



Development of Unified Gauge Theories -- Retrospect<sup>\*</sup>

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On this occasion of Steven Weinberg's being awarded a Heineman prize for his outstanding contribution to mathematical physics, especially for his important contribution to the construction and development of unified gauge theory of weak, electromagnetic and strong interactions, I thought I might review the subject and Steve's leading contributions to it. I soon found that I am not an historian, and I lack the tools, methods and perspective for such an undertaking. I will therefore recount my own recollections on the development of this subject, and Steve Weinberg's influence -- both personal and scientific -- on my own education and work on this subject. My recollections are intensely personal ones; the subject matter is much too fresh in our mind to view dispassionately. It is not an attempt to write, or rewrite history. It is rather a tribute to Steve on this happy occasion from one who has benefitted enormously from his wisdom, insight, and from his friendship.

In 1959, I attended an annual APS meeting in Washington, D.C., for the first time, where Steve Weinberg gave a talk on renormalization, in which he proved a fundamental theorem on this subject, to quote his abstract, "by judicious use of the Heine-Borel theorem." The content of this theorem is a cornerstone in our understanding of field theory, and it is extensively used in the most popular textbook on this subject. In <sup>the</sup> short talk, I grasped very little, but I was enormously impressed by his delivery and by whatever little I understood

of it. I mention this because I believe it was the first occasion I saw him. I was a graduate student, and Steve was already an established physicist, through his work on second class current, K decays, etc. I did meet Steve some years later at the old Bookbinder's in Philadelphia.

Let me digress and say something about my first encounter with gauge theory. During the academic year 1960-1961, I was at the Institute for Advanced Study. I worked mostly on Regge poles in field theory, but I spent some time off studying the paper of Yang and Mills. I was fascinated by the beauty of the concept of nonabelian gauge invariance. I read the paper several times, and found myself more and more confused when I tried to understand the quantum version of the theory as Yang and Mills described it. In this paper, Yang and Mills were trying to use a generalization of the Gupta-Bleuler formalism, and the subsidiary condition imposed on the vacuum was quadratic in field variables. I struggled several days and nights to construct a Fock space in which this condition could be satisfied, but with not success. Frank Yang, who later is to have a most profound effect on me, was upstairs above my office, but, being a fresh Ph.D., I could not muster courage to ask him a question on this point. I suppose I was afraid to expose my ignorance. The second thing I remember about this year was a seminar by T.D. Lee, on the  $\xi$ -limiting process he and Yang invented to regularize electrodynamics of vector bosons. The most important lesson on me was that the

interaction Hamiltonian is not always the negative of the interaction Lagrangian and the folklore version of the Mathews theorem is not always correct.

After a year's sojourn at Princeton, I came back to my alma mater, the University of Pennsylvania, and worked closely with my thesis supervisor -- Abe Klein. At that time,  $SU(3)$  was about to be accepted by everyone, and the BCS theory of superconductivity was being understood at a deeper level. Abe and Sid Bludman were working on spontaneous breakdown of  $SU(3)$ , and soon they called my attention to the very important paper Steve wrote with Goldstone and Salam. I am referring here to the paper in which the celebrated Goldstone theorem was proved in two different ways -- first by what is now known as the current algebra technique, and second by what might now be called as the effective potential method. I must confess that I understood the first, but it took a few more years for me to almost rediscover the second.

In any case, the problem Abe Klein and I were worrying about was this: It would be esthetically nice if the breaking of  $SU(3)$  arose spontaneously, but how are we going to get rid of the Goldstone bosons then? I had written a paper in collaboration with Marshall Baker and Ken Johnson a few months previously, arguing that their version of electrodynamics did not possess a Goldstone boson, even though the formal chiral symmetry of the theory is broken in the solution. It did not seem to Abe and me a satisfactory solution for  $SU(3)$  for reasons that I was then not able to verbalize coherently --

we now know that this situation in the Baker-Johnson electrodynamics is due to the  $\gamma_5$  anomaly. Abe's versatility with many fields of theoretical physics soon led to a clue, and this was that superconductors did not have a Goldstone excitation. In the formulation we were looking at, the BCS pair field develops a nonvanishing expectation value, but there is no phonon-like excitation in the system. Abe and I coauthored a letter, pointing out that an evasion of the Goldstone theorem was possible in this case, by the mechanism since named "seizing the vacuum" by Lenny Susskind and Kogut, and ending with a hopeful note that a similar situation might prevail in a relativistic theory. Our paper was, soon after publication, severely criticized by Wally Gilbert, who was then turning into an able molecular biologist, who showed that what we said, the existence of a "spurious state" of zero energy and momentum, not zero mass, was an impossibility in a relativistic covariant theory. We found ourselves defenseless.

