

SLAC BEAM LINE

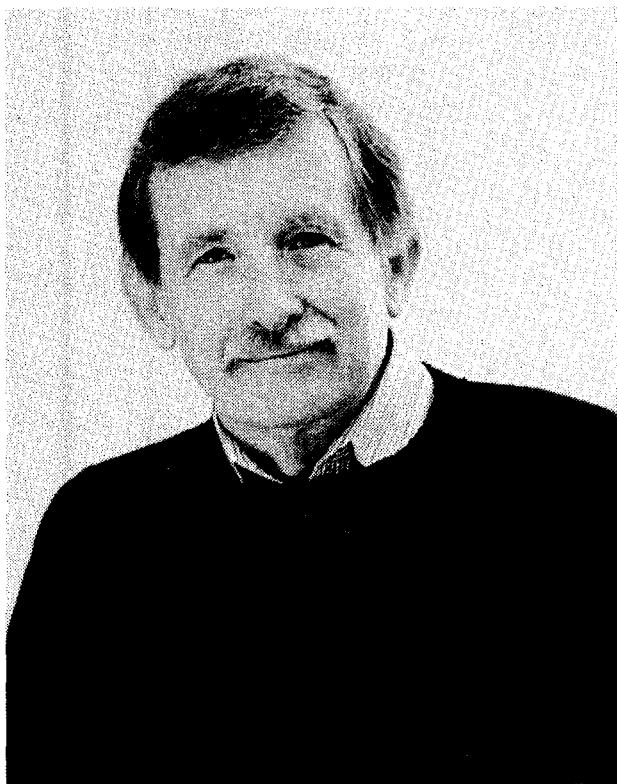
I think the saga of CEA is the Book of Job for the accelerator builders . . . [but] in the end the Lord loved them and they got the right value of R.

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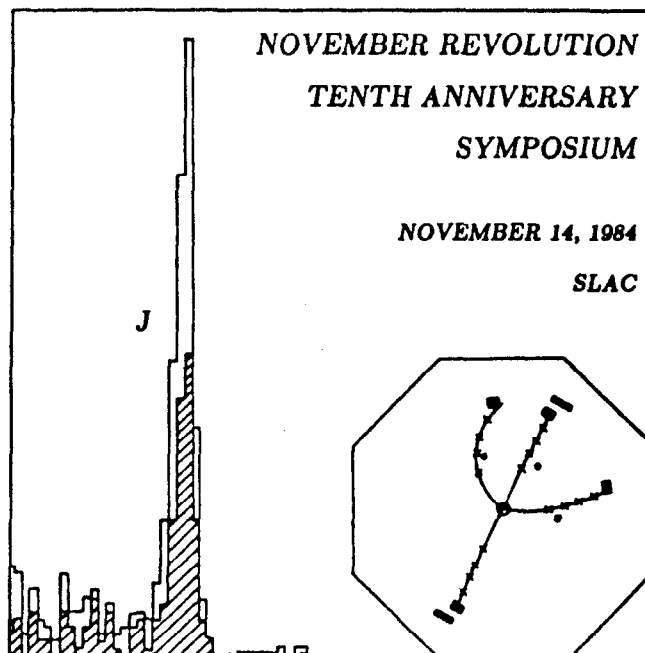
— COLLIDING BEAM STORAGE RINGS — A BRIEF HISTORY

John R. Rees



The discovery of the J/ψ , announced in the fall of 1974, resulted in such a rich flow of new physics and new experimental technique that physicists call the beginning of this era the 'November Revolution.'

The *Symposium on the Tenth Anniversary of the November Revolution*, held at SLAC on November 14, 1984, was a recollection of the discoveries and a review of the consequences and included this talk on the history of storage rings.



The storage ring was crucial to this revolution. SLAC Associate Director John Rees was involved in two such rings before becoming project director of the PEP ring, completed in 1980. He is now project director of the SLC, a new kind of colliding beam machine.

