The Discovery of the τ, 1975-1977: A Tale of Three Papers

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Preface

As an eyewitness to the discovery of the τ, and as a participant in some of the activities that surrounded this discovery, I have been asked by the organizers of this symposium to give a personal history of these events. Two caveats for the listener are in order. First, no personal history will be complete. The recollections of each person will reflect his or her different perspectives, different concerns, and, perhaps, imperfect memory. Only by combining several personal histories will a complete picture of the events emerge.

Second, I am concerned that in giving a personal history, and thereby relating events that I knew about or took part in, the audience may conclude that my role in these events was greater than it actually was. It is thus proper at the outset that I explain what my role was. During the 1975-77 period, I was a member of the SLAC scientific staff, working in Martin Perl's group. During this period, there was an almost unbelievable amount of activity. In 1975 alone, in addition to the discovery of the τ, we published papers on:

- searches for narrow s-channel resonances,[1]
- searches for charm,[2]
- the structure of the total cross section,[3]
- the properties of the ψ states,[4-7]
- the discovery of the χ states and their properties,[8,9]
- the discovery of transverse polarization of the beams,[10] and
- the discovery of jet structure in hadronic events.[11]

I was like a kid in a candy store, hopping from one bin of goodies to the next. Meanwhile, Martin Perl was rather singlemindedly pursuing the search for a heavy lepton. I was fortunate to have the adjoining office to his, and our interactions usually took the form of Martin appearing at my door and saying,

> *I would like to bounce some ideas off you,*

or

> *I have some calculations that I would like you to look at,*

or

> *There is an interesting topic that you might want to look into.*

In short, Martin was at the reins, and I was along for the ride, sometimes riding shotgun, and sometimes just enjoying the scenery.

The Three Papers

When I think about the discovery of the τ, three papers immediately come to mind. These were far from the only papers written during this period, but they were the seminal papers that advanced the state of knowledge. The other papers written during this period, both from our collaboration and from others, mainly confirmed the results of these seminal papers. Martin Perl is the lead author on all three of these papers. They were written during the summers of 1975, 1976, and 1977, spaced almost exactly a year apart from each other. In some sense, they can be thought of as annual reports to the community. In this way, they serve as convenient guide posts to the progress of the discovery of the τ. During this talk, I will simply refer to them as the first, second, and third papers.

Let us turn now to these three papers. In each case, I will quote from the abstract, which in general contains conclusions slightly weaker than those in the body of the paper.

The first paper[12] is entitled "Evidence for Anomalous Lepton Production in e^+e^- Annihilation," and was received on August 18, 1975.

> *We have found events of the form* $e^+e^- \rightarrow e^\pm\mu^\mp$ *+ missing energy, in which no other charged particles or photons are detected.... We have no conventional explanation for these events.*

The second paper[13] is entitled "Properties of Anomalous eμ Events Produced in e^+e^- Annihilation," and was received on July 15, 1976.

> *The simplest hypothesis compatible with all the data is that these events come from the production of a pair of heavy leptons, the mass of the lepton being in the range 1.6 to 2.0 GeV/*c^2.

And the third paper,[14] received on August 17, 1977, almost exactly two years after the first paper, is entitled, "Properties of the Proposed τ Charged Lepton."

> *...the* τ *mass is* 1.90 ± 0.10 *GeV/*c^2*; the mass of the associated neutrino,* ν_τ*, is less than 0.6 GeV/*c^2*...; V-A coupling is favored over V+A coupling for the* τ-ν_τ *current; and the leptonic branching ratios are* $0.186 \pm 0.010 \pm 0.028$ *from the e*μ *events and* $0.175 \pm 0.027 \pm 0.030$ *from the* μx *events....*

I would now like to turn to how these papers came to be written and the other events surrounding them.

The Proposal

Our story begins much earlier than the discovery in 1975. If it were not for the eloquent talk of Nino Zichichi,[15] at this point I would describe the early searches for heavy leptons at Adone,[16-18] which were quite similar to the searches we conducted. Let me move then to the proposal for running time that we wrote in 1971.[19]

This proposal, whose title page is shown in Fig. 1, is a marvelous document to read twenty-one years later. The table of contents (Fig. 2) shows that we proposed to make four sets of measurements. The first two were measurements of meson and baryon form factors, that is, electron-positron annihilation into pion pairs, kaon pairs, proton-antiproton pairs, etc. Eventually, we actually did get some information on these topics by using the ψ as a luminosity enhancer,[20, 21] but in general, the cross sections for these processes were too small to measure.

The third section would be a little more recognizable and relevant to a modern-day particle physicist. One of the measurements we proposed was the total hadronic cross section (Fig. 3). You probably haven't seen the total cross section written in quite this form,

$$\sigma_T(q^2) = \sigma_{\mu\mu}\left|F_T(q^2)\right|^2,$$

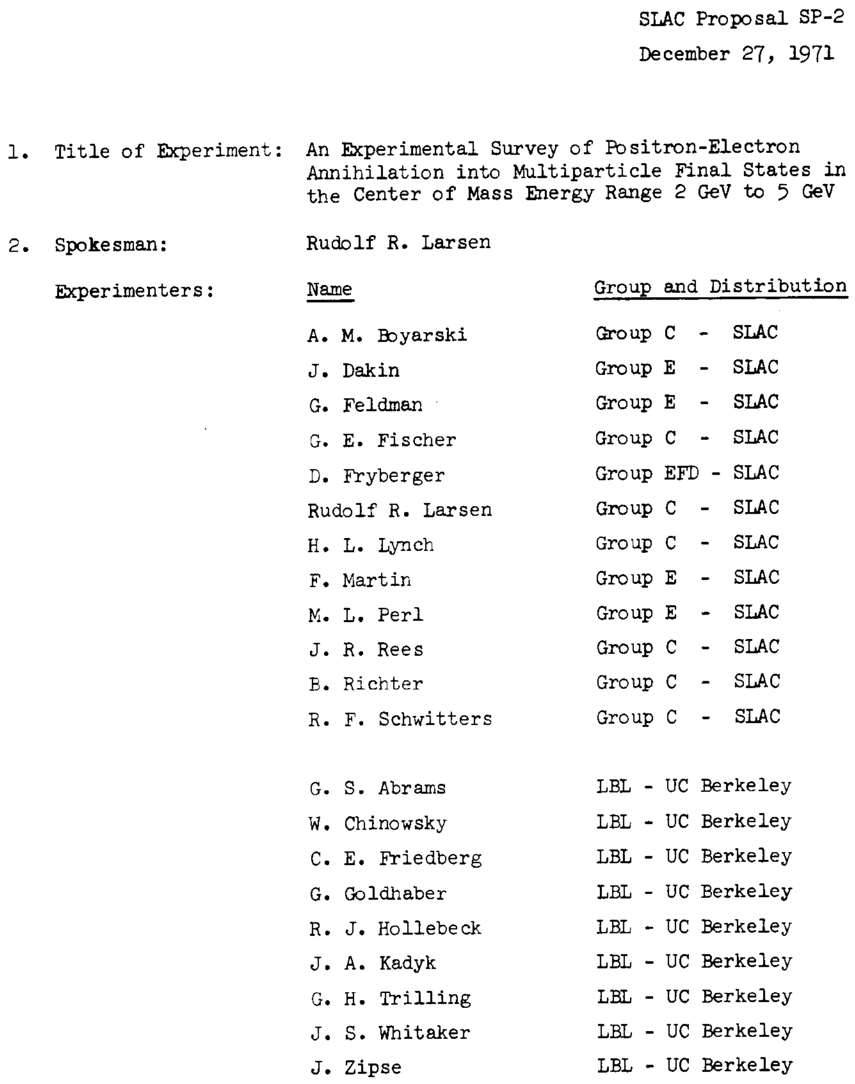
unless you are at least as old as I am. But, of course, the form factor squared is just our familiar R. But look at the options: The pressing question is whether it would be constant, fall like $1/q^4$, or fall like $1/q^8$. Most physicists of the time would have guessed one of the two latter options. A few years earlier, as a graduate student, I remember being told by a distinguished physicist that these proposed electron-positron storage rings were a waste of time. They could only test QED, because everyone knew that hadronic cross sections would fall rapidly with energy.

