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SYMPOSIUM SUMMARY AND PROGNOSIS

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A lot has happened in the last two years since the Bonn Conference, and so I went back to the Proceedings to look over how it was in those days. In particular, since I have to finish up this conference I looked at Sid Drell's talk where he spoke most eloquently about voyaging across the oceans of ν and Q^2 space searching for new lepton lands, new frontiers, and all that. Sure enough, we are still sailing around. But I don't think any of us realized then that two years later some people would be talking about getting to our destination by using magnetic monopoles. We heard about the monopoles this morning, and it is clear that if the monopole event is real it is the most important result in this conference. It is also clear that the story isn't over. The debate must continue as any good scientific debate must, and I am sure we haven't heard the last of it. I myself know very little about the subject and so I won't comment further on it.

I. LOW ENERGIES

A lot has happened since Bonn, especially in colliding beam physics, and you probably remember how it was then. I remember, I had to talk about it. There were a few points, with big errors (Fig. 1a), and there was great excitement because Frascati and CEA found that R was big and increasing with energy. And then later on, by the time of the London Conference, more points were added by SPEAR (Fig. 1b). And we heard about the way it is now (Fig. 1c). But look what's happening at low energy, below the ψ. Last spring, we had a few SPEAR points (Fig. 2a). Now we have some more from Frascati (Fig. 2b), which seem to indicate a rising R again. Maybe by the next conference it will look something like Fig. 2c. We really don't know much yet about what is going on above the ϕ and below the ψ. There could be another new world down there which still hasn't been explored at all. The first generation Frascati experiments which started it all were a great pioneering effort, but the results have errors sufficiently large that we can't draw solid, detailed conclusions. So there is a lot of room at the low energies for new discoveries and even new discoveries of a headline-making sort. The second generation storage ring experiments at Frascati, and the experiments that will be coming on at Orsay will be extremely important for really clearing up the question of whether R in fact is constant at 2-2.5, which complacent theorists prefer, or whether there is actually something going on at the lower energies.

It is not only in the colliding beams where there is a lot of activity and a lot of interest at low energies. Of course all the J/ψ excitement started at low energy machines with Sam Ting at Brookhaven and here at SPEAR. But in addition to that, other big issues will be clarified a great deal by low energy experiments. In the case of neutral currents, we want to know their quantum numbers, e.g., isoscalar versus isovector, and vector versus axial or something else. For such a question it is very important to have clean methods of determining the selection rules, and exclusive channels possess a clear advantage. Thus the low energy neutrino experiments have a

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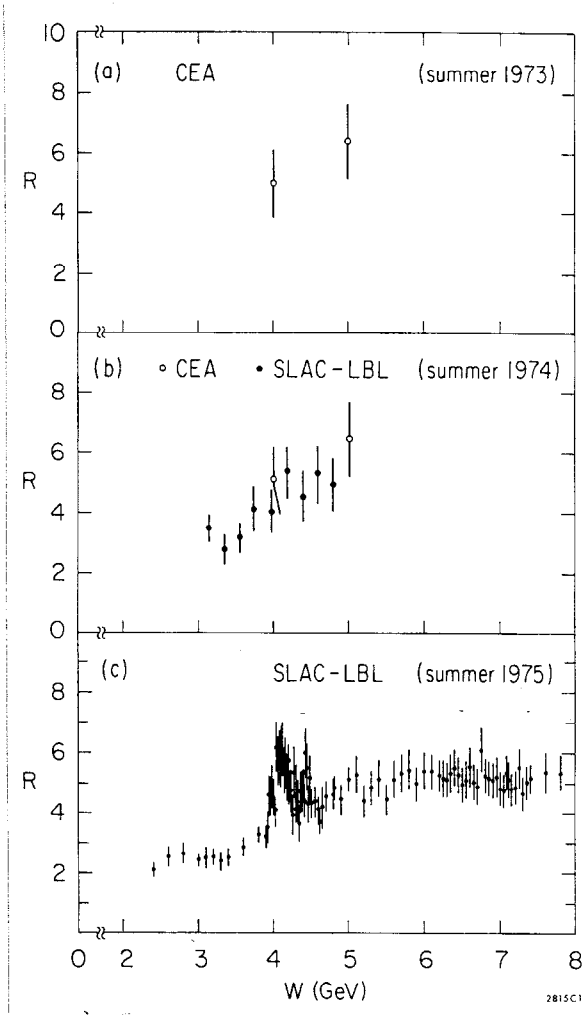

Fig. 1--Data on e^+e^- annihilation above W = 3 GeV the time of (a) Bonn, (b) London, and (c) now.

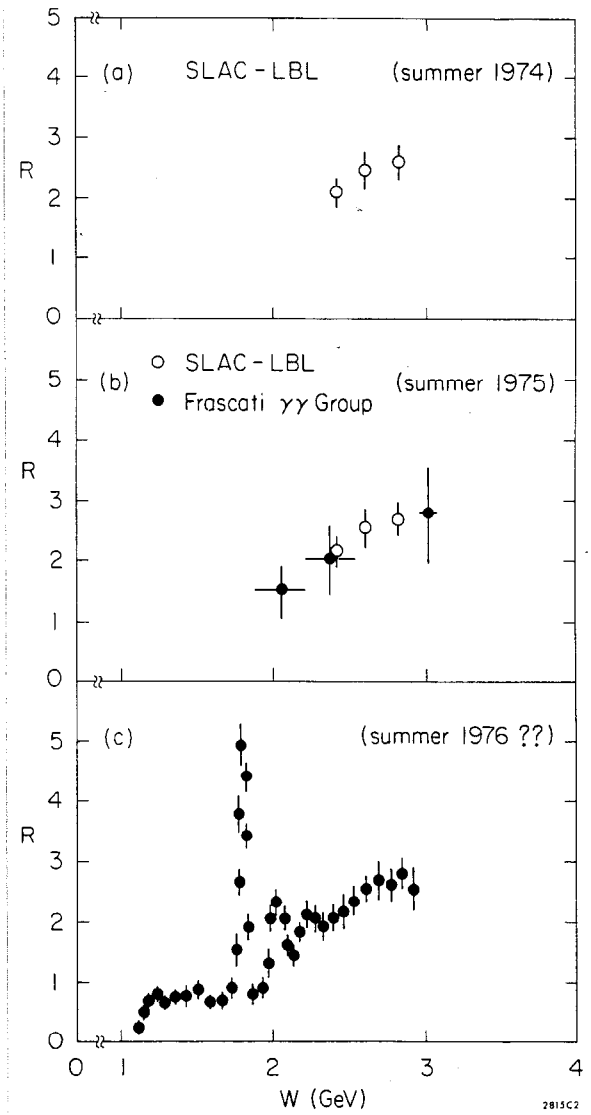

Fig. 2--Data on e^+e^- annihilation below W = 3 GeV, at the time of (a) London, (b) now, and maybe (c) next year. The important first-generation data from Frascati have not been included.

