XVI. HOW MANY QUARKS AND HOW MANY LEPTONS?

Now that we have analyzed various possibilities concerning new quarks and leptons we may review the options which are open to us. We know that above $W\sim4$ GeV we have $R\sim5$ and the new physics corresponds to $\Delta R\sim2.5$. This requires several new fermions. Starting with the well known four leptons (e, ν_e, μ, ν_μ) and three quarks (u, d, s) we now review the possibilities still remaining within the conventional V-A theory:

(A) <u>One new quark (c) and no new leptons</u>

This gives the wrong R and K/π ratio, and does not provide a reasonable explanation of the $\mu^{\pm}e^{\mp}$ events.

(B) <u>Two or three new charged leptons. No new quarks</u>

Does not explain either the narrow ψ, ψ' or the wide ψ'', ψ''' . Solves nothing. Almost certainly wrong.

(C) <u>Three or two new quarks (c, t, b). No new leptons</u>

Does not explain the spectrum of the Ψ-family (unless the quarks are degenerate[30] and more ψ-states are to be found). Gives the wrong K/π ratio and does not provide a reasonable explanation for the $\mu^{\pm}e^{\mp}$ events.

(D) <u>One new quark (c) and two new leptons (U^-, ν_U)</u>

Agrees with all known data. Does not possess quark-lepton symmetry. Anomalies are not cancelled.

We see that, at present, (D) seems to be the only viable scheme from the experimental point of view. Theoretically, however, we prefer to supplement the six leptons:

$$\begin{pmatrix}\nu_e\\ \\ e^-\end{pmatrix}\qquad\begin{pmatrix}\nu_\mu\\ \\ \mu^-\end{pmatrix}\qquad\begin{pmatrix}\nu_U\\ \\ U^-\end{pmatrix}$$

with six quarks. Within a V-A theory, these must be (see section IX):

$$\begin{pmatrix}u\\ d'\end{pmatrix}\qquad\begin{pmatrix}c'\\ s'\end{pmatrix}\qquad\begin{pmatrix}t'\\ b\end{pmatrix}$$

where t, b have electric charges $+\frac{2}{3}$, $-\frac{1}{3}$, respectively. The t and b quarks are presumably produced at energies above $W \sim 7.8$ GeV , or eise we would have already seen the $t\bar{t}$ and/or $b\bar{b}$ vector mesons.

We find no experimental reason to introduce additional V+A currents into the scheme which involves six leptons and six quarks. The two muon events found in neutrino experiments at the Fermi laboratory[13,14] could possibly provide us with such a reason. This might happen if the $\mu^+\mu^-$ events are proved to be too numerous to be produced by -

$$\nu + d \rightarrow \mu^- + c$$

followed by -

$$c \rightarrow s + \mu^+ + \nu_\mu$$

or if the $\mu^-\mu^-$ events[14] are confirmed, and are proved to be too numerous for -

$$\nu + d \rightarrow \mu^- + u + c + \bar{c}$$

followed by -

$$\bar{c} \rightarrow \bar{s} + \mu^- + \bar{\nu}_\mu$$

At the moment we do not believe that the two-muon events necessitate a modified weak current[36], but more data are clearly needed.

The possibility of (V+A) currents is, however, theoretically interesting. We now turn to it.

XVII. DO WE HAVE A (V+A) CURRENT?

Empirically we know that the charged weak currents involving the u, d, s quarks and the four "old" leptons are of the V-A type. Theoretically, there is no reason to exclude the possibility of a V+A current for quarks and leptons. In fact such currents have been proposed with various motivations[37-44], some of which related to the new particles. The right-handed quarks which couple to the (V+A) current cannot follow the same "weak-isospin" classification as the left-handed quarks. In fact, if all right handed quarks belong to weakisodoublets, we <u>must</u> have six quarks, and there is only one weak-isospin assignment. Since u cannot have a substantial (V+A) transition into d or s , it must be in the same weak isodoublet with the right handed b-quark. That leaves only one question open: is the left handed c-quark associated with d or s ? The K_S-K_L mass difference[41] as well as the PCAC analysis of $K\to 2\pi$ and $K\to 3\pi$ decay[45] prevents us from having[39,42] a significant V+A $c\leftrightarrow d$ transition. Hence, the right handed quarks must be classified as:[40,41,44]

