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FROM THE FOURTH LEPTON TO THE FIFTH QUARK

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Invited lecture on "Early History Retrospective", delivered at the mini-symposium on the occasion of the 10^{th} *anniversary of the Upsilon (b-quark), Fermilab, July 10, 1987*

OVERTURE

July 10, 1987

Many of you may wonder why I was invited to give the "Early History Retrospective" in this mini-symposium. The answer is simple: I am an expert on this subject. In fact, by the following definition[1], I am a *great* expert:

> *"An expert is that person who has made the largest number of mistakes in a given field".*

This is not a history talk. First of all, I am not a historian. Second, the events described here are too close to us to be part of a historical analysis. Finally, although my own personal role in what happened was very minor, I can say that "I was there when it happened". That means that I cannot possibly give an objective report. My story will necessarily be influenced by what I saw and heard, by the evolution of my own thinking and understanding and by developments in laboratories in which I happened to have visited at the relevant time. This is a Story, not a History.

My inevitable lack of objectivity will undoubtedly lead to omissions, inaccuracies and perhaps even errors. I apologize to those whose contributions I will fail to mention as well as to some of those who will be mentioned. I will be glad to be corrected by the participants of this mini-symposium or by anyone who may read this account.

We are celebrating today the tenth anniversary of the discovery of the Upsilon particle and, indirectly, of the bottom quark. Ten years ago, on July 1, 1977, the Physical Review Letters received the paper[2] of:

> *S. W. Herb, D. C. Hom, L. M. Lederman, J. C. Sens, H. D. Snyder, J. K. Yoh, J. A. Appel, B. C. Brown, C. N. Brown, W. R. Innes, K. Ueno, T. Yamanouchi, A. S. Ito, H. Jostlein, D. M. Kaplan and R. D. Kephart*

For Lederman and his colleagues this was the culmination of several years of heroic experiments at Brookhaven, CERN and Fermilab, with ups and downs, joy and tears. In the process, they missed the ψ particle, discovered the so-called Drell-Yan process, discovered "prompt leptons", and found a fake Upsilon which was quickly disclaimed. The title of the new paper was:

Observation of a Dimuon Resonance at 9.5 GeV in 400-GeV Proton-Nucleus Collisions.

The fifth quark was discovered.

However, there is another very important anniversary which everyone has forgotten. Twenty five years ago, on June 25, 1962, the Physical Review Letters received another historical paper[3] by Leon Lederman and his friends. The authors were:

G. Danby, J-M. Gaillard, K. Goulianos, L. M. Lederman, N. Mistry, M. Schwartz and J. Steinberger

Their paper signaled the birth of accelerator neutrino physics and proved that there were two different species of neutrinos. It was entitled:

Observation of High-Energy Neutrino Reactions and the Existence of Two Kinds of Neutrinos.

This was the discovery of the fourth lepton.

The story that I will tell you today starts on June 25, 1962 with the discovery of the fourth lepton and ends on July 1, 1977 with the discovery of the fifth quark. As I will try to show, the fourth lepton has led, via an almost incredible chain of events, to the fifth quark. The two discoveries that were made by Leon Lederman and coworkers have, in some unexplained fashion, led to each other, with many crucial theoretical and experimental discoveries, mistakes, false claims, wrong turns and near misses in between.

I will touch only on those events which had a more or less direct impact on the road to the fifth quark. Many other exciting events took place in those fifteen years (1962-1977). I will not mention them here.

Like any good drama, this one has three acts:

Act I:
From the Fourth Lepton (ν_μ) to the Fourth Quark (c).
June 1962-November 1974

Act II:
From the Fourth quark (c) to the Fifth Lepton (τ).
November 1974-August 1975

Act III:
From the Fifth lepton (τ) to the Fifth quark (b).
August 1975-July 1977

ACT I
FROM THE FOURTH LEPTON TO THE FOURTH QUARK
June 1962 – November 1974

Scene I: The Dawn of the New Physics
or
"Throw Deep"[4]
June 1962 – March 1970

Act I, Scene I, starts on June 25, 1962 with the discovery[3] of the second neutrino (and the fourth lepton) ν_μ. Until that time we had only three leptons (e, μ, and "the neutrino", now known as ν_e). We also had what we now call three "flavors" of hadrons. All hadrons could be mathematically constructed from the three baryons p, n, Λ and there was a lot of talk about some kind of a "Baryon-Lepton" symmetry[5]. With the discovery of the fourth lepton, that symmetry (or analogy) broke down. Using today's terminology, we now had three hadronic flavors and four leptons.

In 1963 and 1964 several important events in the history of particle physics took place. Gell-Mann[6] and Zweig[7] introduced independently the quark model; the Ω^- particle was discovered[8] at Brookhaven, confirming what we now call "flavor SU(3)"[9]; Cabibbo introduced his angle[10]; CP-violation was discovered by Christenson, Cronin, Fitch and Turlay[11] and the idea of a fourth hadronic flavor ("Charm") was introduced by Bjorken and Glashow [12] and by several other groups of authors[13].

There were two motivations for suggesting charm at that time. The first was "why not?". There was no clear reason for strangeness, and there was no explanation for nature having chosen SU(3) as the flavor group. If SU(3) did well, why not SU(4)? If there was an inexplicable strangeness quantum number, why not charm?

The second argument for charm was the mythical "baryon-lepton symmetry" which was destroyed by the discovery of the second neutrino and which could now be restored if we had four hadronic flavors. Since there was no real compelling reason to have any connection or symmetry or analogy between hadrons and leptons, this was a purely aesthetic argument. It carried no great theoretical weight and was merely considered as an interesting speculation. With the evolution of the quark model, it gradually switched from a "baryon-lepton symmetry" to a "quark-lepton symmetry".

Meanwhile, throughout the 1960's, what we now call the standard electroweak model was being constructed step by step through the work of Glashow[14], Higgs and others[15], Weinberg[16] and Salam[17]. Very few people paid any attention to this work. Even the authors themselves did not appear to take it too seriously, as can be seen, for instance, by reviewing the rapporteur talks[18] of Weinberg and Treiman at the 1968 ("Rochester") conference in Vienna. The historic 1967 paper[16] is not even mentioned there!

Towards the end of the decade, two important series of experiments led to an almost unanimous acceptance of the quark model as the "real thing": Detailed experimental work in hadron spectroscopy revealed an elaborate spectrum of baryons and mesons in complete agreement with the expectations of the quark model[19]; the SLAC-MIT deep inelastic experiment[20], together with Bjorken's interpretation[21], demonstrated that the proton contained pointlike constituents.

Incidentally, the three known quarks were consistently referred to as p, n, λ. Gell-Mann was almost the only person in the world using his original notation[6] of u, d, s. The p, n, λ quarks had the same spin, isospin and strangeness (but not the same electric charge, baryon number and hypercharge) as the p, n, Λ baryons. That awful notation produced endless confusion but somehow no one seemed to mind (except Gell-Mann...).

