REFLECTIONS ON THE DISCOVERY OF THE TAU LEPTON

Nobel Lecture, December 8, 1995

by

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First thoughts

My first thoughts in writing this lecture are about the young women and young men who are beginning their lives in science: students and those beginning scientific research. I have been in experimental scientific research for 45 years; I have done some good experiments of which the best was the discovery of the tau lepton; I have followed research directions that turned out to be uninteresting; I have worked on experiments that failed. And so while recounting the discovery of the tau for which I have received this great honor, I will try to pass on what I have learned about doing experimental science.

I begin my reflections by going back in time before the tau, before even my interest in leptons. I was trained as an engineer at Polytechnic University (then the Polytechnic Institute of Brooklyn) and I always begin the design of an experiment with engineering drawings, with engineering calculations on how the apparatus is to be built and how it should work. My strong interest in engineering and in a mechanical view of nature carried over into my career in physics.

My doctoral thesis research (Pert, Rabi, and Senitzky 1955) was carried out at Columbia University in the early 1950's under Professor Isidor Rabi. The research used the atomic beam resonance method invented by Rabi, for which he received a Nobel Prize in 1944. My experimental apparatus, Fig. 1, was boldly mechanical with a brass vacuum chamber, a physical beam of sodium atoms, submarine storage batteries to power the magnets - and in the beginning of the experiment, a wall galvanometer to measure the beam current. I developed much of my style in experimental science in the course of this thesis experiment. When designing the experiment and when thinking about the physics, the mechanical view is always dominant in my mind. More important, my thinking about elementary particles is physical and mechanical. In the basic production process for tau leptons

$$e^{+} + e^{-} \to \tau^{+} \; t \; \tau^{-}. \qquad (1)$$

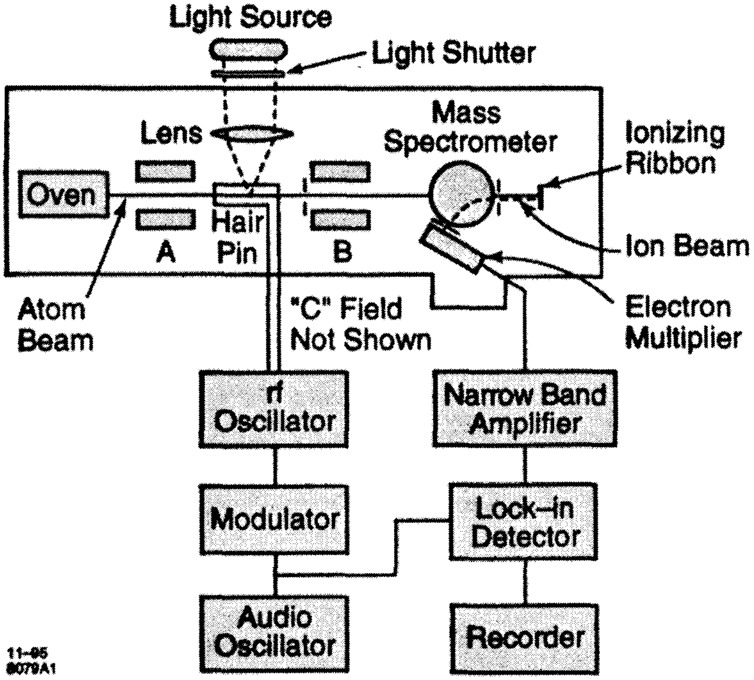


Figure 1. From the author's Ph.D. thesis experiment in atomic beams (Perl, Rabi, and Senitzky, 1955) The caption read:

> "Schematic drawing of the apparatus. The light source is shown on the side of the apparatus for clarity, hut it actually lies above the apparatus. The C magnet, which produce a homogeneous field in the "hair-pin," is also not shown for clarity. The six external boxes which represent the major electronic components do not indicate the physical position of the components."

I see the positron, e^{+}, and electron, e^{-}, as tiny particles which collide and annihilate one another. I see a tiny cloud of energy formed which we technically call a virtual photon, $\gamma_{\mathrm{virtual}}$; and then I see that energy cloud change into two tiny particles of new matter-a positive tau lepton, τ^{+}, and a negative tau lepton, τ^{-}.

In my thesis experiment I first experienced the pleasures, the anxieties, and sometimes the pain, that is inherent in experimental work: The pleasure when an experiment is completed and the data safely recorded, the anxiety when an experiment does not work well or breaks, the pain when an experiment fails or when an experimenter does something stupid. In my thesis experiment the acquisition of a set of data took about a day, and so there were several alternating periods of anxiety and pleasure within a week. When I broke a McCloud vacuum gauge and spread mercury inside the vacuum chamber, the pain of restoring the apparatus lasted but a few weeks. At the other extreme, in the discovery of the tau the ups and downs of my emotions extended over years. This brings me to the research which led me to think about heavy leptons.

From strong interactions to the electron-muon problem

In eight wonderful and productive years at the University of Michigan, I learned the experimental techniques of research in elementary particle physics (scintillation counters, bubble chamber, trigger electronics, and data analysis) working with my research companions, Lawrence Jones, Donald Meyer, and later Michael Longo. We learned these techniques together, often adding our own new developments. One of the most pleasurable experiences was the development of the luminescent chamber, Fig. 2, by Jones and me with the help of our student Kwan Lai (Lai, Jones, and Perl 1961). We photographed and recorded the tracks of charged particles in a sodium iodide crystal using primitive electron tubes which intensified the light coming from the track.

I worked in the physics of strong interactions. Jones and I, using spark chambers, carried out at the Bevatron a neat set of measurements on the elastic scattering of pions on protons (Damouth, Jones, and Perl 1963; Perl, Jones, and Ting 1963). Later, after I left the University of Michigan for Stanford University, Longo and I, working with my student Michael Kreisler, initiated a novel way to measure the elastic scattering of neutrons on protons (Kreisler et al. 1966).

These elastic scattering experiments pleased me in many ways. The equipment was bold and mechanical, with large flashing spark chambers and a camera with a special mechanism for quick movement of the film. Data acquisition was fast, and the final data was easily summarized in a few graphs, but I gradually became dissatisfied with the theory needed to explain our measurement. I am a competent mathematician but I dislike complex mathematical explanations and theories, and in the 1950's and 1960's the theory of strong interactions was a complex mess, going nowhere. I began to think about the electron and the muon, elementary particles which do not partake in the strong interaction.

The electron was discovered in the late nineteenth century: the final characterization of its nature was achieved by J. J. Thomson in the 1890's. He received a Nobel Prize in 1906 for investigation of electrical conduction in gases. The muon was found in cosmic rays in the 1930's. Table 1 lists their properties as known in the 1960's; this table is still correct today

Table 1. Properties of the electron and muon. The electric charge is given in units of 1.60×10^{-19} coulombs. The mass is given in units of the mass of the electron 9.11×10^{-31} kilograms.

Particle	**Electron**	**Muon**
Symbol	e	μ
Electric charge	+1 or −1	+1 or −1
Mass	1	206.8
Does particle have electromagnetic interactions?	yes	yes
Does particle have weak interactions?	yes	yes
Does particle have strong interactions?	no	no
Associated neutrino	ν_e	ν_μ
Associated antineutrino	$\bar{\nu}_e$	$\bar{\nu}_\mu$
Lifetime	Stable	2.2×10^{-6} sec