$$\begin{pmatrix} u \\ b \end{pmatrix}_R \qquad \begin{pmatrix} t \\ d \end{pmatrix}_R \qquad \begin{pmatrix} c \\ s \end{pmatrix}_R$$

as compared with:

$$\begin{pmatrix} u \\ d' \end{pmatrix}_L \qquad \begin{pmatrix} c \\ s' \end{pmatrix}_L \qquad \begin{pmatrix} t \\ b \end{pmatrix}_L \quad .$$

The introduction of similar V+A currents into the leptonic world involves several new neutral leptons[43,44] and theoretical complications related to nonconservation of lepton-number.

The theories with V+A and V-A currents can be rewritten, in the limit of zero fermion masses, as a pure vector theory[42,43,44]. This is a very attractive idea, since it implies that the weak interactions are fundamentally pure parity conserving vector interactions like electromagnetism and, possibly, the strong interactions. According to this philosophy the mechanism which generates the fermion masses and the weak mixing angles, also generates parity violation and axial vector currents. This entire approach, however, is not free of theoretical difficulties, and we will have to watch its development in the next few months.

At this point we must repeat that, with the possible exception of the two-muon events at the Fermi laboratory, we see no experimental or phenomenological reason to introduce V+A currents.

XVIII. WHAT NEXT?

As of now, August 22, 1975, it seems that below $W \sim 8$ GeV , six leptons and four quarks are sufficient for accounting for all the experimental facts. The theory suggests that two additional quarks may be around the corner, and should be found in the next generation of e^+e^- machines.

However, many experimental surprises may appear in the next few months. It is especially crucial to:

(i) Verify that the U particle of the $e^{\pm}\mu^{\mp}$ events[7,10] is indeed a lepton.

(ii) Discover the charmed mesons.

(iii) Study the ψ-spectrum with special emphasis on the C = +1 levels and the missing decays of ψ' .

On the theoretical front, the "burning" issue is, of course, the number of quarks and leptons and the nature of the weak currents.

The total number of "fundamental" fermions is now at least 18 (four tricolored quarks and six leptons) or perhaps 24 (six quarks and six leptons). This is comparable to the number of known hadrons when SU(3) symmetry was introduced. We cannot avoid the feeling that the connection between quarks and leptons[46] as well as attempts to explain the proliferation of "fundamental" fermions will occupy an important place on the theoretical agenda for the next few years.

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DISCUSSION

J. Rosner, Minnesota: As this looks like the last chance for this, can you or someone else show a slide of the fit to the angular correlations of e and mu in the e-mu events based on the heavy lepton hypothesis?

H. Harari: I don't have the slide. Perhaps Martin Perl has it.

M. Perl, SLAC: We don't have the slide with us. I will certainly bring one in on Monday. I would like, however, to make one comment. In my optimistic and cheerful moments I certainly agree with many of Haim's thoughts about the U particle. But sometimes in the middle of the night, or very early in the morning, I wake up with the following worry about all this. When we make these fits and seem to see the heavy lepton as being a reasonable fit, we are assuming that all the e-mu events left after background subtraction are produced by a single mechanism, namely a production of particles of one type only. If the U particles are of several different types (heavy leptons, various charmed mesons, etc.), all being produced together, and the background is mixed into it in a strange way, then we have no real understanding of the events, and many hypotheses can fit again. I want to emphasize that the heavy lepton is the simplest hypothesis at the moment, but it is not the only one. The job of verifying that the U is indeed a heavy lepton requires getting rid of all other possibilities, including very complicated ones such as three or four things happening at once. We fully realize the magnitude of making such a claim.