special advantage, because specific N* states are predominantly excited. This includes not only the 33 resonance, but the higher ones like S_{11}, with its characteristic ηN decay mode. Also, Lincoln Wolfenstein told us about the search for parity violating neutral currents in atomic transitions. Gary Feinberg has suggested that one do low energy electron scattering from nuclei, exciting specific nuclear levels with polarized lepton beams in order to look for very minute asymmetry effects. While minute, the effects are large enough to make it at least thinkable that they are measurable. Leon Lederman told us about direct lepton production. There one of the great issues is what the energy threshold is. Again, the relatively low energy machines are crucial in helping us understand the origin of the direct leptons. And, as you heard, as it stands now we just don't know. Then of course there is the Great Charm Sweepstakes, and other new particle searches, where again the production thresholds are expected not to be tremendously high. I think this is all extremely healthy. We always look towards high energies for the basic solutions of our problems. I think, of course, that it is correct that we do, and that there is no substitute for the high energy machines. But the evidence shows that experimentation on the low energy machines, even of exploratory character, is anything but dead.

There is vitality at low energies not only for the big headline issues but also for more programmatic areas like N* photoproduction. We heard in the summary talks of Fischer and Donnachie the very impressive progress being made there, with large quantities of high statistics data at many neighboring energies. The photoproduction program competes in quality with the program of low energy π-nucleon scattering carried out over the last decade, and which paid off so handsomely. Why do we trust in the quark model? One of the main reasons is that it gives such a good description of not only the nucleon and the Δ but also the myriad of higher N* resonances, which fit nicely into big SU(6) multiplets. Litchfield described all of that at London last summer in a very impressive way. So it is encouraging that the photoproduction is carrying on in the same manner and tradition; perhaps there is an equally large payoff ahead. I also feel that, beyond photoproduction, the electroproduction of nucleon resonances is a field which has an especially pivotal position. This is the one place where the two areas of physics in which we use quark concepts most successfully, namely the deep inelastic region and the baryon-resonance region, overlap and become one. We already know some interesting results in this region: for example, as Q^2 increases the Δ production seems to be less important than elastic or I=1/2 N* production. In the second resonance region the helicity 3/2 production amplitudes seem to diminish with Q^2, in favor of helicity 1/2. All this is what most theorists like; in particular it is dual to the behavior in the deep inelastic region and also is expected in quark model calculations. There are also some peculiarities in the second resonance region which at least puzzle me, although there may be some mundane reason behind them. The D_{13} and S_{11}, which are close cousins (^{2}8; 70 L=1) in the SU_6 description, have form factors which are quite different. The D_{13} form factor falls off rapidly with Q^2, while S_{11} is stiff and does not. Maybe someone has a simple explanation for this. But, aside from what we might learn from N* electroproduction, there is also the important practical use for such data as input for the neutral-current issue we already discussed.

Another low-energy question which shouldn't be forgotten in all the excitement of this last year is the status of meson spectroscopy. We would like this subject to be as clean as in the baryon spectroscopy, but at present it just isn't. The pseudoscalar states puzzle us: η and η' consistently do things that we don't expect them to. And where is the A_1? Sakurai reminded us that a wonderful way of finding it is in the decays of the heavy lepton U (if it does turn out to be a heavy lepton). If low-energy people can't find the A_1 for us, maybe neutrino people can by observing the coherent

production reaction $\nu + A \rightarrow \mu^-(A_1)^+A$. But someone had better find it. It is becoming a crisis situation. There are also all the meson-spectroscopy issues that have been raised by the charmonium interpretation of ψ physics. Is there a ϕ' (and ω')? Is ϕ' to ϕ as ψ' is to ψ? Were ϕ' to be found, what is the dipion spectrum in $\phi' \rightarrow \phi\pi\pi$? In strong interactions are there K pairs produced in association with the ϕ? This is relevant because charm searchers would like D pairs produced in association with ψ's in strong processes. I am sure that the professionals in meson spectroscopy can add much to the above shopping-list.

All of these old-spectroscopy problems will involve slow, painstaking work where we don't really see at any given time spectacular results, but where they creep up on us slowly. Nevertheless they are quite important. Low energy storage rings ($W \sim 1.5$-3 GeV) may also be a vital element in sorting out the meson spectroscopy. They are much better positioned to do that than the higher energy machines.

II. FOUNDATIONS

Amid all the excitement of new physics in this conference, we also had today a great example of what our standards in physics ideally should be. When we are confronted with very new phenomena like we are now, I think it is important to look at what we really believe and what we don't, what can we trust, what we can build on, what our foundations are. The best foundations of physics are gravity theory and quantum electrodynamics. And the g-2 experiment we had described to us is an example of just the very finest that theory and experiment can give us. I have heard that talk on g-2 before. I looked forward to it again, and I know I will look forward to hearing it again and again and again in the same way I look forward to hearing my favorite piece of music or going to a museum to see my favorite works of art. This experiment is in a class by itself, and I know everyone will join with me in thanking Messrs. Combley, Farley, Picasso, and everyone who worked on it for giving us such a beautiful thing. Also, we theorists thank them for getting an answer which agrees with the theory.