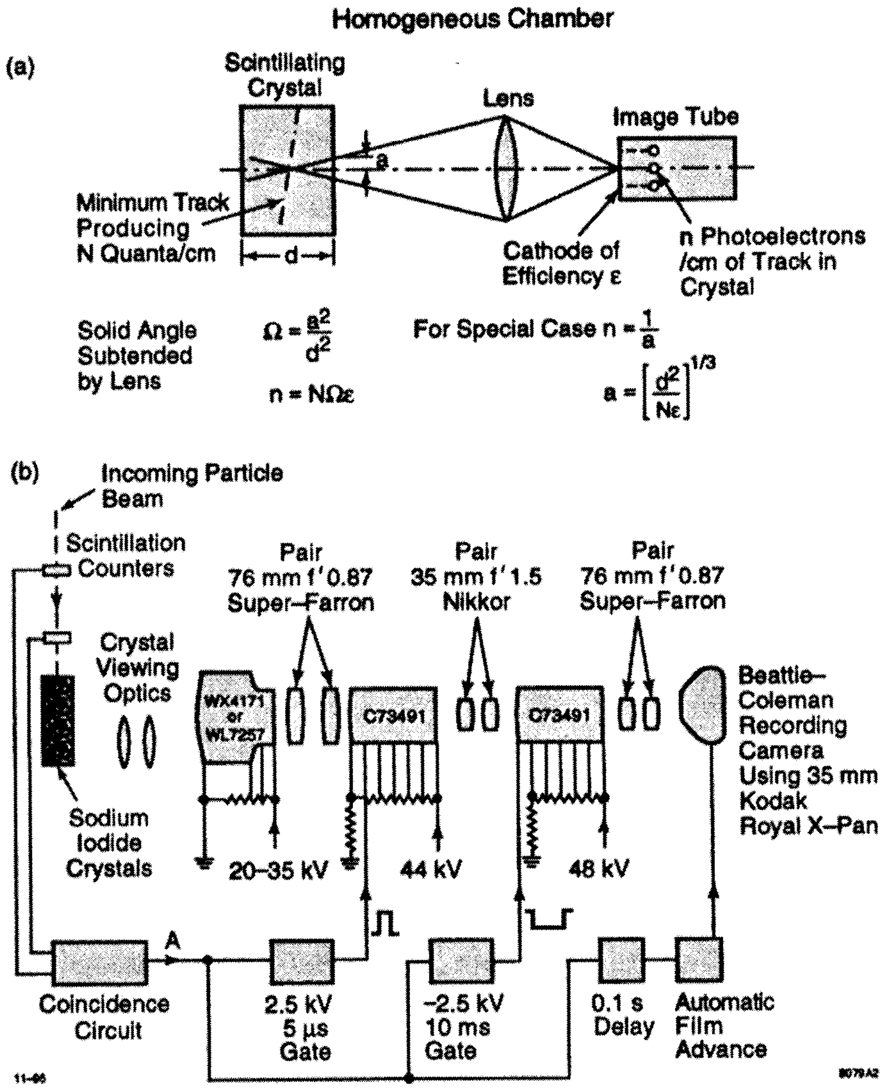


Figure 2. A novel track detector, the luminescent chamber, developed by Lawrence Jones and the author (Lai, Jones, and Perl, 1961) before the advent of the optical spark chamber. The caption read:

> "(a) Relationships between track resolution, a, depth of field, d, and track information, II, for the homogeneous luminescent chamber. For NaI(T1) in which we have N=lO^{5}, n=1.7 mm for d=lO cm and n=lO photoelectrons per cm of track in the crystal.
>
> (b) Schematic diagram of the luminescent chamber system currently in use. The chamber-viewing optics and beam-defining scintillation counters are oversimplified and generalized in this diagram."

There were two puzzles about the relation between the electron and the muon. First, as shown in the table, the properties with respect to particle interactions are the same for the electron and the muon, but the muon is 206.8 times heavier. Why? The second puzzle is that since the muon is unstable, with an average lifetime of 2.2 $X10^{-6}$seconds decay to an electron, one expects that the decay process would be

$$\begin{aligned} &\mu^- \rightarrow e^- + \gamma \\ &\mu^+ \rightarrow e^+ + \gamma. \end{aligned} \qquad (2)$$

Here γ means a photon, and the expectation would be that the γ carries off the excess energy produced by the difference between the muon mass and the electron mass - but this is not the nature of the muon or the electron. The muon decays to an electron by a complicated process,

$$\begin{aligned} &\mu^- \rightarrow e^- + \nu_e + \nu_\mu \\ &\mu^+ \rightarrow e^+ + \nu_e + \bar{\nu}_\mu, \end{aligned} \qquad (3)$$

in which a neutrino and an antineutrino are produced. There is something in the nature of the muon which is different from the nature of the electron. By the late 1950's, there was the electron-muon problem (e-p problem) with two parts:

* Why is the muon 206.8 times heavier than the electron?
* Why doesn't the muon decay through the process $\mu \rightarrow e + \gamma$

While I was at the University of Michigan, I was intrigued by the careful measurements being made on the (g-2) of the muon by Charpak et al. (1962) at CERN, and on the (g-2) of the electron by Wilkinson and Crane (1963) at Michigan. I was also interested in the precision studies of positronium and muonium then in progress, as well as other precision atomic physics experiments. These low energy studies of the charged leptons were in very capable hands, and I could not see how I could contribute.

I knew about the pioneer low energy, neutrino experiments of Frederick Reines and Clyde Cowan, Jr. I must interrupt my narrative to quote two momentous sentences from Reines and Cowan (1953):

> "An experiment has been performed to detect the free neutrino. It appears probable that this aim has been accomplished although further confirmatory work is in progress."

These were extraordinarily difficult experiments, and again I could not see how I could contribute.

I am honored to share this year's Nobel Prize in Physics with Frederick Reines, and I am sad that Clyde Cowan, Jr. is not alive to share this honor.

As for high energy neutrino experiments, they were already being carried out by the powerful set of Nobel Laureates, Leon Lederman, Melvin Schwartz and Jack Steinberger (Danby et al. 1962).

I reflected that it would be most useful for me to consider high-energy experiments on charged leptons, experiments which might clarify the nature of the lepton or explain the electron-muon problem. This is a research strategy that I have followed quite a few times in my life. I stay away from lines of research where many people are working, and in particular I stay away from lines of research where very smart and competent people are working. I find it more comfortable to work in uncrowded areas of physics.

I caution the young scientist about this advice. Almost all the time the best experimenters and the most experimenters work in the most fruitful area. If there are few or no investigators working on a problem, it may be an unproductive problem. In the end, it is a question of temperament and comfort.

SLAC, leptons, and heavy leptons

In 1962, the opportunity arose to think seriously about high-energy experiments on charged leptons when Wolfgang K. H. Panofsky and Joseph Ballam offered me a position at the yet-to-be-built Stanford Linear Accelerator Center (SLAC). Here was a laboratory which would have primary electron beams; a laboratory at which one could easily obtain a good muon beam; a laboratory in which one could easily obtain a good photon beam for production of lepton pairs. And on the Stanford campus at the High Energy Physics Laboratory, the Princeton-Stanford $e^- e^-$ storage ring was operating (O'Neil et al. 1958, Barber et al. 1966).

When I arrived at SLAC in 1963, I began to plan various attacks on, and investigations of, the electron-muon problem. Although the linear accelerator would not begin operation until 1966, my colleagues and I began to design and build experimental equipment. The proposed attacks and investigations were of two classes. In one class, I proposed to look for unknown differences between the electron and the muon; the only known differences being the mass difference and the observation that the decay reaction $\mu \rightarrow e + \gamma$ does not occur. The other class of proposed attacks and investigations was based on my speculation that there might be more leptons similar to the electron and the muon, unknown heavier charged leptons. I dreamed that if one could find a new lepton, the properties of the new lepton might teach us the secret of the electron-muon puzzle.

My first attack used an obvious idea. An intense photon (γ) beam could be made at SLAC using the reactions.

$$e^- + \text{nucleus} + \gamma + \ldots. \qquad (4)$$

The photons so produced could then interact with another nucleus to produce a pair of charged particles , x^+ and x^-,

$$\gamma + \text{nucleus} \rightarrow x^+ + x^- \text{t}\ldots. \qquad (5)$$

Any pair of charged particles could be produced if the γ had enough energy. To the young experimenter, I remark that there is nothing wrong with an obvious experimental idea as long as you are the first to use the idea.

My hope was that we would find a new x particle, perhaps a new charged lepton somehow related to the electron or muon. A vague hope by the standards of our knowledge of elementary particle physics today. We were certainly naive in the 1960's.

We didn't find any new leptons or any new particles of any kind (Barna et al. 1968); as we now know, there were no new particles to find given the experimental limitations of this search experiment. The search used the pair-production calculations of Tsai and Whitis (1966); this experiment was the beginning of a long and fruitful collaboration between my colleague Y-S (Paul) Tsai and myself.

Studies of muon-proton inelastic scattering

Although this first attempt to penetrate the mysteries of the electron and muon failed, we were already preparing to study muon-proton inelastic scattering

$$\mu^- + p \rightarrow \mu^- + \text{hadrons}$$

to compare it with electron - proton inelastic scattering,

$$e^- + p \rightarrow e^- + \text{hadrons} \ ,$$

Extensive studies of e-p inelastic scattering were planned at SLAC. Indeed, some of those studies led to the 1990 Nobel Physics Prize being awarded to Jerome Friedman, Henry Kendall, and Richard Taylor. My hope was that we would find a difference between the μ and e other than the differences of mass and lepton number. In particular, I hoped that we would find a difference at large momentum transfers - another naive hope when viewed by our knowledge today of particle physics. For example, I speculated (Perl 1971) that the muon might have a special interaction with hadrons not possessed by the electron, see Fig. 3.

