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SUMMARY TALK: NEUTRINO '77 *†

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It was only last year, at Neutrino '76 in Aachen, that Benjamin Lee gave the summary talk. And it was, for most of us, only upon arrival at this meeting that we learned of his tragic death. Others will extol his achievements as a scientist, scholar, and leader. I mourn not only for these reasons, but also for the loss of a valued friend. I therefore wish to dedicate this summary to his memory.

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I. Introduction

In preparing this summary I face several difficulties. Foremost is that I came to learn about neutrino astrophysics, not discourse on it. Therefore any remarks of mine on the subject are likely to be at best banal, and I will do my best to keep them to a minimum. Another difficulty is that the flood of fresh accelerator data invites an attempt at compilation. I do not feel especially competent to do that, and in any case there is scarcely time to do a proper job of it. Finally I am, like many summary speakers, tempted to surreptitiously inject a dose of my own recent research ideas into this talk. But I don't like to be surreptitious; therefore I will overtly and unapologetically inject them instead. The outline of this talk is as follows:

II. Astrophysical and cosmic neutrinos.

III. Charm

IV. Heavy lepton τ ?

V. More building blocks ?

VI. Do we understand the structure functions ?

VII. Is the standard SU(2) $\otimes$ U(1) model correct ?

VIII. Exotica, as seen from a particular gauge theory.

IX. Conclusions.

II. Astrophysical and Cosmic Neutrinos

Natural sources of neutrinos include solar, atmospheric, neutrinos from supernovas or other collapsing objects, and possibly other extraterrestrial sources. The solar neutrino puzzle, so splendidly summarized by Professor Zatsepin, is still very much with us. And, while progress is being made in understanding the dynamics of stellar collapse, with general agreement that supernovas within our galaxy emit a detectable flux of neutrinos, the details are still fraught with great uncertanties. Here particle physics plays an especially significant role: the detailed structure of the neutral-current interaction of neutrinos with hadrons has a considerable effect on the dynamics of stellar collapse. On the other hand, in big-bang cosmology, the situation is reversed: there the astrophysicist presents some possible limitations of the classes of theories the particle physicist can contemplate. In particular we heard from Zeldovich and Schramm that the standard phenomenology of the big bang is upset if there are more than seven species of neutrino or if there exist stable neutral leptons with mass between a few electron volts and about one GeV.

Of special pleasure to me was hearing of prospects for the future: of DUMAND and especially of the Neutrino Observatory here in the Baksan Valley. My first introduction to neutrino-physics was through a similar, more modest undertaking in Utah by Keuffel, Bergeson, and their colleagues, and much of my thinking about the behavior of high-energy neutrino cross sections was stimulated by that experience—an experience which also was a splendid education in particle physics in general. Thus it was especially pleasant to relive that experience again, on the grander scale represented by the Neutrino Observatory, and by the DUMAND idea.

III. Charm

Even at the time of the Aachen Conference last year, charm had taken its place as a reasonably solid experimental fact. Nothing in the past year has occurred to weaken the position of charm. Instead there has been considerable supportive evidence, in particular the observation of apparent semileptonic decays in association with K's in e^+e^- colliding beams, as well as the $\mu e V^0$ events in neutrino reactions, which now appear more consistent with the charm hypothesis than they originally did. In addition, the beautiful new dimuon data[1] from the CERN-Dortmund-Heidelberg-Saclay counter experiment is readily interpreted in terms of a charm-production model. With production from the "ocean" quarks comparable to the 4% from valence quarks, along with a 10-15% branching ratio of charmed hadrons into muons, the neutrino data seems to be in reasonable shape.

A new and powerful source of information on properties of D's has recently been found at SPEAR by a SLAC-LBL collaboration and simultaneously by another SLAC-Stanford group (DELCO). These groups observe[2] a resonance at 3.772 GeV, just above $D\bar{D}$ threshold, with width 28 ± 7 MeV, and partial width into e^+e^- of 370 ± 100 keV. Already it is known that it does decay into $D\bar{D}$. Furthermore the DELCO group, whose detector is designed to observe direct electrons with very good rejection of other particles, finds[3] a significant direct electron signal at resonance as well. Such a resonance was quite accurately predicted by E. Eichten and K. Lane[4] on the basis of the charmonium model, in which it is a $^3D_1$ state of $c\bar{c}$, mixed somewhat with the nearby ψ'. It is expected that this ψ (3772) will provide especially clean information on the masses and decay modes of the charged and neutral D.

However, there remain major problems remaining with the phenomenology of charm. The most serious concerns the F: where is it, and how does it decay?