Even believers in the parton model didn't know what the magnitude of the cross section would be. How could they? Asymptotic freedom hadn't been discovered yet.[22-24]

It is remarkable that the words "quark" and "jet" do not appear anywhere in this proposal.

The only part of the proposal that would be fully recognizable to a modern physicist was the fourth section on searches for a heavy lepton, a page of which is shown in Fig. 4. First of all, note the cross section calculations of Paul Tsai.[25, 26] One adds across a row or column to get the leptonic branching ratio, and it comes to 18%, a value completely consistent with modern measurements.[27] Even though Paul has explained to us that the accuracy was somewhat accidental,[28] it seems to me remarkable that Paul was able to do this calculation by putting together the scraps of information on what was then known about hadronic physics, without reference to quarks, color, or QCD.

The proposal goes on to lay out the search almost exactly as it was done.



SLAC Proposal SP-2
December 27, 1971

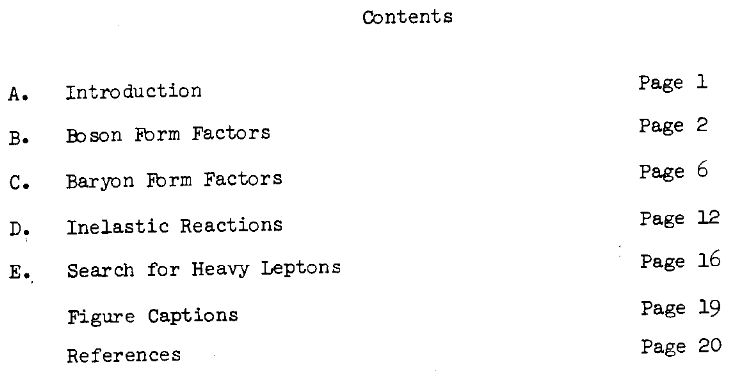
1. Title of Experiment: An Experimental Survey of Positron-Electron Annihilation into Multiparticle Final States in the Center of Mass Energy Range 2 GeV to 5 GeV

2. Spokesman: Rudolf R. Larsen

Experimenters:

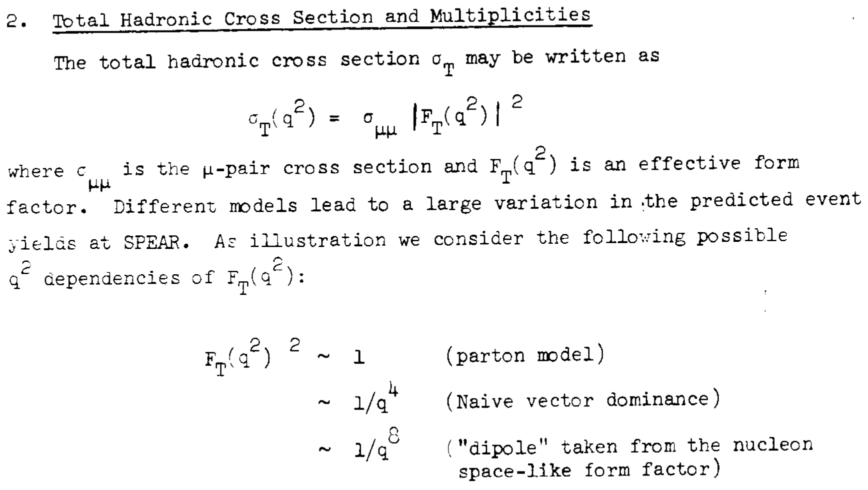
Name	Group and Distribution
A. M. Boyarski	Group C - SLAC
J. Dakin	Group E - SLAC
G. Feldman	Group E - SLAC
G. E. Fischer	Group C - SLAC
D. Fryberger	Group EFD - SLAC
Rudolf R. Larsen	Group C - SLAC
H. L. Lynch	Group C - SLAC
F. Martin	Group E - SLAC
M. L. Perl	Group E - SLAC
J. R. Rees	Group C - SLAC
B. Richter	Group C - SLAC
R. F. Schwitters	Group C - SLAC
G. S. Abrams	LBL - UC Berkeley
W. Chinowsky	LBL - UC Berkeley
C. E. Friedberg	LBL - UC Berkeley
G. Goldhaber	LBL - UC Berkeley
R. J. Hollebeck	LBL - UC Berkeley
J. A. Kadyk	LBL - UC Berkeley
G. H. Trilling	LBL - UC Berkeley
J. S. Whitaker	LBL - UC Berkeley
J. Zipse	LBL - UC Berkeley

Figure 1. Title page from the Mark I physics proposal.



Contents

Figure 2. Table of Contents from the Mark I physics proposal.



2. Total Hadronic Cross Section and Multiplicities

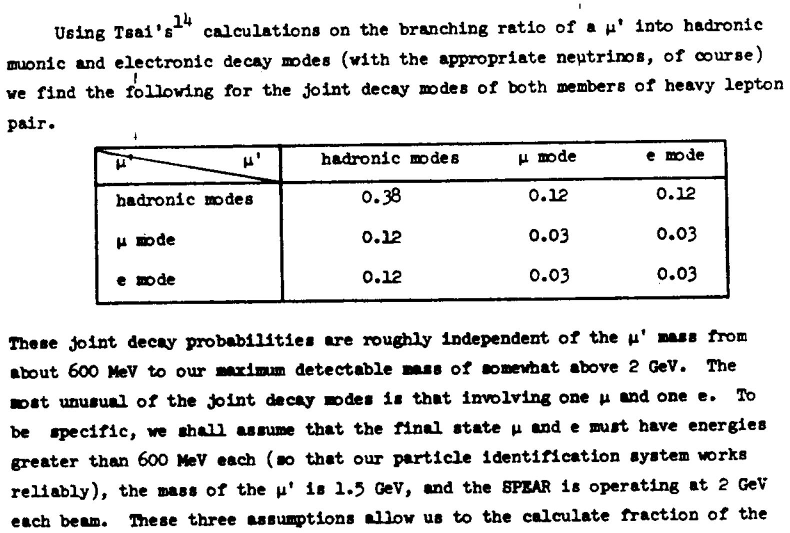
The total hadronic cross section σ_T may be written as

$$\sigma_T(q^2) = \sigma_{\mu\mu} \left| F_T(q^2) \right|^2$$

where $\sigma_{\mu\mu}$ is the μ-pair cross section and $F_T(q^2)$ is an effective form factor. Different models lead to a large variation in the predicted event yields at SPEAR. As illustration we consider the following possible q^2 dependencies of $F_T(q^2)$:

$$\begin{aligned} \left|F_T(q^2)\right|^2 &\sim 1 && \text{(parton model)} \\ &\sim 1/q^4 && \text{(Naive vector dominance)} \\ &\sim 1/q^8 && \text{("dipole" taken from the nucleon space-like form factor)} \end{aligned}$$

Figure 3. A paragraph on the total hadronic cross section from the Mark I physics proposal.



Using Tsai's^{14} calculations on the branching ratio of a μ' into hadronic muonic and electronic decay modes (with the appropriate neutrinos, of course) we find the following for the joint decay modes of both members of heavy lepton pair.

μ^{+} \ μ'	hadronic modes	μ mode	e mode
hadronic modes	0.38	0.12	0.12
μ mode	0.12	0.03	0.03
e mode	0.12	0.03	0.03

These joint decay probabilities are roughly independent of the μ' mass from about 600 MeV to our maximum detectable mass of somewhat above 2 GeV. The most unusual of the joint decay modes is that involving one μ and one e. To be specific, we shall assume that the final state μ and e must have energies greater than 600 MeV each (so that our particle identification system works reliably), the mass of the μ' is 1.5 GeV, and the SPEAR is operating at 2 GeV each beam. These three assumptions allow us to the calculate fraction of the

Figure 4. A portion of a paragraph on heavy lepton searches from the Mark I physics proposal.

> *The most unusual of the joint decay modes is that involving one μ and one e. To be specific, we shall assume that the final state μ and e must have energies greater than 600 MeV each....*

The actual value used in the search four years later was 650 MeV.