In thinking about what distinguishes a really good theory from just any old theory we need just to look at electrodynamics and to our reactions as an experimental result comes in. The first thing we ask is "Does it agree with the theory?" Either it agrees or it doesn't. If it does agree, then almost everyone goes back to doing something else. If it doesn't agree then we ask "Did anybody else do the experiment? Has the calculation been checked?" and so on. Great debates revolve around the experiments and the calculations, with at most a few people thinking about changing the theory (and for a while no one would take them very seriously anyway). Compare that with what happens in charmonium theory, the parton model, or the like. When the experiments appear and disagree, usually the theory will bend to accommodate the experiment. And between quantum electrodynamics and our most speculative theories there is a whole continuum. I have tried to survey what might be the most reliable foundations we have beyond QED and gravity, using this kind of criterion. I found surprisingly that in strong interactions there are quite a few foundations, foremost of which are the simple conservation laws such as isotopic-spin, strangeness, P, C, T, etc. Beyond that are the forward dispersion relations, and the various rigorous bounds a la Froissart and Martin.

In our field of weak and electromagnetic processes, there are also a few foundations. On the top of my list is current algebra, specifically the consequences of the $SU(3) \times SU(3)$ algebra of charges of Gell-Mann. Cabibbo theory, i.e., the theory of the

weak semileptonic decays, comes very close to having the status of being a foundation. When such experiments come along we ask simply, "Does it agree with Cabibbo?" If it doesn't, we really scrutinize what is going on. Maybe a few results from local current algebra or the ideas of scale invariance at short distances might also be elevated to foundation level. There is the Adler sum rule (I have a hard time with that one) or some of the more solid deductions using the ideas of scale invariance at short distances, such as constancy with Q^2 of the first moment of νW_2, the constancy of the colliding-beam R with increasing energy, the Crewther relation, the Adler anomaly, and maybe some of the high Q^2 sum rules (e.g., Gross-Llewellyn Smith) as derived using the BJL method. Whether these really are foundation status, I'm not sure, but they are certainly far up the list of candidates.

III. STANDARDS OF REFERENCE

Theoretical concepts of somewhat lower reliability I call standards of reference. These are what an experimentalist will naturally use to interpret his data. A standard of reference doesn't have to be right or wrong. All it has to be is a reasonably definite working hypothesis and an acceptable language that everyone can use. As the evidence for a given standard of reference gets better and better it might approach the foundations we talked about. I regard the quark model as standard of reference, not as foundation. If we apply the test "Does such-and-such experiment agree with the quark model?" often we don't quite know how to answer, because we don't quite know what the quark model is. The most precise way of phrasing the quark model again is through current algebra. For the resonance phenomenology this is the program associated with the Melosh current-to-constituent-quark transformation. There the formulation is reasonably precise, but the actual phenomenology is complex. There is a considerable amount of representation mixing and ambiguities in determining resonance parameters. Does it all agree with the quark model? Probably Kiskis gives the right answer: "It depends." The other major area of quark model applications lies in the deep-inelastic phenomena. Chris Llewellyn Smith very nicely stated the case of how much evidence for quarks there is in the deep inelastic processes. He argued that there isn't all that much, and that once one utters the words (i) no appreciable contribution from isoscalar photons, (ii) maximum V-A interference, (iii) scaling, and (iv) CVC hypothesis, that one has obtained most of the relationships in the electroproduction and neutrino data without really mentioning the work quark. Now the above words may in fact mean quarks to some people but certainly not to everyone. It seems to me that in the resonance business we also need that same kind of careful criticism along the lines Llewellyn Smith gave. We must be absolutely sure that we are on the right track and that the quark description is right. It will make a very big difference in our interpretation of the new physics if we are making a mistake in building everything in the old physics in terms of tricolored quarks. I worry that the community of potential critics isn't loud enough these days. There used to be an active community of baryon resonance bootstrappers and S-matrix advocates who provided a critical background for the quark model description. Where are they?

Going beyond the quark model, there are many standards of reference of less ambiguity, albeit less credibility. For the strong interactions there are the ideas of limiting fragmentation, duality, Regge-poles, Mueller-Regge formalism, and so on. But the classic example of a standard of reference these days is in the weak interactions. It is the Weinberg-Salam model for neutral currents. You have heard ample evidence in this conference that when an experimental result comes in, everyone asks "Does it agree with Weinberg-Salam?" And if sooner or later the experiments disagree, it won't matter. It won't matter because we can change the model. This can be

done by changing the underlying group, or changing the representations involved without really changing the underlying conceptual structure. Nevertheless, it is very useful to have the Weinberg-Salam model as the simplest prototype—a standard of reference—for the broad class of possible unified gauge theories of weak and electromagnetic interactions. Already, it has clearly played an important role in shaping and stimulating the experimental searches for neutral currents. But it doesn't mean it is right.

I think in some sense the deep inelastic scaling has played a similar role as standard of reference. Certainly the assertion of precise scaling in the parton model doesn't have an absolute foundation. Again, Llewellyn Smith reminded us that even in parton terms one can talk about scaling violations in a way that is similar to the field theorists' approach. And while I am on this subject, I think the whole question of the scaling violations, for which we saw a lot of very beautiful data, is going to be one which isn't answered in 25 words or less, or in a few minutes or even a few months. The subject has become quite sophisticated and, unless the scaling violation in the next-generation experiments at $Q^2 \sim 100$ greatly exceeds the 20% level, it is going to take a long time to straighten out what those scaling violations really mean. However, the pattern of violation does give great encouragement to the honest field theorists who could not accommodate exact scaling even in scale-invariant theories.

Finally, before leaving the topic of electroproduction, I should also mention the problem of the very large-ω data and the whole question of shadows. Such phenomena may be tied up with some aspects of the scaling violations. In any case I don't think any theory is doing well in understanding the question of shadowing. There are quite fundamental issues involved in the space-time structure of large-ω electroproduction and high-energy photoproduction. These issues connect, via vector dominance, smoothly into the issue of very high energy strong interactions, namely the nature of the Pomeranchuk singularity at J=1.