As 1970 approached, the quark model had just become the standard theoretical framework; The weak interactions were still at the level of a phenomenological model with no renormalized theory in sight; CP violation remained a mystery; Most of the experimental and theoretical effort was directed towards the understanding of hadronic interactions.

Scene II: Renormalizable, at Last!
March 1970 – Summer 1972

On March 5, 1970 the Physical Review received a brilliant paper by Sheldon Glashow, John Iliopoulos and Luciano Maiani[22]. This was the now famous GIM paper in which the authors discovered a third reason (and the first *convincing* reason) for introducing charm. They were concerned with the difficult unsolved problem of a renormalizable theory of the weak interactions. At that time it was not known whether or not there were strangeness-*conserving* neutral currents. There was no evidence for them but also no evidence against them. However, it was absolutely clear that strangeness-*changing* neutral currents were absent. GIM tried to understand this and to explain why strangeness-changing neutral interactions must be strongly suppressed even at higher orders of the theory (which did not really exist as a consistent renormalizable theory). The experimental evidence for the smallness of any strangeness-changing neutral interaction came from several processes involving K mesons but primarily from the $K_S^0 - K_L^0$ mass difference which was measured and known to be very small even by the standards of a second order weak interaction.

In their classic paper, GIM showed that by introducing a four-quark scheme (p, n, λ and p', in their notation) they could avoid strangeness-changing neutral currents and suppress the higher-order contribution of two oppositely charged currents to an overall strangeness-changing neutral interaction (the now familiar "box" diagram). Their detailed argument is now standard textbook material and will not be repeated here. It was the first serious theoretical argument for the existence of the charmed quark, going far beyond the "why not?" and the "baryon-lepton analogy" arguments of the 1960's.

The Cabibbo angle was now incorporated into a 2×2 mixing matrix of the quark states. The production and decay mechanisms of the predicted charmed quarks were more or less understood.

Few people believed in charm even after the GIM paper. It was often argued that charmed mesons should be fairly light and were not seen experimentally. In 1970 there were many topics in particle physics (all of them long forgotten) which attracted much more attention than the proposal of charm.

Then came 1971 in which, in the words of Sidney Coleman[23]:

"The frog of Glashow, Weinberg and Salam was turned by 't Hooft into a beautiful prince."

Nonabelian gauge theories for the electroweak interactions were shown to be renormalizable[24]. The now standard $SU(2) \times U(1)$ scheme became a leading contender, but not the only contender, for the correct gauge theory. Field theory returned to fashion after years of underground existence under the regime of the S-matrix priests.

On February 11, 1972, Physics Letters B received a paper by Bouchiat, Iliopoulos and Meyer[25] who discovered yet another powerful theoretical argument for the existence of a charmed quark. Less than three weeks later, on March 1, the Physical Review received a paper by Gross and Jackiw[26], deriving a similar result. The renormalizable $SU(2) \times U(1)$ theory was not really renormalizable. It had one flaw in the form of the divergent triangle anomaly diagram first studied by Adler, Bell and Jackiw[27]. The triangle was formed by two vector currents and one axial vector current with quarks or leptons running around its internal loop. The divergent part of the diagram was proportional to the sum of the electric charges of the quarks and leptons residing in the left-handed doublets. In a world with three quarks (u, d, s) and four leptons (ν_e, e, ν_μ, μ) that sum was -2 (regardless of the number of colors). Such a model therefore contained incurable diseases.

However, Bouchiat, Iliopoulos and Meyer[25] and also Gross and Jackiw[26] noticed that if one added the charmed quark, *and if one counted three colors for each quark*, the sum of all quark and lepton charges vanished! This would save the day, justify the introduction of the charmed quark and provide for the first time a real theoretical justification for the obscure legendary quark-lepton symmetry. In such a model, quarks could not exist without leptons and leptons could not exist without quarks. The quark contributions to the triangle anomaly would be deadly, if they were not exactly cancelled by the lepton contributions and vice versa.

This new argument for charm was independent of the GIM argument. It could not have been proposed prior to the 't Hooft breakthrough. It also could not have been suggested before the concept of three colors became a standard dogma in quark physics. All of these elements were available for the first time in early 1972 and the two groups of authors seized on them with remarkable speed.

By now charm was motivated by the two traditional aesthetic reasons and by the two new independent solid theoretical arguments: suppression of strangeness changing neutral interactions and cancellation of triangle anomalies.

As the summer of 1972 arrived, neutral currents had not yet been discovered and $SU(2) \times U(1)$ had not been established as the standard electroweak model. Gauge theories were riding high.

Intermezzo: Meanwhile, Back in Kyoto...
September 1972

The main international conference (the "Rochester Conference") of the summer of 1972 was held here in the high-rise building of Fermilab (now known as Wilson Hall). Many of us were here in this auditorium, sitting on folding chairs because the seats were not yet ready. The central attractions were the first results from Fermilab and the exciting new developments in gauge theories. Unknown to us, two Japanese physicists by the names of Makoto Kobayashi and Toshihide Maskawa were writing at the same time a remarkable paper[28]. It was received by the Progress of Theoretical Physics on September 1, 1972, and was "discovered" by physicists outside of Kyoto only three years later, in the fall of 1975.

The title of the paper was:

CP-Violation in the Renormalizable Theory of Weak Interactions

Its abstract read:

"In a framework of the renormalizable theory of weak interaction, problems of CP-violation are studied. It is concluded that no realistic models

of CP-violation exist in the quartet scheme without introducing any other new fields. Some possible models of CP-violation are also discussed."

Although the KM paper is now one of the most often quoted papers, most people who quote it have still not read it. It contains some results that are quite remarkable (especially when one remembers that they were derived in the summer of 1972). It does *not* contain other results that many people have associated with KM.

The main important result of the paper was the proof that, in an $SU(2)\times U(1)$ gauge theory with four quarks, there was no room for CP-violation, regardless of the assignments of the right-handed quarks to $SU(2)$ multiplets. The role of the possible complex phase of the Cabibbo angle and the phases which might be absorbed into the definitions of the quark states was fully and correctly analysed within the gauge theory framework.

When we remember that at that time neutral currents have not yet been discovered, Charm was still a prediction believed by few, and renormalizable electroweak gauge theories were approximately one year old, we realize that the KM analysis was a significant achievement.

However, the possibility of a six-quark scheme was mentioned by KM only as the last of several speculative cures for the absence of CP-violations. It is mentioned only twice in the entire six-page paper: On the third page, after completing the proof that no CP-violation is possible in the four-quark case, Kobayashi and Maskawa stated:

"It should be noted, however, that this argument does not hold when we introduce one more fermion doublet with the same charge assignment. This is because all phases of elements of a 3×3 unitary matrix cannot be absorbed into the phase convention of six fields. This possibility of CP-violation will be discussed later on."