It would also be of great interest to have a sizeable sample of charmed baryons; perhaps photoproduction will provide such a source. And sooner or later, we must find charm in hadron-hadron collisions: the limit on the charm cross section, on the basis on emulsion and other experiments, as well as the very nice IHEP-ITEP beam-dump experiment reported here[5], seems to be sinking down below the one-microbarn level. We need good signatures; the direct-lepton signature may not be good enough. Perhaps the peculiar kinematics of the $D^* \rightarrow D\pi$ and $D\gamma$ cascades will help; perhaps there is something still better yet to be discovered.

IV. Heavy Lepton τ ?

As described so well by Meyer and Khose, there is by now a large body of evidence in e^+e^- annihilation that points to the existence of a charged heavy lepton τ of mass $\sim$ 1.9 GeV. Given the confirmatory evidence provided by the PLUTO and DASP measurements, the real existence of anomalous $e\mu$, eh, and μh events seems to be quite firm. No hypothesis accounts so well for the observations as that of a sequential heavy lepton τ, with its own neutrino ν_τ. Even the V-A coupling seems to be preferred, but far from established.

Khose[6] emphasized the important features that remain to be established: foremost is an accurate and comprehensive determination that $e\mu$ events are uncorrelated with the resonance structures in the e^+e^- total cross section. Second is a careful study of the spectrum of emitted hadrons in the eh and μh events: the $\nu\pi$, $\nu\rho$, and νA_1 modes are reasonably well predictable given the heavy lepton hypothesis, and even a good portion of $\nu \times$ continuum is understood from e^+e^- data. As it stands, we should probably consider existence of the τ as not fully established, but as the most probable explanation of the 2-prong $e\mu$ events.

V. Are There More Building Blocks?

Prior to this meeting, data from the HPWF neutrino experiment at Fermilab provided evidence for additional fermion building blocks beyond those we have discussed. The so-called high-y anomaly[7] in antineutrino reactions had suggested the existence of a heavy "bottom" quark of charge $-\frac{1}{3}$ and mass ~ 5 GeV, coupled to the d quark and the $W^{\pm}$ via a right-handed current. However, at this meeting there was presented evidence from three experiments (CITF at Fermilab[8], and CDHS[9] and BEBC[10] from CERN) each of which finds no large energy dependence in $< y >$ and/or $\sigma_{tot}(\bar{\nu}N)$ for $\bar{\nu}$ induced reactions in the energy region from 50 to 200 GeV. While $< y >_{\bar{\nu}}$ appears to be somewhat higher than indicated from antineutrino cross sections at the Gargamelle energies, there appears to be no problem in accommodating that rise in terms of scaling violations of the type seen in muon-scattering experiments, or as expected theoretically in the context of asymptotic freedom and quantum chromodynamics. Another piece of evidence against the 5 GeV b-quark coupled to u with a right-handed current comes from the CDHS measurements of dilepton production.[1] Within such a model there should be a sizeable yield of antineutrino-induced (but not neutrino-induced) dileptons at high energy coming from the semileptonic decay of the b. Then the "ratio of ratios"[11]

$$\frac{\sigma(\bar{\nu} \to \mu^+\mu^-)}{\sigma(\bar{\nu} \to \mu^+)} \Bigg/ \frac{\sigma(\nu \to \mu^+\mu^-)}{\sigma(\nu \to \mu^-)}$$

is expected to rise to a value of 2 to 5 at high energies, while within the standard GIM charm picture it should remain near unity. The CDHS data supports the latter expectation and contradicts the former.

Taking into consideration the much greater potential difficulty with systematic errors in the HPWF experiment (which uses a broad-band beam) than in the subsequent three (all of which utilize narrow-band neutrino beams), I consider the evidence against the (energy-dependent) high-y anomaly quite decisive. While the "standard" b-quark hypothesis ($m_b \sim 5$ GeV; right-handed coupling to u) appears ruled out, this does not rule out the existence of either more massive ($m_b > 9$ GeV) b-quarks <u>with</u> the right-handed coupling to u, or less massive b-quarks which do <u>not</u> have the right-handed coupling to u. Thus the new experimental results have <u>no</u> impact on the various options available to the neutral-current phenomenology.

