SLAC BEAM LINE

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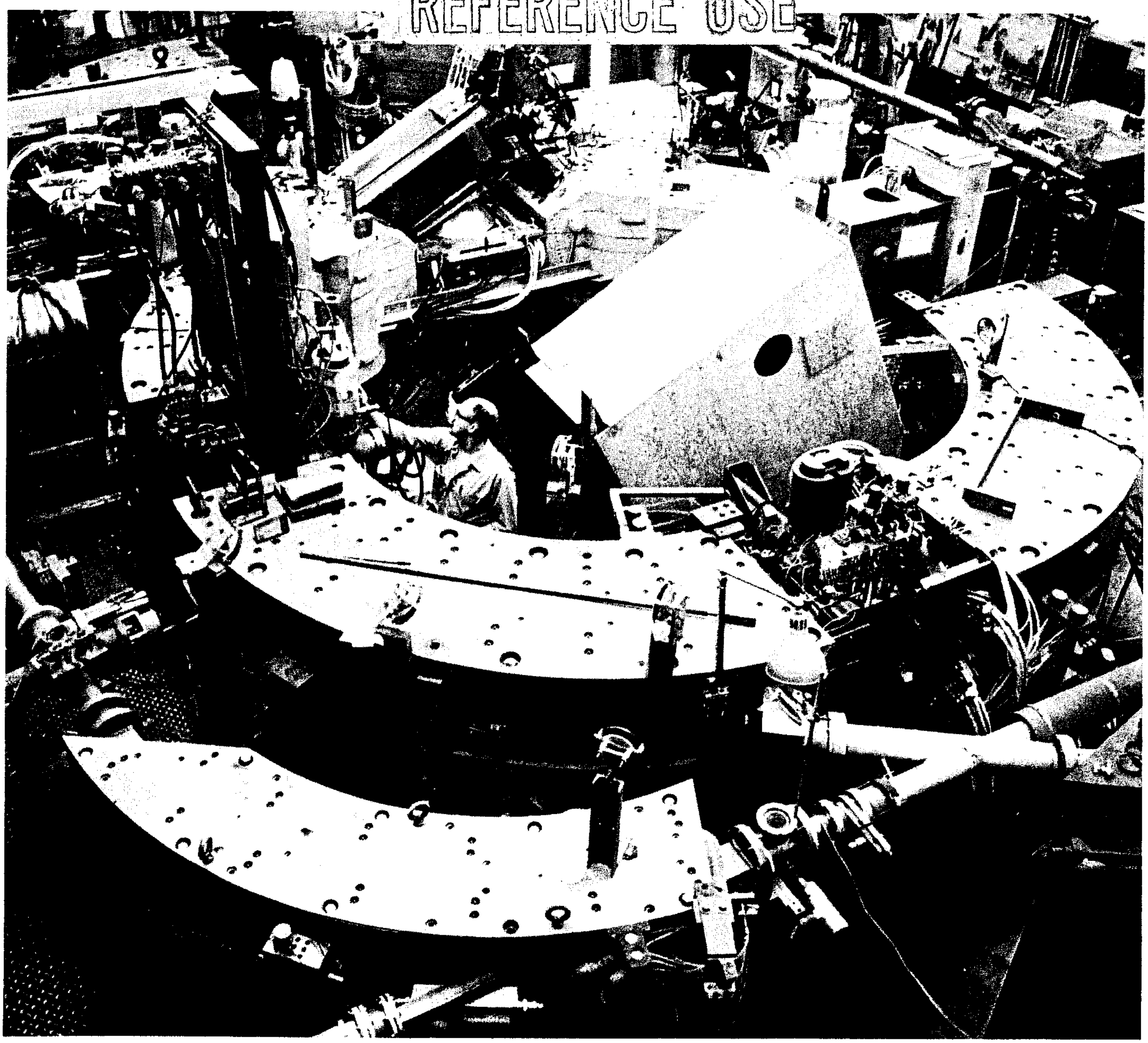
# —AN INFORMAL HISTORY OF SLAC—
# PART III: COLLIDING BEAMS AT STANFORD

**by Burton Richter**





THE STANFORD-PRINCETON STORAGE RINGS, 1965

# COLLIDING BEAMS AT STANFORD — — MACHINES AND EXPERIMENTS

*The following talk was given by Burton Richter in August 1982 as part of SLAC's Twenty-Fifth Anniversary celebration.*

All my work has been with electron experiments and with the technological developments to make those experiments possible. Although most of this has been with colliding beams, it started out quite differently.

When I was a graduate student I used to speculate about quantum electrodynamics (QED), the basic theory of electrons. This was a marvelous theory, but it had some puzzling features. In some calculations infinity had to be subtracted from infinity to get the answer. I thought that this couldn't be quite right and that the place to look for disagreement would be in experiments with high-energy electrons.

I came to Stanford in 1956 because this was the unique place to test quantum electrodynamics, and I had an experiment all designed to do it. Scattering the electron beam of that wonderful linac, the Mark III, on the electrons in the atoms of a target would test QED to a cutoff energy of 30 MeV. I never did that experiment. Instead, I ended up doing an experiment which checked the validity of QED to something like three or four thousand times what I proposed to do with the Mark III linac. That experiment, and several others of the kind, were all done in a different way — with colliding beam machines.

## COLLIDING BEAMS

Colliding beam machines really had their beginnings in 1955 at the Midwestern Universities Research Association (MURA). The MURA people were studying how to build a high-energy proton machine. The objective was to get much more center-of-mass energy for a given cost of the machine. If they took an incident beam with energy $E$ and plowed it into a target whose particles had mass $M$, then the center of mass energy (the energy that goes into producing new particles) would be the square root of $2ME$. If, instead, they collided two beams of energy $E$, they would get $2E$ for the center of mass energy.

*SPEAR — THE SECOND STANFORD COLLIDER The storage ring SPEAR, in the center of the photograph, was the second Stanford colliding beam machine and the first built at SLAC. The beams enter in two lines at the top right. Many of the buildings surrounding the oval ring are for experiments using synchrotron radiation, a byproduct of the stored beam.*

For example, a 10 GeV accelerator shooting protons at a target of 1 GeV protons would give about 5 GeV in the center of mass. If they built two rings of 5 Gev each instead, and collided the beams with each other, they would get 10 GeV in the center of mass. This is about twice the energy out for the same investment in machinery. For larger machines, the benefit is even bigger.

The general idea in the MURA days was to build two rings, fill them with a beam from an accelerator of a lower energy, bring the stored beams up in energy, and collide them. That's generally what we do now, but injection and storing looked extraordinarily difficult in those earlier days when accelerators were not understood in detail. How, for example, could you inject new particles into the rings without disturbing the ones that were already there?

## THE STANFORD-PRINCETON COLLIDER

In 1957 Gerard K. O'Neill, Gerry, from Princeton visited Stanford with an idea that could solve this problem: use electrons, not protons, for the colliding beams. You can avoid interference with the already stored beam by injecting new beams off to the side of the machine. That's fine, but then you somehow have to bring the new particles into the right place. This is where electrons make the difference.

When electrons are bent around in a magnetic field, they lose energy in a process called synchrotron radiation. If you inject an electron far off the centerline of the machine, it will start to oscillate around the proper orbit. As the electron loses energy, however, this oscillation will die away and the particle will end up on the centerline where it belongs. Then you can inject a new bunch off to the side, and let it damp down. By repeating this it is easy to store a large beam.

Here was a way to get into the storage ring business without all the problems of injecting and stacking protons. In addition, there was some physics you could do: test quantum electrodynamics at an absolutely unprecedented energy. If you had two 500 MeV storage rings you could reach 1 GeV in the center of mass in a system that had nothing but electrons in it. The calculations were easy and the predictions were unambiguous. This was more than thirty times higher in energy than the experiment that brought me to Stanford, and was much cleaner as well.

In 1957 a group of us got to work designing this machine, and in 1958 Pief, in his own inimitable fashion, pried eight hundred thousand dollars out of the Office of Naval Research to build the first of the storage rings. This was an unprecedented amount of money at the time for a single experiment. And probably this is the last storage ring for which you can list *all* of the people who worked on the machine. The physicists were Carl Barber, Bernie Gittelman, Gerry, and myself. Our chief mechanical engineer was Jim Walling. We had four technicians: Louie Bogart, Norman Dean, Gerry Gleason, and Clarence Noyer. Dean and Noyer are still at SLAC; I'm still there; Bernie's at Cornell; Gerry's still at Princeton; Carl's at MIT; and that was our entire group.

*COLLIDING BEAMS & BEAMS AND TARGETS*


*The arithmetic of colliding beams is straightforward. In this example a head-on collision of two protons of 5 GeV gives a collision energy of 10 GeV.*


*Colliding a proton of 10 GeV with a stationary proton gives only about 5 GeV of collision energy as most of the energy is wasted by the two particles moving away together after colliding.*

What we built looked much like what was being considered by MURA for protons. There were two rings and electrons circulated in both. A radio-frequency system made up the energy lost by the electrons to synchrotron radiation. The rings in which the beams circulated were under vacuum; at the time this was the largest ultra-high vacuum system in the world. A detector system made up of spark chambers surrounded the interaction region where the beams crossed. [Some of these features are shown in the photograph on the cover.]