The one place the proposal was slightly optimistic was in its ending, which contains the sentence:

> *If such particles exist, it is hard to see how they can be missed.*

We will soon see that it was not quite that easy, and that our friends and competitors in Europe missed them for quite a while.

Now I have to tell you one last thing about this proposal. Of these four sections, the last, on heavy lepton searches, was the only one with which a modern reader would feel completely comfortable today. But, twenty-one years ago, it was quite the opposite. Most physicists considered the first three topics the "real proposal," and this last topic "a joke." I distinctly remember that as we were putting the proposal together in its final form, one senior member of the collaboration quipped,

> *Ha, heavy leptons! If Martin discovers that, we will let him publish it by himself.*

Four years later, that quip had been long forgotten, and almost everyone signed the paper (Fig. 5).

First Analysis

SPEAR had first collisions in April 1972, and took a sizeable amount of data from the spring of 1973 through the spring of 1974. The rf power available during this period allowed a maximum beam energy of about 2.6 GeV, but the practical maximum was 2.4 GeV, or 4.8 GeV in the center of mass, and a large block of data was taken at this energy.

Sometime in 1974, Martin Perl started looking at the 4.8 GeV data and constructed the table shown in Fig. 6 of two charged particles, each with a momentum greater than 650 MeV/*c*, and acoplanar by more than 20 degrees. The issue to be addressed here, as given in the proposal, was "Can these 24 events with an electron and a muon, but no photons be explained by conventional backgrounds?"

To most of us today, who are used to dealing with higher energies and modern detectors, this does not seem to be a very difficult question. To understand why it was not quite so simple to answer, we have to consider the lepton identification elements of the Mark I detector (Fig. 7).

The Mark I was a magnificent concept in terms of a general purpose detector,* and it clearly set the style of all such detectors that succeeded it; however, it was not a state-

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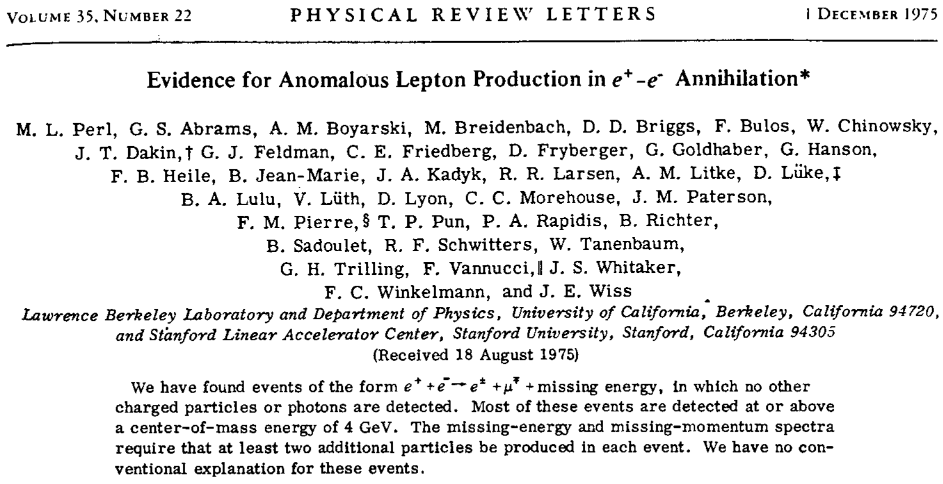
* The Mark I detector was called that only after the Mark II detector was built. During the whole time period of this talk, it was known by the awkward name of the "SLAC-LBL Magnetic Detector," and never had a snappy acronym.



VOLUME 35, NUMBER 22 PHYSICAL REVIEW LETTERS 1 DECEMBER 1975

Evidence for Anomalous Lepton Production in e^+-e^- Annihilation*

M. L. Perl, G. S. Abrams, A. M. Boyarski, M. Breidenbach, D. D. Briggs, F. Bulos, W. Chinowsky,
J. T. Dakin,† G. J. Feldman, C. E. Friedberg, D. Fryberger, G. Goldhaber, G. Hanson,
F. B. Heile, B. Jean-Marie, J. A. Kadyk, R. R. Larsen, A. M. Litke, D. Lüke,‡
B. A. Lulu, V. Lüth, D. Lyon, C. C. Morehouse, J. M. Paterson,
F. M. Pierre,§ T. P. Pun, P. A. Rapidis, B. Richter,
B. Sadoulet, R. F. Schwitters, W. Tanenbaum,
G. H. Trilling, F. Vannucci,‖ J. S. Whitaker,
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(Received 18 August 1975)

We have found events of the form $e^+ + e^- \rightarrow e^\pm + \mu^\mp$ + missing energy, in which no other charged particles or photons are detected. Most of these events are detected at or above a center-of-mass energy of 4 GeV. The missing-energy and missing-momentum spectra require that at least two additional particles be produced in each event. We have no conventional explanation for these events.

Figure 5. Title, byline, and abstract of the first paper (Ref 12).

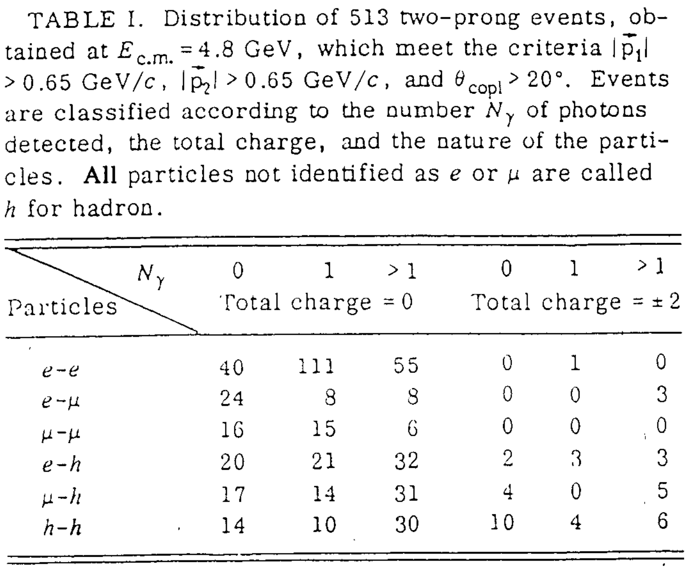

TABLE I. Distribution of 513 two-prong events, obtained at $E_{\mathrm{c.m.}} = 4.8$ GeV, which meet the criteria $|\vec{p}_1| > 0.65$ GeV/c, $|\vec{p}_2| > 0.65$ GeV/c, and $\theta_{\mathrm{copl}} > 20°$. Events are classified according to the number N_γ of photons detected, the total charge, and the nature of the particles. **All** particles not identified as e or μ are called h for hadron.

N_γ / Particles	0	1	>1	0	1	>1
	Total charge = 0			Total charge = ±2		
e-e	40	111	55	0	1	0
e-μ	24	8	8	0	0	3
μ-μ	16	15	6	0	0	0
e-h	20	21	32	2	3	3
μ-h	17	14	31	4	0	5
h-h	14	10	30	10	4	6

Figure. 6. A table from the first paper (Ref. 12).