The authors then proceeded to discuss several possible additions to the four-quark scheme. In retrospect, these proposals do not make much sense. Finally, on the sixth and last page of the paper, almost as an afterthought, we find fifteen lines and one mathematical expression devoted to the possibility of six quarks.

The expression was the now familiar 3×3 matrix of the three mixing angles and the KM-phase. Here is the full text of those fifteen lines:

"Next we consider a 6-plet model, another interesting model of CP-violation. Suppose that 6-plet with charges $(Q, Q, Q, Q-1, Q-1, Q-1)$ is decomposed into $SU_{weak}(2)$ multiplets as 2+2+2 and 1+1+1+1+1+1 for left and right components, respectively. Just as the case of (A, C), we have a similar expression for the charged weak current with a 3×3 instead of 2×2 unitary matrix in Eq. (5). As was pointed out, in this case we cannot absorb all phases of matrix elements into the phase convention and can take, for example, the following expression:

$$\begin{pmatrix} \cos\theta_1 & -\sin\theta_1\cos\theta_3 & -\sin\theta_1\sin\theta_3 \\ \sin\theta_1\cos\theta_2 & \cos\theta_1\cos\theta_2\cos\theta_3 - \sin\theta_2\sin\theta_3 e^{i\delta} & \cos\theta_1\cos\theta_2\sin\theta_3 + \sin\theta_2\cos\theta_3 e^{i\delta} \\ \sin\theta_1\sin\theta_2 & \cos\theta_1\sin\theta_2\cos\theta_3 + \cos\theta_2\sin\theta_3 e^{i\delta} & \cos\theta_1\sin\theta_2\sin\theta_3 - \cos\theta_2\sin\theta_3 e^{i\delta} \end{pmatrix}$$

Then, we have CP-violating effects through the interference among these different current components. An interesting feature of this model is that the CP-violating effects of lowest order appear only in $\Delta S \neq 0$ non-leptonic processes and in the semi-leptonic decay of neutral strange mesons (we are not concerned with higher states with the new quantum number) and not in the other semi-leptonic, $\Delta S = 0$ non-leptonic and pure-leptonic processes."

The title of the classic KM paper, its abstract and most of the six-page text did not mention the possibility of a six-quark scheme. The above two excerpts formed the entire part of the paper that had any relation to six quarks. They contained a rather trivial generalization of the Cabibbo angle but the first analysis of the important issue of complex phases and the first correct assignment of the extra physical KM phase to the generalized Cabibbo-GIM mixing matrix. They also contained (among other possibilities) the first mention of six quarks, well before the discovery of the fourth quark! However, KM did not really "push" the six-quark idea, they never mentioned six leptons, they did not mention any issue other than CP violation and they did not even discuss the $K^0 - \bar{K}^0$ system. Their paper had no influence whatsoever on anything that happened until the fall of 1975 when it appeared on the horizon and captured everybody's attention. More about that later.

Scene III: Gentlemen, Place Your Bets!
1973 - November 1974

The main event of 1973 was the discovery, in the summer, of the neutral currents. The first announcement came on July 19 in an exciting seminar given by the late Paul Musset at CERN, where the Gargamelle collaboration had observed[29] neutral currents in both νN and νe collisions. The Harvard-Pennsylvania-Wisconsin collaboration at Fermilab, after several ups and downs, observed[30] similar events in their new huge detector. This collaboration was beginning to discover that big-time neutrino physics at the newly acquired high energies which only Fermilab had, was a hard way to make a living and that it was going to take a good number of years before this new art could be fully mastered.

The neutral currents were found to be of the same general order of magnitude as the charged currents, as predicted by the $SU(2) \times U(1)$ model. Several other candidate gauge theories were eliminated. The model now known as the standard electroweak model became the clear front-runner. The absence of strangeness-*changing* neutral currents became a crucial problem in view of the "normal" strength of the strangeness-*conserving* neutral currents. Charm was the only reasonable scheme that could cure this problem. It was now promoted from a wild speculation believed by very few into the least embarrassing solution of an extremely embarrassing problem. Most people still did not believe in charm.

On December 3, 1973, a mock round-table discussion was "performed" at Harvard. The four "actors" were Alvaro De Rujula, Howard Georgi, Shelly Glashow and Helen Quinn. They played the roles of an experimentalist, a talking computer, a model-builder and a conservative theorist, respectively. The subject of the discussion, later published[31] in the April 1974 issue of the Reviews of Modern

Physics, was "Fact and Fancy in Neutrino Physics". It was clearly motivated by the discovery of the neutral currents.

Among many other phenomenological remarks, the four "actors" noted that, while the left-handed up and down quarks must be in an $SU(2)$ doublet, their right-handed counterparts could be either $SU(2)$ singlets or they could belong to doublets, *provided that each one of them had a new heavy quark as a companion in the same doublet.* Such new quarks were referred to as "Fancy". They would provide two important experimental signatures. Neutrino experiments above a certain energy would show right-handed charged currents and other new phenomena associated with the new "Fancy" quarks. Neutral current experiments could measure the I_3 values of u_R and d_R. If they were $SU(2)$ singlets, they would obviously have $I_3 = 0$. If they were paired into doublets with "Fancy" quarks, they would have $I_3 = \pm\frac{1}{2}$.

The "Fancy Stuff" of De Rujula *et al* (now known to be wrong) began almost four years of numerous attempts by many authors to arrange the "old" u, d, and s quarks into right-handed doublets with new quarks. It was the second motivation (after KM, whose CP work was not known to the Harvard quartet) for the introduction of quarks beyond charm.

The "Fancy" right-handed currents and their many offspring were *not* related to what we now call "a right-handed W" or to the so-called left-right symmetric $SU(2)_L \times SU(2)_R \times U(1)$ gauge theory. They were right-handed currents associated with the normal W-boson of $SU(2) \times U(1)$, differing from the standard model by the weak isospin of the right-handed quarks.

By the spring of 1974, at least the GIM trio was becoming very confident about the existence of charm. At the Experimental Meson Spectroscopy conference in Boston, Shelly Glashow offered his famous "hat challenge". Addressing an audience of hadron spectroscopists, he concluded his talk with the following pronouncement[32]:

"What to Expect at EMS-76

There are just three possibilities:

1. Charm is not found, and I eat my hat.

2. Charm is found by hadron spectroscopers, and we celebrate.

3. Charm is found by outlanders, and you eat your hats."

At the ("Rochester") London conference in July 1974, John Iliopoulos offered[33] his equally famous "wine challenge":

"I will call these states collectively "charmed", although I do not restrict myself to the $SU(4)$ model. I have won already several bottles of wine by betting for the neutral currents and I am ready to bet now a whole case that if the weak interaction sessions of this Conference were dominated by the discovery of the neutral currents, the entire next Conference will be dominated by the discovery of the charmed particles."