Harari: I think Fred Gilman would agree with me that both our talks were worthwhile if they served to impress you with the crucial importance of fully understanding these e-mu events. Whether or not the simple charm scheme makes sense depends strongly upon the final understanding of these events.

H. Schnitzler, Brandeis University: I would like to point out that if the state found at 2800 MeV is the expected $\eta_c\ 0^-$ of charmonium spectroscopy, this would pose grave difficulties for those who view it as a non-relativistic system, since the $\psi(3.1) - \eta_c(2.8)$ splitting is half the $\psi(3.7) - \psi(3.1)$ splitting. It is hard to believe that this is a $(v/c)^2$ correction in non-relativistic models.

Harari: I didn't have time to discuss that point, but I would add that the problems of the splittings of the states, namely the splittings between the ^3P states (if they are indeed the states observed between the ψ and ψ') and the splittings between each vector and its friendly pseudoscalar, are important to the determination of the parameters of the potential, if indeed you can talk about such a potential. However, that type of problem is really a second-order problem in these charmonium models. You can see that these various splittings give one handle on whether the binding is relativistic or not, what kind of spin-orbit couplings there are, etc.

M. Roos, Helsinki: In the picture presented you put the 2^3S_1 state below the $^3D_1$ state? Are you forced to do that? Or could you identify the $\psi(3.7)$ with the $^3D_1$ state?

Harari: Well, according to these models the D states would have a vanishing wave function at the origin, and the wave function at the origin is the only thing that determines the decay rates of these particles into lepton pairs. Now we know that the $\psi(3.7)$ decay rate into lepton pairs is approximately half of the $\psi(3.1)$ decay rate into lepton pairs. Therefore, the wave function at the origin is presumably of the same order of magnitude in the two cases and is unlikely to be something which is vanishingly small. This is the motivation of the charmonium people for identifying it with the S state. As I said, there is a possibility of a small mixture. Now what is important is that if there is a mixture which is substantial, then this fellow $^3D_1$ should also be seen as a spike at SPEAR (whether narrow or wide is not clear, and the area under it depends on the mixing). That, presumably, has not been seen yet, but maybe it is so small that it will be seen. This is the party line on that issue.

H. Kluberg-Stern, Saclay: In your favored model a new heavy lepton and the charmed quark are solely relevant to the SPEAR data. You mentioned that all experimental SPEAR data are consistent with this model. From your point of view, to what channels do the charmed particles decay, since two- and three-body hadronic channels are still small? Do you expect any hadronic or semileptonic decay to be dominant?

Harari: Obviously, neither I nor anybody else knows what is the best game you can play. However, let me suggest one. The game is to look at $\bar{p}p$ annihilation at rest, which is a system of mass 1.9 GeV (which is close to the mass of the alleged charmed meson) decaying into mesons, and to copy it, except that every time you see a couple of pions there you replace it by a K meson here, because there are many pions there but here you need a K meson. Let's take this as a guess for lack of anything else. If that is the case, then the most favored thing in the final state would be typically something like one kaon and three pions; there will be a fair number of events with one kaon and four pions and of one kaon and two pions, although the latter will be somewhat smaller. There will be very few events with one kaon and one pion, and that is more or less the case. The semileptonic branching ratio is still as unpredictable as ever. It is probably 5% or 10% or whatever, and I think that is the situation. Now, if you are asking how, experimentally, we are finally going to find them, there are obviously two possibilities. One has simply to get one or two or three more orders of magnitude of data on the $K\pi$ final states because that decay must exist at some level. What I mean is that even ψ decays sometimes into $\bar{p}p$, for example, although it occurs only once in 500 decays. Maybe the decay $D \to K\pi$ also occurs only at the 1/5% level. The other possibility is to look at things like K and 3π's in the final state which is supposed to be an abundant decay mode, but this is very difficult to do experimentally. I don't see any alternative.

Kluberg-Stern: When you say abundant, do you mean 10% or more?

