Electron Colliders at CERN

Symposium on the 50th Anniversary of PS and 20th Anniversary of LEP Startups

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Introduction

When I arrived at CERN for this symposium I found a laboratory full of young people, and an excitement in the air at the prospect of the start of the LHC, giving access to a new energy range. All hope we will finally get evidence of what is beyond our Standard Model that has stood unchanged since it was formed in the mid 1970s. Even then we all knew that that was incomplete and hoped that experiments at new energy ranges would reveal what was beyond the theory that explained what we had seen so far. In this talk I want to look back to start of LEP and its role in the hunt for new physics, and look ahead to the next generation of electron machines and what their role might be.

Reminiscences

After I finished graduate school at MIT I wanted to try doing an experiment that particularly interested me. The then new 1-GeV linear accelerator at Stanford's High Energy Physics Laboratory was the place to do it. At that time there were two cultures in particle physics with little overlap. I call them electron people and proton people. The two groups were separate cultures back then, and to some extent remain so even today. CERN is one of the few big labs that have mixed them.

There were many more proton people when I first went looking for a post-doc job, and most thought that the electron people were wasting their time with machines that would never tell anybody anything compared to the wonderful baryon and meson resonances that were being discovered at the proton machines. The big machines of the proton world back then were the 3-GeV Cosmotron and the 6-GeV Bevatron. In the electron world the leading facilities were the 2-GeV synchrotron at Cornell and the 1-GeV linac at SLAC.

There was much contact between Europe and the United States, but the two cultures stayed relatively separate. When I was starting out the closest contacts I had with Europe were with DESY in Germany, Frascati in Italy, and Orsay in France. The AGS at Brookhaven and the PS at CERN were under construction, as were the first of the colliding beam machines, the electron-electron collider at SLAC which I worked on, and a similar though smaller machine at Novosibirsk.

The electron horizon began to expand in the 1960s and early 1970s with the completion of several fixed target machines and a host of colliding-beam machines. The leading facilities were the SLAC linac, and the SPEAR, ADONE, and DESY electron-positron colliders at Stanford, Frascati, and Hamburg respectively. In short order in the first half of the 1970s experiments at both electron and proton machines made a revolutionary change in our understanding of elementary particle physics. The deep-inelastic scattering experiments of Friedman, Kendall, and Taylor showed that Bjorken scaling was correct and quarks were more than a mathematical convenience. The experiments of my group at SLAC and Ting's group at BNL showed that there had to be a fourth quark and that we should think in terms of generations of constituents instead of unrelated entities. The Gargamelle bubble chamber neutrino experiment at CERN showed that neutral currents were really there as required by the Glashow, Salam, and Weinberg model. Perl's analysis of SPEAR data showed that there was a third lepton and that three families were required to complete the subatomic picture as was required to accommodate CP violation in the Standard Model. With those experiments the Standard Model was firmly established and it has resisted ever since all attempts to find what is beyond it.

After the 1963-1970 struggles to get SPEAR funded and the excitement of 1974-1975, I needed a break and decided on a sabbatical at CERN as the best thing for me. I knew some of the accelerator physicists at CERN who had spent time at SPEAR, many of the young theorists who had spent time at SLAC with Drell's theory group, and some of the experimenters. Perhaps more importantly, I knew Willi Jentschke well. As director of DESY, Willi wanted to have DESY move toward colliding beams, but some of the senior scientists wanted to expand the electron synchrotron instead. He and Panofsky conspired to bring a DESY group over to SLAC and have Drell and me explain why colliding beams would be the best way to advance physics. We must have done well because DESY decide to do what Willi wanted them to do. In 1975 he was Director General of CERN, and when I wrote him about my desire to spend a year at CERN, he replied "yes", and my family and I showed up in the late summer of 1975. So began a thoroughly enjoyable period in our lives that had a long lasting effect on my career, and had a major effect on CERN as well.

CERN & LEP

Besides enjoying myself and learning about new areas of physics, I did two major things while I was here. One was an experiment at the ISR working with the Darriulat and Banner group looking for e-mu events with a third spectrometer arm that we built and added to their original experiment. We did indeed find a few, completing the analysis after I returned to SLAC. The people I worked with were more important in the long run. I am still in touch with many including among others Pierre Darriulat who is in Vietnam, and Peter Jennie who came to SLAC to work with me for a while. We tried to keep Peter, but he wanted to go back to Europe and only recently stepped down as spokesman for the ATLAS detector group, leading it through its complex construction phase. Incidentally, it is the 38th anniversary of the start-up of the ISR in 1971, and the 25th of its shutdown in1984. CERN might think of the ISR as the real progenitor of the LHC so you will have an excuse for a fortieth anniversary celebration in only two years.