On August 1, 1974, the Physical Review Letters received the first in a series of papers which claimed what later would be named "the high-y anomaly"[34]. The paper was submitted by the Harvard-Penn-Wisconsin collaboration led by Cline, Mann and Rubbia. In the same Fermilab neutrino detector in which they observed neutral currents, they now found peculiar distributions which later were interpreted by many as being due to charged right-handed currents. The high-y anomaly is now known to be wrong. However, it took a long time to realize it and, while it lasted, it motivated a long sequence of models involving new quarks beyond charm. These models were variations on the "Fancy" theme.

As the curtain falls on Act I, we are in the fall of 1974. The standard $SU(3) \times SU(2) \times U(1)$ gauge theory was widely believed, although many pieces remained to be checked and confirmed. Charmed quarks were not yet discovered but a detailed review article on their expected properties had just been completed by Gaillard, Lee and Rosner[35]. The smart money was placed on charm, but the high-y anomaly was lurking in the wings, raising doubts and confusion. Another "red herring" appeared[36] in the form of the alleged rising value of $R_{e^+e^-}$, the effective cross section for producing hadrons in an e^+e^- collision.

Particle physics was ready for the revolution, but few people realized it. I certainly did not realize it when, in the first week of November 1974, after arriving at SLAC for a sabbatical, I wrote to a colleague back at home at the Weizmann

Institute. I complained that nothing of any great interest was happening at SLAC, and suggested that the real "action" was at Fermilab, where so many new experiments were studying multihadron reactions and neutrino scattering. My letter was mailed on Friday, November 8, 1974.

ACT II
FROM THE FOURTH QUARK TO THE FIFTH LEPTON
November 1974-August 1975

Scene I: The November Revolution
November 1974

On Monday morning, November 11, 1974, two seminars were given at the SLAC auditorium. Attending were not only all SLAC physicists, but for the first time in SLAC's history, many non-physicists, secretaries, administrators etc. Great excitement was in the air. The two seminars were given by Sam Ting and Roy Schwitters representing, respectively, the Brookhaven-MIT collaboration and the SLAC-LBL collaboration. They announced the discovery of the J particle[37] (named by Ting) or ψ particle[38] (the name chosen by Burt Richter and his colleagues at SLAC). It started the "November revolution" and signaled, as we now know, the arrival of the fourth quark.

On November 21, the SLAC-LBL collaboration found[39] the ψ' and we were beginning to get used to a new pace of at least one major discovery every month. Within literally hours from the first announcements, theoretical interpretations began to flow: a charm-anticharm state, a colored meson, a weak boson, other exotic ideas. All of these possibilities were simultaneously weighed. It did not take more than a few days for charm to emerge as the clear front-runner. The colored meson possibility ran a distant second during the first month or so and dropped from the race completely a few weeks later. Even someone like me, who until that time never worked on charm, could produce within two weeks an elaborate set of informal notes carrying the title Ψ*chology*[40], in which the various phenomenological issues related to the new particles were discussed. The notes, dated November 27, 1974, were an attempt to summarize for the benefit of the community some of the many results which were floating around and being communicated by word of mouth. There were no great new theoretical ideas in these notes but they concluded with the following statement:

"Such an informal set of notes would not be complete without a guess. Among the existing models (weak boson, color, $c\bar{c}$) we believe that the $c\bar{c}$ idea is most likely to be correct. However, it would be foolish to preclude the possibility of a totally new idea which will explain it all."

Most theorists shared this conclusion, some with more confidence, others with less. The originators of charm and many of their friends never had a doubt. Others were willing to keep an open mind but were gradually convinced.

Three immediate predictions followed from the charm interpretation of the ψ particles:

(*i*) The R-value in e^+e^- scattering was now expected to have *two* flat regions. It was known that R (defined as the ratio between the total hadronic cross section for e^+e^- and the cross-section for $e^+e^- \rightarrow \mu^+\mu^-$) was supposed to have been equal to the sum of the squared charges of the quarks. For the "old" u, d, s that sum was $R = 2$ (counting three colors) and the addition of charm raised it to $3\frac{1}{3}$. Experimentally, it was already known to be approximately 2.5 below the ψ particles (presumably due to some non-leading terms, QCD corrections, experimental uncertainties and what not). It was now predicted to reach a new plateau of $R = 3.5 - 4$ above the ψ particles, allowing, again, for various extra effects.

(*ii*) The ψ' particle was interpreted almost immediately as a radial excitation of the ψ[41]. The Cornell group, which later established itself as the leader in the theoretical "Charmonium" work, predicted[42], a few days after the discovery of the ψ', that additional states must exist between the two new ψ particles. In a classic paper by Eichten, Gottfried, Kinoshita, Kogut, Lane and Yan[42], they showed that three $C = 1$ positive parity p-states with spins $J = 0, 1, 2$ must exist somewhere below the ψ'. They predicted the masses of these states and suggested that the way to look for them was to search for radiative transitions from ψ' to the new χ–states and from them to the ψ.

(*iii*) Finally, if ψ was a hidden-charm state, bare charm had to exist. Pairs of charmed mesons were expected to be produced above the mass of the ψ' state. They were expected to contribute approximately 40% of the total hadronic cross-section. Since the charmed D-mesons decayed predominantly into strange particles, there were two obvious ways to detect them: A sharp increase in the inclusive K/π ratio was expected for hadronic events above the charm threshold, and peaks in the invariant mass plots of various $K\pi, K\pi\pi$, and $K\pi\pi\pi$ combinations were expected to show up at the same energies.

The R-value, χ-states and D-mesons were three clear and immediate tests of the charm scheme. Most of us believed that they would all be confirmed within a very short time.

Scene II: The Plot Thickens.

November 1974 – May 1975

The following six months were exciting but frustrating. In parallel with the main plot, which was taking place at the e^+e^- machines (primarily at SPEAR), the neutrino experiments contributed their own. A charm candidate was discovered at Brookhaven[43]. Dimuon events, now known to be due to charm, were observed[44] by the HPW (Harvard-Penn-Wisconsin) collaboration at Fermilab. But, at the same time, the same HPW team was producing more evidence for its "high-y anomaly" (now known to be wrong), and seeing like-sign dimuons[45] (now believed to be due to charm and other "normal" sources). All of these, and especially the alleged "high-y anomaly", drove many theorists into the world of "Fancy".

On November 25, 1974, only two weeks after the J/ψ, the Physical Review Letters received a paper[46] written by Harvard postdoc Michael Barnett. He proposed a six-quark model. He suggested three new charmed quarks rather than the "usual" one, and proposed that they were connected to the three old quarks by right-handed currents, motivated by the high-y anomaly. Barnett's six quarks were $p, n, \lambda, p', n', \lambda'$. In modern notation he had *four* down quarks and only *two* up quarks. He had no room for what we now call the top quark.

He suggested that ψ and ψ' were formed from different combinations of the new quarks, much like the old ρ and ω mesons which were two different combinations of u and d quarks.