The HPWFR group has also observed[12] spectacular trilepton events induced by high-energy neutrinos, which have stimulated interpretations [13] based upon production of one or more heavy leptons according to the schemes.

$$\nu_\mu + N \to M^- + X, \qquad M^- \to M^0 \mu^- \nu, \qquad M^0 \to \mu^+ \mu^- \nu$$

or

$$\nu_\mu + N \to M^0 + b + X, \qquad b \to u\mu^- \nu, \qquad M^0 \to \mu^+ \mu^- \nu$$

The masses of these new objects are in the range 4-8 GeV. Given such hypotheses for the production of the trimuons, the CDHS experiment at CERN should have observed several trimuon and a large number of $\mu^-\mu^-$ events. However, the two or three trimuon events and the $\mu^-\mu^-$ events that are seen are consistent with background. The absence of large signals in these modes in the CDHS experiment may be in contradiction with the heavy lepton hypotheses used to explain the HPWF data. But whatever that case may be, there remains the question of whether there is a direct contradiction between HPWF and CDHS trimuon and $\mu^-\mu^-$ data. Until an in-depth and open comparison is made which takes into account differences in detection efficiency and especially in the neutrino spectra, I prefer to remain open-minded on the strictly empirical question of the existence of the trimuon and $\mu^-\mu^-$ phenomena. And there is another possibility (pointed out to me by Burton Richter) which obviates any question of contradiction between the experiments. It is conceivable that the trimuons are produced by a different neutrino ν' produced directly in the primary proton target. Both the CITF and CDHS experiments view the π/K production target at a large angle relative to the primary proton beam, while HPWF views it in the forward direction. To show that the rate might be credible, assume:

1) In pp collisions at 400 GeV, leading baryons (flat x-distribution) containing a heavy quark Q are produced with a cross section $\sim 1\ \mu$b.

2) Q only decays semileptonically with emission of ν'; the probability of ν' having $E_{\nu'} > 100$ GeV is 10%.

3) The mean production angle θ of such an energetic ν' is ~ 15 mrad, while HPWF accepts only $\theta \lesssim 1.5$ mrad, implying a geometrical efficiency $\sim 10^{-2}$.

4) $\dfrac{\sigma(\nu' \to \mu^-\mu^-\mu^+ + \dots)}{\sigma(\nu_\mu \to \mu^- + \dots)} \sim 10^{-1}$ for $E_{\nu'} \gtrsim 100$ GeV .

With these crude assumptions, I estimate a yield of ~ 4 trimuons in the HPWFR detector per 10^{18} protons on target. While optimism is a necessity, the idea is not totally absurd, and deserves an experimental check. The HPWFR group itself may automatically provide this soon; their most recent running has been with a sign-selected antineutrino beam. If HPWFR obtain a comparable number of protons on target as in their previous run they would, according to this hypothesis, still find $\mu^- \mu^- \mu^+$ events, despite the change from an intense ν_μ to a considerably less intense $\bar{\nu}_\mu$ beam.

While the existence of b-quarks and heavy leptons responsible for trimuons have been cast into doubt, there was at this meeting some evidence from SKAT[14] as well as from Aachen-Padua[15] presented in support of a neutral lepton with a (crudely estimated) mass comparable to the $\tau^\pm$. If the one provocative SKAT bubble chamber event is accepted, it follows that either the SKAT team was extremely lucky or that there should be many such μe events, with even larger gaps between τ^0 production point and decay vertex, in the high energy bubble chamber exposures at FNAL and CERN. We should soon have a much clearer picture on this question; samples in excess of 10^4 events should suffice.

The most bizarre candidate for a new particle was that shown by Heusch: a narrow "μ^*" seen by a SLAC-UCSC streamer chamber group in the reaction

$$\mu p \to \mu^* + \ldots, \qquad \mu^* \to \mu \rho \,.$$

The sharply peaked angular distribution of the $\mu\rho$ system is similar to background in neighboring bins and is cause for suspicion. But if real, it may argue for spin higher than $\frac{1}{2}$. The large production cross section coupled with limits from the g-2 experiment argues for a direct lepton-quark coupling not mediated by bosons as underlying the production mechanism. This $\mu\rho$ state should be apparent in e^+e^- annihilation, but despite some searching, has not been seen. On the whole, it is a state that, to my knowledge, no theorist welcomes. It is something that should be searched for in as many experiments as possible, just in case....

VI. Do We Understand the Structure Functions?

Much of neutrino folklore is based on properties of the structure functions W_1, W_2, W_3 which determine the inclusive lepton spectra. This folklore extends not only to the charged-current processes, but also to neutral-current and new-particle (e.g. charm) production processes. Up to now the naive quark-parton model has provided an adequate and simple basis for describing the data. However, with the increased sophistication of the experiments reported here, along with what we can anticipate in the future, a more conservative and model-independent approach is clearly called for. The issues include:

(i) Scaling violations in the x-distribution; increase in the "antiquark content" of the nucleon:

The data on muon-nucleon scattering indicate a sharpening of the x-distribution at small x as Q^2 increases, probably implying a larger antiquark content as small x and at higher Q^2. Such a trend appears to be visible in neutrino data as well. The 10-20% decrease in σ_{tot}/E for neutrinos between $E_\nu \sim 10$ GeV and $E_\nu \sim 60$ GeV as observed by CITF and BEBC, as well as the corresponding decrease in B and increase in $< y >_{\bar{\nu}}$, is superficially quite compaticle with such a picture. All this is in turn in qualitative accord with what may be expected from quantum chromodynamics and asymptotic freedom ideas. I expect the experts will have little difficulty accounting for the new data within that framework. However, I don't think it will necessarily represent any major triumph for those concepts; that requires data over a very large Q^2 range to decisively test the small logarithmic dependences predicted.

ii) Nonvanishing σ_L??