The other speakers at the symposium were James Bjorken, David Hitlin, Roy Schwitters, and Samuel C.C. Ting. Professor Bjorken's talk, *A Theorist Reminisces*, was published in 1985 Special Issue Number 8 of the *Beam Line*. The remaining talks are planned for future issues.

COLLIDING-BEAM STORAGE RINGS: A BRIEF HISTORY

John Rees

This will be a somewhat personal view of colliding beams. Although the field is not a very old one, it is quite impossible to cover all of the developments in it in a half-hour talk. Indeed, my own experience does not cover all of the developments, so I will concentrate primarily on what has happened in the field of electron-positron colliding-beam machines from their beginnings up until the present time, although I will not completely neglect the development of hadron colliders.

To discuss the history of colliding-beam technology at this time is very appropriate, of course, because of the November Revolution that began with the discovery, ten years ago, of the J/ψ particle in November 1974. It is also quite timely from the point of view of the state of accelerator technology, because we are probably seeing right now the twilight of the first epoch of colliding-beam technology and the dawning of a second epoch based on a new technology. But I will come to that later.

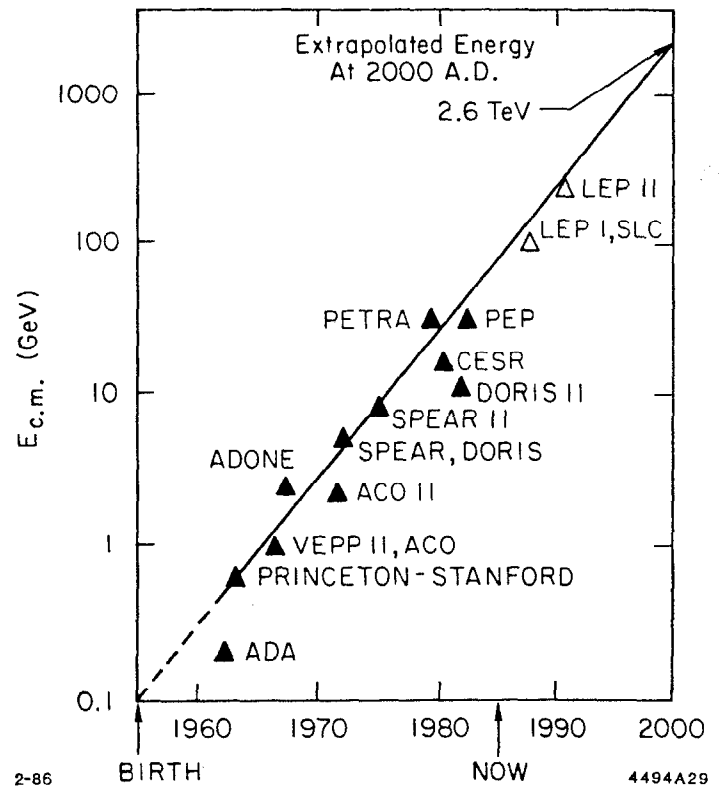
The figure at the right is a sort of map of the territory that we are going to cover. This is a graph that I pinched — I think from Pief. I am not quite sure of its provenance, but it is a graph of the center-of-mass energy achieved in colliding beam machines plotted against time. The machines that are now running or have run are represented by solid triangles; the ones that are still a-building or hoped for are represented by open triangles. If one extrapolates this line backward to low energy, interestingly enough, and for no reason that I can think of, it comes to the time that I would identify as the real beginning of the art as we know it. I mark 1956 as the beginning for reasons that will, I hope, become clear in what follows.