Two weeks later, in Kyoto, Maki and Umemura submitted to the Progress of Theoretical Physics a short note[47], interpreting the new ψ states. It was one of many dozens of similar papers written all around the world during that month. However, in their paper they devoted one or two sentences to the possibility that the new particles consisted of more than one new quark. They referred to an earlier note written in Japanese by Maki, in which a scheme with six quarks and six leptons was apparently proposed. When I visited Kyoto a year later, I learned about this work from Maki who also equipped me with a copy of the earlier note. My limited ability in reading Japanese prevents me from explaining the contents of that two-page note.

During January, February and March of 1975, the race to confirm the three decisive predictions of the charm scheme was on. The R-test, the χ-states and the D-mesons proved to be very troublesome.

The hadronic e^+e^- cross-section was measured at SPEAR by the SLAC-LBL collaboration. It was indeed flat above the energy range of the ψ states[48]. That was a great success of the theoretical prediction. No more "rising R-value". However, the constant value was neither $3\frac{1}{3}$ nor 3.5 nor even 4. It was approximately $R = 5$ with a 10% error. This could not possibly be explained by the charm idea.

The search for the χ states proved very difficult. The SPEAR and DORIS detectors were not designed to look for monochromatic γ-rays. It appeared that the detector most likely to observe the radiative transitions was a large array of sodium iodide crystals, built by a Stanford group headed by Robert Hofstadter and designed to test various QED predictions involving electrons and photons in the final states. It was located in the second interaction region at SPEAR and was supposed to complement the SLAC-LBL detector which discovered the ψ and ψ'. Many of us at SLAC tried to impress upon the members of that Stanford group the importance of searching for the χ states. They were mostly interested in testing QED. However, they certainly agreed to look for the monochromatic photons. As the weeks went by, they accumulated more and more data and there were no χ-states in sight. By the end of March it appeared that the Stanford group could set an upper limit on the monochromatic photons, which was somewhat below the clear theoretical predictions of the Cornell Charmonium experts. At the Washington meeting of the American Physical Society in April, Hofstadter indeed reported[49] such a limit. It was below the predictions. The existence of the χ-states began to appear somewhat doubtful.

The third test was also disappointing. No charmed D mesons were found. It was not clear how many events one would need in order to observe peaks in the $K\pi$ or $K\pi\pi$ invariant mass plots because the branching ratios of the D mesons to these specific final states were not really known. Consequently, the absence of peaks was disappointing but could not be considered negative evidence. However, the inclusive K/π ratio in hadronic final states did not seem to show any increase when the energy was raised above the alleged charm threshold. This began to look serious although there were still some doubts concerning the ability of the SLAC-LBL experiment to tell $K's$ from $\pi's$ with great certainty (especially above a certain momentum).

By the end of March all three "immediate" tests of the charm hypothesis turned sour. The R-test went against charm. The χ states were not seen at the predicted level. The simple signatures of the D mesons were not observed.

It appeared to me that the charm scheme needed to be modified, in order to "save" it from these serious difficulties. By the end of March I was busy inventing a cure, going back to the Barnett idea that the ψ states represented not *one* new flavor of quark but more. My paper[50] was received by Physics Letters B on April 10, 1975 and a more detailed version[51] was received by Annals of Physics seven weeks later, on May 30.

The paper was entitled "A New Quark Model for Hadrons"[50]. In it I proposed six types of quarks: the usual u, d, s and three additional heavy quarks with charges $\frac{2}{3}, \frac{2}{3}, -\frac{1}{3}$. These were, of course, the charges of the six quarks we recognize today. They were different from the Barnett quarks and, although I did not know it at that time, were identical to the KM quarks (except that KM talked about charges Q and $(Q-1)$ rather than explicitly about $\frac{2}{3}$ and $-\frac{1}{3}$).

The new message was that ψ and ψ' consisted of various combinations of the new quarks. We therefore had $R = 5$ (which was now the sum of the squared quark charges) and there was no need to have the χ states between ψ and ψ' because ψ' was not any more a radial excitation of ψ.

Two of the three embarrassments of the charm scheme were gone! We now know, of course, that the absence of χ states was a false alarm. It also transpired that the $R = 5$ value was due to other reasons. More about that later. However, in April and May of 1975, none of this was known.

The names I gave to the new quarks were interesting and in the end, they were the part of my model most likely to be remembered for a long time... The figure caption for figure 1 in the short paper[50] read:

> *"The ordinary u(up), d(down), s(singlet) quarks and the proposed heavy t(top), b(bottom), r(right) quarks."*

I remember spending some time on the choice of these names. I looked for lower-case Latin letters which were not in use as names for particles or groups of particles. Very few such letters remained unused. The letters b, r, t were almost the only ones available. I did not want to use c(charm) for one of the three new quarks because all of them (combined) played the role of charm, and no single one had identical properties to the standard hypothetical charmed quark. So I chose t, b, r and coined the mnemonics "top", "bottom" and "right" because they appeared, respectively, at the top, the bottom and the right of my figure 1. That is how the names of the "top" and "bottom" quarks were introduced.

In addition to the (now known to be wrong) description of ψ and ψ' as bound states of several new quarks, the model did make two remarkable predictions. The first was the introduction of a 3×3 unitary mixing matrix. It said:

> *"The matrix elements of A can be expressed in general in terms of three angles. One of the angles is the Cabibbo angle. We know experimentally that the coefficients of $u\bar{d}$ and $u\bar{s}$ in J^+ are approximately given by $\cos\theta$ and $\sin\theta$. Hence,*
>
> $$A_{11} = 0.97, \quad A_{12} = 0.23, \quad A_{13} \leq 0.1. \text{"}$$

These are, of course, today's V_{ud}, V_{us}, V_{ub} matrix elements. I then noticed that A_{13} was the smallest and considered the approximation in which it could be neglected. I wrote:

"The most general form of the matrix A, consistent with $A_{13} = 0$ is

$$A = \begin{pmatrix} \cos\theta & -\sin\theta & 0 \\ \cos\phi\sin\theta & \cos\phi\cos\theta & -\sin\phi \\ \sin\phi\sin\theta & \sin\cos\theta & \cos\phi \end{pmatrix}$$

where θ and ϕ are two weak rotation angles."

The next order of business was to predict the various decay patterns of hadrons containing the three new quarks. The analysis of weak decays included the prediction (now known to be correct) that the b-quark would decay predominantly to the new heavy quarks. In fact, the entire weak interaction part of the model was essentially correct. However, I definitely did not realize that there was a room for a complex phase and did not consider CP-violation at all. I also was not impressed by the "high-y anomaly" and did not suggest any right-handed currents for the six-quark scheme (no "Fancy").

