to the many colleagues whose work I do not cite and for not offering a more systematic and balanced account of a long and multifaceted tale.

Aging Nobel Laureates are sometimes regarded as authority figures. For example, I am often asked what the lessons of the past can teach us about the future of our discipline. I wish I knew, but instead I offer my last quotation. As it is written in the bible: *"Many are in high place and of great renown, but mysteries are revealed unto the meek"* ...and perhaps, unto my experimental colleagues at the Large Hadron Collider.

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Cern was created in the Fall of 1954, just as I began my graduate study at Harvard. Two years later, Schwinger told me to investigate whether unifying weak and electromagnetic forces with a Yang-Mills-like model was feasible. He had two reasons to advance such a daring notion:

* Versions of this talk were presented in December, 2009 at the Campus de Excelencia and at Ciemat, both in Madrid.

1. Both interactions were known to display a certain *universality,* as shown by the equality of proton and electron charges and by the equal strengths of several disparate weak decay processes.
2. Both interactions were known to be *vectorial:* The then recently established V–A structure of weak interactions required their hypothetical mediators, now called $W^{\pm}$, to be spin-one bosons, like photons. However, Ws would have to be charged and massive, whereas photons are neutral and massless.

I soon found two hints suggesting the self interactions of these three bosons to be of the Yang-Mills form. An obscure note in Comptes Rendu by Tzou Kuo-Hsien showed the zero-mass limit of the field equation for a charged vector boson to make sense if its magnetic moment were that of a Yang-Mills gauge symmetry, but not otherwise. Furthermore, my high-school buddy Gary Feinberg computed the rate for radiative muon decay induced by a W loop. His result conflicted with known data unless the electron and muon neutrinos were different particles, as Schwinger had argued earlier. Gary's calculation required a cutoff, except in the Yang-Mills case, where the divergence was absent. Thus, I realized, the 1-loop contribution to $g - 2$ would be finite were the Ws part of a unified gauge theory.

When my thesis, *The Vector Boson in Elementary Particle Decay* was done, I set forth to Copenhagen on an NSF postdoctoral fellowship. I had not found a suitable electroweak model, but I became convinced, as I wrote in my thesis, that: *"A fully acceptable theory of weak and electromagnetic interactions may only be achieved if they are treated together."*

Niels Bohr hosted dozens of postdocs at his Institut for Teoretisk Fysik on Blegdamsvej. In this wondrously stimulating environment, I wrote several modest papers with Swedish, Polish and American collaborators... and one really awful paper alleging a softly-broken Yang-Mills theory to be renormalizable. Anyone competent in quantum field theory could have spotted my error. Nonetheless, Abdus Salam invited me to speak about my work at Imperial College. My talk was well received and afterward Salam had me to his home for a marvelous Pakistani dinner. But when I returned to Copenhagen, two Imperial College preprints awaited me. Both Salam and Kamefuchi had written papers showing that I was wrong. Couldn't

Salam simply have told me of my mistake?

I would spend my second postdoctoral year at Cern, just as its PS accelerator was commissioned, half a century ago. I was warmly received by its theory group, including (among others) Jacques Prentki, Bernard d'Espagnat and André Petermann. I was amused to learn that Petermann had recently estimated the two-loop contribution to the muon magnetic moment, because my Harvard buddy Charles Sommerfield, another of Schwinger's many advisees, had done the very same thing. Charlie, however, had gotten the exact result.

My stay at Cern overlapped with a visit by Jeffrey Goldstone, during which he devised his eponymous bosons. Listen to the conclusion to his seminal 1960 paper: "A method of losing symmetry is... highly desirable in particle physics, but these theories will not do this without introducing non-existent massless bosons... If use is to be made of these solutions, something more complicated than the simple models considered in this paper will be necessary." That something—the Higgs mechanism—would arrive shortly.

Putting aside the issue of renormalizability, I turned to the algebraic structure of weak and electromagnetic charges. If the two-neutrino hypothesis were true, they would generate the 4-parameter algebra of $SU(2) \times U(1)$. Thus an electroweak model had to involve a heavy neutral boson as well as charged Ws. I was delighted when Murray Gell-Mann, in the Spring of 1960, invited me to speak in Paris where he was spending a sabbatical. Murray's appreciation of my ideas was made doubly clear soon afterward. He presented them (with due attribution) at the 1960 Rochester Conference and asked me to join him as a CalTech postdoc. I accepted his offer and returned to Copenhagen to gather my electroweak thoughts and write the paper which, some two decades later, would earn me a portion of the Nobel Prize. Schwinger's challenge had been met... except for two seemingly insuperable obstacles: how to break the gauge symmetry so that the weak intermediaries could acquire mass, and how to include hadrons in the model. Electroweak synthesis was still just a fable.