It is always a good plan for a speculative experimenter to have two experiments going, or at least one going and one being built. Of course, that was easier in the 1960's than it is now, since most modern high-energy physics

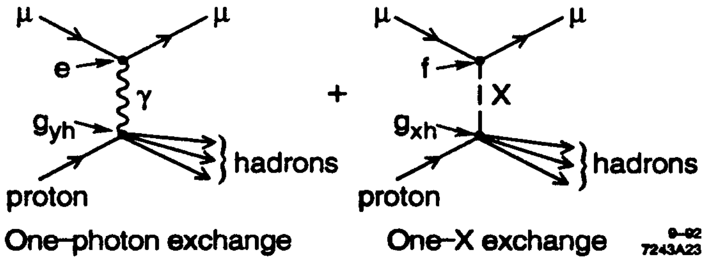


Figure 3. From Perl (1971): "The interaction of a muon with hadrons through exchange of a particle X, an example of the speculation that the muon has a special interaction with hadrons that is not possessed by the electron."

experiments are so large and complicated. Still, it can be done. My main present research is in tau physics, working with the CLEO Collaboration which uses the 10 GeV electron-positron collider CESR at Cornell University. But there is also a small nonaccelerator experiment at SLAC in which a few graduate students (Eric Lee, Nancy Mar, Manuel Ortega), a few colleagues, and myself are searching for free quarks.

Returning to the late 1960's, my colleagues and myself measured the differential cross sections for inelastic scattering of muons on protons, and then compared the μ-p cross sections with the corresponding e-p cross sections (Toner et al. 1972, Braunstein et al. 1972). We were looking for a difference in magnitude, or a difference in behavior of the cross sections. As discussed in Per1 and Rapidis (1974), these differences could come from a new non-electromagnetic interaction between the μ and hadrons or from the μ not being a point particle. However as summarized in Toner et al. (1972), we found no significant deviation.

Other experimenters studied the differential cross section for μ-p elastic scattering and compared it with e-p elastic scattering (Ellsworth et al. 1960, Camilleri et al. 1969, Kostoulas et al. 1974), but statistically significant differences between μ-p and e-p cross sections could not be found in either the elastic or inelastic case. Furthermore, there were systematic errors of the order of 5 or 10% in comparing μ-p and e-p cross sections because the techniques used were so different.

Experimental science is a craft and an art, and part of the art is knowing when to end a fruitless experiment. There is a danger of becoming obsessed with an experiment even if it goes nowhere. I avoided obsession and gave up. That turned out to be a good decision because modern experiment has shown that the scattering experiment does not illuminate any differences between the electron and the muon beyond the mass difference.

Heauy leptons in the 1960's

While building the apparatus using our muon-proton inelastic scattering experiment, and during the first operation of that experiment, I was thinking of another way to look for new charged leptons, L, using the reaction,

$$e^+ + e^- \rightarrow L^+ t L^- .$$

Before turning to this third attack on the electron-muon problem, I describe the general thinking in the 1960's about the possible existence and types of new leptons. By the beginning of the 1960's, there were papers on the possibility of the existence of charged leptons more massive than the e and μ. I remember reading the 1963-1964 papers of Zel'dovich (1963), of Lipmanov (1964), and of Okun (1965). Since the particle generation concept was not yet an axiom of our field, older models of particle relationships were used. For example, if one thought (Low 1965) that there might be an electromagnetic excited state e* of the e, then the proper search method was

$$\begin{aligned} e^- + \text{nucleon} &\to e^{-*} + \ldots, \\ e^{-*} &\to e^- + \gamma . \end{aligned} \tag{6}$$

If one thought (Lipmanov 1964) that there was a μ' which was a member of a μ, ν_μ, μ' family, then the proper search method was

$$\nu_\mu + \text{nucleon} \to \mu^{-\prime} + \ldots . \tag{7}$$

It is interesting to note, in view of the search a decade later for $\tau^- \to \nu_\tau \pi^-$, that Lipmanov (1964) calculated the branching fraction for this decay mode.

By the second half of the 1960's, the concept had been developed of a heavy lepton L and its neutrino ν_L forming an L, ν_L, pair. Thus, in a paper written in 1968, Rothe and Wolsky (1969) discuss the lower mass limit on such a lepton set by its absence in *K* decays. They also discuss the decay of such a lepton into the modes

$$L \to e\,\nu_e \nu_L,\quad \mu\nu_\mu\nu_L,\ \pi\nu_L .$$

Electron-positron colliding beams and sequential leptons

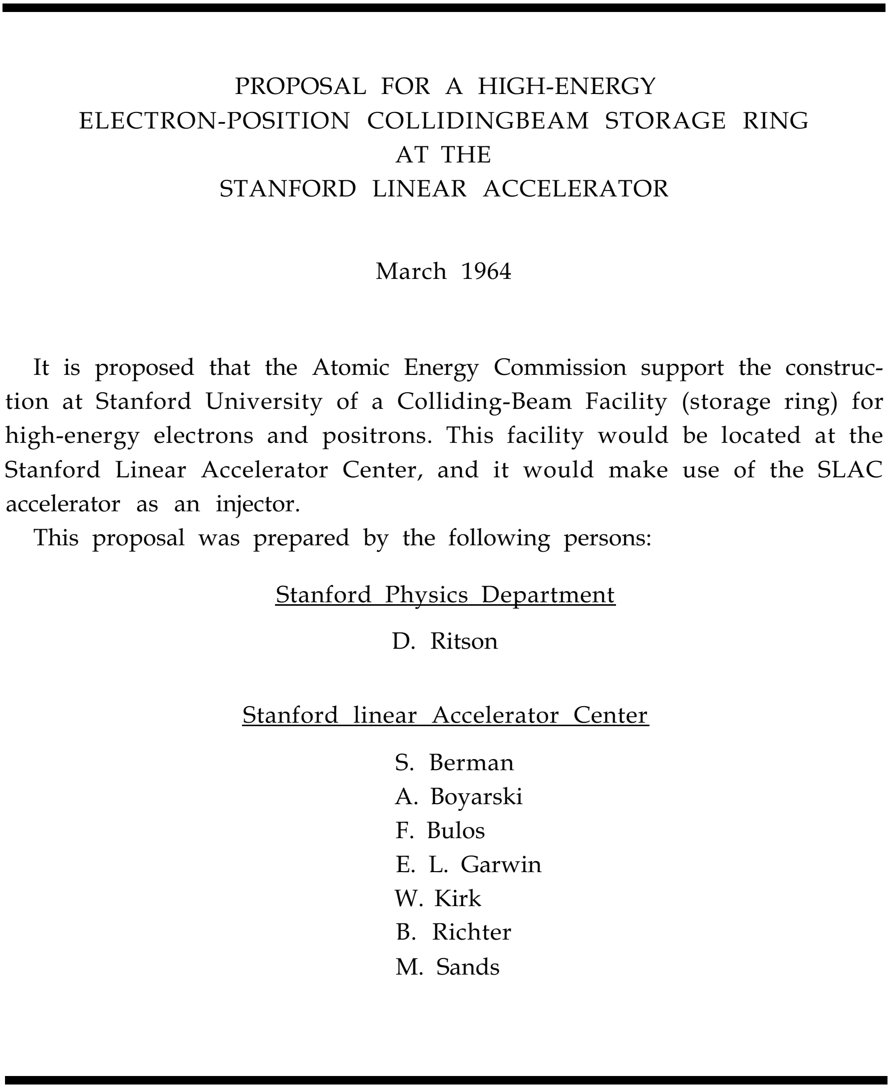
The construction and operation of electron-positron colliders began in the 1960's (Voss 1994). By September 1967 at the Sixth International Conference on High Energy Accelerators, Howard (1967) was able to list quite a few electron-positron colliders. There was the pioneer 500 MeV ADA collider already operated at Frascati in the early 1960's and, also at Frascati, ADONE was under construction. The 1 GeV ACO at Orsay and 1.4 GeV VEPP-2 at Novosibirsk were in operation. The 6 GeV CEA Collider at Cambridge was being tested, and colliders had been proposed at DESY and SLAC (Ritson et al. 1964).