The first funds were available at the end of 1958, but it was a hard job making this machine work. We had preliminary results on electron-electron scattering in July of 1963 and we published the final paper in 1965. In case you're in suspense, quantum electrodynamics worked! In 1967 we set up a short, final experiment in which we looked for muons which might be produced in these collisions. Shortly afterwards, we shut the machine down.

Quantum electrodynamics claims that the electron is pointlike, that it has no size or structure at all. Our experiment showed that if the electron does have a size or structure, it is smaller than $3 \times 10^{-14}$ centimeters. This was a major improvement in our knowledge of the electron and a very good test of QED. Better measurements would require larger machines. The muon measurement showed that muon pair production from two electrons of the same sign was less than 1% that expected from a positron-electron pair. This meant that the quantum number which distinguished electrons from muons was not multiplicative. These were important results, but the main contribution of the Princeton-Stanford storage rings came in accelerator physics and not in particle physics. Had QED failed, of course, the main contribution would have been remembered differently.

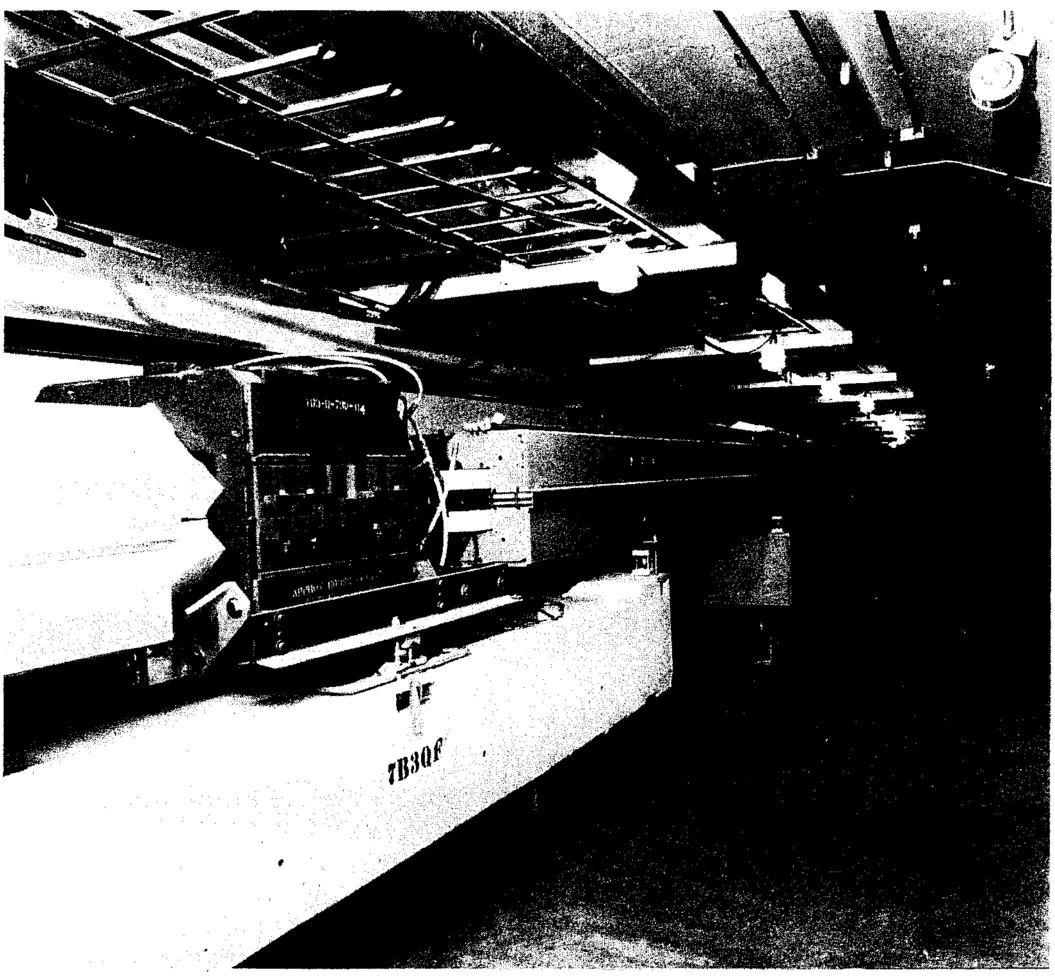
When we set out to build this machine we tried to think of what could go wrong with a beam circulating for hours in a small magnetic storage ring at 25 million times per second. We thought of three possible problems: residual ions, chromatic aberrations, and the beam-beam interaction.

Ions could be produced in the residual gas of the vacuum chamber by the circulating beam, and those ions could get trapped in the beam and give instabilities. We installed clearing plates which could sweep out the ions. And indeed, when you turned off the clearing field, the ions got trapped and, by god, the beam blew up. We'd done one thing right.

The focusing of the magnetic guide field changes slightly with the energy of the particles. The extent of this variation is called chromaticity, and it is the bugaboo of big strong-focusing machines. Chromatic aberrations lead to an instability called the head-tail effect in which the beams cannot be controlled. The aberrations were corrected in this machine and there was no problem with this instability.

When the beams actually collide, they affect one another in ways which lead to another kind of instability called the beam-beam interaction. We calculated the beam-beam interaction completely wrong and got a limit which was much too high and much bigger than actually observed.

Then there were the things that we didn't think of ahead of time. We were surprised by degassing induced by synchrotron radiation. This is now understood as the synchrotron radiation photons bombarding the wall of the chamber and knocking photoelectrons out; and those photoelectrons in turn knock atoms off the chamber wall. The only solution to this was to pump the machine very hard and keep it very clean. We found out about single beam coherent instabilities caused by the electric fields left behind by the beams. Since it only




*PEP — THE THIRD STANFORD COLLIDER*
*The PEP storage ring, with about four times the energy of SPEAR, was completed in 1980. The photo shows some of the magnets which guide the stored beams around the 1.4-mile circumference of the underground tunnel.*

took the beams 40 nanoseconds to go around this small machine, the long-range wake field was still around to affect the beam on the next turn. We fixed this by adding octopole lenses to the ring.

We were surprised by two-beam coherent oscillations. When the beams collide with each other, they can under certain circumstances oscillate out of phase. These oscillations prevent the beams from colliding as completely as they should. This was fixed by slightly changing the focusing properties, called the tune, of the two rings.

Back to the beam-beam incoherent limit — we never solved that problem and nobody has ever solved that problem in a fundamental way. The effect is described by a number called the tune shift. Larger tune shifts mean more beam-beam collisions for a given machine. The Princeton-Stanford rings ran into the limit with a tune shift of .025, not very different from what people find now. The highest tune-shift is in *SPEAR* with .05; *PEP* and *PETRA* get about .03; and Cornell gets about .035. This is now understood only in a most phenomenological way by making a computer simulation of the machine. A value for the tune shift comes out of such a model, but there is no understanding of exactly what is responsible or, more importantly, how to make it bigger.

There were three people who, besides the practitioners on this machine, made most important contributions: Ernest Courant, Dave Ritson, Andy Sessler. Ritson did some important theory work and also helped us build some of the equipment. Courant and Sessler did a lot of work on the beam-beam interaction.

This machine was the first of the storage rings, the pioneer, and its descendents make up a long list which includes those built and being built for high-energy physics and also for synchrotron radiation studies. People learned a lot from that machine and what was found in it. I think it gave a lot of people the courage to go ahead with bigger ones; if that thing could work, larger ones would work.

The children of that machine far outstrip the parents. The parents can point with pride at the activities of their children and say that they have done some very nice things. But we can remind the uppity kids that the Princeton-Stanford storage rings still have the record for current in a single bunch in an electron machine. It was 0.6 *amperes;* it has never been approached by any other storage ring.

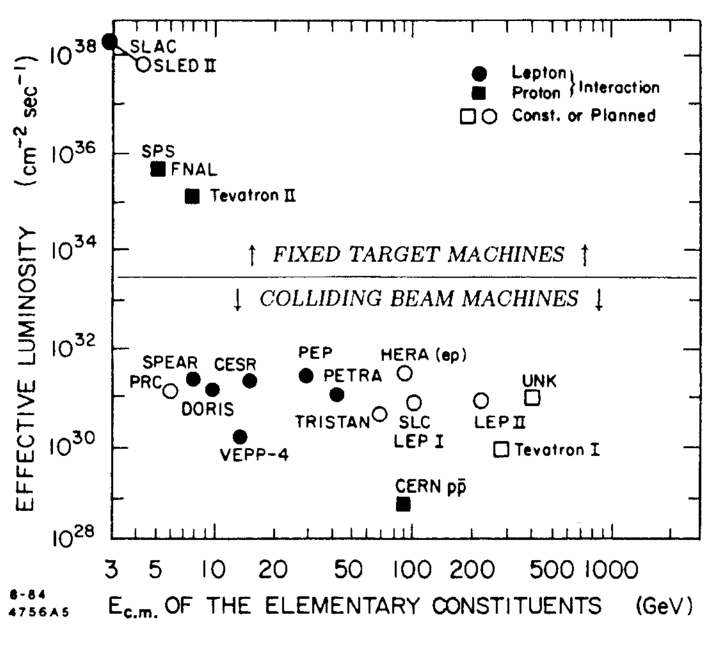
## SPEAR — THE NEXT STEP

Let's pick up the next thread in this story. The Princeton-Stanford storage ring is an aberration in this list of machines. It's an electron-electron machine while all the others are electron-positron machines. So why electron-positron, what's different about it? The thing that's different about the electron-positron system is that the system can annihilate and produce an intermediate state which has high energy, high energy-density, and no other identifying features. That state can then decay into other particles. You can produce *anything,* as long as you obey the basic conservation laws of physics.

