The Signature of Sequential Charged Leptons[*]

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I am honored to be invited to give a talk on this occasion to celebrate Martin Perl's 65th birthday. John Jaros, the organizer of this occasion, wanted me to recollect several things, especially:

1. what motivated me to write my 1971 Physical Review paper[1] on the heavy lepton entitled "Decay Correlations of Heavy Lepton in $e^- + e^+ = \ell^- + \ell^+$";
2. why did I think of muon-electron coincidence; and
3. what were the contributions of Thacker and Sakurai.[2]

Let me answer his requests first and add something else later.

I jokingly told him that I wrote my heavy lepton paper in 1971 because I did not have anything better to do. This was true to a certain extent because in general I tend to avoid working on anything which is too popular and in 1971 hardly anybody was talking about heavy leptons.

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My first encounter with the heavy lepton goes back to 1964 when Dave Coward[3] was looking for the heavy lepton produced by the photo-pair production for his Ph.D. thesis under David Ritson at HEPL of Stanford. I had estimated the muon yields from the SLAC machine for the SLAC Users' Handbook[4] which was written before SLAC was in operation. I was ordered by Dr. Panofsky (Pief) to write the section on theoretical estimates on all the secondary beam productions using the electron beam from SLAC for the Users' Handbook. The heavy lepton cross section Dave Coward wanted was obtained simply by replacing the muon mass by the heavy lepton mass in the photo-production cross section. I was bothered by the fact that nobody had systematically investigated how the heavy lepton should decay if it existed, and the idea of writing a paper on the decay of the heavy lepton occurred at that time.

Meanwhile by the time Dave Coward finished his experiment it became obvious that photo-pair production was not the way to discover heavy leptons because of the backgrounds, and the newly planned e^+e^- colliding beam machine would be the preferred machine. I was fortunate to be associated with the first colliding beam machine at its inception. Pief hired me in 1958 as a fresh postdoc from Minnesota to participate in the first colliding beam machine project in the world: the Princeton-Stanford Electron-Electron Intersecting Beam Machine.[5] My job was to do radiative corrections and other theoretical calculations for the project. I got a Ph.D. in experimental nuclear physics but studied QED for a year with Don Yennie in Minnesota and with his recommendation to Pief I got the job. I was determined to work in theoretical physics for a couple of years and then go back to experimental physics (I never did) so I welcomed the opportunity.

I finished my work on radiative corrections,[6] which was the first radiative correction paper for the colliding beam experiment ever written, and in the appendix of that paper I estimated all possible reactions known to me at that time for the electron-positron colliding beam experiment. G. K. O'Neill of Princeton was the first one to propose the electron-positron colliding beam machine (Pief told me so at this conference) and he was also the original proponent of the Princeton-Stanford Electron-Electron Colliding Beam Project. I would like to remind the reader that in 1960 there was no QCD, not even the parton model. There were some speculations about the existence of 2π[7] and 3π[8] resonances. The physical emphasis was measurement of electromagnetic form factors in the time-like region of various particles: electron, muon, pion, kaon, proton, neutron and the multibody resonances.[6]

I published the results of my calculations in the Physical Review in 1960[6] and gave a talk[9] at the plenary session of the 1960 International Conference on High Energy Physics at Rochester, New York (the last Rochester conference to be held at Rochester). This was also the first time Soviet physicists were allowed by their government to attend such an international conference. Please allow me to describe my first exciting experience in attending such a conference as a young fresh postdoc.

I prepared my lecture well because I realized that practically all the greatest physicists in the world such as Lee, Yang, Nambu, Heisenberg, Feynman, Gell-man,... were in the audience. At that time the overhead projector had just been invented in order to replace the blackboard which was impossible to read in a big auditorium. Instead of writing on the blackboard, the speaker wrote on a big

continuous roll of mylar as he or she lectured. I sneaked into the auditorium the evening before my talk and cut the mylar role into the proper size and wrote my lecture on these sheets of mylar like we do it today. After my talk everybody imitated me. I was very pleased that I contributed something to the art of conference lecturing!

In 1965 I wrote two papers with Tony Hearn[10,11] on the reaction

$$e^+ + e^- \to W^+ + W^- \to e^- + \mu^+ + \nu_e + \nu_\mu \ .$$

This calculation was done before the invention of the Weinberg-Salam model so the diagram involving neutrino exchange was not included and the mass of W used in the numerical example was 2 GeV. Nevertheless this was a very interesting calculation in the following sense:

1. This was the first paper where the algebraic computer routine was used in a very complicated calculation. Tony was the inventor of the famous algebraic computer routine called REDUCE.[12]
2. This paper showed how to deal with decay correlations by treating the W's as vector boson propagators and replacing the square of each denominator by a delta function, a well-known procedure now but I think it was the first time this was shown in the literature.
3. This answered the second question of John Jaros as to the genesis of μe coincidence. Needless to say the W and the heavy lepton can be distinguished from each other by either threshold behavior or decay energy distribution.