Another important standard of reference is the Drell-Yan parton—antiparton annihilation mechanism for producing lepton pairs (or other interesting objects) in hadron collisions. We heard some evidence which, if not conclusive, certainly is positive that the Drell-Yan mechanism may have something to do with the data. At both Fermilab and Brookhaven energies one now sees a ratio of experiment to theory (using tricolored quarks) which is about 2 or 3. In other words if one ignored color, the data would be explained. As Llewellyn Smith emphasized, that result is of vital importance for the future of very high energy proton storage rings.

The standard of reference which I think has taken the biggest jump forward in credibility in this conference has to do with the use of the parton model for deep inelastic production of final state hadrons. There are a host of results which are quite consistent with the idea that hadrons emerge along the direction of the struck parton, that there aren't too many of them, and that they look more or less independent of production mechanism. In the colliding beams we have, because of the discovery of transverse beam polarization, very beautiful evidence of jets. The transverse momenta of the hadrons relative to the jet axis, presumably the direction of the produced partons, is limited. The angular distribution of the jet axes follows what is expected from production of spin 1/2 partons. Furthermore, the inclusive distribution scales and looks quite similar to the inclusive distributions in electroproduction and neutrinoproduction. We saw very significant data from the 15-foot bubble chamber at Fermilab, as presented by Byron Roe. Again, there the idea that the properties of final states at fixed hadron invariant mass are (at the factor of 2 level of approximation) universal seems to work very well. The multiplicities in the neutrino reactions

in particular are low and characteristic of any old reaction at the same hadron total cms energy. This is especially significant because at the highest neutrino energies the mean Q^2 is very large, between 20 and 30 GeV^2. This exceeds all other deep-inelastic experiments (other than the colliding beams) in Q^2-range by a factor of ~ 5. Thus so far the jet idea in lepton-induced processes is working and, if not established, is at least a good standard of reference. This, together with the existence of the Drell-Yan mechanism, would make for wonderful physics in the proposed very high energy pp storage rings such as Isabelle or Popae. We can look for the W or Z by annihilating parton and antiparton from the incident protons, and looking at the decay leptons. Those who don't like to look for decay leptons of the W can even look for the hadron jets into which they most likely decay. The W should decay into hadrons in the same way as a virtual photon does. We now have evidence that virtual photons decay into two hadron jets, and that everything is scaling with energy in that process. Thus one can look with calorimeters for 50 GeV decay jets coming out back to back; they would be very spectacular. And if there are no W's or Z's, and if the weak-interaction q-q cross section continues to rise linearly (as one might expect if there were no phenomenon like W or Z exchange intervening), then just the quarks in the initial proton beam can scatter from each other through weak interactions. We can then again look for the hadron jets from the quark-quark scattering with some confidence, because we will have seen the jets in electroproduction and neutrino-production processes. In fact the biggest problem may be a background of hadron jets produced by strong interactions, as evidenced in the recent ISR data presented by DiLella. But the s-dependences are quite different, and the attainable s and Q^2 values compete with G_F^{-1}, the unitarity bound for weak interactions discovered years ago by Lee and Yang. Proton-proton rings will be great.

IV. THE NEW PHYSICS

For the new physics, we have new standards of reference: SU(4), charm, and charmonium dynamics. We now turn to that subject in detail. The tone of this discussion of new physics will be cautious, conservative, perhaps reactionary. We should not jump to conclusions too soon and keep open as many options as we can as long as we can. Let us start with the colliding beam data. What we have learned so far? From the observed increase in R (i.e., the ratio of the total hadronic cross section to the μ-pair cross section) at $\sqrt{s} \sim 4$-5 GeV, as well as the existence of the ψ and ψ', there is strong evidence that the electromagnetic current has a new piece which has small matrix elements between ordinary hadron states. Because R is constant, the new piece of the current is probably composed (in the same sense we consider the old current being composed of quarks) again of spin 1/2 degrees of freedom. The spin 1/2 is a consequence of the constancy of R and its large magnitude. Constancy of R can be expected theoretically if one has spin zero or spin 1/2 constituents. If the spin is larger than 1/2, one expects R to be a rising function of $\sqrt{s}$. For spin zero constituents, even with integer charge one only gets a quarter of an R unit per constituent. A tremendous number would be needed in order to build up an R of 5. So the new current probably has spin 1/2 constituents. Furthermore they probably interact strongly with each other in order to form the ψ as a bound state. There is a nice argument of Aviv, Goren, Horn, and Nussinov that once one assumes the minimal electromagnetic coupling this is almost forced by the large leptonic width of the ψ, which is order α^2. Minimal electromagnetic coupling means that the virtual photon first creates a virtual charged pair. That costs a power of α in amplitude. Then this virtual pair has to couple to the ψ. This latter coupling must be of order 1, in order that the leptonic width be of order α^2. What are the spin 1/2 constituents in this new current? Either they are new degrees of freedom, or else (as in color models) the new piece of the current is a new combination of the old degrees of freedom (e.g., in such a way that

the new current is color octet instead of color singlet). Haim Harari talked about the difficulties of that idea, and I won't make any more elaborations. While it is certainly premature to reject color, I will assume that there are really new degrees of freedom in the new electromagnetic current. As a consequence of this I find it quite hard to avoid the conclusion that above the rise in R there will be pairs of new particles of narrow width copiously produced. Why is that? In the old physics we think of quarks as the constituents of the current and at high energies we still don't expect quarks to come out into the final state. Here it is different. Why? In this case the current is made of a pair of heavier quarks (because the mass scale for the new phenomenon is so large). After the instant of production, the heavy quarks may go a little distance before they do something (in order to protect free-field short distance behavior), but after they have gone a little distance something may happen. We might think that they reannihilate into ordinary hadrons, in a way similar to $p\bar{p}$ annihilation near threshold, which makes many π's and not much baryon number in the final state. But this mechanism is hard to support, because if the produced constituents in the continuum region do that, then why don't the bound constituents in the ψ do the same thing and give the ψ a large width? For the ordinary quarks, we may think of some kind of vacuum polarization of quark-pairs screening the quark charge and allowing integer charge objects to emerge. However, for the new current that mechanism doesn't work as well. In order to neutralize the quantum numbers of the heavy constituent, at least another pair of heavy constituents would have to again be materialized from the vacuum. We would then expect the threshold to be much higher, at $\sim$6, not 4 GeV. And that is not the case. In addition we might expect a large number of inclusive ψ's produced above 6 GeV. So that leaves two alternatives. Either an ordinary quark pair (as in the standard charm picture) are materialized and attached to the heavy pair to form, e.g., $D\bar{D}$, or else nothing happens and the heavy pair emerges into the final state. But in either case, new objects are going to come out. Furthermore, they have to be narrow in order to protect the narrow width of the ψ. If the D or constituent were to have a large width into ordinary particles the decay process could happen in the ψ, and the ψ itself would have a larger decay width. Thus I find it a good bet that there really are new narrow particles pair-produced in the 4 to 5 GeV region. Somewhere in that e^+e^- data they are there. Hereafter I will call (generically) these objects D, athough they need not be identified in specific properties with the D of the orthodox SU(4) charm model.