Theory describes the interaction of an electron and positron in a two stage fashion: electron and positron annihilate to produce a high-energy intermediate state, and that intermediate state turns back into particles. By studying how these particles are produced you can learn about their structure.

The light bulb turned on for me that this was the right thing to do in 1958. Bjorken was teaching some of the postdocs how to calculate with quantum electrodynamics and assigned a homework problem: calculate the pair production of a pair of spin-zero particles. I realized that pions were spin-zero particles, and that their production in electron-positron collisions could tell you about the structure of the pion. Other people realized this too, and the chase for building electron-positron machines started.

The first of those machines was started by the Italians in 1961: a tiny little machine called *ADA,* with a radius of about half a meter. A much bigger machine in Italy, *ADONE,* was started in 1963. The French began their machine *ACO* in 1962, and the Russians began




*THE GROWTH OF COLLIDING BEAM MACHINES →*
*This chart show most existing and planned high-energy accelerators in the world. The vast majority are colliding beam facilities, and all of these were built after SPEAR.*

their first one, *VEPP-2,* in the same year. In 1961 Ritson and I began the design of the machine that is now *SPEAR*.

Building *SPEAR* was a saga that makes very interesting reading when you go back over the history. I'll give you a few of the milestones, editing out the unprintable comments I usually make when I go down this list. In 1963 the first preliminary plan went into the Atomic Energy Commission. In 1964 the first formal proposal went in at the same time that a ring at the Cambridge Electron Accelerator was proposed. The *AEC* appointed a committee chaired by Jackson Laslett to look at these two proposals. The committee recommended building *SPEAR* at *SLAC*, but felt that things were still too mysterious on the Princeton-Stanford machine. We should wait a year and see whether we understood that machine better.

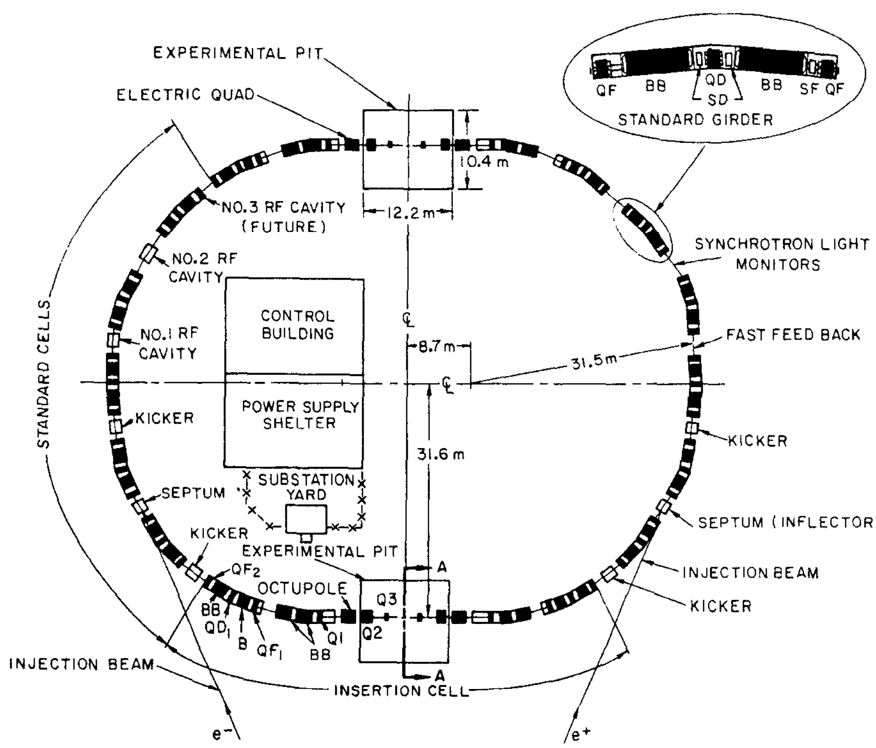
In 1965 the proposal went back in again and the Laslett panel said, Yes indeed they had learned about those things, go ahead. But we had missed the boat; funding for particle physics had begun to get tight. In 1966 the third proposal went in, and the Pake committee made a very strong recommendation for early funding. In 1967 we got tired of writing new books and resubmitted the same book. Still no money.

In 1968 the High Energy Physics Advisory Panel *(HEPAP)* strongly recommended early funding. In 1969 we redesigned the machine to make it a lot cheaper, and *HEPAP*—in what I characterize as a strongly worded recommendation—said to build the damn thing. We didn't get the money.

In 1970 we reduced the machine further, making it one ring instead of two, and were again turned down for a construction proposal. This time, however, we managed to get permission to build the project out of the lab's ongoing budget; until this time that had been forbidden. This change was an invention of the then-comptroller of the *AEC*, John P. Abbadessa. I've had a soft spot in my heart for Mr. Abbadessa ever since.

A schematic of *SPEAR* is shown below. It's a single ring, a collection of magnets, radio-frequency systems to make up for synchrotron radiation , bending magnets to bend the beam, focusing magnets to hold it in, two injection lines going back to the *SLAC* linac for $e^+$ and $e^-$, and two interaction regions to do physics.

The first dollar was available in August 1970. In May of 1971 the first of the girder modules that compose the machine rolled out of the shop. In July of 1971 the site began to look like there might eventually be a storage ring there. In April of 1972 it was all finished and ready to roll. The last wire was hooked up on the 15th of April, and on April 28th of 1972 the first colliding beams were obtained. In the remarkably short time of 20 months we had gone from an empty field to a working piece of physics apparatus.




*SCHEMATIC OF THE SPEAR STORAGE RING The blackened links show the magnets which guide the beams around the roughly 600-foot oval path. This shape is just visible among the buildings in the photograph on page 2.*

THE NEW PHYSICS

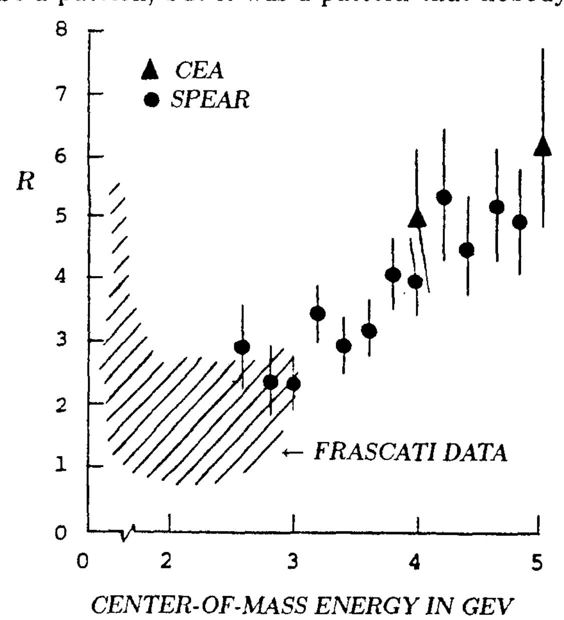
Two physics experiments were built simultaneously with the machine, one for each of the two collision points. These large pieces of apparatus were ready to do physics at the same time as the machine; no time was lost. The two first experiments were a very large sodium iodide array of Bob Hofstadter's and the Mark I detector which had been built by a collaboration of SLAC and LBL physicists.

What was known about $e^+e^-$ physics? By that time it was clear that something strange was going on. Frascati experiments had shown by 1968 that the production of hadrons was very big. 'Big' meant that the production of hadrons was comparable to the well understood processes of quantum electrodynamics in which electrons or muons were created. The ratio of the new process to the familiar one was called $R$, and was about one. There were a few people — a very few people, Bjorken again one of them — who had long expected that $R$ would be big, but when the first results came in people were very surprised.

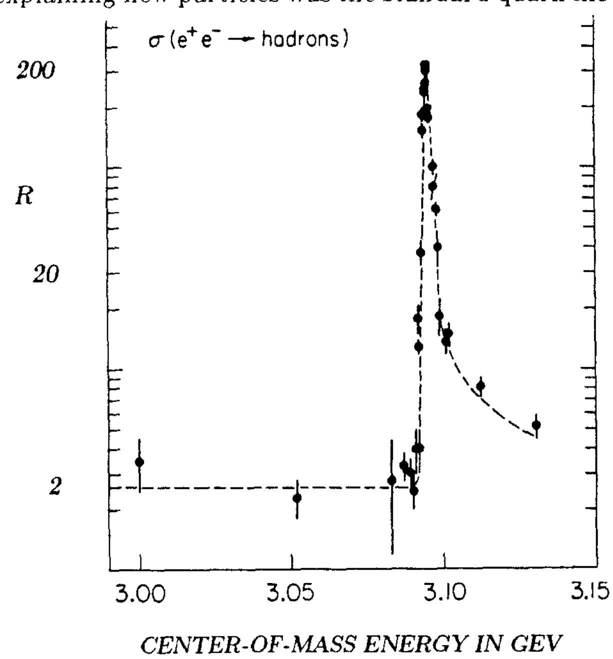
The Frascati results are shown in the figure. $R$ was big but it was messy. It's on the order of a few, not 0.1 or .01 which most would have expected, and there was no clear pattern to its behavior with the energy of the machine. By the summer of 1974 the first results from the Mark I detector were available, as well as two points from the CEA bypass ring. Now there seemed to be a pattern, but it was a pattern that nobody had really expected. An experimenter looking at that graph without the benefit of theoretical prejudice would say that that's clearly a straight line. And that's exactly what I said: this ratio of $R$ is increasing without bound, leptons were hadrons, and they had strong interactions.

