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CONCLUDING REMARKS *

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When I was asked to give a concluding talk at this conference, my immediate reaction was to wonder who needed another conference in high energy physics so soon after the Berkeley Conference. After a few days at the Berkeley conference I began to wonder who needed the Berkeley Conference. Steve Weinberg's summary talk at Berkeley presented the two most important developments since the last Rochester Conference as superstring theory and a new model for neutrino oscillations in the sun. The one ingredient that these two developments have in common is that there is no evidence that either of them has any relation to the real world. I am willing to bet that there will still be no convincing evidence connecting either of them to the real world by the next Rochester Conference.

This conference, on the other hand, has been deeply rooted in the real world. And if the conference was not very exciting, it is because the real world of High Energy Physics has not been very exciting recently. In fact the main developments in the real world seem to be that all the exciting evidence for new physics beyond the standard model following the discovery of the W and Z seems to have gone away. However, the experimental results at this conference are not discouraging. They rather indicate that we are in a period of consolidation, in which experimenters have learned to use the complicated detectors needed for modern experiments, and have achieved a much better understanding of the background effects which produced misinterpretations of previous data. The beautiful results presented on W and Z physics and on heavy quark physics indicate that much finer and more reliable numbers will soon be available, and that if there is any evidence for new physics beyond the standard model hidden in these data it soon will be revealed and interpreted reliably. Even if there is no evidence for new physics, the new data will serve to illuminate an important piece of the standard model; namely QCD, where there is still much interesting physics waiting to be discovered and understood, even if the basic Lagrangian is known.

There is a crucial difference between the experiment which discovered the W and the Z and experiments searching for new physics. The $W-Z$ experiment was designed for a well-defined signal carefully chosen in advance, and planned to optimize the signal-to-noise ratio. The background was well studied and success was guaranteed. Even if the W and Z were not found, the upper limits obtained for their production cross sections would have been significant and sent theorists back to the drawing boards to modify the standard model.

When new unexpected effects are found indicating possible new physics, the situation is very different. The background has not been studied in advance, and cannot be properly understood without considerably more work. But who should do the work and why? What if the effect goes away? A good experimentalist must know how to avoid wasting much time, money and effort chasing effects that go away, while avoiding missing something really significant that may be hiding in the data.

The recent Nobel Prizes are no guide to future experiments. We cannot hope for discoveries like the W and Z, the J/ψ or CP violation. The $W - Z$ discovery followed a clean simple theory with clear predictions enabling the planning of a beautiful experiment to pin down the standard model. Unfortunately, there is no more clear theory and no clean predictions. The standard model is beautiful, but it is just as dead as Newtonian mechanics. It is right where it works and is useless as a guide to new physics. The new theories with their Higgses, Schmiggses, Technicrats and Super-Duper-Whatever have yet to show that they are somehow connected with the real world. They make no clear predictions which can be used as conclusive tests. They may be as dead as the bootstrap in a few years.

CP violation and the J/ψ were found by accident in a search which had no theoretical motivation. But the signal found was so striking that it was immedately clear that there was new physics. Unfortunately we can't depend on such luck for the next new physics. We are in a period of exploration physics where we need contact between theory and experiment in the search for signals in the noise.

One of the previous speakers recalled the 1966 Rochester Conference at Berkeley. At that time the "establishment" believed that nucleons were "elementary" and looked for a kind of "Grand Unification" called "Nuclear Democracy" or a big invariance group including SU(6) $\times$ Lorentz. The suggestion of a composite model made of quarks was heresy. Even Murray Gell-Mann, one of the inventors of quarks was saying that they were only mathematical objects and would never be observed as physical particles. The only respected physicist who took quarks seriously as physical particles at that conference was Dick Dalitz,[1] who was a champion of the quark model from the very beginning, and whose considerable contributions to quark physics have never been given the recognition that they deserve. It was only after the deep inelastic scattering experiments at SLAC showed the existence of point-like constituents with fractional charge in the proton that the establishment finally included quarks in their vocabulary.

Two groups that took quarks seriously in 1966 were in Leningrad and Moscow, where Levin and Frankfurt[2] invented what is now called the "additive quark model" for high energy collisions, and Sakharov and Zeldovich[3] proposed a mass formula for hadrons. Striking evidence that mesons and baryons were made of the same quarks was presented by the surprising agreement with experiment of the Levin-Frankfurt relation between meson and baryon total cross sections

$$\frac{\sigma_{tot}(pp)}{\sigma_{tot}(\pi p)} = \frac{3}{2} \tag{1}$$

and the Sakharov-Zeldovich relations between meson and baryon masses

$$m_s - m_u = M_\Lambda - M_N = (3/4)(M_{K^*} - M_\rho) + (1/4)(M_K - M_\pi) \tag{2a}$$

$$\frac{m_u}{m_s} = \frac{M_{K^*} - M_K}{M_\rho - M_\pi} = 1 - \frac{3}{2}\frac{M_{\Sigma^*} - M_\Lambda}{M_\Delta - M_N} \tag{2b}$$

Sakharov and Zeldovich note that the factor (3/2) in eq. (2b) is absent in a similar relation derived from SU(6) symmetry which disagrees with experiment.[4] This factor (3/2) shows that experiment confirms the composite quark model for hadrons rather than the "grand unified" SU(6) approach.