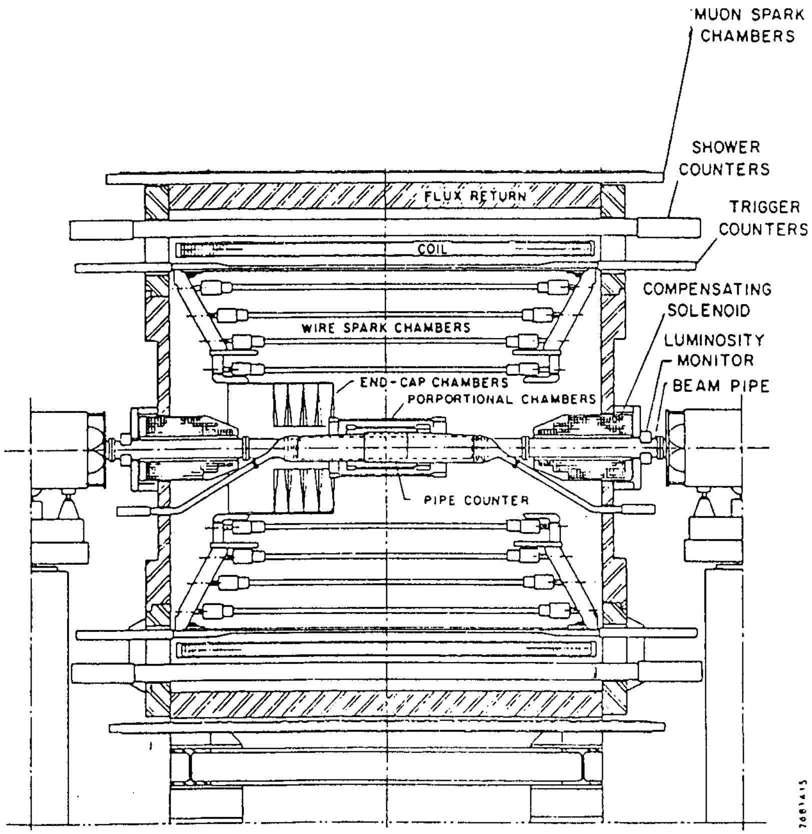

Figure 7. The Mark I detector prior to summer 1974.

of-the-art detector. I remember one discussion on detector issues that occurred at some workshop sometime long after these events. I was trying to make the point that detectors do not necessarily have to be state-of-the-art to be useful, and I used the Mark I as an example. The detector expert with whom I was speaking stuck his nose in the air and said,

> *The Mark I! Huh, it was obsolete before it was built.*

The electron and photon detectors consisted of twenty-four lead-scintillator shower counters, each of which stretched the full length of the detector and was viewed on each end by a single photomultiplier. The scintillators had been scratched in the construction process and had relatively short attenuation lengths. In fact, the attenuation was a factor of 50 from one end of the counter to the other.[29] The sole determination of whether a particle was an electron was a requirement that the pulse height be greater than about four times minimum ionizing.

The muon identification was equally weak. As in most detectors I know of, there was not enough money to provide for a proper muon identification system. The only thing we had was a 20-cm thick iron flux return with a couple of chambers outside. With the calorimeter and coil, this comes to 1.7 nuclear interaction lengths. If there were hits in the muon chambers lining up with a track, it was called a muon. Jim Dakin and I actually wrote a NIM article on how one does muon identification with a 20-cm absorber.[30] The answer was "Not very well."

So the problem that Martin faced was that, although there was no conventional process which could give a muon, an electron, and no other observed particles,* these events could occur through hadron misidentification, which was very probable in the Mark I detector.

The most straightforward way of estimating possible backgrounds was deliberately to overestimate the background by assuming that there were no anomalous sources of leptons in the three-or-more prong data (which of course there were, mainly due to charmed particles), and to use the number of identified electrons and muons in these data as a measure of the misidentification probability. Martin did this as a function of momentum, and found that, averaged over the momentum spectrum of the two-prong events, the average hadron misidentification to electrons and muons was 18% and 20%, respectively. He also had to consider the probability for an electron to be called a muon, or vice-versa. This was to allow for misidentified radiative electron and muon pair events in which the photon was missed. Fortunately, these probabilities were low, of order 1%.

Using this calculation, Martin determined that the expected background was 4.7 events. Even if we allow for some error in determining the background and increase this number to 7 events, the probability of it fluctuating to 24 is less than one in a mil-

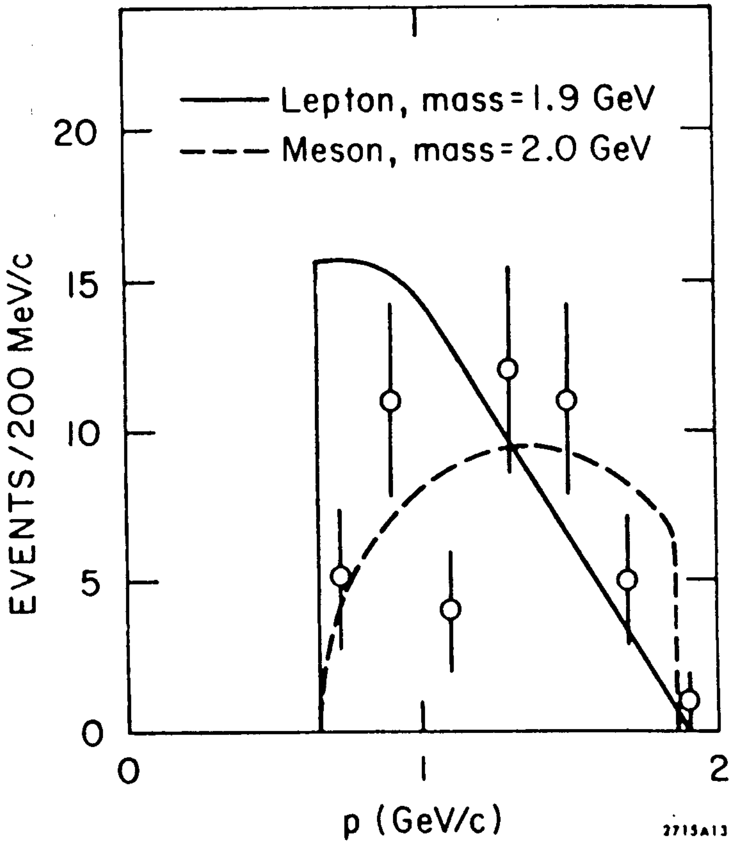

Figure 8. Momentum spectrum of leptons from the original 24 $e\mu$ events from the 4.8 GeV data. The solid and dashed curves represent the expectation of a 1.9 GeV/c^2 lepton and a 2.0 GeV/c^2 meson, respectively. (From Ref. 31.)

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* There is one conventional process which can give this signature, the two photon process, $e^+e^- \rightarrow e^+e^-\mu^+\mu^-$, where one electron and one muon go into the forward and backward directions, and are missed. This process is easily dismissed as a source for the anomalous events, because it gives equal numbers of like- and opposite-sign events, while the data are composed of only opposite-sign events.

lion. Thus, the real issue was not statistics, but whether the misidentifications had been properly determined.

Martin put all this together and then challenged me and many other people, in fact anyone in the collaboration who showed the slightest interest in these events, to see if they could find an error in his method. There were several other ways of determining the misidentifications, and everyone who studied the problem concluded that there was no way in which the events could be explained by backgrounds.

The next question that Martin addressed after the question of validity was "What are the possible sources of these events?" Two sources were possible: a meson (or more generally, a boson) decaying by a two-body decay,

$$e^+e^- \to M^+M^-,\quad M^+ \to e^+\nu,\quad M^- \to \mu^-\bar{\nu}$$

or a lepton decaying by a three-body decay,

$$e^+e^- \to L^+L^-,\quad L^+ \to e^+\nu\bar{\nu},\quad L^- \to \mu^-\bar{\nu}\nu\ .$$

Although one has to consider the mass and form of interaction, these are relatively unimportant, and it basically boils down to the fact that the lepton has one-third of the total energy in a three-body decay and one-half the total energy in a two-body decay. Figure 8, taken from Martin's first talk on these events,[31] shows the momentum distribution of the 48 leptons from the 24 events at 4.8 GeV. One could not tell statistically which hypothesis was correct. A close look at Fig. 8 shows that the meson hypothesis is actually favored, but we didn't stress that point publicly.

Going Public

After everyone had a chance to examine the data and check for errors — and this was a process that stretched out over several months — we went public in a series of summer conferences (Table 1), with the basic message of the first paper, namely that we had found some events that appeared to come from the production of a new particle in the mass range 1.6 to 2.0 GeV/c^2, but that we could not yet determine whether the particle was a lepton or a boson. Martin presented an extensive exposition of the data and analysis in a set of summer school lectures in Montreal.[31] These lectures became the standard reference for these data.