The data managed to wring out from the theorists quite a range of predictions on the value of $R$. A list from John Ellis's talk at the London conference summarized all the theoretical models put up to explain what was going to happen in $e^+e^-$. The predicted values of $R$ went from 0.36 all the way on up to infinity; and you could have had your choice of any one of them. It is indeed true that Iliopoulos at the same conference explained everything, but nobody was listening to him.

Well the explanation for what was going on came in November of 1974 and it came with the discovery of the first of the great resonances, the $\psi$, at about 3.1 GeV. What was seen here as one looked carefully in this energy region of about 3 GeV was a rise of a factor of something like 100 in the hadron cross section as the energy was changed by just a tiny little bit. At the same time the $\mu$-pair cross section increased by about 20, and the electron-pair rate doubled. What was so absolutely shocking about all of that was that these changes came and went with such a very small change in energy; the resonances were so narrow. What most people would have thought of as the proper physics for explaining new particles was the standard quark model.




*UNEXPECTED NUMBERS The number R, which measures the relative number of strongly interacting particles like pions produced in electron-positron collisions, was the first hint of new physics. The early results from Frascati showed numbers around 2, when people were expecting something ten or a hundred times smaller.*




*THE PSI PEAK This graph, which is similar to the one at the left shows a dramatic change in the value of R. The sharp and sudden increase in the number of strongly interacting particles produced clearly signaled new physics. This peak, called the $\psi$, was later determined to be a tightly bound pair of charmed quarks.*

That mode would have said that those peaks ought to be about 600 MeV wide, in sharp contrast to the one or two Mev spread which we saw. The first data had simply skipped over these narrow peaks!

I did something then that I've never done before and never done since. Gerson Goldhaber and I wrote the paper online as the data was coming in. We had seen enough of the peak to know what we should say about it, so we withdrew while the rest of the group sat eating and drinking and enjoying the oscilloscope and looking at the events.

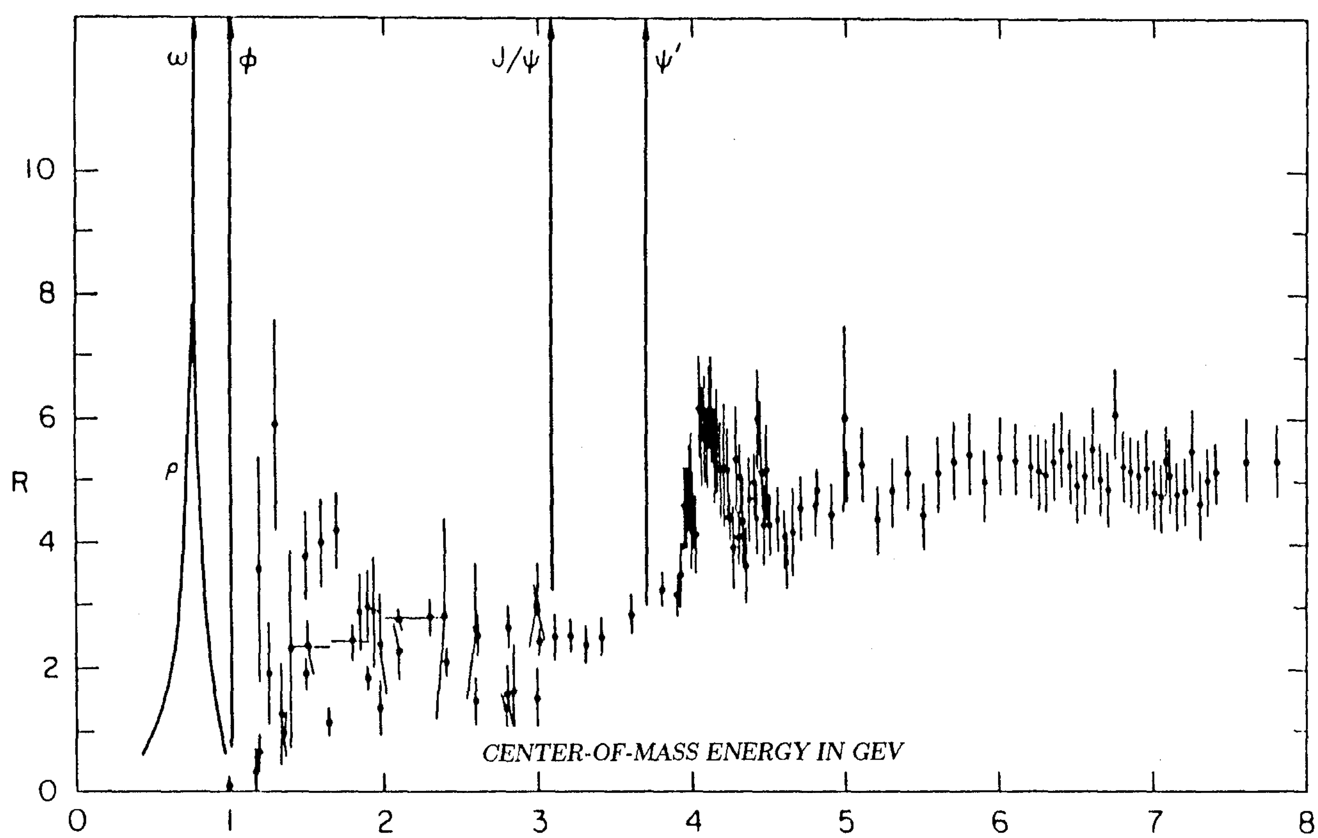
If there was one narrow structure, could there be more? We began to search and 10 days later came the second resonance. With the discovery of this resonance at a mass of about 3.7 GeV we had a better idea of why the ratio $R$ seemed to show a linearly rising cross section. The figure below, which shows what we know now, is perfectly consistent and, had one had the right theoretical prejudice, one would have said in July of 1974 at London: Ah ha, it's all there.

The whole energy region of SPEAR was scanned for possible other narrow resonances and none were found. Those two were the only very narrow ones. The next job that had to be done at SPEAR was to sort out what they were. At that time there were three explanations for what was going on. One of those explanations was that these were the weak bosons of the weak interactions: this was the $Z^0$. There was nothing in the physics done at the time which said that the $Z^0$ couldn't have a mass as low as 3 GeV. Another explanation was that these were the first manifestation of particles carrying color, a postulated property of quarks that was supposed to stay concealed. And the third explanation was that these were the sign of a new quark, a charmed quark.

The experiments done in the next year sorted those things out. One example was a very careful look at the $\mu$-pair channel in that resonance where we saw a destructive interference just below the resonance with the background of quantum electromagnetic $\mu$-pair production. That proved that the state has the quantum numbers of the photon.

There was another experiment done looking at front-back charge asymmetries which proved that there was no axial vector component to these particles. That took care of the $Z^0$; it was not that.

Events were found where these particles decayed into each other with the emission of just gamma rays. Other events showed these particles decaying into hadrons.




*A MARCH OF RESONANCES* *This collection of data from several storage rings shows the two sharp resonances discovered at SPEAR (the $J/\psi$ and $\psi'$) which signaled the presence of new physics. The peaks at low energy ($\rho$, $\omega$, and $\phi$) are combinations of the lighter quarks and had been discovered many years earlier in other kinds of experiments.*

The $\psi$ and the $\psi'$, however, did not decay copiously into a single gamma with hadrons. In the color hypothesis the $\psi$ or $\psi'$ should have done so about half the time. So color was not the answer.

That left the charmed quark hypothesis, and the charmed quark hypothesis turns out to be absolutely right and it had been sitting there all the time. It had been invented, I would say for fun, in about 1963 by Bjorken and Glashow who wanted to see what a theory which had four quarks would look like compared to the theory with 3 quarks which was most popular at that time. The charm hypothesis was taken much more seriously when Glashow started pushing it as an explanation for the absence of strangeness-changing neutral currents in beta decay.

What one had before was three quarks ($u$,$d$ and $s$). All mesons were made from these quarks and antiquarks. All the mesons found to that date were consistent with that hypothesis, and there was nothing that didn't fit with it. In that model you could have very narrow resonances when you produced a quark and an antiquark; it is possible to have a resonance with a $u$-$\bar{u}$ , $d$-$\bar{d}$ and $s$-$\bar{s}$ . Those were all known, in fact; they were the $\rho$, $\omega$, and the $\phi$.