Today the composite model for hadrons is accepted and there is an open controversy on grand unification vs. compositeness at the deeper quark-lepton level. There are no clues from experiment and no crucial experimental tests proposed. It would be very interesting if theorists could find a relation at the quark-lepton level with something like the Sakharov-Zeldovich (3/2) factor to distinguish between grand unification and compositeness. It is unfortunate that there are no representatives from the very active groups in Moscow and Leningrad here at this conference, even though there was one invited speaker.[5] The reasons for this absence and possible ways to obtain broader participation in future conferences were illuminated by the discussion between Arno Penzias and Anatoly Shcharansky shown in a videotape during the conference.

To get a broader perspective on the history of particle physics, it is instructive to go back another twenty years to 1946. The electroweak physics which describes leptons and the strong interaction dynamics which describe hadrons have developed very differently during the past forty years.

Electroweak physics can be characterized by the "standard model syndrome". There has always been a standard model which was generally accepted, and most experiments were either testing this standard model or looking for new physics beyond the standard model. There were periods of crisis when the standard model appeared to be wrong, but these were generally resolved, either by finding that the experiments disagreeing with the standard model were wrong, or that a new concept like parity nonconservation was needed which could easily be fit into the existing framework.

In 1945 the standard model for electroweak physics was the Quantum Electrodynamcs in Heitler's book and the Fermi theory of beta decay. There were indications of "physics beyond the standard model" in the infinities arising in QED calculations and in the Lamb shift experiment, and there were inconsistencies between the Fermi theory of beta decay and the measured beta ray spectra. The QED difficulties were solved by the new formulation of Feynman, Schwinger and Tomonaga. The difficulties with beta ray spectra were first explained by Konopinski and Uhlenbeck who modified Fermi's theory. But subsequent better experiments agreed with the original Fermi theory. And so the standard model evolved. The extension of the Fermi theory of beta decay to the modern electroweak theory was a step-by-step development.

There were a succession of wrong theories proposed to explain wrong experiments. My first theoretical paper, written while I was a graduate student, described an attempt to improve the standard model by what today would be called a "radiative correction" to explain why the experimentally observed beta ray spectra from forbidden transitions had a shape that looked like allowed spectra instead of the unique forbidden shape predicted by the standard model. The theory of course failed, and subsequent experiments showed the spectrum predicted by the standard model. There were then the beta decay experiments which convinced theorists that the weak interaction currents responsible for beta decay were scalar and tensor, rather than vector and axial vector. This led good experimentalists to refrain from publishing right experiments because they did not agree with the accepted wrong theory. The true nature of the beta interaction was eventually revealed by better reliable experiments. But this was immediately followed by the failure to observe the pion decay into an electron and neutrino and another epidemic of wrong theories to explain a wrong experiment. The apparent experimental absence of what are now called flavor-conserving neutral currents led to anther epidemic of wrong irrelevant theories as did a number of wrong atomic physics experiments which seemed to rule out the present standard model. The neutral current confusion was complicated by the failure to note the distinction between flavor-conserving currents and the flavor-changing neutral currents against which there was strong experimental evidence. Again good experimentalists were reluctant to publish right experiments because they did not agree with the accepted prejudices.

On the theoretical side there were the new breakthroughs of parity nonconservation, the two-component neutrino, the universal $V - A$ interaction, the conserved vector current and the Cabibbo angle, and ultimately the present standard electroweak model with the W and the Z and the GIM mechanism for eliminating unwanted flavor-changing neutral currents. But all these were absorbed into the standard model to make a new and better model which did not invalidate the old model. Today the Fermi theory of beta decay and the simple QED of Heitler's book are still considered valid for the problems which they treated.

Hadron physics has had a very different development from electroweak physics. There has never been a sensible "standard model" until the recent development of Quantum Chromodynamics (QCD). There is no relation between today's picture of proton structure with QCD and what was believed in the 1940's, 50's and 60's. Somehow, the particle theory establishment clung to old invalid theoretical prejudices and refused to accept new ideas until they were forced upon them by experimental data. Concepts now generally accepted like spontaneously broken symmetries, chiral symmetry, the unitary symmetry now called flavor-SU(3), the existence of quarks, and the existence of the degree of freedom now called color were ridiculed by the reactionary establishment, as they were dragged kicking and screaming along the path that eventually led to QCD.