To remind us of what was going on in our world in 1956, I checked into what people were doing at that time — the people who are involved in this Tenth Anniversary program here today. Sid Drell had just come to Stanford as an Associate Professor; Sam Ting was an undergraduate at Michigan; Roy Schwitters had just grown old enough to join the Boy Scouts; B. J. Bjorken had just arrived as a new graduate student at Stanford (lured by Sid); and Dave Hitlin was in high school in New York. Burt got his PhD at MIT that year and came here. And Pief was already here at Stanford, the Director of HEPL, and probably the main draw that brought most of these other people here. Finally 1956 was the year that I

got married.

Having thus set the stage, let me now retrace the salient points of the history of colliding beams as I see them.

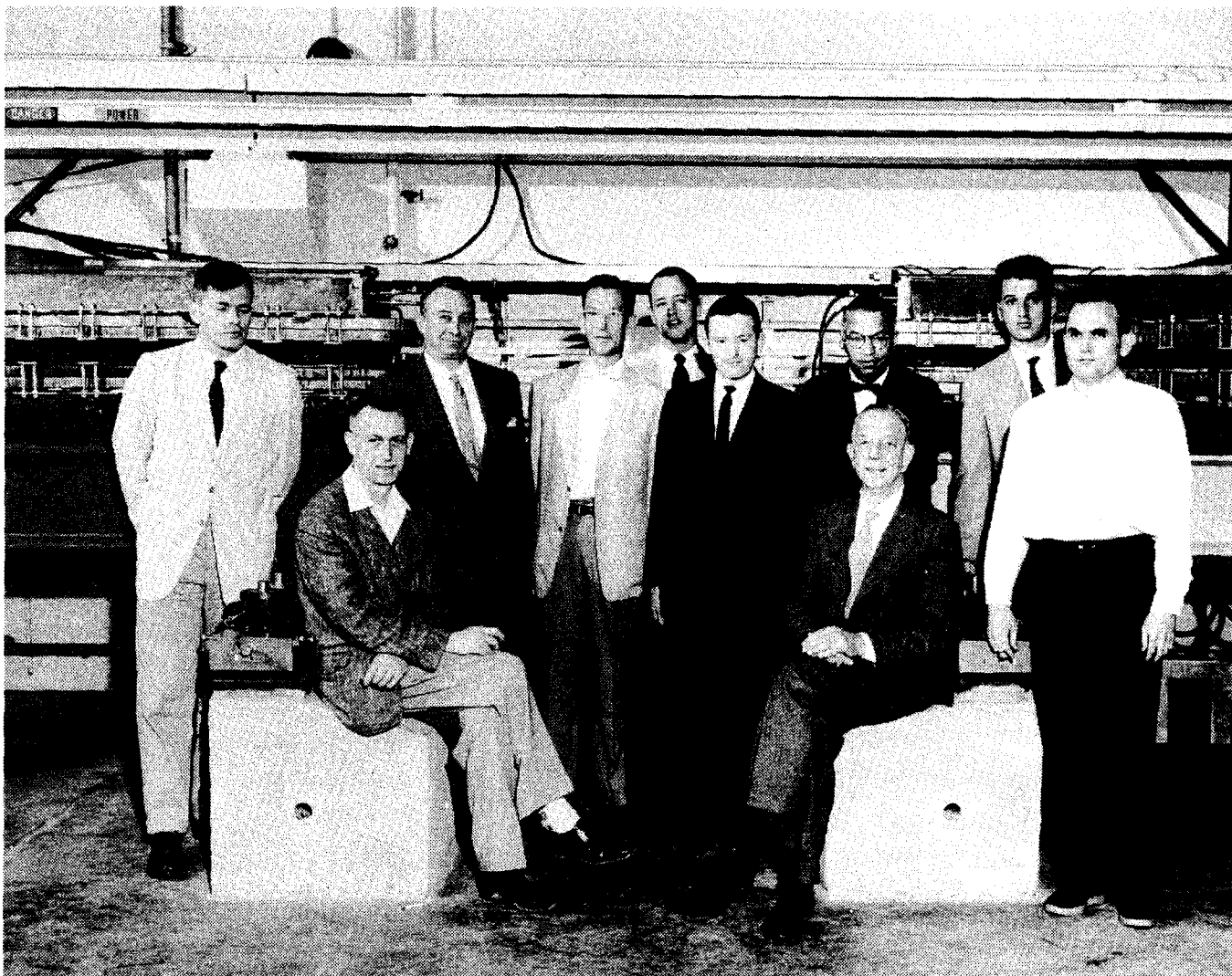
The wish to overcome the handicap imposed by momentum conservation in stationary-target collisions had existed for a long time, but every attempt to imagine a way of overcoming this handicap by colliding one beam against another was frustrated by the tenuousness of the beams that were attainable in the '40s and early '50s. For example, Widerøe had seriously considered colliding protons with negative hydrogen ions but was discouraged by the poor vacuum technology of the day; he knew he could not attain useful beam lifetimes. He had proposed using oppositely charged particles so that they could be contained in a single magnetic ring for the sake of economy. This idea will recur.