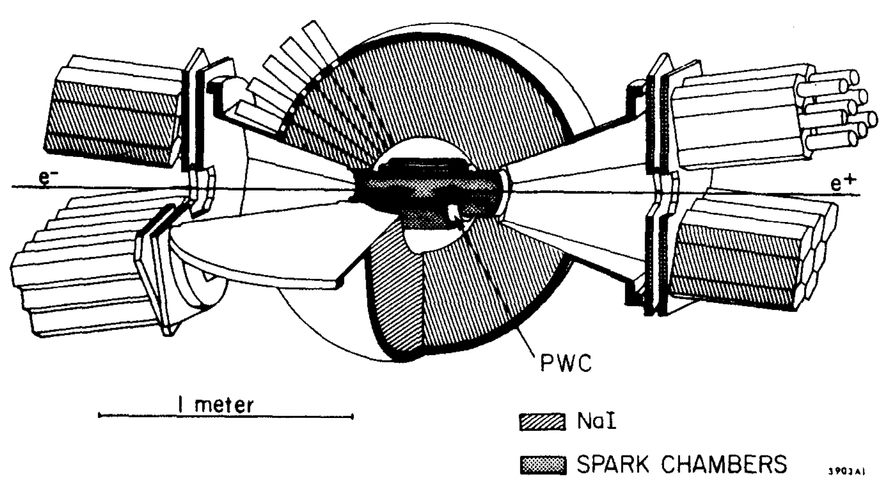
This model also predicted the value for $R$ based on the charges of the three quarks: it should be $\frac{2}{3} \times 3 = 2$.

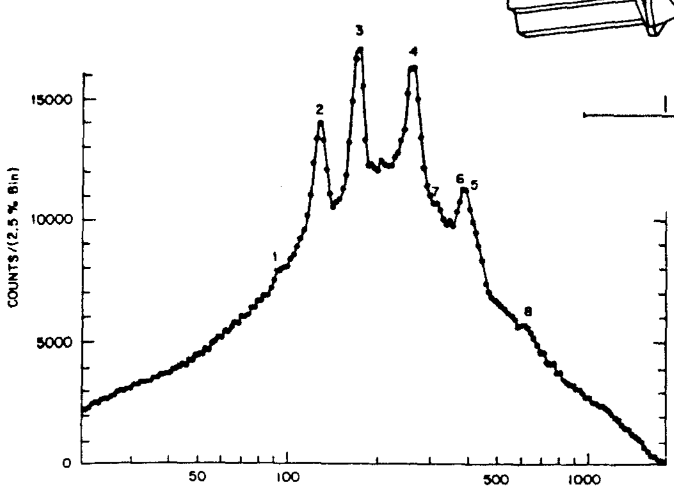
If you introduce a fourth quark into the system, it becomes much more symmetric (this was why Bjorken and Glashow played with it in the first place). We had four leptons (the electron, muon and their two neutrinos) but we only had three quarks. Putting the fourth one in made it look a lot prettier. Now you could produce new resonances based on the $c$-quark and its antiquark — a $c$-$\bar{c}$ system.

The quark-antiquark system has a whole spectrum of excited states; there doesn't have to be just one resonance. In fact it looks exactly like positronium, an atom-like combination of an electron and a positron. You can go from the results of positronium to predictions for the quark system by changing the interaction from the Coulomb potential to the quark potential.

This model predicts that there should be additional states between the two main resonances of the $\psi$ and $\psi'$. These states should not be directly produced in $e^+e^-$ collisions but should be observable in the decay of the $\psi$ and $\psi'$. These states began to be discovered in the year after the first discovery. The most magnificent data on all of this is the final data from the Crystal Ball which has now taken off on a C5-A for Germany and will never run on this experiment again. The figure shows the gamma ray energy spectrum in the decay of the $\psi'$ to other things. Every one of the transitions predicted is measured; every one of them is consistent with resolution of the apparatus; this is an absolutely beautiful job.

*THE CRYSTAL BALL* *The schematic at right shows the heart of this detector, a small sphere composed of several hundred crystals of sodium iodide which measure precisely the gamma rays produced in electron-positron collisions.* →




← *FINE DETAILS* *This plot of the number of gamma rays produced at different energies shows a beautifully detailed structure that corresponds to the transitions between different arrangements of quarks.*

The behavior of $R$ can be qualitatively understood now, too. It should change from a value of 2 below the $\psi$ to a value of 3.3 above the $\psi'$ — a change corresponding to the allowed production of states with the new quark. The actual value of $R$ above the $\psi'$ was closer to 4.3 than 3.3. More about this in a moment.

Mesons which contained the charmed quark combined with one of the old quarks were not so quickly found. The theorists were getting angry with everybody at SPEAR because those damned charmed mesons should be there and we couldn't find them; what was wrong with us? Well they were finally found in the first half of 1976, and they had the expected weak decays. Then combinations of the charmed quark with two other quarks to form charmed baryons were found. In those few years, 10 states of charmonium (the $c$-$\bar{c}$ system) were turned up along with 10 charmed mesons and 2 charmed baryons. It was a thrilling time.

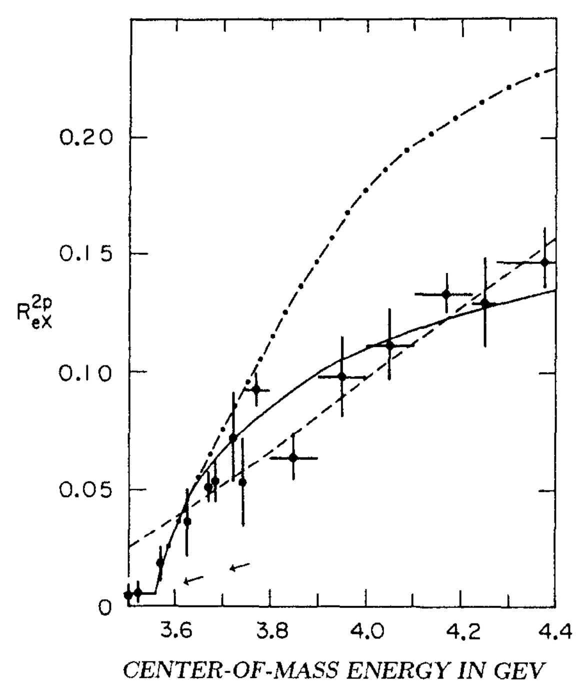
No one has any doubt now that this is the explanation for what went on. But then there was a problem. That value of $R$ above the threshold should not have been a bit more than 4; it should have been 3.3. What was happening? What was happening this time was an absolutely genuine surprise to the entire physics community: there was another member of the lepton family. The leptons that we had then were the electron and its neutrino, the muon and its neutrino. Everyone had assumed that there were no more. However, there was no fundamental reason to have just 2 sets; there might have been 20.

If another standard lepton existed, it should decay in a standard way. There is a particular signal that had long been thought to be a sign of the production of a new lepton pair. You should have events in which one new lepton decayed into an electron plus neutrinos and the other decayed into a muon plus neutrinos. This would give a final state containing an oppositely charged muon and electron, no gamma rays, and no other charged particles. This is forbidden from all we know about $e^+e^-$ and its final states — except as a decay of heavy leptons.

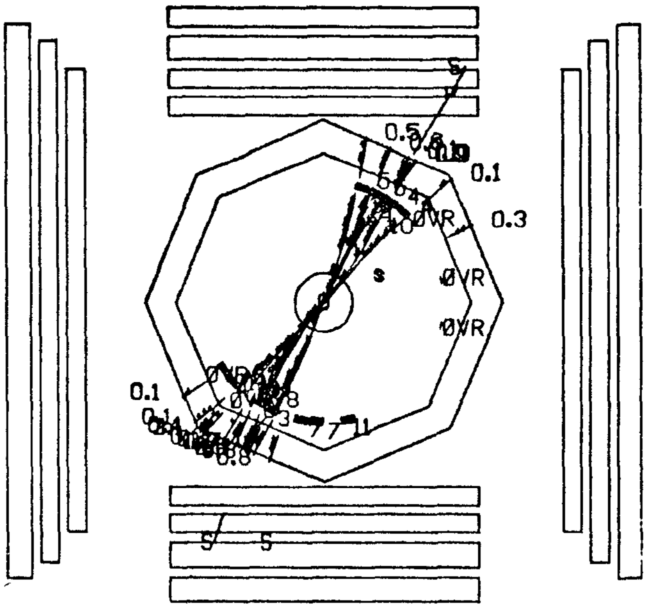
In 1975 Martin Perl turned up 24 of those events, and that was the first sign there was a new member of the lepton family — the $\tau$, or tau. Since that time an enormous number of its decay modes have been studied, its spin has been determined, and its mass has been measured. The best mass measurement comes from a lovely experiment by DELCO just before it left SPEAR. (It seems that everything does its best work just before it's taken out; God knows what these experiments might be doing if they were still there). The figure shows the production rate for leptons. At low energy there is a small background level; when there is enough energy to produce a pair of tau leptons, there is a sharp rise, and from this rise the mass of the tau can be determined to within 2 MeV.

Another part of the SPEAR story is about jets. The theoretical picture of hadron production in electron-positron collisions is that the electron-positron pair turn into a quark and antiquark pair which move away from the collision point in opposite directions. Quarks, however, cannot exist by themselves and must be bound to one or two other quarks to make one of the particles we know and love. So each of these two quarks dress themselves with other quarks to make a bundle of familiar particles. The hadrons that are produced in the collision should appear to be collected into two back-to-back bundles called jets.