Soon after arriving at CalTech I met Sidney Coleman, who was then Murray's graduate student. He would soon become an inspired teacher and an incomparably brilliant theorist, as well as my close friend, frequent collaborator and colleague at Harvard. When Murray invented flavor $SU(3)$, or what he called the Eightfold Way, Sidney and I became its ardent advocates. We developed some of the consequences of the scheme and spent the next few years traveling around the world as disciples of Murray's promising idea.*

During my year in Pasadena, I once had the chance to collaborate with Murray. We examined possible applications of 'partially gauge-invariant' models, by which we meant those whose symmetries were broken only by masses: not a very sensible procedure, but all we could do at the time. Nonetheless, our insignificant paper is interesting in retrospect:

1. "The remarkable universality of electric charge," we wrote, "would be better understood were the photon a member of a family of vector bosons [associated with a simple Lie group]." Years later, Howard Georgi and I realized this notion with a model based on $SU(5)$. Although our attempt at grand unification has been ruled out, many theorists are convinced that the underlying idea is correct.
2. "In general, weak and strong gauge symmetries will not be compatible." Indeed! This problem could not be addressed until the notion of quarks with color arose, thus enabling the formulation of quantum chromodynamics.
3. "It is possible to find a formal theory of [weak and electromagnetic interactions involving] four intermediate bosons." We extended my electroweak model to include hadrons by introducing a precurser to Cabibbo's angle in the context of the Sakata model, but only at the terrible cost of predicting unseen strangeness-changing neutral currents. "We are missing some important ingredient of the theory," we concluded. That important ingredient would be charm!

After my stint at CalTech, I accepted faculty positions at Stanford and later at Berkeley. During this time I continued working out the implications of flavor $SU(3)$ in close collaboration with Sidney and with my experimental colleagues at Berkeley. The weak interaction front heated up in 1962, when Lederman, Schwartz and Steinberger confirmed the two neutrino hypothesis, and a year later, when Cabibbo showed that the weak

* Flavor $SU(3)$ was independently arrived at by Yuval Néeman.

current of flavor $SU(3)$ correctly describes the leptonic decays of hadrons. In the next year, our story became even more thrilling. Here are some of the highlights of 1964:

January: Gell-Mann suggested quarks as hadron constituents, but not specifying whether they were mathematical fictions or real particles.

February: Nick Samios discovered the Ω^- particle, whose existence and properties Murray had predicted.

July: Fitch, Cronin *et al.* discovered CP violation in kaon decay, an effect that was entirely unanticipated.

August: James Bjorken and I proposed the existence of a fourth (charmed) quark to establish lepton-quark symmetry.

October: Oskar Greenberg proposed the additional quark attribute that would evolve to become quark color.

And in August, October and November: Three seminal papers appeared in Volume 13 of the Physical Review Letters. Taken together, they established what is now known as the Higgs mechanism.

Everyone saw the importance of CP violation; most particle physicists accepted the relevance of flavor $SU(3)$ once the Ω^- was found; and many theorists took the idea of quarks seriously; but hardly anyone bothered with charm, quark color or the Higgs mechanism. Much of the fruit of the 1964 vintage would remain on the vine for years.

• Why did it take until 1967 for Steve Weinberg (and Abdus Salam, a year later) to use the Higgs mechanism to explain the breaking of electroweak gauge symmetry? And why was this work ignored until the suspected renormalizability of the theory was established? (There were just two citations to Weinberg's paper prior to 1971, but over 7000 afterward!)

• Why did it take a decade for anyone to see that quark color provides an arena distinct from quark flavor where strong interactions could operate without interfering with electroweak gauge symmetry?

• Why did Bjorken and I not realize that our charmed quark could enable the extension of the electroweak model to hadrons? It seems incredible that nobody made this simple connection until 1970, when I returned to the issue with John Iliopoulos and Luciano Maiani at Harvard.

We three thoroughly enjoyed working together and showing how charm expunged strangeness-changing neutral currents via the GIM mechanism.

When we visited MIT to explain our work to Weinberg, he was amiable but showed little interest. At the time, we ourselves were blissfully unaware of his 1967 electroweak paper and Steve himself seemed to have forgotten about it.