The 1964 SLAC proposal (Ritson et al. 1964), see Fig. 4, had already discussed the reaction

$$e^+ t\ e^- \to x^+ t\ x^- . \tag{8}$$

Of course, x might be a charged lepton. This proposal did not directly lead to the construction of an e^+e^- collider at SLAC because we could not get the funding. About five years later - with the steadfast support of the SLAC director, Wolfgang Panofsky, and with a design and construction team led by Burton Richter-construction of the SPEAR e^+e^- collider was begun at SLAC.

It was this 1964 proposal and the 1961 seminal paper of Cabibbo and Gatto (1961) entitled, "Electron-Positron Colliding Beam Experiments," which focused my thinking on new charged lepton searches using an e^+e^- collider. As we carried out the experiments described previously, I kept loo-



PROPOSAL FOR A HIGH-ENERGY
ELECTRON-POSITION COLLIDINGBEAM STORAGE RING
AT THE
STANFORD LINEAR ACCELERATOR

March 1964

It is proposed that the Atomic Energy Commission support the construction at Stanford University of a Colliding-Beam Facility (storage ring) for high-energy electrons and positrons. This facility would be located at the Stanford Linear Accelerator Center, and it would make use of the SLAC accelerator as an injector.

This proposal was prepared by the following persons:

Stanford Physics Department

D. Ritson

Stanford linear Accelerator Center

S. Berman
A. Boyarski
F. Bulos
E. L. Garwin
W. Kirk
B. Richter
M. Sands

Figure 4. The cover page of the 1964 SL.AC proposal to build an electron-positron collider (D. Ritson et al. 1964).

king for a model for new leptons - a model which would lead to definitive colliding beam searches, while remaining reasonably general. Helped by discussions with my colleagues, such as Paul Tsai and Gary Feldman, I came to what I later called the sequential lepton model.

I thought of a sequence of pairs

$$\begin{array}{ll} e^- & \nu_e \\ \mu^- & \nu_\mu \\ L^- & \nu_L \\ L'^- & \nu_{L'} \end{array} \qquad (9)$$

each pair having a unique lepton number. I usually thought about the leptons as being point Dirac particles. Of course, the assumptions of unique lepton number and point particle nature were not crucial, but I liked the simplicity. After all, I had turned to lepton physics in the early 1960's in a search for simple physics.

The idea was to look for

$$e + \ t \ e^- \to L^+ + L^- , \tag{1Oa}$$

with

$$\begin{aligned} &L^+ \to e^+ + \text{ undetected neutrinos carrying off energy} \\ &L^- \to \mu^- t \text{ undetected neutrinos carrying off energy,} \end{aligned} \tag{10b}$$

or

$$\begin{aligned} &L^+ \to \mu^+ + \text{ undetected neutrinos carrying off energy} \\ &L^- \to e^- t \text{ undetected neutrinos carrying off energy} \end{aligned} \tag{10c}$$

This search method had many attractive features:

- If the L was a point particle, we could search up to an L mass almost equal to the beam energy, if we had enough luminosity.
- The appearance of an $e^+\mu^-$ or $e^-\mu^+$ event with missing energy would be dramatic.
- The apparatus we proposed to use to detect the reactions in Eqs. 10 would be very poor in identifying types of charged particles (certainly by today's standards) but the easiest particles to identify were the e and the μ.
- There was little theory involved in predicting that the L would have the weak decays

$$\begin{aligned} &L^- \to {}^{\nu}L \ t \ e^- + \bar{\nu}_e \\ &L^- \to {}^{\nu}L + \mu^- t \ \nu, \end{aligned} \tag{11}$$

with corresponding decays for the L^+. One simply could argue by analogy from the known decay

$$\mu^- \to \nu_\mu + e^- + \nu , .$$

I incorporated the search method summarized by Eqs. 10 in our 1971 Mark I proposal to use the not-yet-completed SPEAR e^+e^- storage ring.

My thinking about sequential leptons and the use of the method of Eqs. 10 to search for them was greatly helped and influenced by two seminal papers of Paul Tsai. In 1965, he published with Anthony Hearn the paper, "Differential Cross Section for $e^+ + e^- \to W^+ + W^- \to e^- \ t \ \bar{\nu}_e \ t \ \mu^+ + \nu_\mu$," (Tsai and Hearn 1965).

This work discussed finding vector boson pairs W^+W^- by their eμ decay mode. It was thus closely related to my thinking, described above, of finding L^+L^- pairs by their eμ decay mode. Tsai's 1971 paper entitled, "Decay Correlations of Heavy Leptons in $e^+ + e^- \rightarrow L^+$t L^-," provided the detailed theory for the applications of the sequential lepton model to our actual searches (Tsai 1971). Thacker and Sakurai (1971) also published a paper on the theory of sequential lepton decays, but it is not as comprehensive as the work of Tsai. Also important to me was the general paper, "Spontaneously Broken Gauge Theories of Weak Interactions and Heavy Lepton," by James Bjorken and Chris Llewellyn-Smith (1973).

The SLAC-LBL, proposal

After numerous funding delays, a group led by Burton Richter and John Rees of SLAC Group C began to build the SPEAR e^+e^- collider at the end of the 1960's. Gary Feldman and I, and our Group E, joined with their Group C and a Lawrence Berkeley Laboratory Group led by William Chinowsky, Gerson Goldhaber, and George Trilling to build the Mark I detector. In 1971, we submitted the SLAC-LBL Proposal (Larsen et al. 1971) using the Mark I detector at SPEAR. (The detector was originally called the SLAC-LBL detector and only called the Mark I detector when we began to build the Mark II detector. For the sake of simplicity, I refer to it as the Mark I detector.) The contents of the proposal consisted of five sections and a supplement, as follows:

The heavy lepton search was left for last, and allotted just three pages because to most others it seemed a remote dream. But the three pages did contain the essential idea of searching for heavy leptons using eμ events, Eqs. 10.

I wanted to include a lot more about heavy leptons and the e-μ problem, but my colleagues thought that would unbalance the proposal. We compromised on a 10-page supplement entitled, "Supplement to Proposal SP-2 on Searches for Heavy Leptons and Anomalous Lepton-Hadron Interactions." The supplement began as follows:

> "While the detector is being used to study hadronic production processes it is possible to simultaneously collect data relevant to the following questions:
>
> (1) Are there charged leptons with masses greater than that of the muon?
>
> We normally think of the charged heavy leptons as having spin l/2 but the search method is not sensitive to the spin of the particle. This search for charged heavy leptons automatically includes a search for the intermediate vector boson which has been postulated to explain the weak interactions.
>
> (2) Are there anomalous interactions between the charged leptons and the hadrons?
>
> In this part of the proposal we show that using the detector we can gather definitive information on the first question within the available mass range. We can obtain preliminary information on the second question - information which will be very valuable in designing further experiments relative to that question. We can gather all this information while the detector is being used to study hadronic production processes. Additional running will be requested if the existence of a heavy lepton, found in this search, needs to be confirmed."

My first interest was to look for heavy leptons, but I still had my old interest of looking for an anomalous lepton interaction, the idea that led to the study of muon-proton inelastic scattering.

Lepton searches at ADONE

While SPEAR and the Mark I detector were being built, lepton searches were being carried out at the ADONE e^+e^- storage ring by two groups of experimenters in electron-positron annihilation physics: One group reported in 1970 and 1973 (Alles-Borelli et al. 1970, Bernardini et al. 1973). In the later paper, they searched up to a mass of about 1 GeV for a conventional heavy lepton and up to about 1.4 GeV for a heavy lepton with decays restricted to leptonic modes. The other group of experimenters in electron-positron annihilation physics was led by Shuji Orito and Marcello Conversi. Their search region (Orito et al. 1974) also extended to masses of about 1 GeV.

Discovery of the tau in the Mark Z experiment: 1974-1976

SPEAR and the Mark I Detector

The SPEAR e^+e^- collider began operation in 1973. Eventually SPEAR obtained a total energy of about 8 GeV; but in the first few years, the maximum energy with useful luminosity was 4.8 GeV. We began operating the Mark I experiment in 1973 in the form shown in Fig. 5. The Mark I was one of the first large-solid-angle, general purpose detectors built for colliding beams.

