Essay: The Tau Lepton and Thirty Years of Changes in Elementary Particle Physics Research

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Starting with the 1975 discovery of the tau lepton, I look back on the last three decades of change in the substance and style of experimental and theoretical research in elementary particle physics. I recount the major accomplishments of those decades and predict a bright future for particle physics in the next two decades. Turning to three problems, I lament the change in theoretical style and taste, I discuss the growth in the complexity, size, and cost of particle physics experiments, and I conclude with a pessimistic comment on the size of particle physics collaborations.

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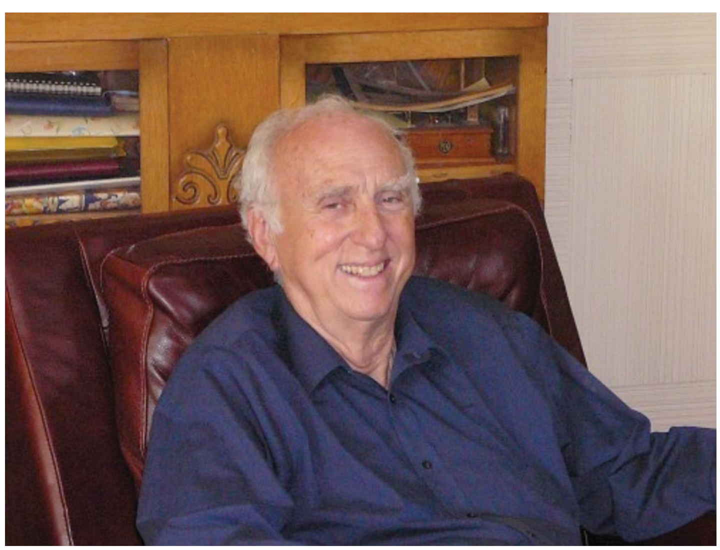
In the December 1, 1975 issue of *Physical Review Letters*, the first evidence for the existence of the tau lepton was published by 34 physicists from the Lawrence Berkeley Laboratory, from the Physics Department of the University of California at Berkeley, and from the Stanford Linear Accelerator Center of Stanford University [1]. This discovery using the Stanford Positron-Electron Accelerating Ring (SPEAR) and the SLAC-LBL particle detector extended the charged lepton electron-muon pair system to the triplet of electron, muon and tau with masses of 0.51, 106, and 1777 MeV/c^2, The tau discovery also introduced the third generation of elementary particle fermions. For me the tau was a dream come true.

Once the dream of the reality of the tau was confirmed, I and perhaps others had two further dreams for the tau. The first dream was that still heavier charged leptons would be found. I thought that there would be a long sequence of heavier and heavier charged leptons with associated neutrinos—the sequential lepton model. I was wrong. Null searches for a fourth charged lepton were carried out at CERN's Large Electron-Positron Collider (LEP) at a total energy up to 200 GeV/c^2. If a fourth charged lepton exists, its mass is more than 50 times the tau mass.

My second dream for the tau was that its relatively large mass would lead to finding unexpected tau decays, indicating new physics. So far my dream has not come true. For example, the lepton flavor-violating decay of a tau to a muon plus a photon has not been



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found. Results from the Belle and BABAR experiments set the branching fraction to be less that 10^{-7} [2]. Searches for unexpected tau decays and properties continue, but a growing role of the tau these days is as a tool for new particle physics studies. For example, in 2007 the first 60 SPIRES High-Energy Physics Literature Data Base references to the tau consisted of 41 concerned with tau properties and 19 concerned with using the tau in other particle physics. Some searches for particles predicted by the supersymmetry conjecture depend upon finding a tau in the final decay state of a new particle. Some aspects of proposed neutrino astronomy depend upon tau neutrinos coming from outer space, colliding with nitrogen and oxygen nuclei in the atmosphere, and producing detectable, charged taus.

PROGRESS SINCE THE EARLY 1970s

Since the early 1970s theory and experiment have combined to make tremendous advances in our understanding of elementary particle physics. We have found and studied the four members of the third particle generation—tau, tau neutrino, top quark, bottom quark. We have discovered that neutrinos have mass and oscillate between types and we have made difficult measurements. Measurements and theory have made great progress on *CP* violation in the bottom quark world. Constraints have been placed on the mass range of the Higgs particle. Much has been clarified about the particle aspects of cosmic rays. The older discovery of dark matter by astronomers has at last been incorporated into the theoretical thinking and the searches of particle physicists. There have been a large number of null searches for new forces, new particles, and new symmetries, disappointing to researchers, but necessary to build a firm picture of what exists. You can certainly add to this list.

The next two decades in particle physics look equally bright because of (i) the start of the CERN Large Hadron Collider (LHC), the proton-proton collider with a total energy of 14 TeV/c^2, (ii) the increasing intermeshing of astrophysics and particle physics, (iii) the next wave of neutrino experiments, and (iv) the continuing creativity and inventiveness of the particle physics community. Given sufficient additional funds, the future will be even brighter with the building of a very high-energy electron-positron linear collider and a very high luminosity electron-positron collider B factory, called a SuperB factory. Among the many virtues of a SuperB factory would be the copious production of tau pairs enabling more sensitive searches for the flavor-violating decay of a tau to muon plus photon; branching fractions as small as 10^{-9} to 10^{-10} can be explored [2].