Then came Higgs. In his short communication, he came to our defense, pointing out that certain relativistic theories -- gauge theories and electrodynamics, in particular -- can be formulated only in a noncovariant way if one is to use a positive definite Hilbert space, and in such theories, what we said might happen could happen! In subsequent papers, he constructed an explicit model, which is now known as the Abelian Higgs model. I have elaborated on the small part I

happen to play in the understanding of the Higgs phenomenon, since in all the reviews I have seen, the clarification Higgs offered to the Klein-Lee-Gilbert controversy did not seem to be appreciated. However, I think the persons who first understood the subject *were* probably Schwinger, P.W. Anderson, Abe Klein, and certainly not I.

I failed to follow up immediately on Higgs' lead, partly because, by training, I was not comfortable in the radiation gauge (which affliction, I hope, has been remedied since), but mostly because I spent the year 1964-1965 again at the Institute, where I was busy learning and working on SU(6) and current algebra. It is an irony of history that the Goldstone bosons we learned to get rid of became an object of adulation and necessity -- they are the pions! Of course the group changed -- it is no longer SU(3), but SU(2)  $\times$  SU(2).

An important advance in my understanding of the current algebra came from Weinberg's paper on chiral dynamics, which was published around 1966. Up to that time, the current algebra seemed to me a highly technical field-theoretical manipulation Steve Adler was better at than anybody else. What Steve Weinberg taught me was that the results of current algebra manipulations *could* be gotten from, and understood as the low energy theorems applied to "almost"-Goldstone bosons and may be derived from any Lagrangian in tree approximation in which chiral symmetry *was* realized in the Goldstone mode. In 1967, Steve wrote a paper with T.D. Lee and Bruno Zumino

on field algebra in which currents ~~were~~ represented by non-abelian gauge fields. Even though I have not discussed this matter with Steve, I think his work on field algebra played an important role, at least at a subconscious level, in his inventing the Weinberg model of leptons. In field algebra, the breaking of degeneracy between  $\rho$  and  $A_1$  is due to a mechanism very similar to the Higgs mechanism.

Unlike most important papers which I have had the privilege of reading in preprint form, somehow I did not read Steve's revolutionary idea on unification of weak and electromagnetic interactions in preprint: I was asked to referee it by Physical Review Letters. I am proud to say that I understood the significance of the paper immediately: I was familiar with the works of Schwinger, Glashow, Bludman, Salam and Ward on unification, but I was not satisfied with the way the mass terms for gauge bosons were introduced by hand. Steve solved that problem at last. At the end of the paper, he gave a reason why the theory might be renormalizable. He said that since Yang-Mills theory was renormalizable, his theory might be. This is a point that would haunt me for several years. At that time, I asked Frank Yang about it. I think he answered that he was not convinced that Yang-Mills theory had been shown to be renormalizable. He did tell me about the work of Feynman, de Witt and Mandelstam. Somehow I failed to follow up on Yang's comments immediately.

Abdus Salam's parallel work, for which he deserves credit, did not come to my attention until 1971, *owing* to the fact that I was ignorant of the Nobel Symposium, and knew nothing about the existence of its Proceedings. I admire Abdus both as a man of science, and a man of Islamic virtue. I regret very much that his wisdom had no impact on my thinking in this period.

I must be very frank with Steve, and tell him now that I was not very happy with the development of chiral dynamics then. The question I repeatedly asked myself then was how the various relations derived in tree approximation of chiral Lagrangian remain intact under renormalization. Of course, the answer was that the current algebra manipulation deals with renormalized quantities. The point really is that renormalization does not mess up things, and I wanted to see them explicitly. In the summer of 1968, Steve was visiting Stony Brook, where I worked, and I casually mentioned that I would like to see that renormalization did not spoil the current algebra and chiral dynamics. I don't believe Steve recalls this, but he expressed that that was a good idea, and he was very encouraging. Since I had a leave of absence coming the following academic year, I decided to study this question then.

I spent the academic year 1968-69 in Paris, or more precisely at Orsay and Bures-sur-Yvette. There were two important things that happened to me that year. I understood the renormalized  $\sigma$ -model. That is, I found that spontaneous breakdown of symmetry does not alter the divergence structure

of the theory at all. I was very fortunate in having Arthur Wightman, Klaus Hepp, Wolfhart Zimmermann as colleagues at Bures-sur-Yvette. The second important thing was that Tini Veltman was visiting Orsay for a year, and he was well on his way in his study of massive Yang-Mills theory. But the great favor he did to me, for which I am very grateful, is to introduce me to the translation of Fadeev and Popov, and Feynman's lecture in Poland, which were not available to me in the States. I worked also on nonlinear  $\sigma$ -model, getting correct Feynman rules, etc.