EVOLUTION OF COLLIDING BEAM MACHINES
This graph summarizes the history of electron colliding beam machines. From an approximate birth in the mid-1950s, the energy of the machines has increased about ten times every decade.

In my view, the main historical precondition for the development of colliding beams that we have seen in the past two decades was the alternating gradient (AG) principle put forward in 1952 by Courant, Livingston and Snyder (and invented two years earlier by Christofilos but not then published). Of course, the AG principle alone was not enough. It did not, in itself, open the door to the higher density of beams required to do physics, but it was a precondition that inspired other developments, in particular the fixed-field, alternating-gradient (FFAG) accelerator and the technique of beam stacking. First the AG principle was applied to fixed field devices with the idea that very high-intensity beams could be achieved.

The early cyclic AG synchrotrons were limited in their beam intensity, because particles could be injected for only a brief time at the bottom of the cycle. This limitation could be overcome in fixed-field (dc) machines of a type developed primarily at the Midwestern Universities Research Association (MURA), initially under the direction of Donald Kerst. Since the main advantage pursued was higher intensity, the MURA scientists had to learn more about stacking particles in machines than had ever been known before. By the way, although it is not obvious from what I have said, the accelerated particle in the plans of the MURA group for a big machine was the proton.



THE CEA TEAM, 1959. The group that led the Cambridge Electron Accelerator (CEA) in Cambridge, Massachusetts. The machine was later converted for colliding beam experiments, testing the technique of 'low-beta' that proved so important in storage rings. Seated from left: Thomas Collins and David Jacobus. Standing from left: Fred Barrington, CEA Director Stanley Livingston, Robert Cummings, Lee Young, John Rees, William Jones, Janez Dekkra, and Kenneth Robinson (deceased).

In the course of their studies Kerst and his *MURA* co-workers realized that, with stacking, they could hope for beam densities sufficient to do colliding-beam physics, and at the International Accelerator Conference at *CERN* in 1956, Kerst made the first proposal for a colliding-beam facility, a pair of clashing-beam accelerators. Although the figure of merit we now use, the luminosity, had not been invented at that time, the luminosity of the machine proposed was about $10^{32} \text{cm}^{-2} \text{sec}^{-1}$ (probably the first time that nearly universal constant of nature for accelerator builders was cited).

Although *MURA* was the most active center, it was not the only place where colliding-beam systems were being devised. At Princeton, Gerry O'Neill was also absorbed in this matter. At the same *CERN* conference, he also gave a paper about storage rings in which he suggested that one need not approach colliding beams via clashing accelerators but rather via storage rings as separate machines — separate from the accelerators that would feed them. In other words any kind of an accelerator could feed particles into two static storage rings in which the particles could be stacked and could collide.