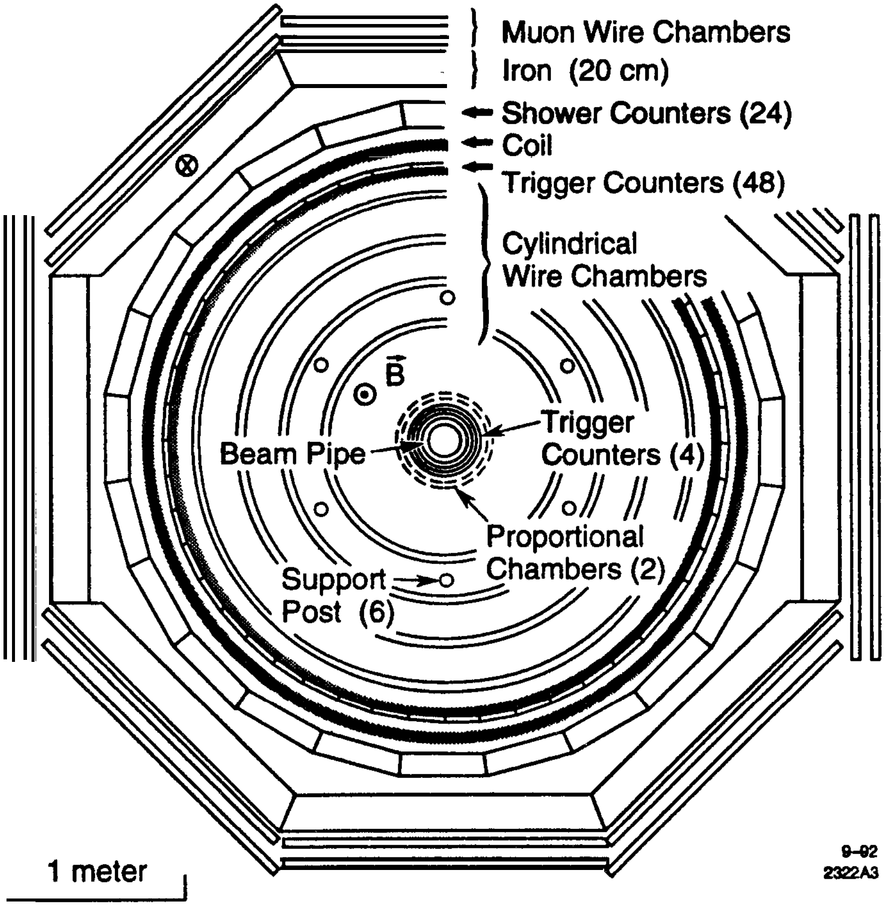


Figure 5. The initial form of the Mark I detector.

The use of large-solid-angle particle tracking and the use of large-solid-angle particle identification systems is obvious now, but it was not obvious twenty years ago. The electron detection system used lead-scintillator sandwich counters built by our Berkeley colleagues. The muon detection system was also crude, using the iron flux return which was only 1.7 absorption lengths thick.

The 1975 Canadian talks

In June 1975, I gave my first international talk on the e-μ events (Perl 1975a) at the 1975 Summer School of the Canadian Institute for Particle Physics. This was the second of my two lectures on electron-positron annihilation at the School.

The contents of the 1975 Summer School talk are shown below:

Contents of the 1975 Summer School talk

1. Introduction
   - A. Heavy Leptons
   - B. Heavy Mesons
   - C. Intermediate Boson
   - D. Other Elementary Bosons
   - E. Other Interpretations
2. Experimental Method
3. Search Method and Event Selection
   - A. The 4.8 GeV Sample
   - B. Event Selection
4. Backgrounds
   - A. External Determination
   - B. Internal Determination
5. Properties of eμ Events
6. Cross Sections of eμ Events
7. Hypothesis Tests and Remarks
   - A. Momentum Spectra
   - B. q_{coll} Distribution
   - C. Cross Sections and Decay Ratios
8. Compatibility of e^+e^- and $\mu^+\mu^-$ Events
9. Conclusions

This talk had two purposes. First, to discuss possible sources of e-μ events: heavy leptons, heavy mesons, or intermediate bosons; second, to demonstrate that we had good evidence for e-μ events. The largest single energy data sample (Table 2) was at 4.8 GeV, the highest energy at which we could then run SPEAR. The 24 e-μ events in the total charge = 0, number photons = 0 column was our strongest claim.

One of the cornerstones of this claim was an informal analysis carried out by Jasper Kirkby, who was then at Stanford University and at SLAC. He showed me that just using the numbers in the 0 charge, 0 photons columns of Table 2, we could calculate the probabilities for hadron misidentification in this class of events. There were not enough eh, μh, and hh events to explain away the 24 e-μ events.

The misidentification probabilities determined from three-or-more prong hadronic events and other considerations are given in Table 3. Compared to present experimental techniques, the $P_{h \to e}$ and $P_{h \to \mu}$ misidentification probabilities of about 0.2 are enormous, but I could still show that the 24 e-μ events could not be explained away.

And so the evidence for a new phenomena was quite strong - not incon-

Table 2. From Perl (1975a). A table of 2-charged-particle events collected at 4.8 GeV in the Mark I detector. The table, containing 24 eμ events with zero total charge and no photons, was the strongest evidence at that time for the τ. The caption read:

"Distribution of 513, 4.8 GeV, 2-prong, events which meet the criteria: $p_e > 0.65$ GeV/c, $p_\mu > 0.65$ GeV/c, $\phi_{copl} > 20°$".

	Total Charge = 0			**Total Charge = ±2**		
Number photons =	0	1	>1	0	1	>1
ee	40	111	55	0	0	0
eμ	24	8	8	0	0	3
μμ	16	15	6	0	0	0
eh	18	23	32	2	3	3
μh	15	16	31	4	0	5
hh	13	11	30	10	4	6
Sum	126	184	162	16	8	17

Table 3. From Perl (1975a). The caption read:

"Misidentification probabilities for 4.8 GeV sample"

Momentum range (GeV/c)	$\mathbf{P_{h \to e}}$	$\mathbf{P_{h \to \mu}}$	$\mathbf{P_{h \to h}}$
0.6– 0.9	.130±.005	.161±.006	.709±.012
0.9–1.2	.160±.009	.213±.011	.627±.020
1.2– 1.6	.206±.016	.216±.017	.578±.029
1.6– 2.4	.269±.031	.211±.027	.520±.043
Weighted average using hh, μh, and eμ events	.183±.007	.198±.007	.619±.012

trovertible, but still strong. What was the new phenomena: a sequential heavy lepton; a new heavy meson with the decays

$$M^- \to e^- + \bar{\nu}_e$$
$$M^- \to \mu^- + \bar{\nu}_\mu$$

My Canadian lecture ended with these conclusions:

"1) No conventional explanation for the signature e-μ events has been found.

2) The hypothesis that the signature e-μ events come from the production of a pair of new particles - each of mass about 2 GeV-fits almost all the data. Only the θ_{coll} distribution is somewhat puzzling.

3) The assumption that we are also detecting ee and μμ events coming from these new particles is still being tested."

I was still not able to specify the source of the eμ events: leptons, mesons or bosons. But I remember that I felt strongly that the source was heavy leptons. It would take two more years to prove that.

First publication: "We have no conventional explanation for these events "

As 1974 passed, we acquired e^+e^- annihilation data at more and more energies, and at each of these energies there was an anomalous e-μ event signal, see Fig. 6. Thus, I and my colleagues in the Mark I experiment became more and more convinced of the reality of the e-μ events and the absence of a conventional explanation. An important factor in this growing conviction was the addition of a special muon detection system to the detector (Fig. 7a), called the muon tower. This addition was conceived and built by Gary Feldman. Although we did not use events such as those in Fig. 7b in our first publication, seeing a few events like this was enormously comforting.

Finally, in December 1975, the Mark I experimenters published Perl et al. (1975b) entitled, "Evidence for Anomalous Lepton Production in e^+- e^- Annihilation."

