Mid-Century Adventures in Particle Physics

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It was in the Spring of 1952 that I joined the Tata Institute of Fundamental Research as a student. The shining light in theoretical research was Homi Bhabba who had completed his work on cosmic ray cascades and on relativistic wave equations by then. Bhabba was already deeply involved in organizing a nuclear research team in Bombay. But he was still very much the academic leader and presided over the weekly colloquia. He also brought to Bombay many excellent physicists like Dirac, Goeppert-Mayer, Levy, Marshak, Pais, Pauli and Tomonaga and got Bernard Peters to join the TIFR faculty. I got to work with Peters, quickly graduating from developing and scanning nuclear emulsions to developing theoretical aspects of experimental particle physics. Several of the τ-decays were discovered in our laboratory; and for myself I got to study in detail Dalitz's papers on τ-decay analysis

Fermi's thermodynamic model and Heisenberg's shockwave model of multiple meson production were of great interest at TIFR. A large "star" was discovered in an emulsion stack and the study of the distribution and interaction of secondaries were among the first lessons in particle physics that I learned. But during all this time the glamour topics were relativistic wave equations and the quantum theory of higher spin fields.

During my second year Robert Marshak came to lecture at TIFR and told us about the Chicago experiments on pion-nucleon scattering and the (3,3) resonance. This was the beginning of an association which has had a decisive effect on my scientific career. Marshak suggested that I come to Rochester as a graduate student. Thanks to Bernard Peters I was detained in Bombay for another year.

This gave me a chance to be apprenticed to Kundan Singwi in statistical mechanics before I could join the University of Rochester. But this time was very usefully spent in hot pursuit of Dirac's innumerable dazzling ideas. Along with Dirac's and Bhabba's papers Carson's "Tensors, Spinors and Relativistic Wave Equations" and Cartan's "Leçons sur la Spineurs" provided an immersion into the mysteries of spinors and the remarkable fact that what we now call the chiral decomposition is respected by the anticommutation relations and the purely kinetic terms in the Lagrangian. It was also noticed that the mass was like a zero wavenumber scalar field.

Graduate Study at Rochester

I joined the University of Rochester in the Fall of 1955 with my new bride Lalita who also joined as a graduate student there. Gell-Mann's classification of the hadrons based on the Gell-Mann-Nishijima scheme was the first "news" that I got. It was a relief to switch from the cosmic ray physics notation to the new notation. That winter when Abdus Salam came to Rochester to lecture about dispersion relations and attend the VI Rochester Conference he suggested that we look at the magnetic moments and mass differences of hyperons. This "new" calculation got me involved with computations in particle physics (as distinguished from quantum electrodynamics) but it also got us involved in the first application of symmetry breaking and the Wigner-Eckart theorem for magnetic moments.

This came about by a curious event. I had calculated, at Marshak's request, the magnetic moments and mass differences of sigma hyperons. In a plane going to Brookhaven Marshak noticed that the neutral sigma moment came out to be the average of the charged sigma moments. He came back and mentioned this curious fact; and while I was busy checking combinational factors Susumu Okubo who knew everything pointed out that this must be the Wigner-Eckart theorem in operation.

We verified this and Marshak, Okubo and I published a paper on this. Okubo went on to generalize this to the SU(3) group to give the general relation now known as the Gell Mann-Okubo formula. Macfarlane and I later on used this for electromagnetic properties generalizing the Coleman-Glashow relations.

Another dramatic result was the G-parity which formed the subject of a short and sweet paper by Lee and Yang. Michel had already given the result; I should have known it from the study of Cartan's book on spinors. Such was the feeble sense in which particle physicists knew group theory!

In Search of Universal Fermi Interaction

But the big excitement was in weak interactions. Soon after I joined the University of Rochester I had to take an exhaustive (and exhausting) examination with Robert Knox, Fred Seward and Giorgio Giacomelli. For this examination I had to have a crash review of nuclear physics and in particular beta decay. It was a good thing since I got acquainted with the crucial experiments. So when parity violation was discovered and beta decay became a hot topic in particle physics I had some advantage. Marshak had done early pioneering work on unique forbidden beta decays and he recognized the importance of Ruderman and Finkelstein's calculation of the electron/muon branching ratio. Since I showed not much interest in the study of nuclear-nucleon forces nor in devising dispersion calculations Marshak suggested that we study weak interactions. It became clear to us from the muon decay that the V, A interaction was the natural one; couple this with the pion decay and we see that unless A (or P) was present the pion could not decay. So we set about examining whether we could challenge the beta decay assignment of S, T which was generally accepted.