The second thing I did here was much more important in the long run. I wanted the relative peace of a sabbatical to think through the scaling laws for and limits to much higher energy electron-positron colliders than existed then or were in the planning stage. My physics interest was in the weak interactions. Perhaps Carlo Rubbia remembers some of our discussions on the subject. I used as a model a 100-200 GeV electron-positron collider of relatively high luminosity tailored to look for the Z-boson, and to uncover all of its properties. The paper I wrote and published in Nuclear Instruments and Methods[1] developed the scaling law for storage ring colliders (size and cost go as the square of the energy), made the physics case for 100-GeV machine, and was, I perhaps immodestly think, the real start of the LEP program.

At the time, the SPS was starting up, and CERN was beginning to think about its next project. Items under discussion with differing degrees of seriousness were a 10 TeV proton machine, a 400x400 GeV super ISR, and an electron-proton collider to be created by adding an electron ring to the SPS. A large electron-positron collider was added to the list, and a group of the younger accelerator physicists began a study. The result was the CERN Yellow Report[2] "CERN-76-18". With an author list that included Mary K. Gaillard, John Ellis, Carlo Rubbia, Jack Steinberger, Bjorn Wiik and me, how could it be resisted?

There is a little known story about a futile attempt to get LEP built at CERN as a U.S. - Europe joint project. The two people trying for a social breakthrough in parallel with the scientific one that LEP would produce were Guy von Dardel (figure 1) and me. The time was in 1976 or 1977 when Guy was Chairman of the European Committee for Future Accelerators (ECFA). I worked on the Americans and Guy worked on the Europeans. I made little progress in the U.S., and Guy made little here. Our attempt to arrange an intercontinental collaboration came to an end at a meeting of restricted ECFA. I was invited to give a presentation and then left the room while the idea was discussed. Guy came out about an hour later and I knew from the look on his face that

[1] B. Richter, Nuclear Instruments and Methods, v136, p47 (1976)
[2] CERN-76-18, Physics with very high-energy e+e− colliding beams

we had failed. If the physicists were against it, there was no point to going to governments. We were too early in out attempt. Projects had not gotten large enough to make interregional collaborations necessary.

It took some time and much maneuvering by the CERN DGs of the time to get the project through the Council, but Herwig Schopper did get it through, and groundbreaking took place in 1983. We physicists are an impatient lot and 13 years from a gleam in the eye in 1976 to collisions in 1989 seemed a long time, but CERN became a major player in electron physics.

Beginnings of Linear Colliders

With the rejection of an international LEP, I began to think about what was next since LEP would be European only. I looked at what I called in some of my later talks LEP 1000. Since my scaling law said size and cost should grow as the square of the center-of mass energy, LEP-1000 was really big having one interaction region in Geneva and the diametrically opposite one in London (figure 2). My cost estimate for LEP-100 was 1.5 times the cost of the SPS, but LEP-1000 would be 150 times the SPS cost. Physicists are not modest in asking for money, but this was too much even for me. I began to look at other possibilities, particularly linear colliders with their first-power scaling law. The SLAC linac could run at 20 megavolts per meter and a 1-TeV machine would only be 50 kilometers long compared to the LEP-1000 circumference of 2700 kilometers. 50 kilometers was big, but not huge compared to the circumference of the LEP tunnel.

The International Committee on Future Accelerators (ICFA) had scheduled a workshop to take place at Fermilab in 1977 on limitations of accelerators and detectors. I discovered at the workshop that Sasha Skrinsky (Novosibirsk), Maury Tigner (Cornell) and I had been thinking along the same lines. We three wrote the section of the report on electron colliders laying out all the conditions for a workable linear collider; luminosity requirements, the beam-beam interaction in a one pass regime, synchrotron radiation in the collision region which John Rees christened beam-strahlung, etc.

I returned to SLAC and began, with the SPEAR group to design what was to become the SLAC Linear Collider (SLC). The two linacs of a true linear collider were folded into one, and a set of magnets separated the beams and then brought them into collision (figure3). The DOE put together a review team led by Dr. Paul Reardon was appointed to see if it all made sense. The issue facing the DOE administrators was how to do a project involving a totally new technology where the final performance could not be guaranteed in advance. Back then the DOE was more flexible that it seems to be today. The committee created the notion of an R&D construction project that did not have a specific number like luminosity as a goal, but was instead to allow the evaluation of the technology. The DOE agreed with the concept and off we went. First beams were in 1987, but while we had anticipated some of the problems of a new kind of collider, we had not anticipated them all. It took two more years for the SLC to start producing physics. The first physics results from both LEP and the SLC were presented at the Lepton-Photon conference of 1989. In the long run LEP became higher in luminosity, but longitudinally polarized electron beams that were produced for the SLC made critical physics contributions. The CERN Courier recently had a photo of the LEP turn on (figure 4) and I thought I should add one of the SLC commissioning group (figure 5) highlighting the leading role of Nan Phinney.