At the 1960 Rochester Conference I mentioned to Nambu that I had heard from John Bardeen in Urbana about his very interesting application of ideas from superconductivity to particle physics. Nambu said I was the only person at the conference who had expressed any interest in this work. At the 1962 Rochester conference in Geneva, the prediction that a particle later called the Ω^- should exist, already proposed in a paper by Glashow and Sakurai, was not considered important enough to be mentioned in any invited or contributed talk. It was mentioned in a comment from the floor by Gell-Mann. Gell-Mann's famous paper proposing the existence of quarks was rejected by Physical Review Letters and accepted by Physics Letters only because it had Gell-Mann's name on it. The editor said "The paper looks crazy, but if I accept it and it is nonsense, everyone will blame Gell-Mann and not Physics letters. If I reject it and it turns out to be right, I will be ridiculed." George Zweig's CERN preprint, which independently proposed quarks at the same time, was somehow not formally published until several years later.

In today's quark-boson euphoria there is a tendency to forget about the existence of hadrons and simple hadron dynamics. Consider for example the plethora of diagrams used to describe nonleptonic decays of charmed mesons.[6] The simplest "spectator" diagram shown in fig. 1a predicts that the D^o decay to $K^-\pi^+$ is allowed while the $\bar{K}^o\pi^o$ decay is forbidden. It is reasonable to try the simplest assumptions first and test them against experiment. However, when experiment shows that the two decay modes have comparable branching ratios, one should think a bit about hadron physics before trying a large number of other weak interaction diagrams. The importance of strong final state interactions in this final state has been pointed out,[7,8] since the $K^-\pi^+$ and $\bar{K}^o\pi^o$ states are not isospin eigenstates but linear combinations of $I = 1/2$ and $I = 3/2$ with definite relative magnitudes and phases. Since the $I = 1/2$ channel has many resonances in this mass region and the $I = 3/2$ channel is exotic and has no resonances, one can expect very different final state interactions in these two channels. One can not take very seriously a selection rule which requires the relative amplitude and phase between these two isospin amplitudes given by the spectator diagram to be maintained while ignoring all strong interactions.

A simple unitarity argument[8] shows that the spectator diagram cannot give the whole story. Let us write the unitarity relation for D^o decay and assume that the unitarity sum is dominated by the $K^-\pi^+$ intermediate state. We then obtain for any final state f,

$$\mathrm{Im}\langle f|T|D^o\rangle = k\langle f|T^\dagger|K^-\pi^+\rangle\langle K^-\pi^+|T|D^o\rangle. \tag{3a}$$

where k is the usual kinematic factor. This relation holds for any final state $|f\rangle$, and in particular for the states $K^-\pi^+$ and $\bar{K}^o\pi^o$. Thus

$$\mathrm{Im}\langle K^-\pi^+|T|D^o\rangle = k\langle K^-\pi^+|T^\dagger|K^-\pi^+\rangle\langle K^-\pi^+|T|D^o\rangle. \tag{3b}$$

$$\mathrm{Im}\langle \bar{K}^o\pi^o|T|D^o\rangle = k\langle \bar{K}^o\pi^o|T^\dagger|K^-\pi^+\rangle\langle K^-\pi^+|T|D^o\rangle. \tag{3c}$$

Dividing these two relations, we obtain

$$\frac{\mathrm{Im}\langle \bar{K}^o\pi^o|T|D\rangle}{\mathrm{Im}\langle K^-\pi^+|T|D\rangle} = \frac{T_{CEX}(K^-\pi^+)}{T_{EL}(K^-\pi^+)}, \tag{4}$$

where T_{CEX} and T_{EL} denote the charge exchange and elastic amplitudes. These are expected to be of the same order if the resonant $I = 1/2$ channel dominates $K - \pi$ scattering in this mass region. Thus unitarity tells us that if the spectator diagram dominates in the weak transition, the ratio of the decays is completely determined by strong interaction dynamics and not by the weak quark diagrams.

Rather than examining all weak interaction diagrams and ignoring strong interactions, one might try explaining the experimental data by adding a Harari-Rosner[9] duality diagram familiar in hadron dynamics to the spectator diagram as shown in fig. 1b.

There are many open questions in hadron spectroscopy still needed for understanding QCD and possible new physics beyond QCD. Here are some:

1) What are the ι, θ, ξ?

2) Are there two states at the iota?

3) Where are the O^+K^* resonances? Could they affect nonleptonic D-meson decays?

4) What is the OZI rule?

5) Does the ψ'' decay to charmless hadrons?