It was early recognized that the Ne^{19} and neutron electron-neutrino angular correlation could be due to either S, T or V, A. If only He^{6} angular correlations could change! The decay of A^{35} was almost pure Fermi and it gave V rather than S;

suggested that the V, A combination was the correct combination.

There were those that were so sure that the Gamow-Teller interaction was T they were willing to consider V, T and abandon all hope of a Universial Fermi Interaction. We were quite convinced that it was V,A; then cane the VII Rochester Conference at which I was to present this theory in outline. But a few days before the Conference opened Marshak felt that a graduate student should not talk at such an august assembly! So we jointly requested Paul Mathews, then a visiting professor at Rochester to present it in my place. Mathews never did. As a citizen at a court trial who cannot speak unless asked by the court even if he had evidence, I had to sit in the sidelines unable to say "I have it, please let me speak". It was only in later years that I realized that I would have to wait twenty-severn more years before getting a chance to talk about it!

Soon after the Rochester conference I had to complete the formalities for my Ph.D. which included a German language test. I got hold of flash cards for vocabulary and books on syntax and somehow managed to get through the examination. These months helped clarify the V, T situation. Felix Boehm's beta-gamma correlation experiment was not consistent with a V,T combination. This was very convincing to Marshak, who was not as easily convinced of the necessity for an A component from pion decay; in fairness to Marshak, I remember that the beta decay of the pion was not yet conclusively demonstrated.

<u>The Padna-Venice V-A Paper</u>

The Summer of 1957 Marshak made an offer to Ronald Bryan and me: if we could get to Los Angeles on our own he would support us as research assistants for two months. We did and he did. At UCLA we were assigned some desks: excepting for Steven Moszkowski most of the people were away. During these months of June and July my task was to write up the first draft of our V-A theory of universal weak interactions. This was a period when there was a large number of experiments carried out but we had been keeping a systematic account of the results and their implications. So we chose to make a systematic analysis of all data and to show that not all accepted experimental results were compatible. We had to make a choice and so in the paper written up for the Padua-Venice Conference in September we identified four experiments as the best candidates. These were:

1) The electron-neutrino correlation in the Gamow-Teller He^6 decay.

2) The sign of the electron polarization for muon decay

3) The branching ratio in pion decay.

4) The asymmetry from polarized neutron decay.

Of these the He^6 experiment was "old", others were "new". Some were by quiet people and some were by unquiet people, but all people of proven ability. So it was quite risky to point to such experiments as being wrong. Fortunately for us all the experiments were repeated in a period of eighteen months and the new results verified V-A theory.

During the first week of July Marshak had arranged that Ronald Bryan and I join him for a lunch with Murray Gell-Mann. I was told that Murray had graciously set up the luncheon with Berthold Stech, Felix Boehm, Marshak and myslef; Leona Marshall was also present at the lunch. It was a pleasure for me to be able to tell our ideas and conclusions to Gell-Mann who was most cordial. And Boehm gave us much reassurance that the beta-gamma correlation for Sc^{46} gave a much larger value than Co^{58} so that V,A was more acceptable than V,T or S,A. This was the final item of experimental confirmation we needed.

The weekend found me furiously busy writing the brief four page paper for Padua-Venice Conference when my wife Lalita left with her friends to see Disneyland; she was disappointed that I could not join her. The very next day we went on a month long much needed vacation and so did Ronald Bryan. The manuscript remained with Bob Marshak until he returned to Rochester several weeks later after I returned to Rochester briefly before going on to Harvard.

The Inscrutable Occidentals

After completing my Ph.D. thesis defense I went to work as a Corporation Fellow at Harvard apprenticed to Julian Schwinger. It was amazing that at that place no one could be bothered to notice that there was a new theory of weak interactions: and those few who noticed it of course conveniently forgot that Marshak and I had done it. Frank Yang came as a Loeb lecturer and towards the end of his characteristically beautiful lecture mentioned the ideas he had heard from Richard Feynman. I looked up, down and sideways at the audience and finally decided that a humble foreigner should speak up: and I did. Not that it made any difference.