It took some time to bring the SLC up to a reasonable level of performance. By 1992 that had occurred, polarized electron beams became available and the SLC's physics output began to contribute to the precision analysis of the electroweak parameters. We had all hoped that precision tests of the theoretical predictions would give a hint of what lay beyond the standard model. It was not to be. What was to be were the beginnings of studies in Europe, Japan and the US of much higher energy linear colliders.

In 1993 the SSC was killed by the US Congress. There have been many suggestions of why it was killed, but the one that affected what I called the Next Linear Collider was that there had not been any meaningful international collaboration in setting its parameters or in its design. Bjorn Wiik, Hirotaka Sugawara and I discussed what had gone wrong with the SSC and concluded that the NLC might avoid the same fate if it were international from the very beginning, and so began a collaboration that lasted for many years. The first technology review took place in 1995. The candidates at the time were an S-band, room-temperature linac; an X-band, room-temperature linac; and a superconducting linac. Gus Voss of DESY was a strong advocate of proceeding to the next machine with the tried and well known S-band (3 GHz) technology and aiming advanced R&D at the machine after that. On the occasion of his 80th birthday, I was to give a talk and looked up the old 1995 table that compared the three technologies (figure 6). I was surprised to find that S-band used less power than the superconducting option. I told the DESY audience that, with the benefit of hindsight, Gus was correct. We should have gone with S-band for the 500 GeV machine and developed he new technology for the machine beyond that. If we had, a 500 GeV machine might be turning on about now as a companion to the LHC. We had forgotten an important lesson; it is not the technology that matters it is the physics.

After several more years we three realized that we had made a serious error in setting it up the collaboration. We had allowed technologies to become linked to laboratories. DESY was superconductivity, while KEK and SLAC were room-temperature. What we should have done was to have both technologies under development at all three labs. It would not have cost significantly more and would have made the final technology selection much easier since all labs would have been practitioners of all technologies.

There is a lesson here for the ILC/CLIC groups. Do not let two beams be the sole property of CERN while superconducting RF remains the sole property of the ILC collaborators. It might be useful to remember that CERN developed superconducting niobium-on-copper cavities for LEP that performed better than their all niobium counterparts. Might they have a role for ILC? Though CERN says it is in a money squeeze, having some of the CLIC work done elsewhere frees up funds for other things here.

What should be the energy of the NLC?

Many years ago we all hoped that a linear collider of 0.5 TeV to 1.0 TeV would be a companion to the LHC in exploring the lands where we might find what lies beyond our standard model. However, the ILC became too expensive for the funding agencies, and major international attention on science projects became focused on fusion energy and the ITER project.

We will not know much about the new high-energy physics landscape until 2012 and perhaps even later. I think it unlikely in the extreme that any one will pay attention to our desire for still another expensive accelerator until we know something about what will come from the one that is turning on even as I speak. That being so, a fast track schedule might be as follows;

- 2012 - LHC results indicate that XXX is the desirable minimum energy for a future linear collider
- 2014 - Final design starts and international discussions begin
- 2017 – An international agreement is reached and construction starts
- 2025 – the new machine turns on

I find it impossible to believe that 500 GeV will be of much interest in 2025. It is true that only an e+e- machine can find a new heavy lepton, and that an e+e- machine is probably best suited to find all the couplings of a relatively light Higgs boson (if there is such a thing), but that is an awfully light meal for a 5-star price amounting to many billions of Dollars, Euros, Yen or Yuan. Also, experimenters are very clever and things that today seem hard to do at a proton machine will likely get done given enough time. B_s mixing is an example. It was supposed to be too hard to measure at FNAL, but the experimenters did it, though it took more time than it would have taken at an electron collider.

I do not see the sense of the ILC or CLIC groups' discussion of 500 GeV for an initial energy, nor sticking to 500 GeV in the evaluation of detector designs. It makes even less sense to have the CLIC group discussing low luminosity versions.

I would like to end by saluting another sure to be long remembered year in the history of accelerators and of CERN; the zeroth birthday of the LHC. We have been waiting a long time for an accelerator to tell us what is beyond the standard model, and we all hope that this will be forthcoming soon. CERN is now the leading lab in accelerator based high energy physics and everyone hopes that by the 10th birthday of the LHC in 2019, we will be celebrating the discovery of the something really new that not even the theorists have thought of as yet. Congratulations to CERN on its past and on the next step in its sure to be illustrious future.