Had PEP been around back then there would never have been a question about whether jets existed. The figure across the page shows an event from PEP, not from SPEAR, of a 15 GeV on 15 GeV collision. All you see is these collimated jets of particles of limited transverse momentum. At the SPEAR energy the multiplicity is lower and the mean transverse momentum is much closer to the average momentum of the particles themselves. The jets do not show up so dramatically, and it took some rather sophisticated mathematics to reveal them. They were indeed found, and that was the first evidence that there were jets in hadronic final states, and that quarks could dress themselves in jets.




*THE TAU STEP After the discovery of the tau by Perl in the Mark I, the DELCO experiment carefully tracked the energy at which it began to be produced. This step at the left of the graph can be used to measure its mass precisely.*



*JETS This event from the Mark II detector at PEP shows two tightly bunched sprays, or jets, of tracks. This structure points to an initial production of two quarks moving in opposite directions.*

One of the most remarkable hadron experiments at SPEAR was done with transversely polarized beams, and personally this was the thing that got me to believe that quarks might be real after all and not a convenient aid to calculation. If you have transversely polarized beams and you produce a pair of spin-$\frac{1}{2}$ particles, you can look azimuthally at the rate of production with respect to the direction of polarization. If these things have a spin of $\frac{1}{2}$, you see a dip in the production along the direction of polarization. If they're bosons (particles with integer spin), then you see a peak in the production.

Now consider events with jets containing only pions, which are integer-spin particles. If you look for events in which one of the pions in a jet has a very large share of the momentum, our plot starts to look like the plot expected for spin-$\frac{1}{2}$ particles. The only way these integer-spin pions can behave this way is if they came directly from a pair of spin-$\frac{1}{2}$ particles— the quark pair. There are more complicated ways to show this, but this is what made me believe in quarks.

SPEAR has some remarkable statistics. From 1973 to the end of 1980 there are 113 papers in the major journals, 98 invited papers at major conferences, 26 PhD theses, and something over 3000 or 4000 citations to those 113 papers. All this came out of those two experimental areas, a remarkable job. Some people have said that is the most productive single experiment in high-energy physics in a long time. I, of course, modestly agree with them.

SPEAR still has work to do; there are still papers coming out of it. The latest paper from Mark II on SPEAR data has just gone out and there are more papers to come out of the Crystal Ball. A brand new detector called the Mark III has just been installed and is starting to do physics now. There's lots more to come.

I would be remiss if I didn't say something about what started out as a cottage industry at SPEAR but now threatens to devour the whole place: the Stanford Synchrotron Radiation Laboratory. In 1971 Gerry Fischer and Ed Garwin argued very hard that the x-rays emitted when particles are bent in a storage ring (the synchrotron radiation) would be very useful to do all kinds of surface physics, atomic physics, solid state physics, what have you. Their arguments were so convincing that a port was built on SPEAR to let this radiation out, although there were no people proposing at that time to use it. In 1973 Seb Doniach and Bill Spicer got very interested in this, and their interest started the development of the Synchrotron Radiation Laboratory. By 1980, 50% of the SPEAR time was dedicated to the synchrotron radiation work. They now have 7 ports, each of which splits into several beams for separate experiments. Their publication list for last year alone has 141 entries covering fields from chemistry to x-ray lithography. They're big and getting bigger. Soon you won't be able to see the ring all; you'll only be able to see the SSRL buildings.

We have to take a brief detour away from colliding beams for another piece of the story. The nice thing about that charmed quark at SPEAR was that it made the number of quarks equal to the number of leptons, a nice symmetry. But then SPEAR discovered another lepton, the tau, which presumably had its own neutrino. Now the symmetry was gone, for we had 6 leptons but only 4 quarks. And yes, people started looking for two more quarks (tentatively name $t$ and $b$, for truth and beauty or top and bottom). The $b$-quark was soon found, not in a storage ring but in muon-pair production in a hadron beam experiment by Leon Lederman and collaborators at Fermilab. It was a very similar kind of experiment to that done at Brookhaven in which Ting co-discovered the $J/\psi$ in 1974.

Physics initiative soon returned to the storage rings when the CESR machine at Cornell began operations with an energy sufficient to produce the $b$-$\bar{b}$ states. Now, the German lab DESY has increased the energy of its ring DORIS to the same level to join the study, and has inherited the Crystal Ball to do it. That leaves the $t$-quark, which presumably is just waiting for a storage ring of still higher energy to discover it.

## PEP — THE NEXT RING AT SLAC

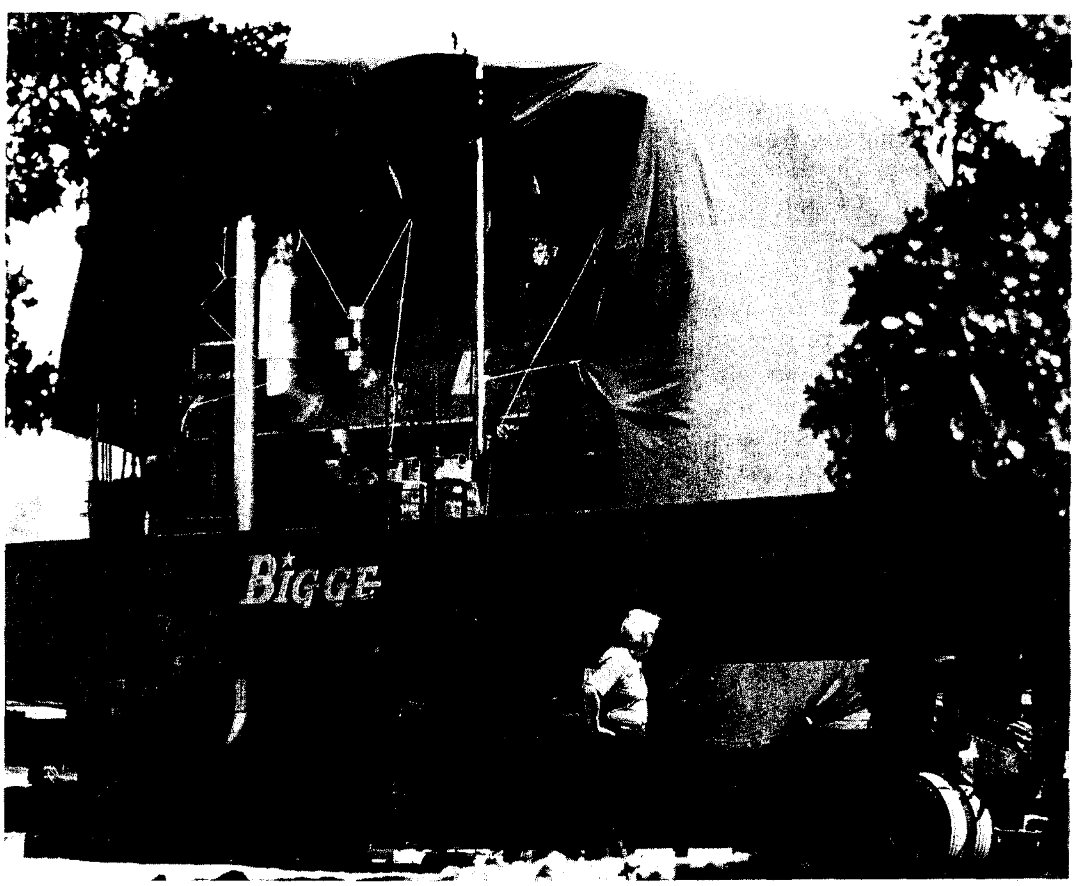
PEP, the next of the SLAC storage rings, originally meant the Positron Electron Proton project. The first design studies were begun in 1970 and 1971 when SPEAR was still being built. Two rings were proposed: one for $e^+e^-$ colliding beams, a second for protons to allow either electron-proton or positron-proton collisions. This project also went through some funding vicissitudes which I won't describe; they pain John Rees even more than they pain me. But it finally did get funded and construction started in 1976.

It was completed in 1980 as the Positron Electron Project instead of a Positron Electron Proton project. It had only the one ring designed for electrons and positrons. The ring has six places to do experiments, a maximum energy of 18 by 18 GeV, and a luminosity as of this spring of $10^{31}\text{cm}^{-2}\text{sec}^{-1}$ which is close to its design goal. PEP is just beginning its physics career, and so results are just starting to come out of it. But there is already one thing about PEP which is already a remarkable achievement. It was built as a collaborative project between two of the major laboratories in the United States, LBL and SLAC. The project leader was John Rees from SLAC and the deputy leader was Tom Elioff from LBL. I don't think anything has ever been done like that before. And I'm not sure when I talk to either the SLAC people or the LBL people that anyone would like to something like that again. But it certainly was an interesting adventure, and we all had rather a good time doing it.

Let me briefly go around the interaction regions and tell you what physics is going on at PEP. The TPC and the Two-Gamma Experiment are in region 2. The TPC has particle identification for all momenta over its large central region, and the Two-Gamma experiment covers the forward and backward region. It is now in the debugging stage, and when it gets working it will be an extraordinarily powerful experiment. It can, for example, identify every particle in a final state jet.