The final paragraph reads:

"We conclude that the signature e-p events cannot be explained either by the production and decay of any presently known particles or as coming

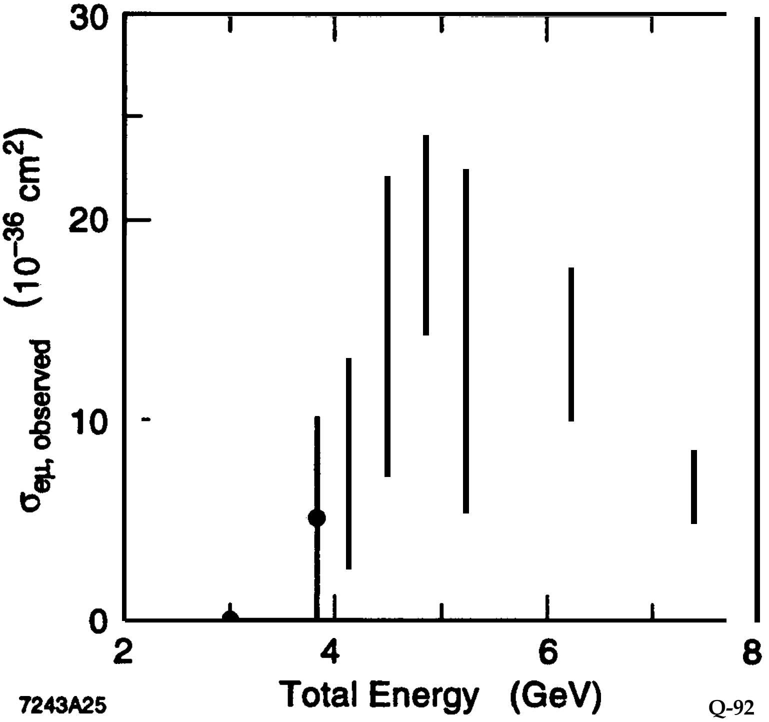


Figure 6. From Perl et al. (1975b): "The observed cross section for the signature eμ events from the Mark I experiment at SPEAR. This observed cross section is not corrected for acceptance. There are 86 events with a calculated background of 22 events."

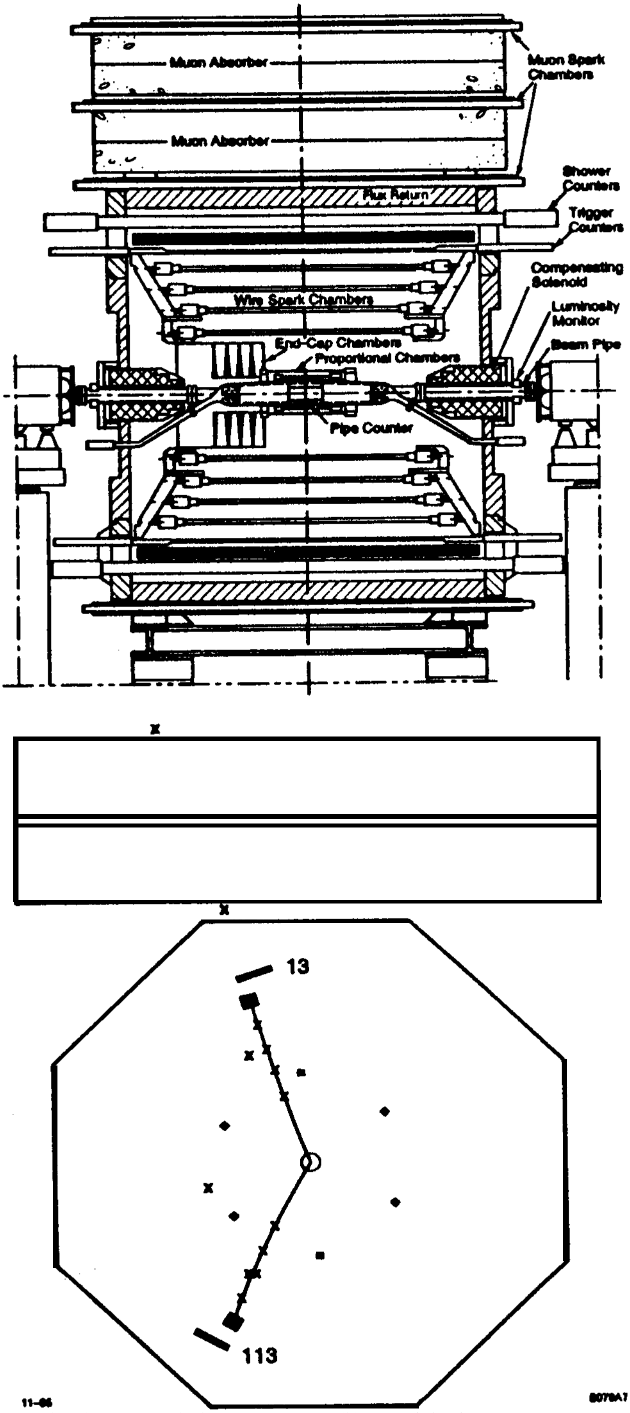


Figure 7. (a) The Mark I detector with the muon tower: (b) one of the first eμ events using the tower. The μ moves upward through the muon detector tower and the e moves downward. The numbers 13 and 113 give the relative amounts of electromagnetic shower energy deposited by the μ and e. The six square dots show the positions of longitudinal support posts of the magnetostrictive spark chamber used for tracking.

moves upward through the muon detector tower and the e moves downward. The numbers 13 and 113 give the relative amounts of electromagnetic shower energy deposited by the μ and e. The six square dots show the positions of longitudinal support posts of the magnetostrictive spark chamber used for tracking.

> from any of the well-understood interactions which can conventionally lead to an e and a μ in the final slate. A possible explanation for these events is the production and decay of a pair of new particles, each having a mass in the range of 1.6 to 2.0 GeV/c^2."

We were not yet prepared to claim that we had found a new charged lepton, but we were prepared to claim that we had found something new. To accentuate our uncertainty I denoted the new particle by "U" for unknown in some of our 1975-1977 papers. The name τ was suggested to me by Petros Rapidis, who was then a graduate student and worked with me in the early 1970's on the e–μ problem (Per-l and Rapidis 1975). The letter τ is from the Greek triton for third - the third charged lepton.

Thus in 1975, twelve years after we began our lepton physics studies at SLAC, these studies finally bore fruit. But we still had to convince the world that the e–μ events were significant and we had to convince ourselves that the e–μ events came from the decay of a pair of heavy leptons.

Reflections on the discovery

This is a good place to reflect on the elements of the research which led to the discovery of the tau. First I had chosen a research area in which there were few investigators. Second, we had cast a wide net in studying the electron-muon problem: an attempt to photoproduce new leptons, experimental comparisons of muon-proton inelastic scattering with electron-proton inelastic scattering, and the use of the general reaction $e^+ + e^- \to L^+ + L^-$ to try to produce a heavy lepton. Third, a new technology, the electron-positron collider was available to carry out the L^+L^- production. Fourth, I had a good way to detect the L^+L^- production, namely the search for eμ events without photons. Fifth, I had smart. resourceful and patient research companions. I think these are representative of the elements which should be present in speculative experimental work; a broad general plan, specific research methods, new technology, and first-class research companions. Of course the element of luck will in the end be dominant. I had two great pieces of luck. First, there was a heavy lepton within the energy range of the SPEAR collider. Second, the Mark I experimental apparatus was sufficiently good to enable us to identify the e–μ events and prove their existence.

Is it a lepton? 1976-1978

Our first publication was followed by several years of confusion and uncertainty about the validity of our data and its interpretation. It is hard to explain this confusion a decade later when we know that τ pair production is 20% of the e^+e^- annihilation cross section below the Z^0, when ττ pair events stand out so clearly at the Z^0.

There were several reasons for the uncertainties of that period. It was hard to believe that both a new quark (charm) and a new lepton (tau) would be found in the same narrow range of energies. Also, while the existence of a fourth quark was required by theory, there was no such requirement for a third charged lepton, so there were claims that the other predicted decay modes of tau pairs, such as e-hadron and μ-hadron events, could not be found. Indeed, finding such events was just at the limit of the particle identification capability of the detectors of the mid-1970's.

Perhaps the greatest impediment to the acceptance of the τ as the third charged lepton was that there was no other evidence for a third particle generation. Two sets of particles - u, d, e^-, ν_e, and c, s, μ^-, ν_μ- seemed acceptable, a kind of doubling of particles. But why three sets? A question which to this day has no answer.

It was a difficult time. Rumors kept arriving of definitive evidence against the τ: e-μ events not seen, the $\tau \to \pi\nu$ decay not seen, theoretical problems with momentum spectra or angular distribution. With colleagues such as Gary Feldman, I kept going over our data again and again. Had we gone wrong somewhere in our data analysis?