There are of course crucial assumptions in making this whole line of argument. First of all, whether R itself is constant with energy is not so clear from the experimental point of view. You may use your imagination and put several resonances on top of a rising background. And also there is the assumption that R really is the sum of squares of charges as in the naive picture. That is not true in field theories with anomalous dimensions, where, as Polyakov mentioned yesterday, R is a constant but can't be calculated. And there is a certain amount of loose language about constituents and what they do which has been used.

What is implied by these conclusions is, first, the production of new particles and, second, the existence of charmonium spectroscopy. If we have a thing and an antithing bound to each other strongly, there can be many excited states, triplet and singlet, S and P, and so on. As we saw in the beautiful experimental talks on the storage ring results, there is now considerable support for the existence of such a new spectroscopy. This will be a fine playground for both experimentalists and theorists, and it is clear there is lots of fun ahead for us all. This spectroscopy will be an extremely good testing ground for theoretical ideas of the dynamics of strong interactions. There was hope in the early days that the ψ would be the hydrogen atom of

strong interactions. But the potential (if there is one) has the wrong shape. It is not clear how things will turn out in the long run, but at least we have got a spectroscopy.

What is not implied so far by this line of argument is charm itself. We haven't mentioned the weak interactions or its problems. It is not even necessary for the ψ and ψ' to be hadrons. What is a hadron? I would propose that the definition of a hadron be any particle that interacts (nonelectromagnetically) with protons and neutrons with a typical cross section greater than or equal to a millibarn. The millibarn is negotiable, but not by much more than factors of 2. Now it was argued by my good friend Haim Harari that in fact we know ψ is a hadron. So I would like to go over those arguments again:

1. $\psi(4.1)''$ and $\psi(4.45)''$ are broad.
2. Existence of P_c, χ, $x(2.8)$: many states of different J^{PC}.
3. $\psi \rightarrow$ hadrons with $G=-1$; $I=0$ selection rule.
4. $\sigma(\gamma p \rightarrow \psi p)$ and VDM imply $\sigma_{tot}(\psi p) \sim 1$ mb.
5. If ψ is fermion-antifermion bound state, the binding is $\gg \alpha$.

Items 1, 2 and 5 we have discussed. I agree with them, but they just say the constituents inside the ψ interact strongly with each other. They need not interact with the ordinary quark strongly. Item 3 has to do with symmetries. Symmetries of the couplings for ψ decay do in general look like strong interaction symmetries. But by the definition of hadron I have adopted it is the magnitude of the coupling constant and not what kind of isotopic matrix that multiplies it which counts. Therefore I don't think item 3 is relevant. That leaves only item 4 on ψ photoproduction. It is the well known statement that the ψ-nucleon cross section is a millibarn if vector dominance is assumed. But we know that is not the whole story: not only is vector dominance assumed, but also the imaginary part of the ψ-nucleon amplitude is assumed to be large compared to the real part. This is very natural if ψ is a hadron. If ψ isn't a hadron it is very unnatural, because then one does lowest order perturbation theory, and the ψ-nucleon amplitude will be real, not imaginary. In such a case the total cross section is roughly the same as the elastic cross section, $\sim$20-50 μb. The cascade decay $\psi' \rightarrow \psi\pi\pi$ provides another way to estimate interactions of the ψ with hadrons. Using crossing symmetry, one can turn that process around and talk about the process $\pi\psi \rightarrow \pi\psi'$, and calculate the low energy cross section. Again it is of order 20-50 μb. One might also cite the strong production of ψ (the details of which we are now beginning to learn from the new Fermilab experiments). However, estimates of W-production (using the Drell-Yan mechanism) are less than a factor $\sim$100 smaller than those observed for the ψ. I conclude that there is no experimental evidence that ψ is a hadron. And it is hard to find out the answer. One way is to look at the A-dependence of ψ photoproduction. That will be tried, but it is only sensitive down to about the millibarn level. If an effect were found we would have the answer. But were it not found, we could not conclude ψ is not a hadron. A better answer is to first find the (generic) D's. If something like the charm picture is right the D's should contain a nonstrange quark and therefore the D-nucleon cross section will be much bigger than the ψ-nucleon cross section. Once it is determined that the D is strongly interacting with hadrons and the ψ is strongly coupled to the D's the question is answered. This, by the way, suggests that study of the A-dependence of ψ' production may be more favorable than ψ production, inasmuch as there may be mixing of the ψ' 2S charmonium states with nearly degenerate $D\bar{D}$ states. If such mixing is present, part of the time ψ' will be a $D\bar{D}$ system with stronger absorption in nuclear matter. [Notes added: 1. I was mistaken; the evidence from photoproduction for ψ being a hadron is much stronger than I implied. Given, via VDM, that ψ photoproduction measures the modulus of the ψN forward elastic amplitude, the phase is constrained by dispersion-relations: it turns out there must be a sizable absorptive part. Given the validity of VDM, ψ photoproduction provides strong evidence that ψ is a hadron. This argument has been worked

out by E. Hendricks, A. Sanda, and V. Rittenberg. I thank Edward Hendricks for informing me of this work prior to publication. 2. Sam Ting has reminded me that the interference of Bethe-Heitler dilepton photoproduction with dilepton production via the ψ measures the phase of the ψ nucleon scattering amplitude. This process therefore also can shed light on these questions.]