Region 4 contains an experiment called MAC which has a calorimeter to measure hadron energy, another calorimeter for electrons and photons, and momentum measurement for muons. It's running and doing physics.

In region 6 is an almost unique situation. The Free Quark Search experiment has finished taking data and is gone. It is not gone to another laboratory, it's been taken apart and is gone. It is a very rare thing in storage rings for an experiment to finish up and leave. In its place there is the HRS, built around a huge superconducting magnet. This device has the best momentum resolution and mass resolution of any detector at any storage ring in the world. It is now running.




*MOVING MARK II*
*Burt Richter watches the ponderous journey of the Mark II detector from SPEAR to PEP in 1979. The Mark II will also see service in the new linear collider, requiring yet another move.*

DELCO is in region 8. This experiment has special Cerenkov counters to single out kaons and electrons in the final state. It's running.

In region 10 is the Monopole Search, a specialized experiment that's the smallest and lightest of the PEP experiments. It weighs 2 kilograms and you can carry it around in one hand, and that is no mean feat when you look at a photograph of a more typical experiment.

And finally there is the Mark II detector in region 12. It is a general purpose detector and has a new vertex detector which measures decays very well. It's also running.

Let me give a very quick summary of the results from these experiments. First we have the searches for new particles. At the top of the list is the $t$-quark, a natural for discovery in this higher energy range. Neither PEP nor PETRA has seen a glimmer of it, however, so it is either at still higher energy or our expectation of symmetry in the number of quarks and leptons is wrong.

The Free Quark Search found no unbound quarks of mass less than 14 GeV. The Monopole experiment found no stable Dirac monopoles with mass less than 14 GeV. The MAC and Mark II detectors found no supersymmetric electrons and muons and no Higgs particles. No heavy leptons were discovered by Mark II, MAC or DELCO.

Both the Mark II and MAC have measured the lifetime of the tau lepton. The value agrees with what's predicted from theory assuming that now there is $e$, $\mu$, $\tau$ universality. The Mark II has a measurement of the lifetime of the charmed meson called the $D^0$. This doesn't agree with anyone else's measurement, but within errors they'll probably all come together.

Theories predict how the quarks turn into the known particles. The kind and momenta of the produced particles are given by something called a fragmentation function. A measurement of the charm fragmentation function from DELCO and Mark II shows this function as rather hard, giving large momentum particles. The Mark II also has a $b$-quark fragmentation function; this seems to be even harder than that of the $c$-quark. MAC and the Mark II have both measured the fraction of time that the $B$ meson decays into a lepton plus anything; it's around 10% .

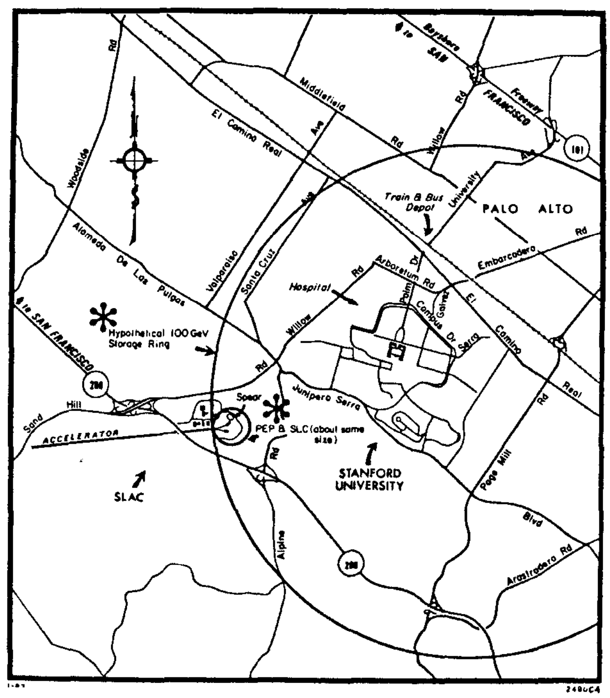
Measurements of the strong interaction coupling constant, $\alpha_s$, have been made in three ways: three-jet properties, energy moments, and energy flow asymmetries. The most remarkable thing about all these measurements is that they all agree to within 15% . I think people put too much credence in the determinations of $\alpha_s$; things are not as perturbative as they seem. So it surprises me that in analyses as different as these that you get the same answer.

There are still more results, involving the interference of the weak and electromagnetic forces and so on. PEP is a young machine; it's just getting rolling, and I expect we're going to see lots more.

## A NEW KIND OF COLLIDER

When SPEAR was just getting started, we were already at work designing the next machine, PEP. Now that PEP has started, what are we talking about for the next step? The next step is something different, a new direction. We are thinking about a new kind of a machine because a problem has come up with these storage rings and how much bigger they can be made.

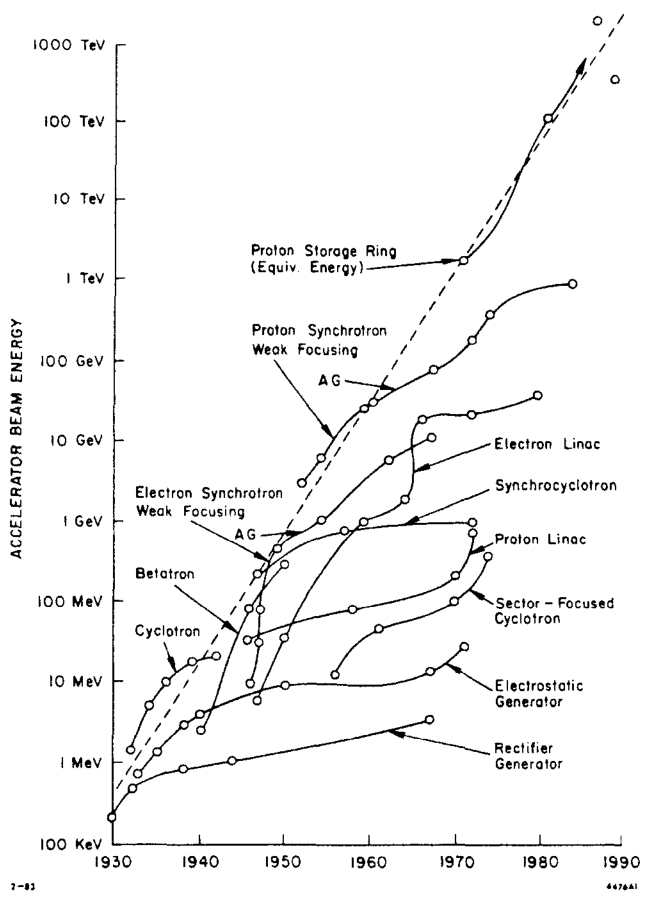
Based on the machines that have already been built, you can make up scaling laws that tell you how big a ring of, say, twice the energy must be and how much it will cost to build and run. The dominant factor in this scaling for $e^+e^-$ storage rings comes from synchrotron radiation. The power lost to this radiation increases as the fourth power of the energy and decreases as the first power of the radius. A machine of twice the energy of SPEAR but with the same radius, for example, would lose 16 times as much power to synchrotron radiation. This could be brought back down by making the ring 16 times bigger. When you put in all the factors, including the beam-beam interactions and the expected rates for physics events, you get a scaling law that says that the cost and size of storage rings goes as the square of the energy in the center of mass.




*HYPOTHETICAL SUPER-PEP If PEP were scaled up for 50 GeV beams it would engulf Palo Alto. A linear collider to achieve the same energy, however, requires arcs no larger than the PEP ring.*

The largest of the storage rings, LEP, is now being built by CERN outside Geneva. It has a circumference of 27 kilometers, a cost of over half a billion dollars, and a beam energy in its first phase of about 50 GeV. Now scale this up with the quadratic law to 500 GeV, but allow for a decrease per unit cost of 30% because some things will get easier or cheaper as we learn more about building machines. The result would be a machine that costs 18 billion dollars, has a circumference of 675 kilometers, and has a power consumption of 2 Gigawatts.

We have to find another way to go, but that is nothing new in particle physics. The figure is the famous Livingston plot of the energy of accelerators through the years. The equivalent energy seems to keep going up by about a factor of ten every 6 or 7 years. But it keeps on going up only by changing techniques; each technique runs out and something has to come along and take over to continue the increase.




*NEW IDEAS This Livingston plot shows that the continuing increase in accelerator energies over the the past 50 years has required a sequence of new kinds of machines.*

We think there's a different way to do $e^+e^-$; we think it can be done with linear colliders: fire two linac beams at each other. This has a different scaling law that looks more like the first power of the energy. We have begun to work on this, we have a design for the first such machine, and we have asked the government for money for the SLC, the SLAC Linear Collider. You accelerate both the electrons and positrons in the SLAC linac, split them up at the end, bring them around in an arc, let them collide with each other and never use them again. The energy for the first phase is 100 GeV and the luminosity is $6 \times 10^{30} \text{cm}^{-2}\text{sec}^{-1}$.