Harari: I would guess that $K3\pi$ is probably something of the order of 30% maybe, but this is just a wild guess. Don't forget that there are probably half a dozen different charged decay modes, so we are making life very difficult for the experimentalists (but then, they are reciprocating).

G. Karl, Guelph University: I would like to ask you about a point that I didn't understand. Why do you feel that the discovery of natural parity states between the ψ and ψ' forces you to assign ψ' to a radial excitation of ψ rather than having a model with several quarks in which ψ and ψ' are composed of different quarks? Or have I got you wrong?

Harari: No, no, you have got me right, except for the word "forces". I don't think it really forces anything but, in my opinion, there is a very strong indication in that direction because it is the simplest explanation. If that would have been the only thing that happened in the last month or so, I would have said that we have a big puzzle. In fact, that is what I did say a month ago, namely that the ψ spectrum does look like radial excitations and one new quark; but the value of R, which is a very important clue, certainly doesn't look like one new quark. If, however, the following things are happening and are descending upon you at the same time--(1) the e-μ events begin to look more and more like a heavy lepton, (2) the ψ spectrum begins to look more and more like that of a radial excitation, and (3) the two things are really performing the perfect crime by concealing everything in such a clever way--then I find this to be the simplest possibility at the moment. I am not forced, but I find it attractive.

C. A. Heusch, UC-Santa Cruz: I have a comment concerning your remark that one of the principal tasks of the experimentalists will be to dig up information that would make us understand the Zweig rule better. I feel that the first task in front of us is to prove or disprove that there is such a thing as the Zweig Rule. If, say, we don't find more strange particles in fully reconstructed $pp \to \phi + \ldots$ events, then the Zweig Rule as we invoke it to explain the narrow width of the ψ's is dead.

Harari: We are not sure whether the Zweig Rule is right. So far it has been right wherever it has been tested, but wherever means about four or five places so we are not sure. We have to test whether it is right or wrong in as many experiments as possible, and I made a partial list in my talk. Every such piece of quantitative evidence will give us parametrizations, and we will start to understand the importance of whether there are two or three gluons, of whether it is a light or heavy quark, etc., etc. In this way we will get more and more clues. Then the people who write the underlying theory behind all of this may have some kinds of things to contradict the theory or to prove it. It is that kind of thing which I am looking forward to. Any experimental piece of work that you can do that will kill or improve or explain or parametrize the Zweig Rule is very relevant, I think. If you can kill it you will give work to the speaker at the next conference.

J. Kirkby, SLAC: In my opinion, the easiest experimental way to pin down charm is the K/π ratio. I don't understand why you say that this extra lepton makes everything OK now. The present data shows a rise in this ratio of 10% ± 10% as the center-of-mass energy is swept through 4 GeV, whereas in the absence of the lepton one would expect it to rise by roughly 50%. Would you go through the arithmetic to show how accurately the experimentalists need to measure this quantity before charm is in serious trouble?

Harari: I cannot give you a quantitative estimate of that, but I do want to give you two or three numbers. Let us take the most stupid possibility as an example. Suppose that 5% of the heavy lepton decays have K-mesons in the events, and 60 - 70% of the charm particle decays have K-mesons, which is a typical estimate. Now what you have to do is to average these two numbers, with somewhat more weight for the charm number because the lepton gives you one unit of R, while charm gives you one and one-third or perhaps somewhat more. So you have as an average, say, 35%, perhaps an additional 5%, and that is it, roughly. Now in the "old" physics, approximately 40% of the events have some kind of K-meson in them. So what you find is that this crazy mixture of--half beef and half chicken, or whatever you want to call it--this mixture of heavy lepton and charm is really imitating the 35 - 40% very cleverly. Now you are asking me to put better numbers on it. I haven't done it as carefully as I would like to do, and as I will do, but I can guarantee that for anything I do, somebody else will get 10% more or 10% less. It is very hard to get better accuracy than that because you are averaging two uncertain numbers like this and asking only for variations. I think that if the heavy lepton is confirmed, the global K/π ratio will teach us very little. I say if the heavy lepton is confirmed. That is very crucial.