The old and new experimental measurements of D-decay branching ratios[6] disagree with one another by a factor of 2. Experimentalists insist that there is no problem, but each group[6,10,11] says that the other is wrong. If both are right, then half of the ψ'' decays go to charmless hadrons. This possibility can be tested by looking for decays of the ψ'' into odd numbers of pions, e.g.

$$\psi'' \rightarrow \rho\pi,\ 3\pi,\ 5\pi,\ 7\pi,\ \text{etc.}$$

The nonstrange odd G final states discriminate against photon background, since the nonstrange isoscalar piece is only 1/12 of the photon. However, nobody is doing this—it is not considered interesting enough for a student to waste his time on. But is all this spectator-color suppression, annihilation, etc., etc. more interesting?

There is a suggestion that OZI violation is much greater at the ψ'' than elsewhere.[12]

A threshold effect predicted by unitarity gives the relation

$$\sum_{f \neq (D\bar{D})} \frac{|\mathrm{Im}\langle f|T|\psi''\rangle|^2}{|\mathrm{Im}\langle D\bar{D}|T|\psi''\rangle|^2} = \frac{\sigma_{\mathrm{inel}}(D\bar{D})}{\sigma_{\mathrm{el}}(D\bar{D})}. \tag{6}$$

Thus the ratio of OZI violating transitions to the OZI allowed transition is given by the ratio of the inelastic to elastic cross sections for $D\bar{D}$ scattering in the relevant partial wave. Since there are many open OZI-allowed inelastic channels described by the standard allowed Harari-Rosner diagrams[9], the right hand side of Eq. (6) has no OZI-forbiddenness factor and may very well be of order unity. But this only holds above $D\bar{D}$ threshold and below $D^*\bar{D}$.

There is also a suggestion that OZI violations in heavy quarkonium might occur via off-shell intermediate states with naked charm or beauty; e.g.

$$\psi'' \to D\bar{D} \to \eta\psi \tag{7a}$$

$$\psi'' \to D\bar{D} \to \pi\pi\psi \tag{7b}$$

$$\Upsilon'' \to B\bar{B} \to \eta(b\bar{b})_{^3P_1} \tag{8a}$$

$$\Upsilon'' \to B\bar{B} \to \pi\pi(b\bar{b})_{^3P_1} \tag{8b}$$

and other transitions in which any heavy pseudoscalar (D or B) meson in one of the transitions (7-8) is replaced by the corresponding vector (D^* or B^*) mesons.

CONCLUSIONS

Many experiments are now ready to give interesting data. There will soon be better values for standard model parameters, more information on QCD, heavy flavors, decays. Is the discovery of the top quark imminent? There will be new W, Z, $t(?)$ physics. Hopefully there will be more excitement by the next conference.

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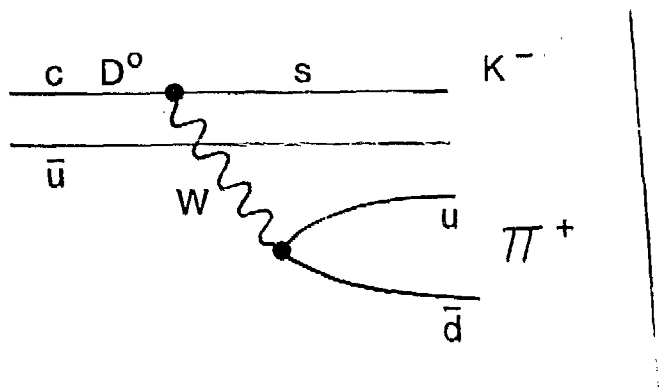

Figure 1(a). Spectator Diagram for Nonleptonic *D*-Meson Decays.

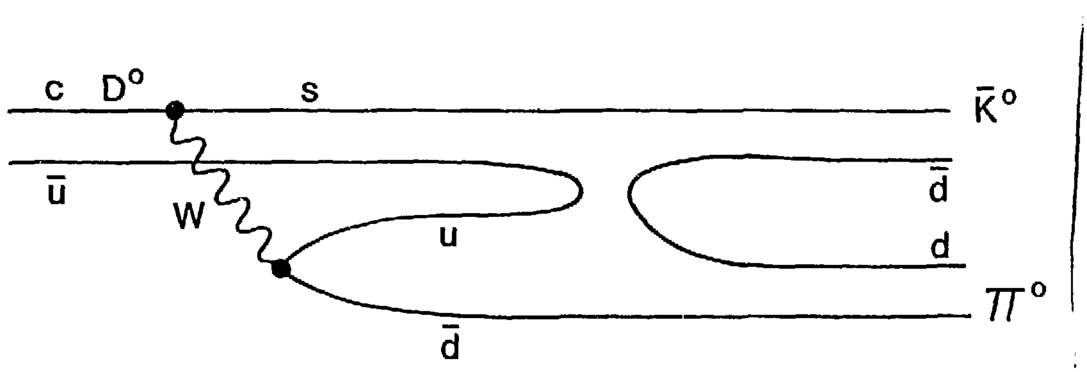

Figure 1(b). Final State Interaction in Nonleptonic *D*-Meson Decays.