During my first weeks at Harvard I heard from Sheldan Glashow that Feynman and Gell-Mann had sent in a paper suggesting a V-A interaction. I telephoned Marshak to ask about the status of our paper: he said that it was already presented at Padua-Venice and was sent out as a Rochester preprint. His assurance was that it was sufficient way to inform all concerned. Years later when it became a habit for people to refer not at all to our work or refer to our second paper rather than the first paper it puzzled me a lot why anyone who had read our paper and seen the analysis of experimental data and the subsequent outline of the chiral V-A theory would ignore it and quote the paper of Feynman and Gell-Mann for their theory (rather than the excellent proof of nonrenormalization of the vector coupling constant). Robert Oppenheimer, more forthright than most, told Marshak that he had never read our paper!

Even more curious is the mischief of quoting the work on mass reversal along with ours. At least one of the authors of mass reversal had a preprint of our paper in his hands before he wrote his paper!

Back to the V-A interaction. There were three anomalies regarding universality. One was the apparent suppression of strangeness changing leptonic decays: this was parameterized by Nicola Cabibbo within the context of SU(3) and is now understood on the basis of the Kobayashi-Maskawa mass matrix. The second was the absence of the electron-photon mode of the muon: this was resolved by the recognition that the muon neutrino is different from the electron neutrino. The third concerns the magnitude of the vector and axial vector coupling constants in beta decay. Feynman and Gell-Mann explained the vector nonrenormalization in terms of a conserved vector current of isospin, an idea earlier mentioned and discarded by Gerstein and Zeldovich. This conserved vector current and its relation to weak magnetism was developed by Murray Gell Mann subsequently and verified experimentally by Mo and Wu. The axial vector renormalization constant was related to pion-nucleon scattering via the PCAC hypothesis and current algebra by Adler, by Weisberger and by Tomozawa.

"Nonlocal" Weak Interactions

My involvement with weak interaction physics continued for the next few years. During the two years that I spent at Harvard I used to take the night bus to Rochester where we had a small working group consisting of Robert Marshak, Susumu Okubo and myself. In his travels Marshak met a young theorist by the name of Steven Weinberg who joined some of the sessions; and the late Werner Teutch made a fifth. We wrote two papers in collaboration. Steven Weinberg wrote the draft of one and I the other. But the major effort was contained in an unpublished manuscript written with a carbon copy. (Xerox was not so efficient in its home town!) This involved the use of a derivative coupled realization of PCAC and had identified mass as a zero frequency external scalar field which violated chirality. I am ashamed to say that today I do not even have a copy of this working paper. I could not get my then colleagues to be too enthused about mass being only a component of a scalar field. And none of us at that time had any ideas about either charm or the virtues of spontaneously broken Yang-Mills theories. And I could not impress on my colleagues the notion that the chiral components were independent kinematic entities which happened to get coupled by the interaction with the "mass".

I had made some effort to interest Julian Schwinger in our work; Julian and I used to have lunch twice or thrice a week with Paul Martin, Stanley Deser and Walter Gilbert. It was quite difficult to get Schwinger's attention away from Green's functions to weak interactions. To my surprise Schwinger seemed to believe that in his paper on "Fundamental Interactions" he had predicted V-A! The nonrenormalization of the vector coupling he took in his stride but the second order Dirac equation that Feynman and Gell-Mann had used he did not like at all. He pointed out to me that if it entered the action, this would imply that the field and its time derivative would obey nontrivial anticommutation relations while the field components amongst themselves would have a trivial anticommutator. This would introduce the indefinite metric and negative probabilities. Meanwhile he suggested that I look into integral representations of Green's functions. This involved me in a program of research into which I invited Stanley Deser and Walter Gilbert.

Gatlinberg and After

Meanwhile Susumu Okubo, Bob Marshak and I continued our research collaboration on various aspects of weak interactions, in particular the three body decays of kaons. It was in this second postdoctoral year that the Gatlinberg Conference on Weak Interactions took place. I was invited to chair a session where Murph Goldberger and Henry Primakoff spoke and was duly flattered but failed to note that I was not invited to talk at this conference even though my work was the reason the conference was held! And Bob Marshak was not even invited; talk of representative invitations! Again one had the feeling of a witness in a trial court not allowed to speak even when what you had to say was important. But worse was to come. Several years later Argonne National Laboratory held a conference on Weak Interactions and neither Marshak nor I had even a ceremonial role, not to mention an invited talk at this meeting. When I asked my dear friend Kameshwar Wali, a king-pin of this Conference "How come?: his response was "George, you make it difficult for me to be your friend". My ceremonial role was restored at the Kiev Conference in 1967 and at the Paris Conference in 1982 and Bob Marshak got the speaking role at the Racine meeting in 1984. I am puzzled: I brush my teeth and bathe regularly, use proper personal care products, do not pray in public and speak English.