We should remember that, despite some theoretical predilection toward a very small σ_L/σ_T, electroproduction experiments do indicate a nonvanishing value which hovers around 0.2 even at rather large Q^2. In neutrino language this would mean

$$\left(\frac{d\sigma}{dy}\right)_{ep} \sim 0.5 + 0.5\,(1-y)^2 + 0.2\,(1-y)\ .$$

It is time for the neutrino physicist to consider the question of such $(1-y)$ terms in the y-distribution. He must also watch out for an x-dependence of the coefficient. To extract any such term even in electroproduction is a great effort; we must expect similar difficulty in neutrino physics. Nevertheless it is time to make the try.

iii) A-dependence

We may expect shadowing effects in neutrino reactions at very small x ($\lesssim$ 0.03??) and perhaps even antishadowing at somewhat larger x ($\sim$ 0.1 - 0.15?). These have been nicely discussed by Zakharov[16] and Nikolaev[17] and will not be elaborated here.

While not a major topic at this meeting, I also found it encouraging that there is some growth of interest in study of the A-dependence of inclusive hadron production by neutrinos. There are all kinds of questions regarding the propagation through nuclear matter of quarks produced in "status nascendi," for which these studies would shed light. The first thing to check is that the spectra of energetic produced hadrons ($E > 20$ GeV??) is A-independent, both with regard to their $p_{\parallel}$ and $p_{\perp}$. Such an observation would strengthen the idea that energetic quark partons do not interact significantly with the wee partons present in nuclear matter.

Finally, I will add one trivial point: it is high time that data on structure functions be plotted versus $\omega = x^{-1}$, not x. To do it in the conventional way is an insult to accelerator builders and funding agencies; it is tantamount to plotting total cross sections versus E^{-1}.

VII. Is the Standard SU(2) ⊗ U(1) Model Correct?

There are strong subjective reasons why the weak-electromagnetic gauge theories nowadays enjoy such overwhelming favor. These include (i) the desire to synthesize the weak and electromagnetic forces within a common framework, (ii) the fact that the theory is based upon a gauge principle, in concordance with pure electrodynamics and general relativity, and (iii) renormalizability: right or wrong, at least the theory allows a systematic and predictive calculational procedure.

Nevertheless, in the long run, we should base our judgment of the correctness of the gauge theory ideas on objective reasons. At present there are such objective reasons but, in my opinion, not many. They deal with the successful quantitative predictions of the SU(2) ⊗ U(1) model, in particular for the ratios R and $\overline{R}$ of neutral current to charged current deep inelastic cross sections. It is quite remarkable that the data for R and $\overline{R}$ lie on the one-parameter curve in R-$\overline{R}$ space allowed by the simple theory. This success gives objective support not only to the applicability of the gauge theory concepts, but equally strongly to the simple SU(2) ⊗ U(1) model.

Some people maintain that just the existence of $\Delta S = 0$ neutral currents argues for the gauge theories. However one could easily expect the existence of neutral currents with strength comparable to charged currents in a much broader class of theories of weak interactions; it is in fact difficult to avoid them. It is also argued that the existence of charm, rather strongly demanded by the gauge theories, is another objective argument in support of the gauge theory concept. However, I again believe that charm provides a solution to the problem of the absence of $\Delta S \neq 0$ neutral currents which is of more general applicability than to just the renormalizable gauge theories.

Thus, for me it is the quantitative agreement of neutral current data with SU(2) ⊗ U(1) predictions which comprises the one major objective piece of evidence in favor of the gauge theory concept, and if SU(2) ⊗ U(1) were found to be incompatible with experiment it would be appropriate to thoroughly reexamine alternatives to the gauge theories themselves.

Thus it is of special interest that we might be heading for a potential crisis situation with regard to SU(2) ⊗ U(1). The first element of the crisis concerns the results from the atomic physics parity violation experiments in Bi.[18] There the experimental limits are about an order of magnitude smaller than expected on the basis of the original Weinberg-Salam model. If one can trust the atomic theory calculations (and on this there is not universal agreement) it would seem there is a serious problem. However, as Sakurai discussed here, even if one disallows conspiratorial models which appeal to accidental cancellations, there is an easy solution to this problem <u>within the SU(2) ⊗ U(1) framework</u>:

one simply reassigns the right-handed electron to a weak doublet, whose partner is a neutral heavy lepton E^0 (of indeterminate mass). This leaves unaffected the successful prediction of R and $\overline{R}$ for semileptonic neutral currents, while predicting a null result for the Bi experiment (but not for similar experiments which may be performed in hydrogen).