In the introduction of my 1971 paper[1] I wrote, "Since muons exist in nature for no apparent reasons, it is possible that other heavy leptons may also exist in nature. If one discovers heavy leptons, one may be able to understand why muons exist and why the ratio of muon mass to the electron mass is roughly 207."

Now the tau has been discovered but we still do not understand why muons and taus should exist, nor do we understand their mass ratios of roughly 1: 207: 3477. However we have learned several interesting things about nature since the discovery of the tau. From the width of Z_0 we now know[13] that there are three and only three generations of leptons. Since the whole universe must be neutral there must also be three and only three generations of quarks. Three is the number just enough to have CP violation so that preponderance of matter over matter, and thus our existence, becomes possible according to the CKM theory. At this moment we still do not know whether ν_τ has any mass or not, but we know it cannot be heavier than 35 MeV.[14] It is very difficult to obtain a better limit than this from the measurements of decay products of the tau. Only if we observe neutrino oscillation does the hope of obtaining better estimates of neutrino masses become possible. Observation of neutrino oscillation will tell us not only whether leptonic flavor is conserved, but also whether dark matter in the universe is due to neutrinos.

Of course how the heavy lepton decays depends upon the mass as well as whether it is a sequential lepton or something else. I assumed[1] it to be sequential and the mass equal to 0.6, 0.8, 0.938, 1.2, 1.8, 3.0, 6.0 GeV. Needless to say the case for m_ℓ = 1.8 GeV happened to be the closest to the presently accepted value of 1.777 GeV from the Beijing Electron Positron Collider.[15] The branching

ratios calculated at that time using 1.8 GeV are substantially correct even today and they were definitely good enough for Perl to design the experiment[16] which eventually led him to discover the tau lepton at around 1975.[17] Since we always learn something from past mistakes maybe it is of interest to point out some aspects of the calculations which did not turn out correctly due to ignorance at that time. Fortunately a large fraction of the decay rates were independent of these uncertainties and furthermore errors in these uncertain channels almost cancelled among themselves. In evaluating the decay into A_1 I have used Weinberg's sum rule[18] assuming the ρ and A_1 dominance in the difference of the vector and axial vector spectral functions. I stated in my paper[1] that this estimate of the partial rate is probably accurate to within a factor of 2. Looking at the data[19] today this procedure underestimated the partial width of the tau into A_1 by about a factor of 2. The second thing I did which was wrong was that in estimating contributions from the high mass non-resonant states, I used the parton model without color. The concept of color[20] was still in its infant stage so I could not be faulted for not using it. I also pointed out in the paper[1] that the contribution from continuum states can be estimated from the total cross section for the reaction $e^+ + e^- \rightarrow$ hadron continuum. At that time the only data available was the controversial results from Frascati[21] which was correct—but at that time very few people believed in its validity—so I merely mentioned the possibility but did not use the result in the final tabulation. Retrospectively, the large cross section obtained by Frascati was the first indication of the existence of color quantum number! The two mistakes made above tended to underestimate the width by about 10%. These were compensated for by another mistake, namely by my disrespect for the empirical rule called duality.[22] I integrated the continuum contribution from 1 GeV to the lepton mass which is clearly against the principle of duality because I had already included the contribution of A_1; and thus a double counting was involved here. Even today I am still amazed by the fact that QCD, which is just the parton model with radiative corrections, works so well in predicting the leptonic branching ratio of the tau particle (slightly less than 20% for the electron). QCD is supposed to be true only in the asymptotic region, not at a very low energy region such as the decay of the tau into $\pi + \nu_\tau$.

I met the late J. J. Sakurai for the first time at the 1960 Rochester conference. We became very good friends. He spoke some Chinese and I speak fluent Japanese. During the Second World War he studied at Gakushiuin, the school for the Japanese imperial family and noblemen, and I studied at Doshisha, a Christian school in Kyoto built mostly by American donations. We had a lot to talk about besides physics. Even though we were very good friends I did not know that he was working on the heavy lepton until I almost finished my long paper[1] (17 pages in the Physical Review) when his short paper with Thacker[2] appeared in Physics Letters. They were interested in the possibility of finding the heavy leptons at ADONE which had the maximum energy of 1 GeV so their letter essentially consisted of a graph plotting the decay branching ratios of a heavy lepton as a function of its mass from 0.6 to 1.2 GeV. Experimentalists did not use their results because the mass range covered was too low and also the treatment of the subject was too sketchy compared with my paper.

My paper was written in the typical style of all my papers, long and detailed. Because I am an ex-experimentalist all my papers tend to be written in such a way that they can be understood and used by experimentalists immediately.

The energy and angular distributions of the decay products of the heavy lepton depend upon the polarization of the parent particle. This was treated in great detail in my paper but did not play a crucial role in the discovery of the tau. However recently this fact was used extensively as an analyser of the polarization of the tau pair produced by the decay of Z_0 at LEP.[23] This gave the best value of the Weinberg's angle θ_W.[24]

I met Martin Perl around 1965 when he first came to SLAC from Michigan. We have interacted on many occasions since then. I will try to recollect the items on which we interacted very closely.