At the same time, I was dispatched to Europe to spread the word there, first at the neutrino conference at Lake Balaton in Hungary[35] and then at the European Physical Society meeting in Palermo, Sicily[36], which Nino Zichichi hosted in a most elegant way. My talks at these conferences covered a wide range of topics and concluded with a short discussion of the anomalous $e\mu$ events. As a young physicist, these conferences were a wonderful experience for me. The anomalous events were a topic of much discussion, and I remember being able to meet the Russian theorist Lev Okun for the first time at Lake Balaton and discuss these events with him.

Table 1. First Published Talks on the τ in the Summer of 1975.

Speaker	Dates	Meeting	Location	Ref.
M.L. Perl	Jun 16-21	McGill Summer School	Montreal	31
	Jul 7-10	Hadron Spectroscopy	Argonne	32
	Jul 21-31	SLAC Summer Institute	SLAC	33
	Aug 27-29	APS-DPF	Seattle	34
G.J. Feldman	Jun 12-17	Neutrino	Lake Balaton	35
	Jun 23-28	EPS High Energy	Palermo	36
	Aug 21-27	Lepton/Photon	Stanford	37

The conference at Argonne in July[32] was notable for one thing. At this conference, Martin gave the mystery particle a name — a capital U. The U was to stand for "unknown," since we didn't know what the particle was. This was supposed to be a temporary name, to be changed when we identified the particle.

I think that Martin was fond of the name, but I detested it and I don't think I kept my dislike of it a secret. I remember that Martin defended the name to me once with the following joke:

> *The advantage of the name is that if someone asks you what it is named for, you can say that it is named for you.*

This joke did not increase my affection for the name. How the τ finally got its present name comes later in our story.

The Tower of Power

To repeat, there were two major questions in the summer of 1975: first, were we making a systematic mistake in our misidentifications, or in other words, could we or others confirm these events, and second, assuming that our identification was correct, what was the nature of the particles we were producing? Let's consider the former question first.

Statistics was not an issue. Although we emphasized the analysis of the 24 events at center-of-mass energy 4.8 GeV, analyses of other energies yielded similar results, and in the first talks and in the first paper, we mentioned that adding up data from all energies, we had 86 events with 22 of them estimated to be background. The first internal confirmation came by the time of the Lepton-Photon Symposium held at Stanford in August 1975. The story of this confirmation takes us back in time a bit and actually had nothing to do with heavy leptons.

In April 1974, seven months prior to the discovery of the ψ, I attended the Meson Spectroscopy Conference held in Boston. On the final day of this conference, Shelly Glashow gave a talk in which he challenged the meson spectroscopists to find charm.[38] He ended his talk with these now famous statements:

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.*

Although there is no indication in the proceedings to that effect, I believe that at the next meson spectroscopy conference,[39] candy hats were passed around for all the participants to eat.*

In any case, I was impressed by this speech. I realized that charmed particles would decay to muons, and that we would need improved muon detection to identify them. Upon returning to Stanford, I discussed this with my colleagues, and we decided that the only place for additional absorbers was on the top of the detector. We would normally use iron, but it would take a fair amount of time to get iron, and, in any case, we didn't have the funds to buy any. Our chief engineer, Bill Davies-White, suggested that we make the absorber out of barium-loaded concrete, which has about half the density of iron. We quickly set up some casting pads, cast the concrete, mounted it on top of the detector, and borrowed a couple of chambers from the side of the detector for the readout (Fig. 9). This new detector was dubbed the "Tower of Power," named after a local rock group, but I usually just referred to it as the muon tower. The solid angle of the tower was quite small, but the hadron misidentification was quite low for a muon that passed completely through the absorbers.

With this preface, we can move to the Lepton-Photon Symposium in August 1975. This, of course, was the major international conference of the year. I would like to digress for a minute on this conference, since I know of no conference that ever had an opening with the impact of this one. By some combination of luck and planning, SLAC was hosting the conference and could set the order of the scientific program. The conference opened with three talks on results from the Mark I detector. First, Roy Schwitters showed the measurements of the total cross section.[41] A year earlier, at the international conference in London, the delegates had seen Burt Richter present the data shown in Fig. 10.[42] The data seemed to show R increasing monotonically with energy, and there was a great deal of speculation and confusion surrounding those results. When Roy showed the new data, shown in Fig. 11, one could literally look around the room and see people's jaws drop open in amazement.

Roy went on to discuss the discovery of transverse polarization of the beams and the newly discovered evidence for jet structure in the hadronic final states.

The second speaker was Gerry Abrams.[43] A year before, the ψ had not yet been discovered. Less than a year later, Gerry was able to discuss detailed measurements of the properties of the ψ and ψ', and to show long lists of branching ratios that had been measured.

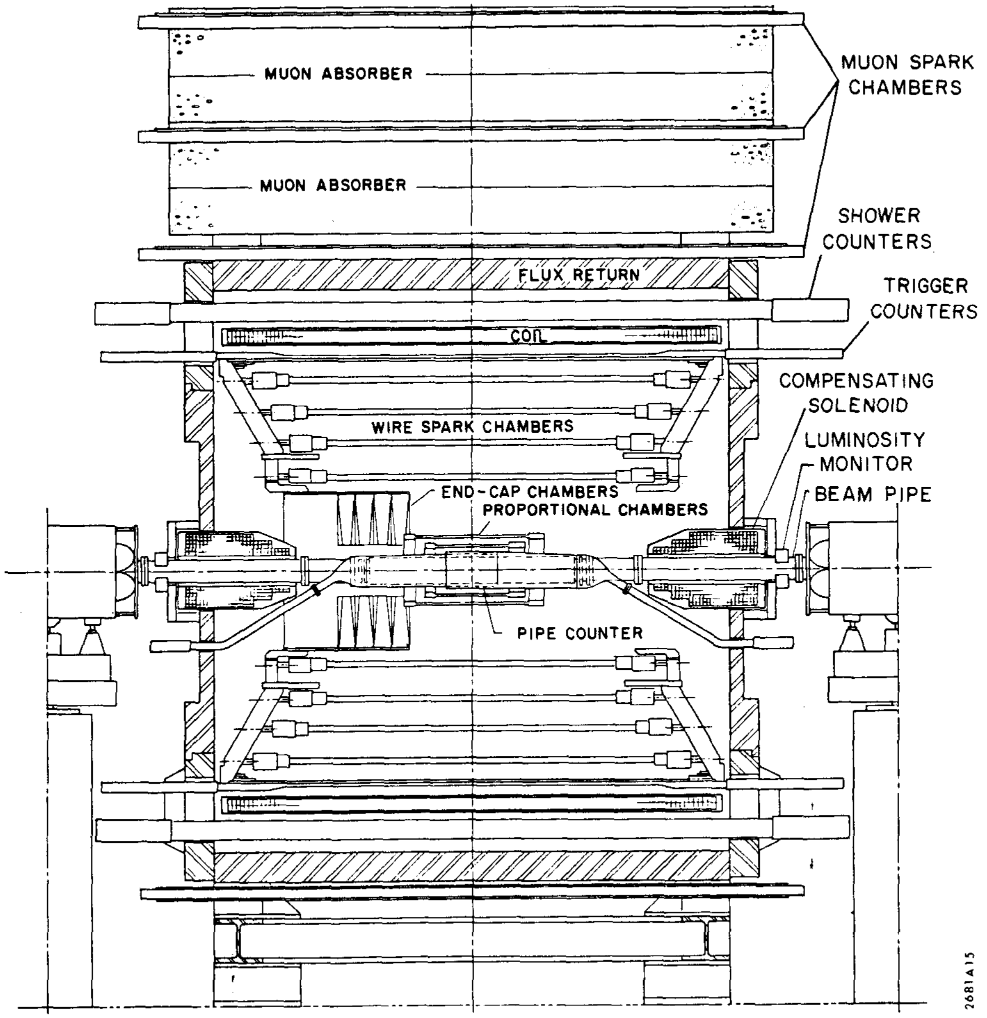

Figure 9. The Mark I detector after the addition of the muon tower in summer 1974.

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[*] The next meson spectroscopy conference was postponed from April 1976 to April 1977. If it had been held at is normal time, Shelly Glashow would have had to eat his hat, since the charm discovery did not come until June 1976.[40] The postponement was presumably to avoid this spectacle. I am indebted to Haim Harari for pointing this out to me.