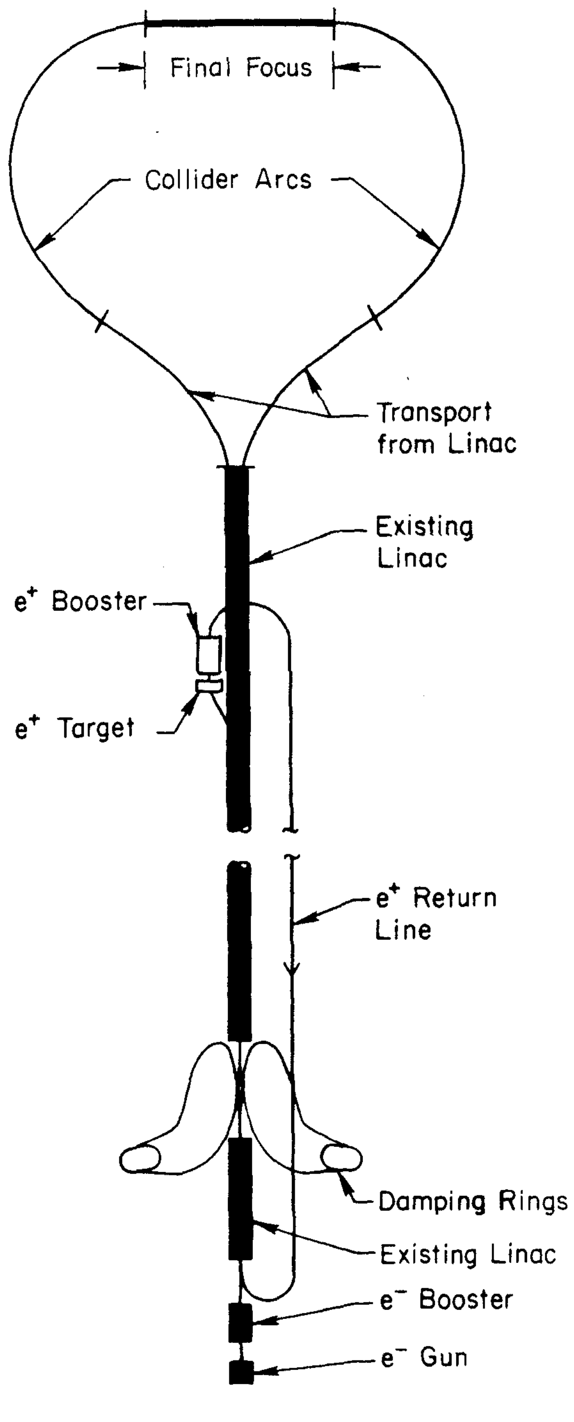
The SLC is not a true linear collider since it uses only one linac and not two. But it tests the same trick that makes a true collider work: producing a tiny beam size of one or two microns. What does this do for us?

There are two things that contribute to high rate in a storage ring or linear collider: colliding the beams often and squeezing the beams into the smallest spot possible to increase the chances for particle collisions when the beams do cross. The linac beams collide only a few hundred times per second compared to the millions of times per second that the beams in a storage ring cross. In a storage ring, however, you cannot focus the beams down to tiny spots. If you try, the beam-beam interaction destroys the beams. In a linear collider, however, you don't have to worry about keeping the beam unperturbed enough to continue circulating in a ring of magnets; you don't care, you're going to dispose of it anyway. So you can compensate for the small repetition rate of a collider by going to these very small size beams.

Several things have to be done to make the SLC work at SLAC. First, the linac has to be modified to give 50 GeV beams. I didn't even know until I heard Ed Ginzton's talk that way back in 1957 it was designed to have an ultimate energy of 50 GeV. [see Special Issue Number 2, April 1983]. They must have been prescient; they must have known that the $Z^0$ mass was 100 GeV.

We have to build 2.3 kilometers of tunnels at the end to bring these beams around to a special final magnet system to focus them down to 1.4 microns. We have to produce positrons in a tricky way and bring them back to the beginning of the accelerator so they can be boosted to 50 GeV. We have to build small storage rings to shrink the transverse emittance and make the beams very small before we accelerate them down the linac.

All of this will bring us two big things. It will prove a new kind of machine, and it will give us a tool to study the new physics of weak interactions. We have talked about the new machine, what about the new physics?




*SLAC LINEAR COLLIDER — SLC*

*This schematic shows the many new components of the linear collider: a high-intensity electron gun; two damping rings near the injector; a positron target and return line; two arcs to bring the beams around into single-pass collisions; and a final focus system to achieve the micron-sized beam spot. The linac will have new klystrons to get to higher energy and new beam controls to maintain the exacting tolerances.*

We have been talking about quantum electrodynamics as the theory that tells you what happens to electrons and positrons when they collide. This is the theory which describes the familiar electromagnetic interaction between charged particles. There is another way for electrons and positrons (and other particles, too) to interact, and that is called the weak interaction. As the name implies this other interaction is pretty small compared to the usual electrical force, but that is only true at the lower energies where we have been working.

The rate for electromagnetic interactions in an $e^+e^-$ machine decreases as the square of the energy of the beams; there is less and less rate as the energy goes up, and this could cause problems for higher energy machines. The weak interaction, however, increases as the square of the beam energy, a behavior first described by Enrico Fermi. Well, there must be some point where the weak force takes over and becomes the dominant effect in an $e^+e^-$ machine, and we have a pretty good idea where that will happen.

The Weinberg-Salam model says that the weak interaction cross section is going to increase to a huge resonance which has a value 4000 times bigger than that of the electromagnetic process. This resonance corresponds to the mass of the carrier of the weak force, a particle analogous to the photon of the electromagnetic force. This particle, called the $Z^0$, has a mass of 93.8 Gev, or so I'm told this week.

When we look back at the pioneer Princeton-Stanford project, we see that it accomplished two things: prove a new machine technique and test a theory. The SLC is also advertised as having two goals: one to develop a new technique of colliding beams; the other to carry out a brand new physics program and to look at this new regime of energy. History repeats.

It's an exceptionally vital time in high-energy physics. We got here with a major contribution from the storage rings and from the colliding beam experiments done at Stanford. In the 1950s, when it all started, the world was discussing resonances, and experiment was leading theory. We were discovering resonances and no one knew where to put them. In the 1960s, when the quark model came along, the theory led the experiments. The quark model told you what resonances to look for and the experimenter went to look for them. They were found and people were quite happy with the quark model.

In the early 1970s there was no clear leader. Theory had available dozens of alternatives to what turned out to be right: the four-quark model, the charm hypothesis, and the weak neutral currents. They were all there and the experiments went and found them. Let's say that theory and experiment were tied.

Right now theory is way in the lead of experiment. The theories of today discuss things that happen at very high energy and need experiments to verify and validate them. These experiments are not possible with our present tools.

It's time for an expansion in the tools. We need new accelerators of the proton type; we need new accelerators of the electron type. The SLC, I hope, is our next step up in energy at SLAC. I hope we will find some surprises there and not merely validate the theory of current fashion, the Weinberg-Salam model.

But we have to go beyond that too. In parallel with all the work on the SLC, we have to get started on the big linear colliders, for which the SLC is the predecessor. We have begun that kind of work now at SLAC. And I hope that ten years from now we will be talking not only about the results from the SLC but also about the next machine and how it will be along soon.

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BURTON RICHTER

*The following biography is adapted from Modern Scientists and Engineers, McGraw Hill, 1980, and from a scientific autobiography printed in the November 1976 Beam Line.*

Richter's introduction to the electron-positron system began as an undergraduate working part-time in the MIT magnet laboratory. He entered graduate school at MIT in 1952 and continued work in the subtle effects of atomic physics. These experiments incidentally involved the machinery of high-energy physics and he completed his Ph.D. training in 1956 in the new field.

He joined the faculty of Stanford in 1956 and began his studies of the short-distance behavior of the electromagnetic interaction. That interest led to the building of an electron-electron storage ring at Stanford and, in 1970, to the electron-positron storage ring SPEAR at SLAC.

On November 11, 1974, Richter and S.C.C. Ting announced the independent discovery of a new elementary particle with highly unusual properties. Richter and Ting shared the 1976 Nobel Prize in physics for this discovery. Richter's work at SPEAR led to what has been called the 'November Revolution' of 1974, including the family of $\psi$ resonances and the charmed quark.

Richter became Director of SLAC in September, 1984.

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POSTSCRIPT — 1984

In the two years between this talk and its publication here, a few pages have been added to SLAC history.

The Mark III detector at SPEAR has accumulated nearly 3 million $\psi$ events and is studying very rare decay modes. The luminosity of SPEAR has been increased by moving focusing magnets closer to the detector. This scheme will significantly increase the Mark III's data-taking rate.

The MAC, Mark II, and DELCO detectors at PEP have measured the lifetime of particles containing the $b$-quark. The magnetic monopole experiment and the DELCO detector have completed their physics programs at the ring, and a new experiment, ASP, has been installed to look for anomalous single photons. Plans to increase PEP's luminosity are being considered.

A new injector has been added near the end of the accelerator to allow more efficient running of low-energy beams. This will be used in a new nuclear-structure physics program in End Station A, the site of the traditional electron-scattering experiments.

And, the next chapter in the laboratory's history is underway. The SLAC Linear Collider, the SLC, has been funded, construction is underway, and first beams are scheduled for the end of 1986.

SLAC Beam Line Special Issue 7
Bin 94 Bill Ash, Editor

*Stanford University operates SLAC under contract with the US Department of Energy.*