Clearly other tau pair decay modes had to be found. Assuming the τ to be a charged lepton with conventional weak interactions, simple and very general theory predicted the branching fractions

$$B(\tau^- \to \nu_\tau + e^- + \nu_e) \approx 20\ \%$$

$$B(\tau^- \to \nu_\tau + \mu^- + \nu_\mu) \approx 20\ \%$$

$$B(\tau^- \to \nu_\tau + \text{hadrons}) \approx 60\ \%$$

Experimenters therefore should be able to find the decay sequences

$$\begin{aligned} &e^+ + e^- \to \tau^+ + \tau^- \\ &\tau^+ \to \overline{\nu}_\tau + \mu^+ + \nu_\mu \\ &\tau^- \to \nu_\tau + \text{hadrons}\ , \end{aligned} \tag{13}$$

and

$$\begin{aligned} &e+ + e^- \to \tau^+ + t^- \\ &\tau^+ \to \overline{\nu}_\tau + e^+ + \nu_e \\ &t^- \to \nu_\tau + \text{hadrons}\ . \end{aligned} \tag{14}$$

The first sequence, Eqs. 13 would lead to anomalous muon events

$$e^+ + e^- \to \mu^\pm + \text{hadrons} + \text{missing energy} \tag{15}$$

and the second, Eqs. 14 would lead to anomalous electron events

$$e^+ + e^- \rightarrow e^\pm + \text{hadrons} + \text{missing energy} \qquad (16)$$

Anomalous muon events

The first advance beyond the e-μ events came with three different demonstrations of the existence of anomalous μ-hadron events:

$$e^+ + e^- \rightarrow \mu^\pm + \text{hadrons} + \text{missing energy.}$$

The first and very welcome outside confirmation for anomalous muon events came in 1976 from another SPEAR experiment by Cavilli-Sforza et al. (1976). This paper was entitled, "Anomalous Production of High-Energy Muons in e^+t e^-Collisions at 4.8 GeV."

I have in my files a June 3, 1976, Mark I note by Gary Feldman discussing μ events using the muon identification tower of the Mark I detector (see Fig. 7a). For data acquired above 5.8 GeV, he found the following:

> "Correcting for particle misidentification, this data sample contains 8 e-μ events and 17 μ-hadron events. Thus, if the acceptance for hadrons is about the same as the acceptance for electrons, and these two anomalous signals come from the same source, then with large errors, the branching ratio into one observed charged hadron is about twice the branching ratio into an electron. This is almost exactly what one would expect for the decay of a heavy lepton."

This conclusion was published in the paper, "Inclusive Anomalous Muon Production in e^+e^-Annihilation," by Feldman et al. (1977).

The most welcomed confirmation, because it came from an experiment at the DORIS e^+e^-storage ring, was from the PLUTO experiment. In 1977, the PLUTO collaboration published "Anomalous Muon Production in e^+e^- Annihilation as Evidence for Heavy Leptons," (Burmester et al. 1977); Fig. 8 is from that paper.

PLUTO was also a large-solid-angle detector, and so for the first time we could fully discuss the art and technology of τ research with an independent set of experimenters, with our friends Hinrich Meyer and Eric Lohrman of the PLUTO Collaboration.

With the finding of μ-hadron events, I was convinced I was right about the existence of the τ as a sequential heavy lepton. Yet there was much to disentangle: it was still difficult to demonstrate the existence of anomalous e^- hadron events, and the major hadronic decay modes

$$\tau^- \rightarrow \nu_\tau + \pi^- \qquad (17)$$

$$\tau^- \rightarrow \nu_\tau + p^- \qquad (18)$$

had to be found.

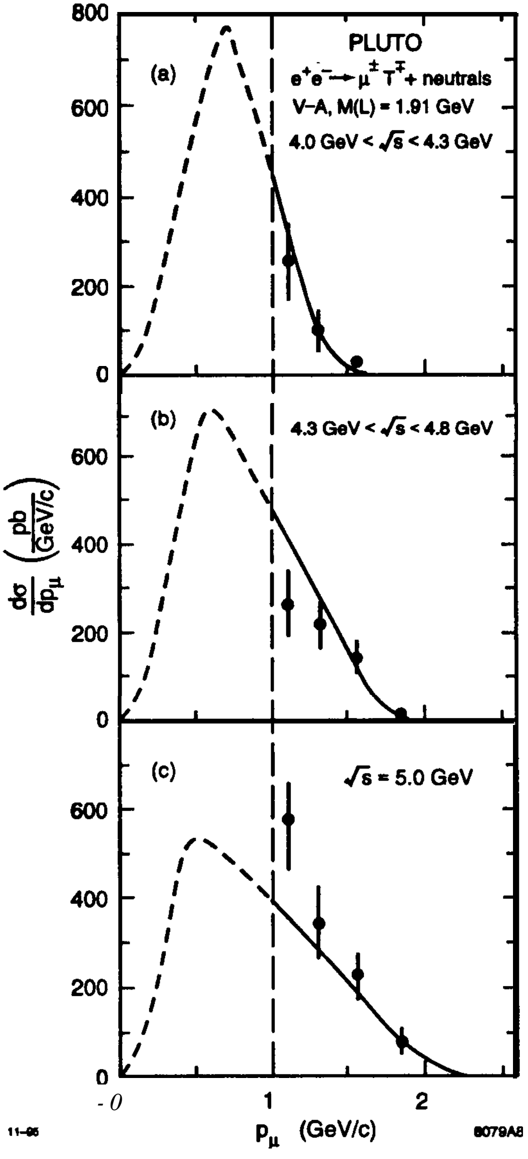


Figure 8. The momentum spectra of μ's from anomalous muon events found by the PLUTO experimenters using the DORIS e^+e^- storage ring (Burrnester et al. 1977).

Anomalous electron events

The demonstration of the existence of anomalous electron events

$$e^{+} + e^{-} \to e^{\pm} + \text{hadrons} + \text{missing energy}$$

required improved electron identification in the detectors. A substantial step forward was made by the new DELCO detector at SPEAR (Kirkby 1977, Bacino et al. 1978). In Kirkby's talk at the 1977 Hamburg Photon-Lepton

Conference, "Direct Electron Production Measurement by DELCO at SPEAR," he stated,

> "A comparison of the events having only two visible prongs (of which only one is an electron) with the heavy lepton hypothesis shows no disagreement. Alternative hypotheses have not yet been investigated."

The Mark I detector was also improved by Group E from SLAC and a Lawrence Berkeley Laboratory Group led by Angela Barbaro-Galtieri; some of the original Mark I experimenters had gone off to begin to build the Mark II detector. We installed a wall of lead-glass electromagnetic shower detectors in the Mark I (see Fig. 9). This led to the important paper entitled, "Electron-Muon and Electron-Hadron Production in e^+e^- Collisions," (Barbaro-Galtieri et al. 1977b). The abstract read:

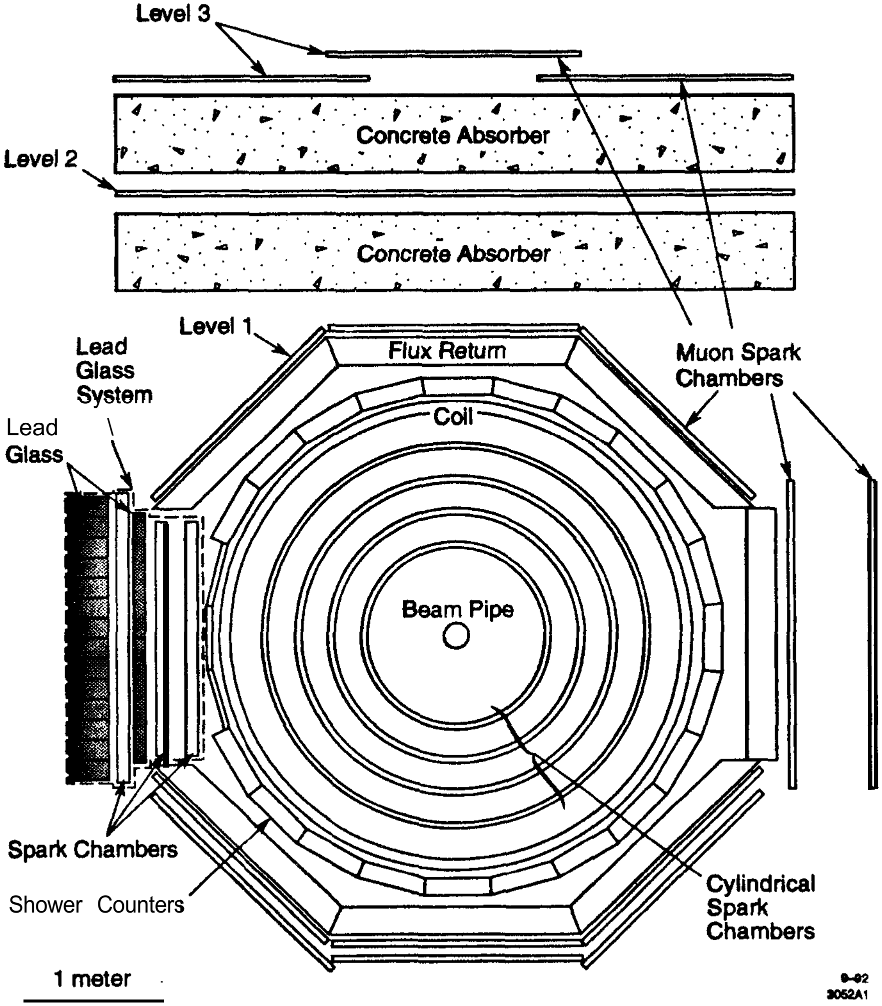


Figure 9. The "lead glass wall" modification of the Mark I detector used at SPEAR to find anomalous electron events.