H. Lipkin, Weizmann Institute: Continuing along this same line, another possible signature for charmed pair production would be an apparent strangeness violation if one charmed particle decays with a kaon in the final state and the other without. If you had just a few events, with one kaon and three or five pions, and if you knew that all the particles had been seen, that would be enough to tie it down. What are the experimental possibilities for detecting this?

Harari: This question is simple enough so that even I can answer about the experimental situation. There are very, very few events which are fully reconstructed, namely that satisfy four-constraint or one-constraint fits, so that you can be absolutely sure that no K escapes. There are even fewer events which are fully reconstructed and have K/π identification on all particles. In fact, I have never heard of even one such event above 4 GeV although there might be one among the 50,000 or so events observed above that energy. So, at present, it is hopeless. I don't know what the chance for seeing this might be elsewhere, or with the new magnetic detector at SPEAR, which will have a much better solid angle and somewhat better K/π identification. I hope we don't have to wait for that, since it is two years away.

E. Derman, University of Pennsylvania: About a month ago, at the SLAC Summer Institute, you attributed the dimuon events of the same charge, seen in neutrino scattering, to the t and b quarks, which you now "postpone." How can you now account for these events with only c quarks in a V - A framework?

Harari: I think that I said quite clearly that my opinions have changed in the last month. I think that is perfectly legitimate; this happens--to some of us at least. More seriously, I did not discuss dimuons since they will be discussed by Professor Wolfenstein next week. Incidentally, the V and A issue will be discussed by Ben Lee in his talk. I just didn't have enough time to get into all of this.

I would say that the opposite-charge dimuon events would be likely to come from the production of charmed particles. I don't know how the like-charge dimuon events are explained in this way. The last I heard there were only 4 such events, but perhaps next week we will have more. If they are confirmed, then this would be a possible motivation for introducing V + A currents. Remember that what I said was that I do not see a definite motivation for introducing V + A currents. Some of the people who introduced V + A currents originally, for example, Glashow and company, did so in that context and liked it as a possible explanation. It is a fairly involved question, and I hope that Wolfenstein will discuss it.

P. Condon, UC-Irvine: Have the experimentalists looked at the K/π ratio in events that have been selected to have a muon or electron present?

Harari: That is a very good question, but I don't know the answer.

Perl: The answer is no.

J. J. Sakurai, UCLA: I would like to point out that the decay of a heavy lepton provides a fantastic opportunity for cleaning up the "old" spectroscopy of G-odd pseudoscalar and axial vector states. It seems a little strange to me that we know so much about the ^3P bound states of c and $\bar{c}$ quarks while we don't even know whether the ordinary axial vector meson nonet is complete. For example, the question of whether or not the A_1 exists can be conclusively settled by looking for $U \rightarrow A_1 + \nu$. There are certain chiral symmetry estimates which were made long before heavy leptons became fashionable. By studying the decay products of the heavy leptons we can get information that could otherwise be obtained only by constructing an electron-antineutrino colliding-beam apparatus with an astronomical luminosity.

Harari: The whole situation is ridiculous. We have four excitations of the ψ but we haven't yet found the φ', and, as you say, there is the problem of the A_1. I mean that there are many respects in which the ψ spectroscopy is already better understood than the old spectroscopy, although far from being understood in all respects, of course. However, this is partly because they are narrow states and partly because of the unusual experimental circumstances.

G. A. Snow, University of Maryland: What would you speculate about the mass of the neutral brother of the heavy lepton?

Harari: That is a very good question. I have no speculations. As far as I am concerned, it could either be a massless neutrino, or it could even have a small mass. There is no evidence against some mass. Presumably, most of the analysis of the SPEAR people was done assuming a massless neutrino, but I am sure that, with the present data, much of the analysis wouldn't be affected if it had a mass of a few hundred MeV. I would find it very hard to believe that this neutrino has a very large mass because that would presumably affect all the momentum distributions, etc., but it is clearly an open question theoretically and experimentally.