At Gatlingberg Richard Feynman gave a leisurely evening lecture on beta decay, and while it contained some amount of "second order Dirac equation", he concentrated on the numerical calculation of decay rates. The next evening Dick and I sat next to each other; he very graciously told me that he had been told that I had the essential V-A ideas much earlier than anyone else. I was very please to meet him; and told him that even as he spoke at the VII Rochester Conference I could have presented the theory. He could not understand why I did not do it, especially since the Conference was at Rochester. I must say that Dick Feynman, consistent with this conversation at Gatlinburg spoke again at Neutrino 1974 at the University of Pennsylvania. The Grand Masters sometimes forget, but the camp followers are the ones who rewrite history.

A more patient and systematic review is given in the Racine paper "Origin of the Universal V-A Theory".

Hadron Resonances

Already at this time strong interactions had shifted from one-dimensional dispersion relations to hte Mandelstam representation and Geoffrey Chew's enthusiastic analytic function models and the subsequent developments of Regge pole formulations. The eightfold way reincarnation of SU(3) got everyone's attention after the brilliant presentation by Gell-Mann at the La Jolla Conference where Susumu Okubo presented the general theory of broken SU(3). Charles Goebel at Rochester was my friend, guide and philosopher with regard to the S-matrix models; my expertise in deciphering Susumu had to be extended to cover Charles also, to my great advantage. I did some subsequent work with Alan Macfarlane and Narasimha Mukunda on higher groups as applied to particle physics, particularly sigma-lambda mixing and nucleon resonance magnetic moments.

But the more interesting involvement was with pion resonances. With my background in multiple meson production it became clear that pion-pion resonances were seen more easily in antinucleon annihilation in terms of effective mass plots for two- and three-pion complexes. With two of my students, Gabriel Pinsky and Kalyana Mahantappa I calculated such plots and presented it at the X Rochester Conference. William Chinowsky and Frank Solmitz were at the session and categorically denied any such evidence for resonances in effective mass plots. My efforts to get Lawrence Radiation Laboratory films for analysis at Rochester were not successful. But a couple of months later Bogdan Maglic telephoned me from Berkeley with great enthusiasm saying that he had read my paper and had, in fact, carried out such mass plots and found the two-pion resonances but did not find any evidence for three-pion resonances. I did some calculations on the expected effective mass which gave a value less than the then-popular California estimates. He called me back the next day in great excitement and told me he found it within a few MeV of the value I had quoted and sent me, two days later, the draft of a paper in which my contribution was given due prominence. But two weeks later I got a "revised" version with more authors which washed out any reference to my role in this discovery. Bogdan was very apologetic and said the matter has been settled at "higher levels" but that if ever he got to write about it he would surely bring out the whole story: Maglic obviously could not get in even a reference to my contribution printed in the Proceedings of the X Conference!

In the next few years I got interested in Heisenberg's nonlinear spinor field theories and in axiomatic field theory; and of course in classical mechanics. I returned to symmetries after I left Rochester on sabbatical for the Institute for Exact Sciences, Bern, Brandeis University and the Institute of Mathematical Sciences, Madras. I worked on the dynamical origin of symmetries inverting the Smushkevich theorem; and on combining Lorentz group with broken symmetries. The results on the latter topic got tangled up with a moronic referee for Journal of Mathematical Physics but it did serve to instruct Lochlainn O'Raifeartaigh whom I met in Europe and took first to Madras and the to Syracuse.

Concluding Comments

In the Indian traditional lore it is said that any encounter can change one's future but only the encounters with Vidyā, the godess of knowledge can change one's past. There must be many people who have encountered Vidyā since for many, at least to the extent their recollections of the history of particle physics goes, the past has changed and in diverse ways.

As for me it has been a sad but wise experience that in the land of the free most people find it easy to be free with history. It has been an education that the practice of science, particularly particly theroy, is political economy in the age of robber barons. There is no dishonour in being dishonourable; and if you have done, written and published good physics there is no assurance that your work would be publicly recognized and quoted. You need alliances and powerful sponsors. If you cannot assure yourself of either, you must be strong enough to derive joy from the act of discovery itself. Over the years I have developed this skill and I have contribued to many areas of physics even outside particle physics. I hope to continue to do so.