However, there is a change in the predictions for $\nu_\mu e$ and $\overline{\nu}_\mu e$ scattering: the electron coupling to Z is vector, and we have

$$\sigma_{\nu_\mu e} = \sigma_{\overline{\nu}_\mu e} \quad .$$

Furthermore the magnitude of $\sigma_{\overline{\nu}_\mu e}$ is suppressed relative to the standard model by a factor

$$\frac{\sigma_{\overline{\nu}_\mu e}}{\left(\sigma_{\overline{\nu}_\mu e}\right)_{\text{standard}}} = \frac{4}{3\left(\dfrac{1}{2\sin^2\theta_W} - 1\right)^{-2} + 1}$$

$$\sim \; 0.3 \text{ for } \sin^2\theta_W = 0.33 \; .$$

While at this time there is nothing decisive (other than perhaps existence) about $\nu_\mu e$ and $\overline{\nu}_\mu e$ data, the Aachen-Padua results presented at this meeting[19] tend to favor the standard model over the modified model with the right-handed electron in a doublet, as well as disfavor the equality of $\nu_\mu e$ and $\overline{\nu}_\mu e$ cross sections. In any case, a definitive measurement of $\overline{\nu}_\mu e$ scattering is of pivotal importance. Paradoxically, if the result were to agree with the original Weinberg-Salam model, this would produce greater difficulties for $SU(2) \otimes U(1)$ than if it were to turn out to be smaller.

If $SU(2) \otimes U(1)$ does not succeed, it is clear that many alternative gauge theories will be proposed to fix up the situation. A preview has been provided by the theoretical response to the HPWFR trimuons, which are themselves another serious challenge to $SU(2) \otimes U(1)$. Alternative theories[20,21], such as $SU(3) \otimes U(1)$ and $SU(2) \otimes SU(2) \otimes U(1)$, tend to describe the semileptonic neutral current quantities R and $\overline{R}$ by at least two parameters, and to me do not persuade in the same way as the original model did. However an exception[22] is an $SU(2) \otimes SU(2) \otimes U(1)$ model of Fritzsch and Minkowski[23], which preserves the usual Weinberg-Salam predictions for neutrino-induced neutral current processes, while predicting neutrinoless neutral current phenomena to be parity conserving. Thus failure of $SU(2) \otimes U(1)$ need not imply complete lack of objective evidence for gauge theories (according to my own criteria), and the Fritzsch-Minkowski model might become an equally credible successor to $SU(2) \otimes U(1)$.

VIII. Exotica, As Seen From A Particular Gauge Theory

The discussion which follows is based on work in progress, and carried out in collaboration with Kenneth Lane. Beyond the obvious reasons, there is another reason for elaborating on it here. This conference was characterized, more than usual, by considerations of lepton number or baryon number non-conserving processes. As we heard from Professor Pontecorvo, in the past year there has been a revival in interest in such subjects, especially with regard to $\mu \to e\gamma$ and $\mu \to eee$. Not always, but quite often, such exotica seem to lie somewhat beyond the mainstream of theoretical work. However, the model Lane and I are working on is a piece of evidence to the contrary. In most respects it looks like just another routine gauge theory, indistinguishable from the countless number of such theories that pollute the literature. On the other hand, it necessarily has lepton number non-conservation built in (although <u>not</u> $\mu \to e\gamma$), and the processes which limit the parameters of the theory (in particular masses of the many gauge bosons beyond W and Z) are a showcase for exotica. It is this feature I wish to exhibit.

The gauge group G is $SU(3) \otimes SU(3)$, along with a reflection symmetry equating the two gauge coupling constants. It is motivated by the following assumptions:

1. $SU(2) \otimes U(1)$ describes data correctly.
2. $SU(2) \otimes U(1) \subset G$, with G simple or pseudosimple (only one independent coupling constant).
3. No leptoquarks; leptons and quarks form separate representations of G.
4. $\frac{1}{4} < \sin^2\theta_W < \frac{1}{2}$.
5. No $SU(2) \times U(1)$ weak triplets.
6. Nonproliferation:

   $4 \leq$ number of flavors < 10

   $3 \leq$ number of positively charged leptons ≤ 6.