I came back from Paris, in 1969, and Frank Yang asked me to give an impromptu seminar on what I did in Paris. I talked about the quantum theory of spontaneous symmetry breakdown with the  $\sigma$ -model as an example. Afterwards, Frank told me privately that he thought that the Yang-Mills theory should be quantized in a similar way, around a stable classical solution. I immediately mentioned the Higgs model, but he said that that was not what he had in mind, but that he meant the kind of classical solution of pure Yang-Mills field that he and T.T. Wu discovered, which gives rise to a finite action. His comment did make an impression on me, but I failed to follow up this lead. The general idea he prophesied was to have fruition in the study of instantons.

Sometime later I visited MIT, and on the way to the Faculty Club for lunch, Steve told me about a problem he and

his colleagues were worried about. It had to do with the one loop correction in nonlinear chiral Lagrangians: Charap found that one loop corrections *did* not satisfy current algebra constraints. This was a problem I looked into in Paris, and I suggested that the solution lies in the extra term in the interaction Hamiltonian which arises from canonical quantization. This term is the analogue of the Fadeev-Popov ghost loops (and as far as I know, it was written down first by T.D. Lee and Yang in their paper on the  $\xi$ -limiting process). After lunch, Roman Jackiw, Ira Gurstain, Steve and I worked out on blackboard the solution to the puzzle. I mention this because Steve told me sometime later that he applied the same canonical quantization to his theory and obtained the correct interaction Hamiltonian in what is now known as the U-gauge, quite independent of the later development.

In the summer of 1970, I lectured on my work on chiral dynamics at Cargèse summer school. I remember a young Dutch student, who looked always pensive and serious, and who was camping near the Institute building.

I spent the Spring quarter of 1972 at Caltech, learning mostly about deep inelastic scattering and light cone expansion and I spent a few months studying the quantization of the Higgs model, trying to combine what I *knew* about the  $\sigma$ -model, and quantum electrodynamics in the Landau gauge. On balance it was not a success, and I believe I said so to my frequent conversation companion at the time, Jeff Mandula. I could see cancellations

of unphysical singularities at the tree-diagram level, and in some simple loop diagrams, but was at a loss to see how these miraculous cancellations persisted<sup>ed</sup> in any order. The matter rested there.

In 1972, there was the Amsterdam Conference, and as soon as we exchanged greetings, Tini Veltman handed me two preprints by his student with the statement that the student solved the massive Yang-Mills theory. It turned out that the student in question was the young physicist I saw in Corsica, Gerard 't Hooft, and Tini told me 't Hooft combined what he heard about spontaneous breakdown in the  $\sigma$ -model at Cargèse with what he learned from Tini about the Yang-Mills theory to reinvent a kin to the Weinberg theory on his own, in a formulation in which renormalizability and cancellation of unphysical singularities were more or less plausible, modulo the question of renormalization not spoiling the cancellation of unphysical singularities. In any case, despite the extreme fatigue due to jet lag, I was up almost all night studying his papers, excited.

I came back from Amsterdam and moved on the NAL at the invitation of Sam Treiman, to spend the remainder of that Summer, and began to study renormalization of the abelian Higgs model anew, under active encouragement from Sam. Somehow everything clicked on our trip back east, while my wife was driving and I doodling in the jump seat, and I could see

clearly that divergences can be cancelled without affecting the Ward-Takahashi identities, and these identities guarantee the cancellations of unphysical singularities.

Somehow the word got around to Steve, and Steve was kind enough to give me a ring to compare notes. This was the beginning of our frequent long telephone conversations which prove to me always inspirational. That year, in 1971, Jean Zinn-Justin came to Stony Brook, and he proved to be a man of Cartesian mind. We collaborated happily to extend the proof to nonabelian case. It must be said that we were not the only ones to do this. 't Hooft and Veltman worked out the proof in their own way, and much of what we were doing was also done by Slavnov in Russia.

The development since then was well-recorded in various Proceedings of international conferences, I shall not dwell too much on it. In my view, Steve's contributions in the post-revolutionary period have been as important and prolific. In fact the whole program Steve outlined this morning reflects his efforts and achievements.

On the experimental side, we have found the neutral current as reviewed by Charlie Baltay this morning, and most probably charmed particles, as discussed in yesterday's session.

The predictive power of the unified theory, I believe, is amply demonstrated in the estimate of the charmed quark mass before the November revolution, from the suppression of  $K_L-K_S$  mass difference and nonsuppression of  $K_L \rightarrow \gamma\gamma$ . Mary K. Gaillard and I suggested, for example,  $m_c \leq 1.5$  GeV, which is not a bad estimate after all in view of the many uncertainties involved.

Do we understand, or hope to understand, weak interactions as well as, say, electrodynamics, in the present framework? Perhaps. We have yet to come to grips with CP violation and ultrahigh energy behaviors of weak interactions, on which subjects I have a few remarks to make. But I am more optimistic than ever that we are on the right track, and I can say that Steve has earned the honor bestowed on him today.