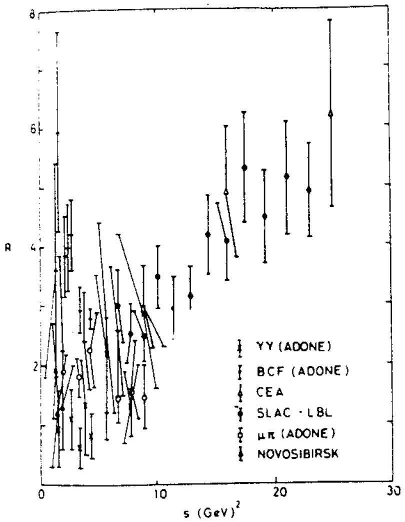


Figure 10. Measurements of R shown at the London conference, July 1974 (Ref. 42).

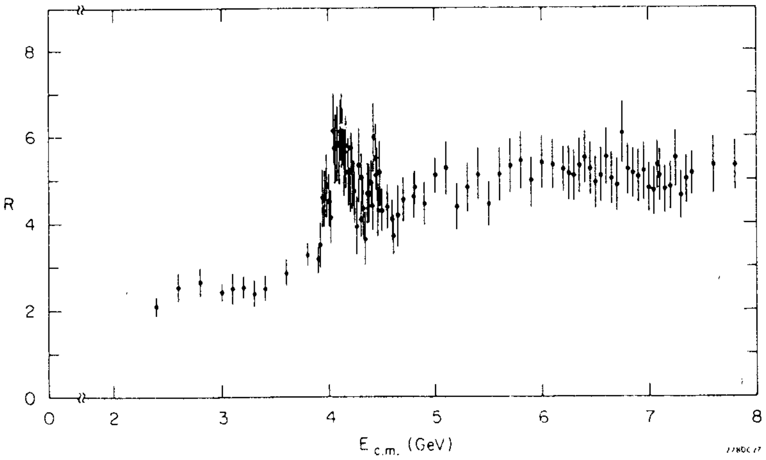


Figure 11. Measurements of R shown at the Stanford conference, August 1975 (Ref. 41).

I was the third speaker.[37] The first part of my talk dealt with the newly discovered χ states in both their radiative and hadronic decays. The second and concluding section of my talk dealt with the anomalous $e\mu$ events. The main focus of my remarks was that we now had some data from the muon tower, which had lower misidentification probabilities, and that these data were confirming our earlier results. There were five $e\mu$ events in which the muon penetrated at least half of the muon tower with an estimated background of 0.6 events. I was able to show an event (Fig. 12), in which the muon penetrated all three absorbers. It is as close as we ever came to a "golden event" in the Mark I detector. Still, outside confirmation was needed.

Identification

While there was nothing we could do about getting outside confirmation, we could address the second issue of the nature of the new particle. I can't place the date precisely, but at some point around the fall or winter of 1975, I was sitting at my desk, working on some problem unrelated to the anomalous lepton events. I am sure I had not given them any thought for some time, because I was taken completely by surprise when Martin Perl appeared at my door and said simply,

> *It's a heavy lepton.*

I responded with some sage comment such as

> *Oh, really?*

Martin invited me into his office and we went over the data and calculations that he had put together, which were to be the start of the argument of the second paper. The data set had grown from 86 events of which 22 were estimated to be background to 139 events with 34 of them background. Figure 13 shows the scaled momentum spectrum for three different energy bins. Martin had defined a scaled momentum variable ρ, such that each event could be plotted on scale of 0 to 1, 0 being the cut momentum of 650 MeV/c and 1 being the kinematic maximum.

The overall χ^2 distributions were correct for a three-body V-A decay, but totally unacceptable for any form of a two-body decay. The original data at 4.8 GeV had been an aberration. No other data set ever favored a two-body decay.

When the second paper (Fig. 14) was written the following summer, it continued with a tight argument, which is outlined in Fig. 15. If the decays were three-body, there were two missing particles in each decay. Could they be K_L's, photons, or charged particles? By comparing $e\mu$ events with these particles (and using K_S's as a substitute for K_L's, since they had to be the same), we could determine an upper limit on the number of anomalous $e\mu$ events which had missing hadrons or photons. This very conservative limit, obtained by adding all of the upper limits linearly, was 39%. Thus, missing particles had to be neutrinos, because that was the only thing left. Thus, each decay had to have a lepton and two missing neutrinos. The only particle with this signature was a heavy lepton.

I was always very pleased with this paper and its tight argument.

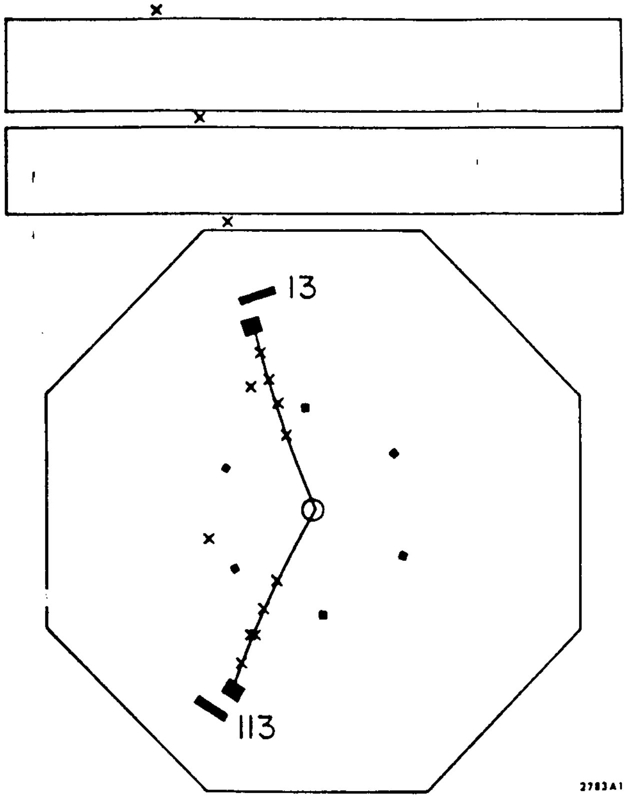


Figure 12. An $e\mu$ event in which the muon penetrates both layers of the muon tower. Shown at the Stanford conference, August 1975 (Ref. 37).

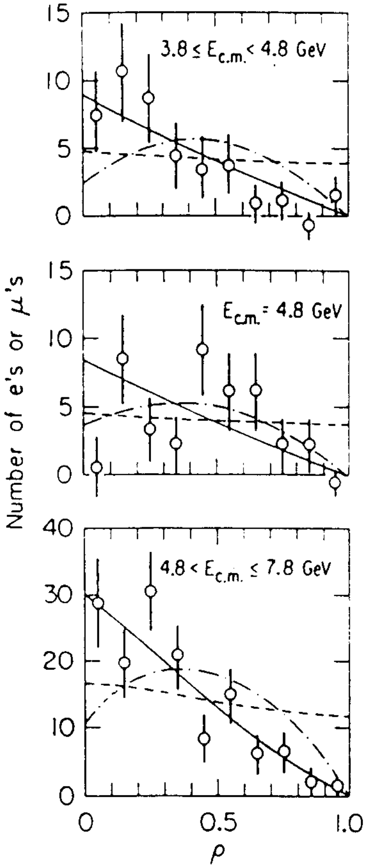
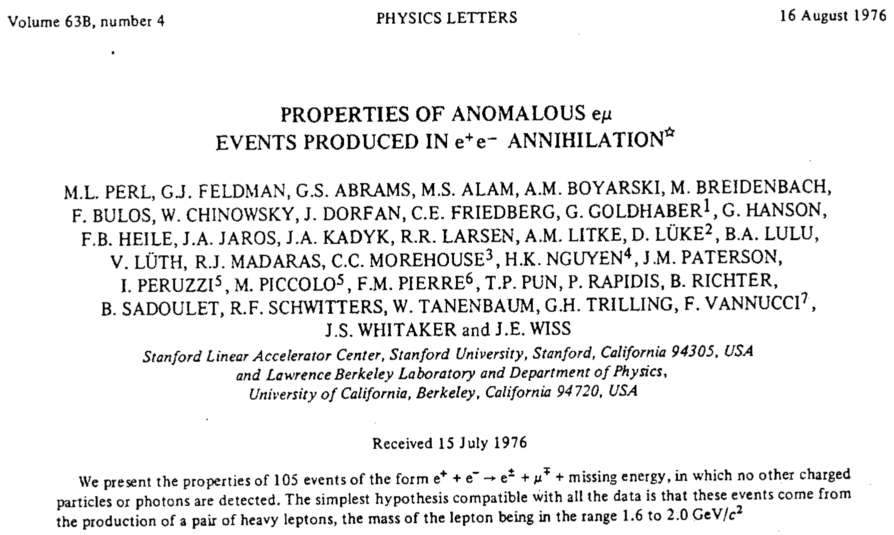