Given these assumptions, $G = SU(3) \otimes SU(3)$ appears to be a unique solution. Furthermore the fermion representations are also almost unique; the simplest solution for the fermions is isomorphic to the E6 model proposed by Gursey, Ramond, and Sikivie.[24] There are 6 quarks, with only $(u\ d')_L$ and $(c\ s')_L$ coupled to the W, 4 charged leptons, 2 neutral heavy leptons, and 4 new neutrinos. The Weinberg angle is fixed: $\sin^2\theta_W = \frac{3}{8}$. More specifically, we have

i) Quarks in $(3,1) \oplus (3,1) \oplus (1,3) \oplus (1,3)$:

$$q_L = \begin{pmatrix} u \\ d' \\ b \end{pmatrix}_L \quad Q_L = \begin{pmatrix} c \\ s' \\ h \end{pmatrix}_L \quad q_R = \begin{pmatrix} u \\ s \\ h \end{pmatrix}_R \quad Q_R = \begin{pmatrix} c \\ d \\ b \end{pmatrix}_R$$

The charges of b and h are $-\frac{1}{3}$.

ii) Leptons in $(\bar{3},3) \oplus (\bar{3},3)$:

$$E = \begin{pmatrix} \bar{\tau}^o & e^+ & \tau^+ \\ e^- & \nu_e & \nu_E \\ \tau^- & \nu_\tau & \tau^o \end{pmatrix}_L \qquad M = \begin{pmatrix} \bar{M}^o & M^+ & \mu^+ \\ M^- & M^o & \nu'_M \\ \mu^- & \nu_\mu & \nu_M \end{pmatrix}_L$$

iii) Two octets of gauge bosons

$$\mathscr{W} = \begin{pmatrix} \cdots & W^+ & V^+ \\ W^- & \cdots & U^o \\ V^- & \bar{U}^o & \cdots \end{pmatrix} \qquad \mathscr{W}' = \begin{pmatrix} \cdots & W^{+\prime} & V^{+\prime} \\ W^{-\prime} & \cdots & W'_1, U'_2 \\ V^{-\prime} & U'_1, U'_2 & \cdots \end{pmatrix}$$

where

$\ldots$ = linear combinations of 4 self-conjugate bosons A, Z;B, C .

Notice that this unification attempt differs from most others in that leptoquarks are assumed absent. In the more "conventional" attempts, the leptoquark masses must be chosen to be enormous ($\gtrsim 10^{15}$ GeV, the Planck mass ?) in order to protect the observed stability of the proton.[25] While this feature invites vigorous efforts to improve the experimental limits[26] on proton life-time, it also implies a rather large commitment to our understanding of dynamics over 10-15 orders of magnitude in distance below what is presently attainable. We choose a more modest goal, but one lacking the possibility of unifying the strong force with the weak and electromagnetic.

Because the model contains $SU(2) \otimes U(1)$ as a subgroup, we may in principle let the masses of other gauge bosons become large (but considerably less than 10^{15} GeV) and suppress any predictions not contained within the $SU(2) \otimes U(1)$ framework. However, it is of interest, while not a necessity, to determine what phenomena constrain the masses of the other gauge bosons. It turns out that all other gauge bosons must have masses at least 3-4 times larger than W and Z, and that the limits come mainly from low-energy measurements, some of which need improvement. The main constraints are as follows:

i) Muon decay gets extra contributions from V, W', and V' exchange:

$$\begin{aligned} \mu^- \to\ & e^- \bar{\nu}'_E \nu'_M \\ & e^- \bar{\nu}_M \nu_e \\ & e^- \nu'_M \nu_E \,. \end{aligned}$$

The W' and V' exchange contributions are V + A and diminish the parameter[27] ξ describing the angular distribution of electrons relative to muon spin:

$$\xi = 1 - \left(\frac{\mu_W}{\mu_{W'}}\right)^4 - \left(\frac{\mu_W}{\mu_{V'}}\right)^4 = 0.973 \pm 0.014 \,.$$

ii) There are extra contributions to $\Delta S = 0$ and $\Delta S = 1$ semileptonic decays; however Cabibbo universality

$$G_\mu^2 = G^2_{\Delta S = 0} + G^2_{\Delta S = 1}$$

is disturbed by the V and V' exchange contributions to μ-decay, which have no semileptonic counterpart;

$$\frac{2\delta G_\mu}{G_\mu} = \left(\frac{\mu_W}{\mu_V}\right)^4 + \left(\frac{\mu_W}{\mu_{V'}}\right)^4 < 10^{-2} \,.$$

However it is interesting that W' exchange contributions are Cabbibo universal, and incoherent to W-exchange contributions. They do <u>not</u> upset universality.