The next crucial event took place at the Amsterdam Conference in 1971, where Gerard 'tHooft announced his proof of the renormalizability of the electroweak model with spontaneous symmetry breaking provided by the Higgs mechanism. I learned about this seminal work during my honeymoon, when I attended the Marseille Conference in the Summer of 1972. Tini Veltman explained what his student Gerard had done and thus, how his own years of work on non-Abelian gauge theories had paid off... although seventeen more would pass before he and Gerard would share the Nobel Prize. Soon after Gerard's bombshell, physicists recognized that the electroweak theory could describe all weak interactions, with the GIM mechanism excluding the unseen strangeness-changing neutral currents. Schwinger's fable had at last emerged as a plausible, predictive and mathematically consistent theory.

Scientists at Cern and Fermilab quickly set out to find the predicted strangeness-conserving neutral currents. Cern succeeded in 1973, as did Fermilab shortly thereafter. "Unified physics theory confirmed... a finding of historic importance" wrote the New York Times. However, Tini Veltman believes that neutral-currents could and should have been discovered a decade earlier. He feels that "there was a rather heavy bias against this type of event," and "experimentally it [was] made sure that even if there were such events they would not have been discovered."

But what about the charmed quark? In April, 1974, at a conference on meson spectroscopy, I promised to eat my hat if charmed particles were not found prior to the next such meeting. Only a few months later, Sam Ting invited me to his MIT office to tell me about his startling discovery of a new particle at Brookhaven Lab. On that same day, November 11, 1974, scientists at SLAC announced their independent discovery of the very same particle. Thus, this doubly discovered particle bears the double name J/Ψ. Eight theoretical explanations soon appeared in the same issue of Physical Review Letters. Most were dead wrong, including one by Schwinger and another by Maiani, but two of the papers, both from Harvard, were on the money: one by Tom Appelquist and David Politzer, the other by Alvaro de Rújula and me.

The J/Ψ, we insisted, is a bound state of a charmed quark and its antiquark, a system which Alvaro christened charmonium. Most particle physicists remained unconvinced, until the p-wave excitations of the charmonium 'atom' were found to lie just where they had been predicted by theorists at both Cornell and Harvard. But what about particles containing a single charmed quark, or what Alvaro and I called 'bare charm'? The Samios group at Brookhaven reported one likely candidate for a charmed baryon in 1975, but it took until the Spring of 1976 for charmed mesons to be seen at experiments performed at SLAC. At this point, almost everyone agreed that there had to be four quark flavors.

Among the exceptions were two Japanese physicists, Makoto Kobayashi and Toshihide Maskawa. In 1973, well before the fourth quark found its way into textbooks, they advocated a theory with six quark flavors. Only then, they showed, could CP violation be neatly described by the theory. Their brilliant idea was ignored until evidence began to accumulate for the existence of unanticipated new particles. A third charged lepton, tau, was discovered in 1975 due to the perspicacity and persistence of Martin Perl. Much the same could be said about Leon Lederman, whose group discovered the bottom quark just two years later. This time around, no one doubted that these particles were part of a third family of fundamental fermions, even though two decades would elapse before top quarks and tau neutrinos would be seen.

Meanwhile, our understanding of the strong force had evolved. Quantum Chromodynamics, an unbroken non-Abelian gauge theory acting in the arena of quark color, arose in the early 1970s. Its property of asymptotic freedom explained the narrow width of the J/Ψ, and would enable perturbative calculations of many strong-interaction phenomena. QCD and the electroweak model, two mutually compatible gauge theories, constitute our standard theory of particle physics. By 1979, even the Nobel Committee trusted in electroweak unification, even though its central predictions, those of W and Z bosons, had not yet been verified. Carlo Rubbia and his colleagues at Cern would soon remedy that.

Despite heroic efforts of experimenters at Fermilab's Tevatron and

Cern's Lep collider (whose 20th anniversary we now celebrate), many daunting problems remain unresolved. Some will soon be addressed: (1) No Higgs boson has yet been seen. Furthermore, the Higgs mechanism *per se* cannot fully explain electroweak symmetry breaking. We do not know just what kind of 'new physics' is needed, but we are counting on LHC data to provide at least part of the answer. (2) Nonbaryonic dark matter, whose existence astronomers seem to have established, may consist of new kinds of particles. If so, they may be produced, observed and studied at the LHC.

Other questions cannot so "easily" be answered: What is the origin of neutrino mass? Why is the cosmological constant so tiny? But my own most vexing problem is that of flavor: At least 20 parameters are needed to describe the various masses and mixings of quarks and leptons. Most of these have been measured, but no plausible theoretical relation among them has ever been found. Are we likely to find such relations in the future? Or are these 20 numbers simply accidents of birth of the universe, just as the radii of planetary orbits are accidents of birth of the solar system. Some of my string-bound colleagues advocate just such a gloomy philosophy. For them I would pose one last question: How can we ever learn whether superstrings are the correct approach to fundamental physics?

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