Figure 13. The scaled momentum spectrum of leptons from $e\mu$ events in three energy regions. The solid curve represents the expectation of a 1.8 GeV/c^2 lepton with V-A interactions. The dashed and dot-dashed curves represent the expectations from a 1.8 GeV/c^2 boson with spin 0 and spin 1, helicity 0, respectively. (From the second paper, Ref. 13.)



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PROPERTIES OF ANOMALOUS $e\mu$ EVENTS PRODUCED IN e^+e^- ANNIHILATION[☆]

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We present the properties of 105 events of the form $e^+ + e^- \rightarrow e^\pm + \mu^\mp$ + missing energy, in which no other charged particles or photons are detected. The simplest hypothesis compatible with all the data is that these events come from the production of a pair of heavy leptons, the mass of the lepton being in the range 1.6 to 2.0 GeV/c^2

Figure 14. Title, byline, and abstract from the second paper (Ref. 13).

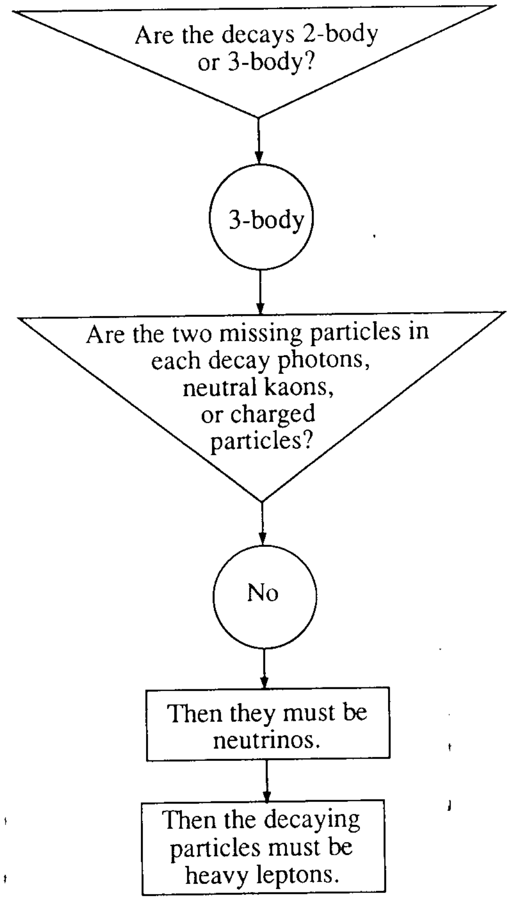

Figure 15. Outline of the second paper (Ref. 13).

Doubts and Uncertainty

With the submission of the second paper, one would think that July 1976 would have been the high point in the discovery of the τ. It was, in fact, the low point. Never, before or since, was the credibility of the τ as low. To understand why, consider the major international conference that year, which was the Rochester Conference held in Tbilisi, Georgia, in what was then the Soviet Union. Neither Martin Perl nor I attended this conference for personal reasons. Although there were two Mark I speakers in the parallel sessions of the conference, Gail Hanson and François Pierre, neither spoke about the anomalous lepton events.

The plenary speaker on new particle production was Bjorn Wiik.[44]* He presented our data well and then went on to discuss the confirming evidence, or lack of it. The Pavia-Princeton-Maryland group from the other SPEAR pit did have a positive result;[45] but they were suspect since they did their experiment only a few hundred feet from ours. Confirmation was needed from the two experiments that had been running for almost two years at DORIS.

One of them, Pluto, searched for inclusive muons in two-prong events. They did seem to have a few events, but not enough for a heavy lepton. They set a lower limit on the mass of a heavy lepton at 1.95 GeV/c^2, just barely compatible with our mass estimate of 1.6 to 2.0 GeV/c^2. Bjorn concluded,

> *From the present muon inclusive data there is no convincing evidence for the production of a new heavy lepton.*

The other DORIS experiment DASP had been searching for inclusive electrons. They found them, but they appeared to be coming from charmed particles, based on their momentum spectrum and event multiplicity. Bjorn concluded,

> *The DASP group ... excludes a heavy sequential lepton as the sole source of the events.*

Of course, both charm and the τ were in the data, so this statement was perfectly correct. Bjorn never claimed that the DASP data ruled out a heavy lepton, but there was certainly an implication that DASP was casting some doubt on its existence. Word got back to us that the discussion in the halls of the conference was worse. The argument I heard from people went like this:

> *Look, everyone knows that Martin Perl has always wanted to find a heavy lepton, and people find what they look for. We know charmed particles are in this mass region, and he is probably just confusing the leptons from the charmed particles with a heavy lepton signal.*

The argument was absurd. In the second paper, we had already ruled out a much more general case than charmed particles. We had ruled out any hadrons in the final state. We knew that there was no way that these anomalous events could come from charmed particles, and it was never a major concern of ours.

When these reports came back to Stanford, I told Martin that this was a terrible situation, and that, in the future, one of us should go to these major conferences to refute these kinds of statements. Martin gave me some fatherly advice:

> *No, it's not important. You see, that is the great thing about science. It doesn't matter what people think or say. The truth comes out in the end.*

Confirmation

Of course, Martin was right. In the year between the second and third papers, the truth began to emerge. We published our work on inclusive muons from the muon tower,[46] and also events with much better electron identification from a lead-glass wall which had been added to the Mark I in collaboration with a new group from LBL headed by Lina Galtieri (Fig. 16).[47] However, as far as the rest of world was concerned, it was the confirmation from DORIS that mattered. In May 1977, the Pluto experiment decided that their inclusive muon measurements were consistent with a heavy lepton and, in fact, provided positive evidence for one.[48] And in June 1977, in a paper entitled "On the Origin of Inclusive Electron Events in e^+e^- Annihilation between 3.6 and 5.2 GeV," the DASP Collaboration decided that there were actually two components to the inclusive electron spectrum, one consistent with coming from charmed particles, and the other one not.[49] At the next international conference, the Lepton-Photon Symposium in Hamburg in August 1977, Martin Perl reviewed the data on the τ and was able to conclude that its existence was no longer in doubt.[50]

A Proper Name

As we approached the writing of the third paper, I realized that this was the last chance for the τ to get a proper name. You will remember that it was still being called the *U* particle at this time. I reminded Martin that *U* stood for unknown and that it was meant to be a temporary name. Now that we had identified it as a heavy lepton, the name should be changed to one that reflected that identity. And we had to do it now, because if we published one more paper with the name *U*, it would stick forever.

There was some discussion within the collaboration over this point, because some members of the collaboration felt that once a name was given, no matter how illogical it was, it should not be changed. They pointed out that many particle names made no sense. (This was before the Particle Data Group rationalized the meson- and baryon-naming conventions.[51]) However, Martin agreed that the name should be changed, and we began searching for a proper name.

Everyone felt that a lower case Greek letter was called for, in analogy with the μ. The problem was that most good Greek letters were already in use. The iota was not

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* Roy Schwitters was also a plenary speaker, but for other aspects of the data.