Before going on to the question of how to find D's, I will mention those problems of the new spectroscopy which I think provide the largest difficulties. First, the radiative widths seem to be too small, at least by an order of magnitude. Next, there are the remarkably narrow widths of the ψ and ψ' themselves. We keep saying to each other Zweig's rule, Zweig's rule. Over and over again we say Zweig's rule. We draw funny little pictures of hairpins on pieces of paper, and repeat again ZWEIG'S RULE. And after a few weeks of this we understand everything. Also, the mass spectrum of the dipions in the cascade $\psi' \to \psi\pi\pi$ is peculiar. While the Adler zero and the low energy PCAC argument helps, that solution wasn't really required by PCAC. It is consistent with PCAC, but other mundane alternatives also are consistent with PCAC. There is also the peculiarly large $\eta\psi$ decay mode of the ψ'. There is just barely enough energy to make the state. In addition the η is in a p state, and there is a lot of SU(3) violation if ψ and ψ' are really SU(3) singlets. The ratio $\Gamma(\psi \to KK^*)/\Gamma(\psi \to \pi\rho)$, which should have been one, is $\sim 1/3$. Maybe that is not so bad, but according to the Zweig-rule the ratio $\Gamma(\psi \to \phi\pi\pi)/\Gamma(\psi \to \omega\pi\pi)$ should be very small. Experimentally it isn't and in fact is comparable to the $KK^*/\pi\rho$ ratio. Also, the mass of the 2.8 is too low to make Howard Schnitzer happy, as we heard from the floor the other day. So there are many serious questions for which we need answers.

But even more crucial than straightening out the spectroscopy is to find the D's. Of course everybody is looking. I sometimes worry that everyone is looking in the same corner—for $K\pi$, direct leptons, and the other states of orthodox SU(4) charm. I think that someone who had never heard of such things, and only knew the facts about the e^+e^- data, might well conclude that the characteristic feature of the D was that it always decayed into at least one neutral particle (ω, η, π^0, γ, ν, ... ??). We clearly need as diversified a search as possible.

Going beyond e^+e^- reactions, the neutrino events in the 15-foot chamber at Fermilab are a fine place to search for D's, etc. And there are already the ν-induced dimuon events, which may well involve the D's. We shall return to that result in the next section. The existence of dilepton production by neutrinos invites the same kind of search in muon or electron interactions. Also especially attractive is trilepton production by muons or electrons, in particular deep-inelastic production of the ψ. While this is off the subject of D searches the deep-inelastic production of ψ by muons will tell us much about the validity of vector dominance, and the structure of the γ-ψ coupling constant, which in turn tells us whether the ψ is a big loose structure or a tightly bound system like we usually think it is. Those experiments will be done at Fermilab, and eventually at the CERN SPS.

How to search for the D in the strong interactions is a big problem. It is like the searches for CP-violating effects: if one searches and finds nothing it is hard to know what to conclude. Perhaps a good criterion for a sensitive search for 2-body decay modes of new states is that the sensitivity should be sufficient to see the $p\bar{p}$ or $\Lambda\bar{\Lambda}$ decay mode of the ψ. We saw very pretty data on new particle searches—especially the MIT-BNL data presented by Sam Ting—but it still hasn't reached that sensitivity level. But, speaking as one more frustrated theorist, the most important task is for theorists to help experimentalists in figuring out the best way of finding the new states. This is a tough problem, and it means really getting in close and studying backgrounds and production mechanisms so that we can realistically estimate the sensitivity of the experiments before they are done. It doesn't help much for us theorists to explain to

the experimentalists (after the experiments are done, of course) why their experiment wasn't good enough.

Now let us look at the weak currents in the same way. We shall, however, be more speculative here. [As usual, the degree of speculation is inversely proportional to the quantity of data.] The neutrino induced dilepton events, plus the interpretations given by Carlo Rubbia, plus all the ideology of the light cone analysis for deep inelastic processes, together imply that there should exist a new charged weak current in addition to the ordinary Cabibbo current. The new current is distinguished from the old by the existence of a direct lepton in the final hadron system produced by the current acting on the target nucleon. From the observations of the x and y distributions of the μ^- in the very high energy neutrino induced events, there is a strong suggestion (but nowhere near a certainty) that the d quark enters into the new current. The old quark is there because otherwise the x distribution would be moved down to lower values. That needs more verification. But if it is true, we can draw a lot of conclusions. Again, we first must choose whether charged weak current will involve new degrees of freedom rather than a linear combination of old degrees of freedom. Without justification we again choose the former, assume the current is vector and/or axial and write

$$\mathcal{J}_\mu^{\text{had}} = \mathcal{J}_\mu^{\text{Cabibbo}} + \mathcal{J}_\mu^{\text{new}} \; (= \bar{d}'\gamma_\mu(1-\gamma_5)u + \bar{d}\gamma_\mu(V-A\,\gamma_5)t + \ldots \;?)$$

If the d quark does enter into the new current, then the new current may not commute with the old current. From our experience with current algebra, that may wreck universality unless we are careful. A careful choice, popular these days, is to choose A=-V, making the new current a pure right-handed current, in which case it will commute with the old left-handed current. In any case, because the d has fractional charge, then its new companion t will also be charged, in fact fractionally charged. Therefore the t should contribute to the new colliding beam physics. The alternative to that is what Harari calls postponement, i.e., the phenomenon occurring in the neutrino experiments occurs at such a high invariant mass level that the colliding beam people haven't attained it yet. But if there is no postponement and the Fermilab dilepton events are related to the new colliding beam physics, this would furthermore imply that ψ is a hadron. This follows because the t has fractional charge and it is confined. The t, after being virtually produced, doesn't get out into the final state; it has to pick up a quark. Therefore it is coupled strongly to the quarks: it is a hadron.