After I gave a seminar about the paper, Itzhak Bars, then an Assistant Professor at Stanford, asked me if I did not think I could include CP-violation in the model. Neither of us knew about Kobayashi and Maskawa. I answered that I did not know how to do it. He said something about possible complex angles but neither of us pursued the matter any further. I am not even sure that Itzhak remembers this conversation.

My ill-fated model had one additional striking prediction. It said:

"The Charm scheme has four quarks and four leptons. We have six quarks. We may achieve a similar quark lepton symmetry by proposing a new charged heavy lepton and its neutrino. In fact, such a six-lepton scheme is necessary if we wish to preserve the condition

$$\sum_{quarks} Q_i + \sum_{leptons} Q_i = 0.$$

This condition is required in a unified theory of quarks and leptons if *we want to eliminate the asymptotic contribution of the triangle anomaly diagrams which occur in triple-current vertices (such as two vectors and an axial vector). If these additional heavy leptons exist, we will eventually have $R = 6$. Experimentally, it is entirely possible that pairs of leptons are produced somewhere above $W \sim 3.5 - 4.5$ BeV, and are partly responsible for the rise in R."*

The new model did not have much of an impact. The charm enthusiasts were not at all perturbed by the R-problem and the χ-problem. They had no solution to it except to say that wrong experiments were not unusual and one should simply be patient. They did not want ψ states which corresponded to several different quarks and they were eventually proven right.

By the end of May 1975 charm did not look good. The alternatives were variations on the charm theme (such as my model) but not profound new ideas. The experiments were not giving the expected results.

Scene III: The Perfect Crime... Is Solved!

June 1975 – August 1975

As June 1975 arrived, the search for the χ states and the D mesons continued, without success. However, one person was finding something else which few theorists really wanted. That person was Martin Perl, one of the leaders of the SLAC-LBL collaboration. Perl always wanted to find a new lepton. Now he was finding a few dozen events of the type:

$$e^+ + e^- \to e^\pm + \mu^\mp + missing\ neutrals.$$

Such events *could* be due to a new heavy lepton which sometimes decayed to electrons and sometimes to muons. They could also be due, in principle, to charmed mesons which could do the same. They could also be pions, misidentified as muons and, less likely, as electrons.

Most members of the SLAC-LBL collaboration did not believe that the events were real. There were three powerful arguments against these events: first of all, Martin Perl always wanted to find a new lepton. That made the whole affair somewhat suspicious. Second, the collaboration was discovering new things every month. How lucky can one be? It was unlikely that there was yet another, unrelated, gold mine in the same place. Third, many members of the collaboration doubted the ability of their own detector to identify electrons and muons with a sufficiently high probability. They were giving Perl a very hard time.

By the end of June, Martin Perl "bootlegged" an announcement of his effect by describing[52] the detector and its capabilities at a summer school which was held at McGill University in Montreal. He described his events but stopped short of claiming a new particle. Even if it were a new particle, it could still be the elusive D meson, rather than a new lepton.

By early July both DESY and SLAC were beginning to see the χ states. The first announcement[53] came from the DASP detector in DORIS. It was followed by reports[54] from the SLAC-LBL collaboration. Between these two experiments, all the p-states predicted by the Cornell team[42] were discovered at the right masses and approximately the right transition rates. Needless to say, the charm advocates were very happy. My own six quark model with top and bottom was clearly in trouble. After all, one of its main two motivations was the absence of χ states. Those existed now and there was no reason at all to suggest that ψ and ψ' were anything but two different bound states of $c\bar{c}$.

In the meantime, as the high-y anomaly was still popular, several teams of theorists tried to combine the "Fancy" idea[31] of De Rujula and his fellow "actors" with the six quark scheme which I suggested. The first of these teams was Pakvasa, Simmons and Tuan[55] in Hawaii, followed by others. More about that later.

By the end of July, the SLAC-LBL collaboration finally became convinced that the Perl events were real. On July 29, Martin Perl announced to the topical conference at the annual SLAC Summer Institute the discovery[56] of a new particle, temporarily named U (for Unknown). There was no proof that this was a heavy lepton. This was indeed the τ lepton, but it was not at all clear at the time.

During July and August, new discoveries came at a furious pace. New χ states; new decay modes of $\chi's$; New decay modes of ψ states; the U-particle; first observation of quark jets in e^+e^- collisions[57] and more and more. During July and August I remember lecturing in four different meetings: a symposium at Argonne, the SLAC Summer Institute, the Gordon Conference in New Hampshire and finally the rapporteur talk on the new particles at the Lepton-Photon Symposium which was held at Stanford. I mention these lectures because over a period of 5 weeks I could never give the same talk twice. From one talk to the next, new data appeared but also new understanding of older facts was emerging. A particularly awkward moment came in my Argonne talk. By then I knew that the SLAC-LBL collaboration had clear evidence for the χ states. However, I was literally under oath not to say it without permission from the group. They were not yet ready to announce their result. That was before the DESY announcement of the same states and I knew nothing about the DESY data. Since I knew that χ states existed, I knew that my six quark model was doomed. I therefore hardly mentioned it at Argonne and certainly did not advocate it. Two people from the audience attacked me. They thought that my model was wonderful and that it explained both the large R value and the absence of χ states. I could not argue with them, but I did not want to push a model which I already knew to be wrong. I ended up doing something I never do: I mumbled something which no one could understand and just avoided the questions and comments. It was a very bad moment for me.

As the Stanford Lepton-Photon Symposium approached, I was back to studying the mystery of the missing D mesons. I spent hours going over the data with the SLAC-LBL experimentalists, with Fred Gilman and especially with Bjorken. The inclusive K/π ratio was still refusing to increase at energies above the ψ particles. The peaks in $K\pi$ and $K\pi\pi$ final states still refused to show up[58]. Bj, Fred and I concluded that this was fine only if the branching ratio of a D meson into a two-body and three-body final state was relatively small. However, in that case, the most frequent D decay should be into four or more particles and the average multiplicity of the decay products of a D meson should be fairly high. If that was the case, we should see a clear increase in the charged multiplicity of the hadronic events above the charm threshold. No such increase was observed by the SLAC-LBL team[59]. We convinced ourselves that, if charm existed, we *had to* see *either* peaks in low multiplicity final states *or* an increased multiplicity. At least one of the two effects *had to* exist. Neither existed. It was very frustrating.

In preparing for my rapporteur talk I got from the SLAC-LBL collaboration their latest data on three quantities below and above the alleged charm threshold:

(*i*) The R-value was flat with a value of $R \sim 2.5$ below the suspected new threshold and $R \sim 5$ above it. That was not very good for charm.

(*ii*) The K/π ratio was essentially constant throughout the entire range of measurements. That was very bad for charm.

(*iii*) The average charged multiplicity was also essentially unchanged as one crossed the alleged charm threshold. That could coexist with charm if the D-mesons had low multiplicity decays but then the peaks at low-multiplicity final states should be found. They were not.