Now I believe (this is a speculation on my part) that O'Neill was somewhat frustrated by the fact that things were not moving very rapidly toward the realization of a big *FFAG* machine. Such machines were inherently very large and therefore very expensive. Indeed, the high-energy *FFAG* machine proposed by *MURA* was never built. In any case, inspired by frustration or not, O'Neill realized that electrons could be used instead of protons. In fact, electrons had advantages over protons. The radiation damping that came automatically with electrons because of their low rest mass would make stacking easier.

In the case of protons stacking required putting a new swarm of particles into a region of phase space very close to an old swarm of particles. But the swarms could not overlap, and in the end the density of the particle points in the full six-dimensional phase space could never exceed the density of the injected swarms. With electrons, on the contrary, the particle points move inward in phase space because of radiation damping, so that the density increases greatly. That made stacking an easier process. The radiation damping also had the potential of ameliorating instabilities that clearly might arise.

O'Neill believed that a relatively inexpensive pair of rings could not only test colliding beam principles but could also do forefront electromagnetic physics, because the center-of mass-energies that could be reached in the electron-electron system far exceeded that available at that time by any other means. Of course, these inexpensive storage rings had to be cou-

pled to a suitable source of electrons.

This is where Stanford entered the picture. Stanford's High Energy Physics Laboratory had an ideal source in the *Mark III* linear accelerator, so O'Neill came to Stanford with Bernie Gittleman, his student, and discussed these ideas with Pief Panofsky, Burt Richter, and Carl Barber. And the Princeton-Stanford colliding-beam experiment (which came to be known around Stanford as the *CBX*, although I find now there are few people who remember that) was proposed in a report dated in 1958, and work began in that same year.

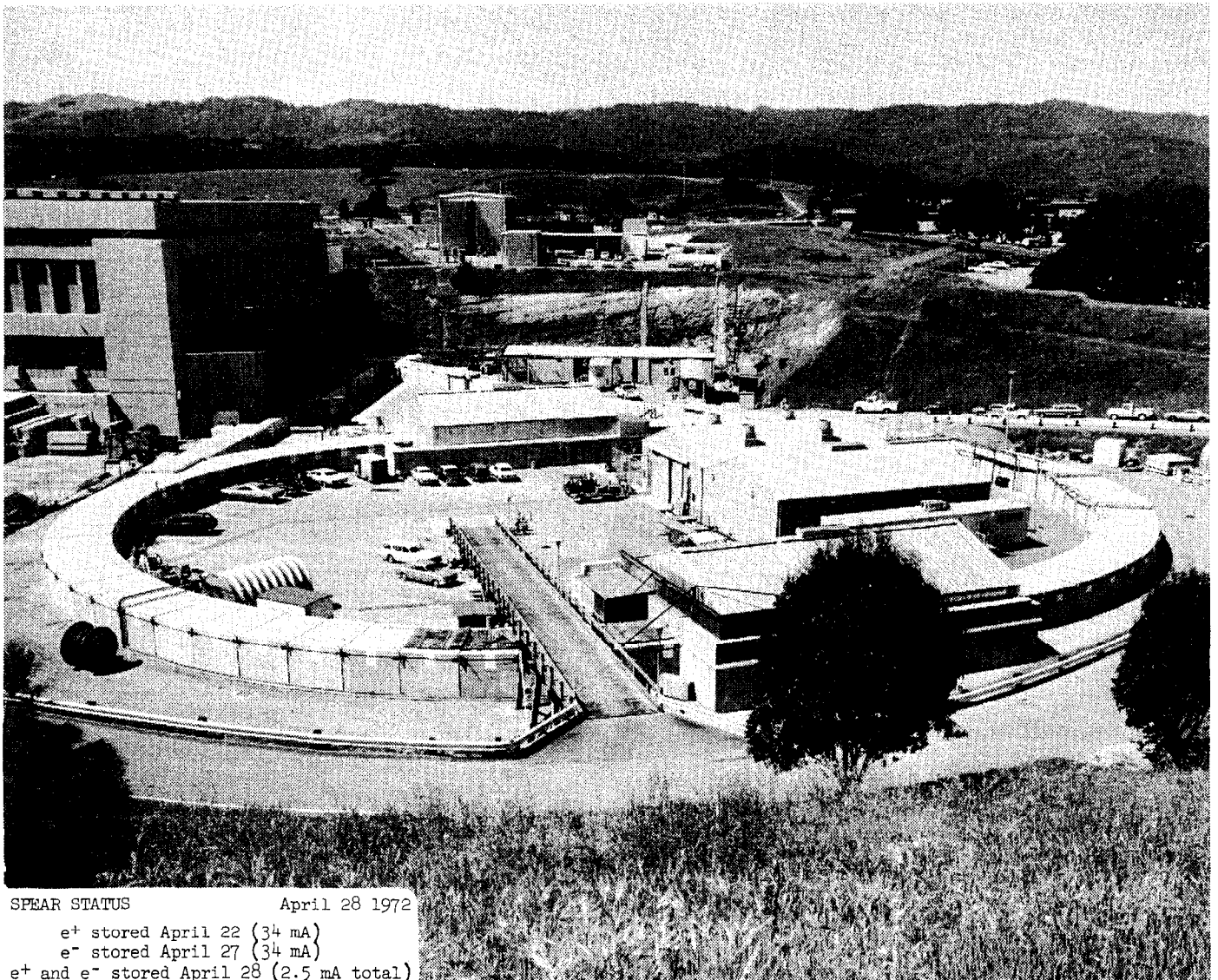
The beam energy of this experiment was planned to be 500 MeV; the radii of the rings were about 1.4 meters and they were weak focusing. (These figures are approximate; I will continue to be approximate about energies and radii throughout this talk.)