There may be more currents—very new currents. If there are several new constituents then we could have weak transitions between them. This may be in fact a nice mechanism for explaining the same-sign dimuon events at NAL. I learned this from Haim Harari. The picture is in Fig. 3. First the neutrino hits the d quark, changing it into a t quark with the emission of a μ^-. Then the t quark decays either hadronically or semileptonically to some other heavy quark, say b. Then the b decays to a u quark with the emission of another μ^-. In this way one can get $\mu^-\mu^-$ as well as $\mu^-\mu^+$ pairs. Occasionally there should be trimuon events as well as dimuon events, at probably a level of 10% or so of the dimuons.

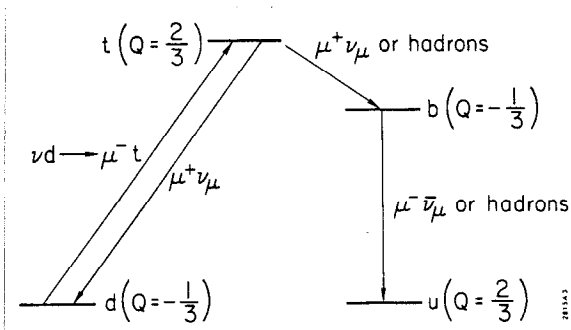

Fig. 3--Possible cascade-decays of new hadron constituents.

V. PROLIFERATION

In the above arguments we ended up introducing several new degrees of freedom. New degrees of freedom are needed just to have a colliding-beam R greater than equal to 5. (Provided we accept that $R = \sum e_i^2$.) With many new degrees of freedom this can give us many new quantum numbers. It has been proposed that these new quantum numbers have the generic name "flavor" to go along with color and all of the other sensual labels that we have. I am slightly disquieted by all this, and think that maybe odor would be a better name than flavor. But in any case proliferation is with us even without the new degrees of freedom, because we already have a lot of them. I count basic degrees of freedom in our theories in the following way: each two component fermion (like a left-handed neutrino or a left-handed electron) is one degree of freedom. A single four component quark of a given color including its antiparticle counts for two degrees of freedom. To count this way is natural because weak interactions are chiral, and even the strong interaction symmetry is an approximate chiral $SU_3 \otimes SU_3$. We shall not count bosons as degrees of freedom, since it is bad enough without them. The table below shows the degrees of freedom in various models.

Fundamental Degrees of Freedom

			Number
	Old Leptons	ν_μ, ν_e	2
		e_L, e_R	2
		μ_L, μ_R	2
	Old Partons	u, d, s	6 (no color)
			18 (3 colors)
	New Leptons	U_L, U_R	2
		ν_u ?	≥ 1 (??)
	New Partons	c	6 (with color)
		t	6 (with color)
		b	6 (with color)
		... ?	
Totals			
12	irreducible minimum	$\mu, e, \nu_\mu, \nu_e, u, d, s$ (no color)	
24	last year	$\mu, e, \nu_\mu, \nu_e, u, d, s$ (color)	
33	"minimum theory"	$\mu, e, \nu_\mu, \nu_e, u, d, s, U, \nu_u, c$	
45	six-quark theory	$\mu, e, \nu_\mu, \nu_e, u, d, s, U, \nu_u, c, t, b$	
48	vector-like 6-quark theory	$\mu, e, \nu_\mu, \nu_e, u, d, s, U, \nu_u, c, t, b, N_1, N_2, N_3$	

With just the old leptons and the 3 quarks without any color we get 12 degrees of freedom. Adding color but nothing else new brings the number up to 24. A new lepton (if more data confirms the indications from SPEAR) and its neutrino is a negligible number of degrees of freedom on this scale. But new partons, especially if color is included, make a substantial increase. So depending on which of the various theories we

choose, we get anywhere from 24 up to 48 degrees of freedom. There are still more elements than elementary constituents, but the margin is going down. Sooner or later, and maybe the time is sooner, we will have to worry about consolidation, and consider how these objects might be comprehended in terms of a much smaller number of degrees of freedom. Eventually it will have to be done, and maybe we have to jump our thinking ahead to the consolidation stage, to solve the problem of how to deal with the proliferation we have at present. I wish I knew how to do it.

VI. THEORY

If we talk again about standards of reference, Roger Dashen just showed us very nicely what our theoretical standards of reference are. The best candidate for a theory of strong interactions is SU(3) color gauge theory, which is supposed to confine the quarks. Then there are the spontaneously broken gauge theories for the weak and electromagnetic interactions, which are supposed to unify them. And there is occasional epic poetry on the possibility of some Grand Synthesis which unites everything.

Looking back 10 or 15 years ago to where I came in there is a tremendous change in our theoretical viewpoints. In the good old days Heisenberg pragmatism was what was popular. Predominant was the emphasis on observables as in the S matrix approach to strong interactions. And those S-matrix ideas have been extremely successful. They have provided us with useful ways of organizing and interpreting a great amount of data in strong interactions. In addition there are the dispersion relations and the rigorous results like the Froissart bound on σ_{tot}. And in weak and electromagnetic interactions the S-matrix approach had its parallel in the development of current algebra, which again is the epitome of Heisenberg pragmatism. What has happened to all that? Even strong interaction and S-matrix people have been going in a direction which had led away from this pragmatism. First came phenomenological duality which led to the Veneziano formula, which led to strings and now even into modern field theoretic lines. Our description of lepton-induced processes evolved from the algebra of charges to local current algebra. When experiments showed the existence of deep-inelastic scaling, scale invariant field theory had a very fruitful development. This in turn led eventually to the asymptotically free gauge theories. The other element in these developments is of course the quark, which is never seen but refuses to go away. So we have arrived with a conceptual structure which looks like a set of very wild ideas from the point of view we had 10 or 15 years ago. In the old days everyone was very happy because we had an S-matrix without field theory. Nowadays everyone is very happy because we have a field theory without an S-matrix.