"We observe anomalous e-μ and e-hadron events in $e^+ + e^- \rightarrow \tau^+ + \tau^-$ with subsequent decays of $\tau\pm$ into leptons and hadrons. Under the assumption that they come only from this source, we measure the branching ratios $B(\tau \rightarrow e\nu e\nu,)$ = (22.4±5.5)% and $B(\tau \rightarrow$ h+neutrals) = (45±19)%."

Semileptonic decay modes and the search for $\tau^- \rightarrow \nu_\tau \pi$ and $\tau \rightarrow \nu_\tau \rho^-$

By the time of the 1977 Photon Lepton Conference at Hamburg, I was able to report in "Review of Heavy Lepton Production in e^+e^- Annihilation," (Perl 1977) that:

"a) All data on anomalous eμ, ex, ee and $\mu\mu$ events produced in e^+e^- annihilation is consistent with the existence of a mass 1.9±0.1 GeV/c^2 charged lepton, the τ.

b) This data cannot be explained as coming from charmed particle decays.

c) Many of the expected decay modes of the τ have been seen. A very important problem is the existence of the $\tau^- \rightarrow \nu t \pi^-$ decay mode."

The anomalous muon and anomalous electron events had shown that the total decay rate of the τ into hadrons, that is the total semileptonic decay rate, was about the right size. But if the τ was indeed a sequential heavy lepton, two substantial semileptonic decay modes had to exist: $\tau^- \rightarrow \nu_\tau \pi^-$ and $\tau^- \rightarrow \nu_\tau \rho^-$.

First, the branching fraction for

$$\tau^- \rightarrow \nu_\tau + \pi^- \tag{19a}$$

could be calculated from the decay rate for

$$\pi^- \rightarrow \mu^- + \nu_\mu \tag{19b}$$

and was found to be

$$B(\tau^- \rightarrow \nu_\tau \pi^-) \approx 10\,\%. \tag{19c}$$

Second, the branching fraction for

$$\begin{aligned}\tau^- \rightarrow \nu_\tau + \rho^- &\rightarrow \nu_\tau + \pi^- + \pi^\circ\\ &\rightarrow \nu_\tau + \pi^- + \gamma + \gamma\end{aligned} \tag{20a}$$

could be calculated from the cross section for

$$e^+ + e^- \rightarrow \rho^\circ \tag{20b}$$

and was found to be

$$B(\tau^- \rightarrow \nu_\tau \rho^-) \approx 20\%. \tag{20c}$$

One of the problems in the years 1977-1979 in finding the modes in Eqs. 19a and 20a was the poor efficiency for photon detection in the early detectors. If the γ's in Eq. 20a are not detected then the π and ρ modes are confused with each other. Probably the first separation of these modes was achieved using the Mark I-Lead Glass Wall detector. As reported at the Hamburg Conference by Angelina Barbaro-Galtieri (1977a)

$$B(\tau^- \to \nu_\tau \pi^-)/B(\tau- \to \nu_\tau\ \mathbf{p}^-) = \mathbf{0.44 \pm 0.37}.$$

Gradually, the experimenters understood the photon detection efficiency of their experiments. In addition, new detectors (such as the Mark II) with improved photon detection efficiency were put into operation. In our collaboration, the first demonstration that $B(\tau^- \to \nu_\tau p^-)$ was substantial came from Gail Hanson in an internal note dated March 7, 1978.

Within about a year, the $\tau^- \to \nu_\tau \pi^-$ decay mode had been detected and measured by experimenters using the PLUTO detector, the DELCO detector, the Mark I Lead-Glass Wall detector, and the new Mark II detector. These measurements were summarized (Table 4) by Gary Feldman (1978) in a review of $e^+ + e^-$ annihilation physics at the XIX International Conference on High Energy Physics. Although the average of the results in Table 4 is two standard deviations smaller than the present value of (11.1±0.2) %, the $\tau^- + \nu_\tau \pi^-$ mode had been found.

Table 4. From Feldman (1979), the various measured branching fraction\ B in percent for $\tau- \to \pi^-\nu_\tau$ in late 1978.

Experiment	Mode	Events	Background	$B(\tau \to \pi\nu)$
SLAC–LBL	$x\pi$	≈200	≈70	9.3±1.0±3.8
PLUTO	$x\pi$	32	9	9.0±2.9±2.5
DELCO	$e\pi$	18	7	8.0±3.2±1.3
Mark II	$x\pi$	142	46	8.0±1.1±1.5
	$e\pi$	27	10	8.2±2.0±1.5
Average				8.3±1.4

The year 1979 saw the first publications of $B(\tau^- \to \nu_\tau p^-)$. The DASP Collaboration using the DORIS $e^+ + e^-$ storage ring reported (Brandelik et al. 1979) (24±9)% and the Mark II Collaboration reported (Abrams et al. 1979) (20.5±4.1)%. Crude measurements, but in agreement with the 20% estimate in Eq. 20c. The present value is (24.8±0.2) %.

By the end of 1979, all confirmed measurements agreed with the hypothesis that the τ was a lepton produced by a known electro-magnetic interaction and, that at least in its main modes, it decayed through **the** conventional weak interaction. And so ends the sixteen year history, 1963 to 1979, of the discovery of the tau lepton and the verification of that discovery.

Reflections on the present and future of tau physics

Since 1979, there has been a tremendous amount of experimental and theoretical research in tau physics. There are recent reviews by Weinstein and Stroynowski (1993), Montanet (1994), and myself (Perl 1996). The proceedings of the Third Workshop on Tau Lepton Physics (Rolandi 1995) are a treasure house of information and speculation on the tau and its neutrino. There are very active experimental programs on the tau using the CESR electron-positron collider at Cornell, the LEP electron-positron collider at CERN, the SLC electron-positron collider at SLAC, and the BEPC electron-positron collider at IHEP in Beijing. This experimental research uses numbers of tau decays which are much larger than the numbers that were available during the discovery years-1000 to 10,000 more events. There are, in addition, active experiments at CERN and experiments in preparation at Fermilab that are designed to detect tau neutrinos and to look for oscillations between the tau neutrinos and other neutrinos.

There are two broad goals in tau research. One goal is to learn as much as we can about the expected behavior of the tau lepton and tau neutrino. The second goal, which is perhaps only a dream, is to find some unexpected behavior of the tau lepton, behavior that will lead us to a deeper understanding of elementary particles and basic forces. The tau is a fine candidate for such speculative research because the tau and the tau neutrino are the only particles in the third family that can be examined in a pure, isolated state. Remember that the electron-muon puzzle which set all this in motion is still not solved. The electron-muon puzzle has expanded into the electron-muon-tau puzzle. We still do not know why there are three charged leptons or understand the ratios of their masses.

In the future, there will be another increase by a factor of 10 to 100 of the number of recorded tau decays. This increase will be achieved with the high luminosity B-factories now being constructed at SLAC and KEK, and by further increases in the luminosity of the CESR electron-positron collider. And there is the special hope that a Tau-Charm Factory will be constructed at the Institute for High Energy Physics in Beijing.

I am fortunate that a short time ago some SLAC colleagues and I were able to join the CLEO Collaboration which uses the CESR collider, and so I am continuing to work on tau lepton physics. I don't have any original ideas for tau research, but I do know that the only way I get ideas is to work experimentally on a subject.

My final remark to young women and men going into experimental science is that they should pay little attention to the speculative physics ideas of my generation. After all, if my generation has any really good speculative ideas, we will be carrying these ideas out ourselves.

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