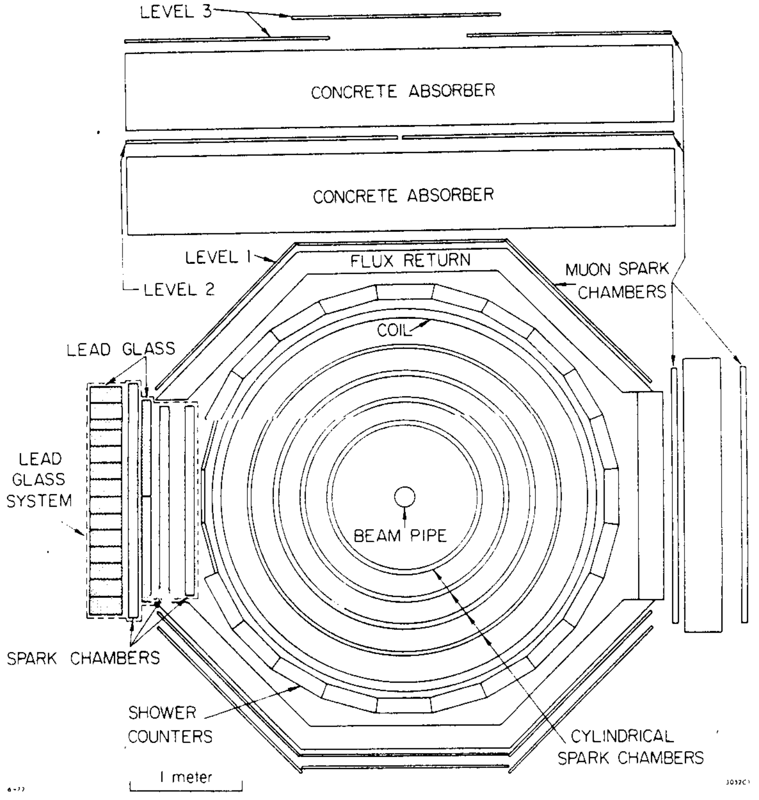


Figure 16. The Mark I detector with the lead-glass wall added in 1976.

used yet (although we would later use it for the name of a meson[52]), but it was clearly too insignificant a name for such a grand particle. The omicron was not used but it was useless, since it could not be distinguished from an "oh," or worse, a zero. To make a long story short, the finalists were λ and τ.

Each had an argument for it and against it, as shown in Table 2. The argument for λ was that it had not yet been used for any particle. The argument against was that it was a useful symbol to represent a generic lepton, although I have to admit to never having seen it used this way, either before or since. The argument for τ was that it had a meaning. τ was to stand for τριτον, the Greek word for "third." (Having a Greek graduate student, Petros Rapidis in this case, is very handy when it comes to naming particles.) The argument against was that τ had already been used for the three pion decay of the *K* meson, as in the "τ–θ puzzle." There was a fair amount of concern over this point, but it was decided that Greek letters were too valuable not to be recycled when they became obsolete.

Table 2. Heavy Lepton Name Candidates

	λ	τ
Pro	Not previously used.	Has meaning: τ for "τριτον," meaning "third."
Con	Should be saved for a generic symbol for a lepton.	Previously used for the three-pion decay of the kaon, as in "τ–θ puzzle."

In the process of making this decision, we asked our group secretary, Karen Goldsmith, for her technical opinion. She would have to type symbols such as m_λ or m_τ Which would be more esthetic? She opted for τ, and I remember this as the final piece of evidence that caused us to adopt τ as the name.

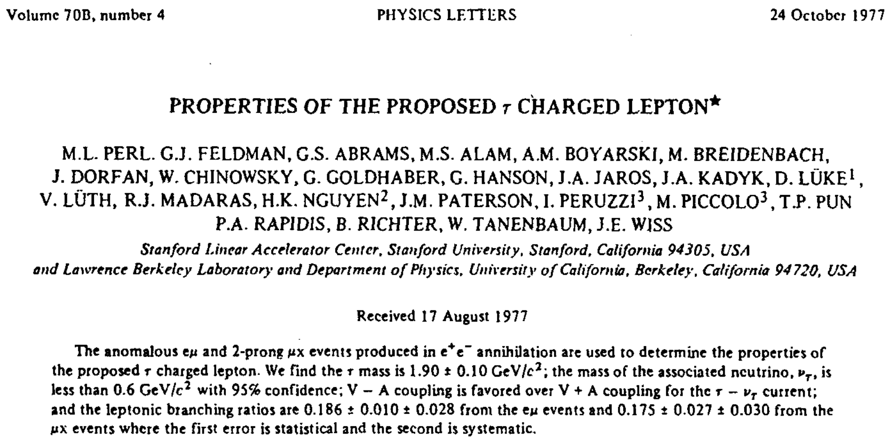
Martin Perl introduced the name to the world at the Rencontre de Moriond,[53] which was held in March 1977 at Flaine, in the French Alps. Although there had been fights over names during this period, the J[54] and the ψ[55], and the χ[8] and the P_c,[56] given the history, there was no question of priority here. Martin received word from a senior physicist at DORIS, who said,

> *We will call it anything you say.*

The name quickly caught on, and by the time of submission of the third paper, there was no need to explain it. However, we stuck it prominently in the title, just so that it would not be missed (Fig.17).

Transition

The third paper marks the end of the discovery of the τ, and is transitional to the next period, the detailed study of τ properties. Unlike the first two papers, which only dealt with *eμ* events, this paper also included the two-prong inclusive muon events from the muon tower. It presented measurements of τ properties, not only for their



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PROPERTIES OF THE PROPOSED τ CHARGED LEPTON★

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The anomalous $e\mu$ and 2-prong μx events produced in e^+e^- annihilation are used to determine the properties of the proposed τ charged lepton. We find the τ mass is 1.90 ± 0.10 GeV/c^2; the mass of the associated neutrino, ν_τ, is less than 0.6 GeV/c^2 with 95% confidence; V − A coupling is favored over V + A coupling for the $\tau - \nu_\tau$ current; and the leptonic branching ratios are $0.186 \pm 0.010 \pm 0.028$ from the $e\mu$ events and $0.175 \pm 0.027 \pm 0.030$ from the μx events where the first error is statistical and the second is systematic.

Figure 17. Title, byline, and abstract of the third paper (Ref. 14).

own value, but also as a way of verifying that the τ was a sequential heavy lepton, the partner of the electron and muon.

The mass was measured three ways, from a pseudo-transverse momentum, from the acoplanarity angles, and from the inclusive momentum spectrum. The mass measurement gave 1.9 ± 0.1 GeV, 1.2 standard deviations above today's accepted value. However, the three measurements served another purpose. They would be consistent only if we had the right hypothesis. In fact, they were consistent for a V-A interaction, but not for a V+A interaction. The direct measurement of the momentum spectrum also ruled out V+A.

We set an upper limit on the mass of the τ neutrino at 600 MeV/c^2. It is curious to note that the precise value of the τ neutrino mass is a hot topic today. If its value is about eight orders of magnitude lower than our upper limit, then it will account for most of the mass of the universe.[57]

Finally, we used the trick that the $e\mu$ cross section is proportional to the square of the leptonic branching ratio, while the inclusive muon cross section is linearly dependent on it, to measure the total cross section for the production of τ's. The result was an R value of 0.9 ± 0.4, in complete agreement with the notion that the τ is a point particle.

The contemporary literature gives evidence of the transitional nature of this period. I was asked to be the plenary speaker on e^+e^- annihilation at the Rochester conference in Tokyo in 1978.[58] I chose to spend most of my time reviewing the growing data on τ properties, but I started with a brief review of the history of the τ to 1976, then continued:

> *This was the state of the τ at the last conference in this series. All of the evidence for a new lepton came from a single experiment and one that admittedly had poor lepton identification. Independent confirmation was badly needed. It came during the following year from the PLUTO and DASP experiments.*
>
> *It is clear that at this conference we are entering a new stage in the history of the τ. Its existence and general identification are accepted and we are beginning the detailed measurements of its properties.*

Acknowledgements

Thanks to many of the Mark I collaborators for helping to jog my memory of events that took place 15 to 17 years ago, to Martin Perl for sharing his historical records with me, and to the organizers of this symposium for throwing a great party.

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