iii) Because W' exchange in semileptonic processes is $\Delta S = 1$ (see item vi) there is a possibility of observation of other kinds of neutrino emitted in $\Delta S = 1$ semileptonic processes. The most prominent case is $K_{\mu 2}$ decay:

$$\frac{\Gamma(K^+ \to \mu^+ \bar{\nu}'_M)}{\Gamma(K^+ \to \mu^+ \nu_\mu)} = (\sin^2 \theta_c)^{-1} \left(\frac{\mu_W}{\mu_{W'}} \right)^4 .$$

This implies

a) The polarization of μ in $K_{\mu 2}$ (and $K_{\mu 3}$) decays is less than 100% because $\bar{\nu}'_M$ has helicity opposite to ν_μ. To my knowledge[28] this polarization is only known to be complete to an accuracy $\sim$ 10-20%. I should think this could be improved.

b) Because $\bar{\nu}'_M$ does not couple to W, not all neutrinos from kaons will interact in neutrino detectors; i.e. the apparent cross section $\sigma_{\nu_{\mu K}}$ (E) will be less than $\sigma_{\nu_{\mu\pi}}$ (E). An experiment at FNAL is in progress to test this.[29]

iv) In τ^- decay U' exchanges interfere with W-exchange leading to a violation of μ-e universality:

$$\frac{\Gamma(\tau^- \to e^- \nu \nu)}{\Gamma(\tau^- \to \mu^- \nu \nu)} > 1 .$$

v) While the assignment of e^-_R to a weak doublet eliminates the Z-exchange contribution to the atomic parity violation in Bi, B-exchange does contribute, providing a good limit on μ_B (which must be considerably heavier than Z).

vi) The assignment of right-handed quarks we have made is controlled by the absence of neutrinoless double β-decay. If d_R is mixed with s_R, with mixing angle θ, then diagrams with joint W and W' exchange between electron and a nucleon pair induce the $\beta\beta_0$ reaction. Relative to the "standard" rate Γ_0, one has

$$\frac{\Gamma}{\Gamma_0} \sim \left(\frac{\mu_W}{\mu_{W'}}\right)^4 \theta^2 \lesssim 10^{-7 \pm 1} .$$

Thus the experiments described by Fiorini[30] are a quite significant constraint on the structure of the model.

vii) Finally there is the question of $\mu \to e\gamma$, $\mu \to eee$ decays, and μ capture to an electron final state. The analogy

$$\mu \leftrightarrow s$$

$$e \leftrightarrow d$$

along with the known Cabibbo mixing of s and d argue in general for a similar mixing of μ and e. However, in this model the situation is not too clear. The $(\bar{3},3)$ representation of leptons is structurally different from the quark representation, and while mixings of E and M multiplets may be possible, there are difficulties as well.

But independent of this model, the recent theoretical developments summarized by Pontecorvo underline the importance of pushing the experimental limits as much as possible.

Before leaving the $SU(3) \otimes SU(3)$ model, I list some of its most characteristic features:

i) There is no high-y anomaly; W does not couple to right-handed quarks.

ii) The b-quark decays only semileptonically.

iii) M and b are <u>stable</u> in the $SU(2) \otimes U(1)$ limit. Thus their lifetimes will be at least $\sim$ 100 times longer than given by conventional estimates.

iv) We expect $m_{\tau^0} \cong m_{\tau^-}$. However we have no easy interpretation of the SKAT event. The τ^0 should be visible at PEP and PETRA via the reaction

$$e^+e^- \to \nu_e \bar{\tau}^0$$
$$\qquad\qquad \bar{\tau}^0 \to e^+\pi^- .$$

v) There are four new neutrinos. They might mix and oscillate, but we have nothing specific to offer, because our understanding of fermion mass generation is not good.

vi) There are many mechanisms of multilepton production by leptons. But rates and branching ratios are low, and sensitively depend on the unknown masses of the heavy gauge bosons. There is a mechanism for trilepton production via U' exchange.

$$\nu_\mu d \to M^0 + b$$
$$\qquad\qquad b \to u\mu^-\nu$$
$$\qquad M^0 \to \mu^-\mu^+\nu .$$

However, we expect the rate is low compared to what is suggested by HPWFR.

vii) The $U'-\bar{U}'$ mixing leads to mixing of the $b\bar{d}$ meson with $\bar{b}d$. These $b-\bar{b}$ oscillations enrich the class of multilepton final states which can exist.

Regardless of whether this model turns out to be correct, there may be lessons to learn from it: in particular that all selection rules should continue to get the most careful examination, that there is still great importance in improving measurements of low energy decay processes, that the issue of neutrino identity needs continuing scrutiny, and that hypothetical objects with lifetimes much longer than standard estimates are credible and deserve careful searches.