But problems have arisen in the past three decades in the practice of particle physics and I now turn to those problems. I am unhappy with the changes over that period in the style and dignity of theoretical elementary particle physics. In the 1970s experimenters expected to be able to understand mathematically and to evaluate new theoretical ideas; we knew group theory, could calculate in field theory, and could follow the development of quantum chromodynamics. Experimenters did not expect to follow in detail higher order corrections in quantum electrodynamics, but they could depend on the few practitioners of that intricate science to check each other's results.

The one limitation in the particle theoretical perspective of the early 1970s was that both theorists and experimenters were too conservative about speculation. If a speculative idea could be explored by data acquired for other purposes, the speculation would be tested, but if a dedicated experiment was required the chance of support and accelerator time was small. In the heyday of the bubble chamber this limitation was not serious since with enough patience obscure signals could be exhumed. Fortunately, since the 1970s speculative particle physics ideas have become much more acceptable and testing of speculative ideas has become easier as the large solid angle, multilayer collider detectors have become more powerful.

PARTICLE THEORY IN THE LAST TWO DECADES

Unhappily, in the last two decades speculation has overwhelmed disciplined theoretical work in particle physics. By disciplined I mean that new theoretical work must have some, not necessarily all, of the following properties: the work might mathematically simplify older work or might require fewer arbitrary constants or might use better motivated mathematical functions; the work might connect to older established theory; the work might predict new particles or phenomena, but also predict what unknown particles or phenomena do not exist; the work might provide quantitative explanations for parameters already well measured, such as the masses of the charged leptons.

There are dozens and dozens of speculative proposals for new particles to be found at the LHC. Many of these proposals are based on intricate mathematics that experimenters and most theorists do not have the time to study. There are two causes for the explosion of undisciplined theories. One cause is the belated involvement of the particle physics community, experimenters and theorist alike, in the great discoveries in astronomy and astrophysics. Most embarrassing for particle physicists is that it took us so long to accept the existence of dark matter. For many years we preached the success of the standard model of elementary particles while ignoring dark matter. Finally, astrophysics and particle physics have meshed—a very good outcome. But the price we pay is that speculation is now out of control. These days the only requirements on speculative particle theory are that there be no logical errors, no mathematical errors, and no contradiction with astronomical observations or particle physics measurements. As Cole Porter titled his 1934 musical, “Anything Goes.” What is the harm, particularly if the paperless office and library finally occur? The harm is that the experimenter already overwhelmed by the complexity of the hardware and software in detectors has no time to sort out the reasonable from the unreasonable. And the student theorist is being brainwashed while still in the theoretical nursery. The student must eventually learn the hard lesson that most new ideas in science are wrong.

I also lament the attendant publicity given to speculative theory. The string of wellness articles in the *New York Times* Tuesday science section is often interrupted by articles touting string theory. I do not like the wellness articles because they confuse the general reader who cannot distinguish between proven medical knowledge and wished-for medical cures. For example, if you had eaten the right diet you would not have cancer. And I do not like the string theory articles because they confuse the general reader who cannot distinguish between proven particle physics knowledge and what some members of our community wish would be true.

I live in Silicon Valley and most of my friends are engineers, scientists, and computer experts who work in industry. When they ask me if I pay attention to string theory predictions my answer is as follows: I tell them the tau lepton has the same electromagnetic properties as the electron but is 3500 times more massive. I ask them to think about what new kind of electronics could be invented by replacing electrons with taus, just mentioning at the end that the tau lifetime is less than 10^{-12} seconds. Of course they then say that they have better things to think about with their time and their company’s money. So it is with particle experimenters. We have better things than string theory to work on with our time and limited money.

THE COST OF EXPERIMENTAL PARTICLE PHYSICS

Having made sufficient theory enemies for 2008 I now turn to the increasing cost of experiments. In the interest of full disclosure, I tell you that I am a chemical engineer turned physicist and have spent 50 years in experimental work. The technology used in particle physics experiments—particle detectors, analysis software, colliders—has improved immensely in the past 30 years: Accelerator physicists and engineers have done more profound theoretical work, vastly improved simulations, and created many ingenious inventions that led to greatly increased luminosity and higher collider energies. SPEAR’s luminosity was between 10^{30} and 10^{31} per cm^2 sec, depending on the energy. The present day B factories, KEK-B and PEP (Positron-Electron Project) II, reach about 10^{34} per cm^2 sec. The LHC is designed to reach 10^{34} per cm^2 sec with a planned upgrade to 10^{35} per cm^2 sec. Accelerator physicists in Italy and Japan are designing B factories with luminosities of up to 10^{36} per cm^2 sec, an increase of 10^5 in luminosity over SPEAR!

The increased construction costs of particle physics experiments, particularly particle colliders, has led to worry inside and outside our community. Our community pessimists worry that the LHC will be the final high-energy collider. Scientists in other areas of physics and outside physics complain that we spend too much money compared to their fields. And funding agency officials worry about how they can convince those who control our national budgets of the worthiness of our work. The main cause of these worries and warnings is the increase in construction costs over the years. I will show that these cost increases have been exaggerated.