In spite of this, the χ states were such a remarkable victory for charm that one could not possibly doubt its validity. There were also the neutrino dimuon events[44] which looked like charm and the Brookhaven neutrino event[43] which looked like a charmed baryon. What was going on?

The solution was amazingly simple but it appeared very artificial, at first. I concluded that charm did exist but there was also a new heavy lepton. They both had the same threshold. (Sounds crazy! Why would they have the same threshold? There is no relation between them!)

The heavy lepton would mostly decay to hadrons, contributing an apparent extra unit of R, explaining the $R \sim 5$ value.

The D mesons decayed mostly into strange particles, but the heavy leptons decayed into strange particles only a few percent of the time. The new physics consisted of two *comparable* pieces, one made almost purely of events containing strange particles, the other containing almost no strange particles. Therefore, above the common new threshold, the K/π ratio would neither increase nor decrease!

The heavy lepton always decayed with the emission of neutrinos. Its dominant decay modes were $e^-\nu\bar{\nu}$, $\mu^-\nu\bar{\nu}$, $\pi^-\nu$, $\pi^-\pi^0\nu$. In all of these decays, the charged multiplicity was one! Therefore, the new physics consisted of two pieces: one (D mesons) with higher than normal charged multiplicity; the other (heavy leptons) with smaller than normal charged multiplicity. The overall charged multiplicity should neither increase nor decrease as we climb through the common threshold of the new physics! The peaks in $K\pi$ and $K\pi\pi$ final states were not seen because D mesons mostly decayed to higher multiplicities. This was now fine, because these higher multiplicities were counterbalanced by the low multiplicities of the decays of the new lepton.

Even as I write these things now in 1987, knowing that they are absolutely correct, they appear concocted and artificial. The heavy lepton and the D meson conspired to commit a perfect crime. They almost succeeded, but were finally caught!

So now I was going to claim in my Stanford talk that the absence of any change in the K/π ratio and in the average multiplicity were good signs! That reminded me of a story:

Italian archeologists excavated under the Roman Forum and found a wire. An Italian theorist wrote a paper, claiming that the ancient Romans must have used a telephone. A year later, Israeli archeologists were digging under the old city of Jerusalem. They found no wires. An Israeli theorist then wrote a paper, deducing that the ancient Jews must have used a cordless telephone!

Here I was, not finding anything in the data, but claiming that this is marvelous evidence for having both charm and a heavy lepton...

In my rapporteur talk[60], I reviewed all of this information and concluded:

"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 $W \sim 4$ GeV we have $R \sim 5$ and the new physics corresponds to $\Delta R \sim 2.5$. 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) One new quark (c) and no new leptons
This gives the wrong R and K/π ratio, and does not provide a reasonable explanation of the $\mu^\pm e^\mp$ events.
(B) Two or three new charged leptons. No new quarks
Does not explain either the narrow ψ, ψ' or the wide ψ'', ψ'''. Solves nothing. Almost certainly wrong.
(C) Three or two new quarks (c, t, b). No new leptons
Does not explain the spectrum of the Ψ family (unless the quarks are degenerate 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) One new quark (c) and two new leptons (U^-, ν_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:

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

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

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

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 else we would have already seen the $t\bar{t}$ and/or $b\bar{b}$ vector mesons."

This time, for a change, my summary of the situation was correct and, even in hindsight, there is no reason to change a single word in it. As far as I know, this was the first time in which the correct picture of the six quarks and six leptons was presented. However, it was not a brilliant theoretical prediction like the prediction of charm. It was a common-sense analysis of available data, guessing correctly almost all the missing pieces. The only missing element was CP-violation. The Kobayashi- Maskawa paper had not yet been "discovered".

In fact, as I was preparing my rapporteur talk for the Stanford symposium, I received approximately 150 theoretical papers on the new particles. They were usually fresh preprints, discussing the implications of the latest data. Among all of these papers, there was a three-year-old *reprint* which arrived from Japan. I read the title and the abstract. It said something about CP and gauge theories. I could not understand why it was sent to me, what was its relation to the new particles or why I, as rapporteur, should discuss a three-year-old paper. I did not have time to read the full paper, especially since the abstract did not advertise anything which looked relevant. Only three months later, when I first learned about the KM paper, I returned to my enormous pile of Stanford Conference papers and discovered that the old reprint was sent to me by KM, probably because they read my six-quark paper and wanted me to know about their much earlier work.

On August 21, 1975, during the Stanford symposium, a press conference was held. The speakers were the heroes of the day: Richter, Ting and representatives of DESY. I was a very minor player in all of this but since I was the theoretical rapporteur on the new particles, I was invited to attend. I kept quiet while Richter and Ting described their discoveries. Then Ting told the press that the theorists claimed that the new particles were due to charm, but he looked for charm, did not find it, and was absolutely convinced that it did not exist. Indeed, he brought to the conference new negative results of a search for bumps in $K\pi$ invariant mass plots in his Brookhaven experiment. I had to defend the prestige of the theoretical community, of which I was the only representative in the room. I could not possibly allow the San Francisco Chronicle and the Palo Alto Times to conclude that we, theorists, did not know what we were talking about... I immediately offered Sam Ting a $10 bet, stating that within a year charm would be found. I even suggested that Sam himself would be the judge, in case that there would be doubts. Sam accepted and the bet occupied a prominent place in the report of the Palo Alto Times on the following day[61].

ACT III
FROM THE FIFTH LEPTON TO THE FIFTH QUARK
September 1975-July 1977

Scene I: OOPS!
September 1975-February 1976

The fall and winter of 1975/76 were a period of false starts. The six quarks were slowly becoming popular, but the high-y anomaly[34] was still very much alive in the minds of many people. At least four groups of authors[55,62] combined the "Fancy" of the Harvard group[31] with the new six-quark scheme[50,60] in order to create a "unique vector-like theory" of six quarks. The idea was to claim that all left-handed *and right-handed* quarks were in doublets of the standard model $SU(2)$. The left-handed doublets were clear:

$$\begin{pmatrix} u_L \\ d_L \end{pmatrix} \quad \begin{pmatrix} c_L \\ s_L \end{pmatrix} \quad \begin{pmatrix} t_L \\ b_L \end{pmatrix}$$

How about the right-handed doublets? The u quark could not be paired with d or s because that would lead to right-handed currents in ordinary β-decay or K-decay and hyperon decay. Therefore, u could only be a partner of b. The c-quark then had to choose between d and s. A right-handed $c\bar{d}$ current would spoil the explanation of the $K_S^0 - K_L^0$ mass difference. Therefore the right-handed companion of c had to be s. That left the d-quark as a partner of the t-quark. The unique right-handed assignment would then be:

$$\begin{pmatrix} u_R \\ b_R \end{pmatrix} \quad \begin{pmatrix} c_R \\ s_R \end{pmatrix} \quad \begin{pmatrix} t_R \\ d_R \end{pmatrix}$$

Such a model would explain the high-y anomaly and would be free of triangle anomalies in the quark sector. That would require further modifications in the leptonic sector which could allow some new neutral leptons, accounting for some of the other effects which were observed by the HPW collaboration (such as the tri-leptons, now known to be wrong).