IX. Conclusions

The first conclusion is that neutrino physics is growing up: much of the new data is of unprecedented refinement and precision. While such progress should have considerable impact on the important dynamical issues such as the question of scaling and its violation or the properties of hadron final states, our attention remains largely focused on structural questions such as the number of building blocks and the basic interactions between them. New building blocks, the charmed quark and τ^-, have become better established while others, such as the b-quark or the heavier leptons used to interpret trimuon data, have lost some support. And a new candidate—a neutral lepton—has appeared on the scene, a candidate which yet needs much more study and confirmation before it can begin to be considered established.

And what about the prognosis from theory? I think we should expect more building blocks, and that the situation will become more complex before it becomes simpler. We may well be in the same position as the chemist discovering elements; a sufficient number must be found before a pattern emerges which allows further synthesis. And we must recognize that while there is broad consensus regarding the applicability of gauge theories to weak and electromagnetic interactions and of "quantum chromodynamics" to the strong, none of this is firmly founded as yet. We may have big surprises ahead of us. In particular, the spectrum of masses of the building blocks remains utterly baffling. It is here that the present theoretical framework faces its greatest challenge: we have many clues such as massless (or almost massless) neutrinos, an incredibly light electron, large ratios of bare ("current algebra") masses of other constituents (e.g. m_u/m_c), curious degeneracies ($m_\tau \sim m_c$; $m_s \sim m_\mu$), and the tantalizing relation $\theta_c^2 \sim m_d/m_s$. But if there is a message in such relationships, it has not yet been deciphered.

But no matter how uncertain or certain is the theory, it takes new data for certain progress. The vitality of present-day experimental neutrino physics is assurance that there will be plenty of progress in the near future.

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References

1. R. Turlay, these Proceedings.
2. P. Rapidis et al., SLAC/LBL preprint SLAC-PUB-1959/LBL 6484.
3. J. Kirkby, private communication.
4. K. Lane and E. Eichten, Phys. Rev. Letters 37, 477 (1976).
5. A. Muzhin, these Proceedings.
6. V. Khose, these Proceedings.
7. A. Benvenuti et al., Phys. Rev. Letters 36, 1478 (1976) and 37, 189 (1976).
8. A. Bodek, these Proceedings.
9. R. Turlay, these Proceedings.
10. D. Cundy, these Proceedings.
11. M. Barnett and F. Martin, SLAC preprint SLAC-PUB-1892; A. Bouquet and J. Kaplan, Paris preprint PAR/LPTHE 77/10; J. Bernabeu and C. Jarlskog, preprint CERN-TH-2313.
12. A. Benvenuti et al., Phys. Rev. Letters 38, 1110 (1977).
13. A compilation is given by M. Barnett in his report at the Budapest Conference; preprint SLAC-PUB-1961. See also M. K. Gailliard, these Proceedings.

14. E. Kuznetzov, these Proceedings.

15. H. Faissner, these Proceedings.

16. V. Zakharov, Proceedings of the 18th International Conference on High Energy Physics (Dubna, 1977), Vol. II, p. B69.

17. N. Nikolaev and V. Zakharov, Yad. Fis. 21, 434 (1975).

18. P.E.G. Baird et al., Nature 264, 528 (1976) and results reported at the 1977 Spring Meeting of the American Physical Society, Washington D.C.

19. H. Faissner, these Proceedings.

20. For example, B. Lee and S. Weinberg, Phys. Rev. Lett. 38, 1237 (1977).

21. For example, R. Mohapatra and D. Sidhu, Phys. Rev. Lett. 38, 667 (1977). A. de Rujula, H. Georgi, and S. Glashow, preprint HUTP 77/A028.

22. This is mentioned in the contribution of J. J. Sakurai, these Proceedings, and especially emphasized to me after the meeting by H. Harari.

23. H. Fritzsch and P. Minkowski, Nucl. Phys. B103, 61 (1976); see also J. Pati and A. Salam, Phys. Rev. D10, 275 (1974).

24. F. Gursey, P. Ramond, and P. Sikivie, Phys. Lett. 60B, 177 (1976).

25. See for example H. Georgi and S. Glashow, Phys. Rev. Lett. 32, 438 (1974).

26. It was good to hear here of the vigor that does exist; c.f. R. Steinberg, these Proceedings.

27. I. Gurevich et al., Phys. Letters 11, 185 (1964), and V. Akhmanov et al., Sov. J. Nucl. Phys. 6, 230 (1968).

28. D. Cutts et al., Phys. Rev. 184, 1380 (1969).

29. This is an adjunct to the CITF experiment described by A. Bodek, these Proceedings. It was proposed by E. Fowler and G. Kalbfleisch together with the CITF group; analysis of the data is in progress.

30. E. Fiorini, these Proceedings.