I have found it impossible to establish reliable costs for colliders. Costs are obscured by differences in accounting practices, by currency differences, by the hiding of construction costs in operation budgets, by not including the costs of previously existing facilities such as tunnels, and by other human failings.

The SPEAR electron-positron collider cost about 5 million 1970 U. S. dollars [3]. However SPEAR was an unusually inexpensive collider because it was built above ground—no tunnel. A better benchmark starting point is the cost of the CERN Intersecting Storage Ring (ISR) proton-proton collider completed in the early 1970s. Its cost was 330 million 1965 Swiss francs [4]. (By the way, using the 1970 exchange rate of about 4.4 Swiss francs per U. S. dollar, the ISR construction cost was about 75 million U. S. dollars.) I could not establish a definitive benchmark cost for the CERN LHC. For example, should its benchmark cost include the cost of building the LEP tunnel used by the LHC? I estimate the LHC cost in the range of 5 to 10 billion euros equal to 8.5 to 17 billion 2007 Swiss francs. The ratio of LHC cost to ISR cost is thus in the range of 25 to 50. But it is necessary to correct for inflation, about a factor of 7 for scientific facilities in the last three decades. And we certainly should correct for the growth in the real gross domestic product (GDP) of Europe, about a factor of 2. The product of inflation and the growth in real GDP is about 14. Dividing the 25 to 50 by 14 yields 1.8 to 3.5 for the increased cost of particle colliders in three decades after correcting for inflation and GDP growth. This is the true increased price to society for our continuing broad, highest energy research in elementary particle physics. As the economy grows, a society should be willing to support more scientific research. I believe high-energy physics is well worth the cost. However, it is clear that the world's resources must be combined to build these highest energy accelerators.

For my friends not in particle physics I present the following comparison: The U. S. National Institutes of Health (NIH) biomedical research budget increased in real dollars by a factor of 8 [5] from 1970 to 2005. Correcting this by the real growth in the GDP, a factor of 2, the increased cost of NIH funded biomedical research is about 4. This is about the same as my 3.5 upper limit for the growth in collider costs.

PUBLISHING IN EXPERIMENTAL PARTICLE PHYSICS

In arguing for new accelerators, experimenters must return to less publicity and more dignity. I am dismayed by press releases that announce the null result of a new particle search. Such a result is analogous to an electric car company announcing that its quest for a lighter storage battery has failed. In the case of failed particle searches, only a scientific paper is needed so that the search information is known and to ensure that those repeating the search have made substantial improvements in sensitivity.

Much of the pressure for publicity comes from the natural desire to be first, even in a null search, combined with the speed of the Internet. When we announced the tau discovery in 1975 we did not have the Internet. Before publication I gave talks on our evidence; most talks were on the West Coast but I did venture to Canada [6]. I brought the questions and criticisms back to our SLAC-LBL Collaboration. We considered the criticisms carefully before we finally published. There was no great rush. And all the while we kept collecting data.

There was an even more leisurely publication process for my Columbia University Ph.D. measurement of the nuclear quadrupole moment of ^{23}Na (sodium 23) [7]. Eager to publish and leave Columbia, I discussed my final results with my advisor, Isadore Rabi. I was tired of being a poor graduate student with a wife and child living in a rundown apartment. He said that we had to check my result with that of the French physicist Alfred Kastler, [8] who had invented an optical pumping method for measuring the quadrupole moments of alkali atoms. No need to telephone or TELEX, he said. Rabi would write to Kastler. I was afraid to ask Rabi if he would use airmail. Six long weeks later Kastler replied that my results agreed with his; I began to look for a job and ended up as a University of Michigan instructor in physics.

THE SOCIOLOGY OF EXPERIMENTATION IN PARTICLE PHYSICS

These reminiscences bring me to the last part of this essay and the most painful part for me, the changes in the sociology of experimentation in particle physics—the enormous size of detector collaborations using major particle physics facilities. Collaborations now have as many as two thousand physicists, engineers, and students. One justified reason for such a large size is the enormous amount of complex technical and physics work in the construction, operation, and use of such detectors. Two unjustified reasons are: that the larger collaborations can gather more funds and that funding agencies push small groups into these collaborations.

The net effect of this large size is negative. Creativity and invention are suppressed, and difficult personalities are weeded out. Some of my best collaborators have been difficult personalities—not amenable to authority and always introducing unpopular technical and physics ideas after the group had made final decisions. I was a difficult personality until I reached my late seventies. At that age I finally understood two sayings of my old relatives from Russia-Poland. Translated from the Yiddish: "If you fight City Hall you will lose." And, "it could be worse." Fortunately, I was kept within bounds by Wolfgang Panofsky, a great physicist, the founder of SLAC, and a wise leader in physics and in scientific life [9,10].

I see no cure for the large size of particle physics collaborations. If a physicist wants to work in such a collaboration, she or he must be sure to have a tough mind and sharp elbows and should occasionally ponder the sayings of my relatives.

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