1. SLAC Secondary Beam Production Survey:
   Martin and his Group E collaborated with Group C in the measurements of secondary beam productions using the 16 GeV electron beam from SLAC. It was anticipated (especially by Drell[25]) that SLAC could produce enough $\mu, \pi, K_\pm, K_2, \bar{p}$ beams to compete against proton machines. So a survey was conducted with great expectations. The work was closely related to theoretical estimates of the secondary beam productions I made for the SLAC Users Handbook. The conclusion[4] was that SLAC could produce these secondary beams, but their intensities are about 100th of those from a proton machine if the electron and proton machines had equal energy and intensity. The muon beam was the only exception to this rule.

2. Experimental Limit on High Energy Diffraction Photoproduction of the ϕ meson:
   This was the by-product of the beam survey experiment. By attributing all the yield of K_- beam to the decay of ϕ, we obtained upper limit of the ϕ photoproduction cross section. I did this[26] in collaboration with Martin and his group. It was a puzzle at that time why the photoproduction cross section for the ϕ was only about 1/30 of that for the ρ. Even 25 years later, today I still do not understand it.

3. Muon Scattering from Nuclei:[27]
   This was the Group E project. I was kind of a consultant to them because I had the muon beam calculations[4] and was supposed to be an expert on radiative corrections.[6,28,29] At the 1960 Rochester conference after reporting the results of my calculation for radiative corrections to e-e scattering,[6,9] I noticed that Hofstadter *et al.*[9] were not doing the radiative corrections correctly. They were using the experimental width of the elastic peak as ΔE in Schwinger's formula! The experimental width of the elastic peak was caused mostly by machine width and had very little to do with ΔE in Schwinger's formula, which was given a very misleading name "energy resolution" at that time. This problem was clarified, together with effects due to the nucleon recoil and emission of photons by the target nucleus in my paper in 1961.[28] Hofstadter was most graceful on this matter. He gave me the whole credit for my contribution to this subject. For inelastic electron and muon scatterings from a nucleon or nucleus I wrote a long article in the Review of Modern Physics with Luke Mo[30] which, together

with my unpublished lecture note[31] at the NATO Summer School, became the standard reference on the subject.

4. Search for Heavy Leptons by Photopair production:

   This was again the Group E effort under the leadership of Martin. I was again the consultant because I had calculated the cross sections earlier in the Users Handbook. They did not find anything because, as stated earlier, the cross section was too small and the background too high. I got something out of this endeavor however. Because of this effort I was able to write a long Review of Modern Physics paper[32] reviewing and deriving all the useful formulae for bremsstrahlung, pair production, electroproduction, the thick target problem, the Weiszacker-Williams approximation, a complete recalculation of the radiation lengths of all the materials, etc., etc.

5. Discovery of the Ψ:

   This was the joint effort of LBL and SLAC of which Martin and his group were members. As soon as we heard news of the discovery, the theory group organized a study group under the leadership of J. Bjorken. The objective was to give theoretical tools to experimentalists to analyze and understand the data. This group effort[33] was unprecedented and resulted in SLAC–PUB–1515 (1974), which was a collection of quickie papers by all the participants. I contributed a paper dealing with a very practical problem, namely how to obtain the width of a narrow resonance (64 KeV) which is buried under a much wider machine width (2.2 MeV) and, at the same time, do the radiative corrections to the width. Now everybody knows how to do this, but I am very proud that I was the first one to figure out how to do it. There are many imitation versions but I regard my original version as still the best. This was rather easy for me because of my experience in dealing with the radiative corrections to the elastic e-p scattering which was also smeared by the machine width[28] and also my familiarity with approximating the narrow Breit-Wigner peak by a delta function which I did in many of my previous works[10,1] including my 1971 heavy lepton paper.

6. The discovery of the τ:

   It is very interesting to notice that of all the items that were contained in the original proposal[16] of the MARK I detector only the item on the heavy lepton was carried out. My paper was used in the original proposal. One day in around 1974 Martin came to my office and told me about his handful of μ-e events and asked for my opinion. I told him that the μ-e events could be due to heavy leptons, W-mesons, or other particles, and they could be distinguished easily by both the threshold behaviors and the energy distributions of e and μ. Whatever particles these events might be due to, he better be ready to go to Sweden to dance with the Queen! Meanwhile I knew many other people in the collaboration and I asked their opinion about the experimental evidence of the events. Many of them told me that all those events most likely existed only in Martin's mind! Please do not force me to reveal the distinguished names of those who said that!! It took almost four years of hard work by the people at MARK I, MARK II, DELCO, and PETRA to finally convince the world that what Martin saw were really heavy leptons and not anything else.[34] The reasons it took so long were, in my opinion, that practically everything about the heavy lepton except its mass

was known beforehand and the experiments better agree with all theoretical predictions, otherwise it could not be called the sequential lepton.

7. Search for a neutral boson coupled to the electron:

   This was the effort of Martin and his group. My contribution was to write a theoretical paper on the subject.[35]

I am fortunate to be able to contribute something to the discovery of the tau and I am grateful to Martin for discovering it.

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