For weak and electromagnetic processes the standard of reference is the spontaneously broken gauge theories. There is the opinion, as mentioned by my good friend Ben Lee, that existence of neutral currents and charm is experimental evidence in support of the gauge theories. But I think this view is conditioned more by history than by the facts. In order to see whether there is support for the gauge theories we should look at possible alternatives to the gauge theories. Consider four alternatives:
(i) theories with no W's, but where unitarity damps the high energy behavior,
(ii) theories with W's which possess strong self interactions which in turn provide the damping, (iii) intrinsically broken gauge theories which contain massive W's, but where there are no Higgs particles (again strong W-W interactions are needed to provide the cutoff), and (iv) the orthodox gauge theories, with Higgs particles introduced. In that case only the Higgs scalars have the strong self-interactions (in order to generate the spontaneous breakdown). Now in all four cases, there are neutral currents predicted. They are ubiquitous. They are there in almost anybody's theory.

Furthermore, without being clever the strangeness-changing neutral currents will be there in all cases. But if you are clever (as clever as Glashow) and introduce charm, the Glashow, Iliopoulos, Maiani idea rids them in all cases in the same way. So I think neither the neutral currents nor charm are by themselves support for the gauge theories. Support will exist to the extent that there is numerical agreement with accurate experiments. But there are strong points in favor of the unified gauge theories, the major one being that this is the only case for which we can calculate. Therefore it becomes by default a standard of reference. Indeed it has been an extremely useful standard of reference, in particular, in helping experimentalists discover neutral currents.

Renormalizability is often used as an argument for the unified gauge theories. But just the fact that we can calculate something seems to me no reason for making a physical principle out of it. The gauge principle is a much more powerful motivation for such theories; the idea of a gauge theory goes very very deep. After all the gauge principle is there in both gravity and in quantum electrodynamics. But in the presence of proliferation, do we deserve to be able to penetrate to that depth? And there is in such theories a complex world of strongly-interacting Higgs scalars between us and any kind of Ultimate Simplicity. We have a long way to go.

VII. WHERE ARE WE GOING?

The answer is easy. We are going to do more experiments and (our patrons, the general public willing) build new machines. And maybe even the theory will progress a little. The new physics of this year will in time become established physics, and we will be again in pursuit of the highest possible energies to find out, among other things, the structure of weak interactions.

But in looking at the long view, there is the nagging question of whether and how we are really making progress. We penetrate one layer of structure, only to find more complexity and richness of structure beyond. Is the only light at the end of our tunnel that produced by yet another set of radiative transitions in yet another spectroscopy? In such an exploratory field as particle physics, it is usually the case that one need ask such a question only over a pitcher of beer. However, in a costly enterprise such as ours, I don't think we can responsibly duck the question. Are all these new expensive machines and experiments worth it?? We should be able to define broad goals and mileposts along the way to those goals. Such goals and mileposts have existed in the past, and in fact history shows us that expectations have been fulfilled. One GeV accelerator physics was supposed to tell us about the internal structure of individual nucleons and mesons. It appears to have done that very well. 10-30 GeV accelerators were designed to attain a distance-scale small compared to nucleon size and to reach into strong-interaction asymptopia. This also succeeded: the deep-inelastic world was found, QED was verified far below the subnucleon distance-scale, and the Regge power-law approaches to strong interaction asymptopia were discovered, and by now reasonably well comprehended. Of course we also found that asymptopia had to be renamed logarithm-land, and that true asymptopia ($\alpha' \log s \gg 1$) is probably beyond reach of any attainable energy.

The next milepost in energy (most appropriate for IHEP and FNAL) is the one associated with the weak-interaction cutoff ($\sim$4-15 GeV) needed to limit the size of higher-order weak contributions to the K_L-K_S mass difference and the decay $K_L \to 2\mu$. This is nowadays associated with the mass-scale implied by charm and/or the new physics. It is too early to tell whether the K_L-K_S and $K_L \to 2\mu$ problem will be solved

when we comprehend the new physics, but there is at least plenty of optimism on that point.

What are the mileposts for the future? The weak interactions provide them. There is the expectation from the gauge theories that $m_W \sim m_Z \sim 100$ GeV, and beyond that milepost is in any case the Lee-Yang unitarity limit at $\sqrt{s} \lesssim 1000$ GeV. The next generation of pp storage rings would reach directly into these regions. And e^+e^- and ep rings and also multi-TeV conventional proton machines could have sufficient energy to teach us, by more indirect means, about the internal structure of weak interactions as well.

Thus the next mileposts are tangible, attainable, and justifiable. But will our basic problems be solved at that stage? I think we'd be very lucky if they were. No matter what our picture of weak interactions is, it is hard to avoid a new strongly interacting world in the 100-1000 GeV energy region. The four classes of weak-interaction models mentioned earlier exhibit this. Either the fermions themselves, or intermediate bosons, or Higgs scalars undergo strong interactions at a very high energy scale. And we may have to penetrate that complexity before finding some simplicity and the synthesis of the disparate elements that we have at present.

What kind of simplicity are we ultimately after? That is too intangible a question for me. But a tangible goal is to reach a level of understanding where lepton and hadron are really treated on the same footing. Almost everything about leptons and hadrons are the same: they share the same weak and electromagnetic interaction, and even their patterns of chiral symmetry breaking are very similar: $e, \nu \leftrightarrow u, d$ and $\mu \leftrightarrow s$. Perhaps we will even have a heavy lepton bearing the same relationship to charm: $U \leftrightarrow c$. So the only lepton-hadron distinction is in the strong interaction: quarks have it and leptons don't. And maybe the clue here is color: quarks have it and leptons don't. But even if this is a correct direction, we should eventually expect a much closer synthesis between lepton phenomena and quark phenomena: sooner or later bridges between the two worlds will be built. With luck we may see this attained by the time we open up fully the weak-interaction world in its natural energy regime. If this happened, even were it to happen in the midst of proliferating degrees of freedom and the onset of yet another round of strong interaction and its concomitant spectroscopy, attainment of such a goal would be quite enough, well worth the effort getting there.

VIII. SOME CLOSING WORDS

As a theorist, I feel a little awkward in giving a summary of a conference where almost all the news is experimental. I should like to express this sense of humility and gratitude to all accelerator physicists and experimentalists who have put in such tireless efforts to provide us all with such beautiful results.