Theoretically, a vector-like theory could be attractive because it would mean that the weak interactions were parity conserving in the exact symmetry limit and parity violation was a by-product of the mass-generating mechanism of the standard model. Among many other experimental implications, the vector-like theory predicted:

$$I_3(u_R) = \frac{1}{2};\ I_3(d_R) = -\frac{1}{2}.$$

This could be tested in neutral current experiments even at the atomic physics level and did not require any high energies or a discovery of the top and bottom quark. In fact, the neutral currents were predicted to conserve parity! Needless to say, all of this is now known to be wrong.

As this was happening, the world finally learned about the KM paper. On September 29, 1975 the Physical Review received a paper by S. Pakvasa and H. Sugawara[63] discussing CP-violation within the framework of the new six-quark model. A month later, Physics Letters B received a paper by Maiani[64], presenting a similar discussion. Both papers referred to the three-year old Kobayashi-Maskawa paper[28]. To this date I do not know to what extent Pakvasa and Sugawara were inspired by KM or whether they learned about the KM paper only after their work was done. I know that Maiani heard about KM only after his work was completed and he added the reference to them in the last moment. The CP phenomenology of the $K - \bar{K}$ system was discussed in these papers for the first time. KM never discussed any of it.

We now had three independent theoretical reasons to expect six quarks:

(*i*) The existence of six leptons (which was still far from being confirmed) together with the requirement of anomaly cancellation. This reason is still valid today, in 1987.

(*ii*) The high-y anomaly and the implied right-handed currents (now known to be wrong).

(*iii*) CP-Violation. Today, in 1987, we are still not sure whether the observed violations of CP are indeed due to the KM phase, but they are certainly consistent with it.

On January 28, 1976 a new e^+e^- resonance was announced at Fermilab by Lederman and collaborators[65]. It was a peak around 6 GeV observed in an improved Fermilab version of the original Brookhaven experiment of Lederman *et al.* Another group at Fermilab[66] claimed a similar bump in a beam dump experiment, submitted to Physical Review Letters two days before the Lederman bump. A quick scan of the relevant energy region at the SPEAR e^+e^- machine soon showed that the new bump did not exist and the Lederman group confirmed the negative verdict by a further run of their own experiment[67]. The 6 GeV bump, named Upsilon by Lederman *et al*, became known as the Oops (while some evil minds called it Oops-Leon).

As the spring of 1976 arrived, the D mesons were not yet found. Iliopoulos needed charm before the summer, in order to win his bet. The Experimental Meson Spectroscopy Conference (were Glashow would win or lose the "hat challenge") was not held in 1976 and was postponed to 1977. On a much more minor scale, I was getting worried about having to pay Sam Ting $10 on August 21, 1976. There were also some new doubts, coming from a DESY experiment, on the Perl lepton. The high-y anomaly was at its peak. There was a mixed bag of arguments for the existence of six quarks.

Scene II: The Dust Settles
Spring 1976– Spring 1977

In May 1976 John Iliopoulos was visiting us at the Weizmann Institute. On Tuesday afternoons our theory group used to play soccer in the recreation center of the campus. Iliopoulos joined us for a rough (but not very good) soccer game. I remember returning home with both knees bleeding, very tired and somewhat disgusted. I found a message from my daughter saying that Fred Gilman phoned from SLAC and said he would call again later. He asked her to tell me that "they have found it". Fred called again a few minutes later and told me how Gerson Goldhaber and Francois Pierre from the LBL part of the SLAC-LBL collaboration finally found a clear peak in the $K\pi$ invariant mass plot at the expected mass. This was the D^0 meson[68]. It took me two hours to track Iliopoulos among the few not-very-good restaurants of Rehovot and to have the pleasure of breaking the news to him. His wine was now safe.

In the following few months everything suddenly turned well. The D^+ was found[69]; the high-y anomaly began to look shaky and was finally demolished by a beautiful CERN experiment of Jack Steinberger and collaborators[70]; the τ lepton was confirmed as a new lepton and its decay modes were being measured, in agreement with the theory; neutral current experiments were excluding the vector-like theories; the HPW dileptons were shown to be consistent with charm.

In the summer of 1976 Iliopoulos won his wine. On August 21, 1976 I walked into the CERN cafeteria to have dinner and met Sam Ting. I had not seen him since the press conference in Stanford, exactly a year earlier. Without either of us saying a word, he pulled a $10 bill out of his wallet and handed it to me. I demanded a check. He said: If I give you a check, you would hang it in your office and you would never cash it. He was right.

In the spring of 1977, the Experimental Meson Spectroscopy conference was held again in Boston. Glashow was invited to give the summary talk. Before the talk, Roy Weinstein, the Conference organizer, announced that we all had to eat our hats. Several secretaries from Northeastern University went down the aisles in the auditorium, handing everybody small hat-shaped candies. Everybody except Glashow ate them.

Glashow and Iliopoulos won their bets on their own great prediction of charm. I won my $10 simply by standing on the sideline and betting on the right horse.

During 1976 and 1977 the fourth quark and the fifth lepton were confirmed. Richter and Ting received their Nobel prize. I told Sam Ting that our respective profits (his Nobel prize and my $10) reflected our relative contributions to the new physics.

Most theorists were sure that we needed more quarks. Various conference and summer-school lectures were given under titles such as "Beyond Charm"[71], "Charm is not Enough"[72], "Three Generations of Quarks and Leptons"[73] and "Charm, Apres-Charm and Beyond"[74]. We all expected the next quark to arrive any moment although the theoretical motivation was not compelling and no one had a convincing prediction for its mass.

Scene III: Υ

July 1, 1977

On July 1, 1977 the Physical Review Letters received a paper from Leon Lederman and collaborators at Fermilab. This time there could be no doubt. The Upsilon was there. The fifth quark was discovered. The fourth lepton of Lederman and friends had finally yielded a great-grandson: the fifth quark of Lederman and friends. Another chapter of physics had ended. A new one began.

FINALE: In the Fox Valley Movie Theater, Aurora, Illinois

July, 1987

A few days before this symposium, I saw a new film in a movie theater near Fermilab. In the movie (named Roxanne) the heroine (a high-brow lady) tells the hero (a modern-day version of Cyrano de Bergerac) that there are six types of quarks: up and down, charm and strange, top and bottom. She then continues (to my utter amazement) and says that the top and bottom quarks are the most common kinds(!), and that only in very rare and unusual collisions, the charmed and strange quarks sometime turn up. Hearing these words on the tenth anniversary of the b-quark discovery, I knew that the bottom quark "has arrived". Not only is it mentioned in a Hollywood movie, but it is alleged to be the most common quark. Hollywood must know something that we don't.

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