At the same time, Budker's Institute for Nuclear Physics in Novosibirsk started work on a pair of rings called *VEP-1* to collide electrons of 140 MeV. The scientists at Frascati, Italy, noting what was being proposed for the Princeton-Stanford experiment, were thinking along a slightly different line. Their idea was to put electrons and positrons in a single storage ring for both beams, which offered an economic advantage. But perhaps a more important advantage was the fact that the events that ensued from collisions would be dominated by a well-known and well-understood intermediate state, namely the virtual photon. They became extremely enthusiastic about the idea, and in 1960, just two years after the Princeton-Stanford proposal, they proposed a program that began with the construction of a small machine called *ADA* and continued to the construction of a much larger one. The first storage ring was to be a 250-MeV machine, with a radius of only 60 centimeters, and like the *CBX*, weak focusing. But at the outset they stipulated that their plans called for proceeding to a larger machine which they even then named — *ADONE*, for 'big *ADA*' — with an energy of 1.5 GeV, if possible. So *ADA* was to be a proof-of-principle, and *ADONE* was to follow on right away. Thus in the four years from 1956 to 1960 the first generation of colliding-beam storage rings was well and truly launched. I want to emphasize that, although protons were the particles that were contemplated originally, they had by this time disappeared temporarily from the scene. Furthermore, although I have asserted that the strong-focusing principle was essential to this development, it was not used in the design of any of these first three machines. (Its use began in the second generation.) It was essential to get *FFAG* thinking started, but these first machines were attempts to find out whether this vein had any gold in it, and they were not pushed to the limits of accelerator technology, which I think was very wise.

The builders of the first generation faced what all pioneers have to face: the banes of the first generation, the unanticipated problems that plague new machines, to which their builders have to find solutions — solutions which became part of their heritage to the next generation.

In the case of the Princeton-Stanford machine a very fast kicker was required, a magnet that switched on and off very rapidly to inject the electron beam from the linear accelerator, and that was a technological challenge that occupied the attention of the builders for some time. They recognized that this magnet would be difficult to realize, and they met the challenge.

But then they encountered their first real surprise. At very low stored currents — far below what they required — they observed a large evolution of gas accompanied by an intolerable pressure rise, and they were led to learn about the desorption of molecules from the surfaces of the vacuum system. It is noteworthy that the vacuum system for this machine was, at the time, the largest ultrahigh vacuum system in the world. They learned that they could not pump it with oil pumps, and they learned that they had to bake it very well. They overcame the problem, but it took a long time, because rebuilding an ultrahigh vacuum system is very difficult.



SPEAR STATUS April 28 1972
 e⁺ stored April 22 (34 mA)
 e⁻ stored April 27 (34 mA)
 e⁺ and e⁻ stored April 28 (2.5 mA total)
 luminosity measured = $1.5 \times 10^{28} \text{sec}^{-1} \text{cm}^{-2}$

SPEAR — 1972. This photo of the first storage ring at SLAC was proudly completed with typed-on performance figures. The view is from the east end of the site looking over the Research Yard.

And finally, after they had solved that problem, the Princeton-Stanford physicists came up against what may fairly be characterized as the Fundamental Limit, the beam-beam limit of the colliding-beam storage ring. This phenomenon has proved to be insurmountable and continues to place the basic limit on the performance of colliding-beam storage rings. The theorem deserves to be capitalized.

ADA, the first Frascati ring that I mentioned earlier, being more limited in its goals, never reached the beam-beam limit, although it did uncover a basic limitation on the low-energy beam-storage capacity of storage rings. It was at first curbed by its injection scheme. A gamma-ray beam was directed onto a target at the edge of the aperture, and some of the electrons and positrons produced went into the ring. Subsequently a more sophisticated system of time-varying inflection equipment was installed, but the power of the Frascati synchrotron was inadequate, so *ADA* was eventually moved to Orsay, France, and put at the end of the Orsay linac. Then it was able to accumulate enough current to find a new limitation on storage-ring behavior, the Touschek effect, in which Coulomb scattering within the bunch transfers transverse momenta into longitudinal momenta and causes intensity-dependent particle losses.

But now I want to come back to the Fundamental Limit and its elucidation. It is sometimes called the incoherent limit and more often the beam-beam limit. I think it was appreciated early by people at *MURA*, at Stanford and at other places that there was some inevitable limit to be encountered simply owing to the beam dynamics that result from the interaction of the bunches with each other — the effect on the particles of one bunch of the macroscopic or collective field of the other bunch, which is highly non-linear. But it remained for Fernando Amman and Dave Ritson (who was visiting Frascati) in 1961 to elucidate the consequences of this interaction in a way that made it fairly clear. I don't believe that the outlines of the problem have changed since they stated them.

Amman and Ritson said that the transverse beam density — the number of particles in the bunch divided by its interacting area — would be restricted by a 'tune shift,' namely the shift of the betatron frequency of the individual particles of one bunch by the collective electromagnetic field of the other bunch, treated in the linear approximation, i.e., treated as a lens. They wrote their formula this way:

$$\frac{N}{A} \leq \frac{\Delta\nu_v \nu_v \gamma}{r_e R F_v}$$

where $\Delta\nu_v$ is the vertical betatron-frequency tune shift, and ν_v is the vertical betatron frequency itself. (I've used the American notation, ν rather than Q .)

Here γ is the energy, r_e is the classical radius of the electron and R is the gross radius of the machine. F_v is a function called the beat function, and in this formula it is evaluated at the interaction point. $F_v(s)$ is the 'focusing function' and is dimensionless. It is more common today to use $\beta(s)$ as the focusing function. More on that in a moment.

Amman and Ritson pointed out that the tune shift could not increase in magnitude beyond the value that would carry the particle's betatron frequency to the nearest proper resonance. There, the particle's motion would become unstable. This was certainly an upper limit on the tune shift.

Later Ernest Courant at Brookhaven did simulations and suggested that the tune shift would prove to be limited to something of the order of 0.1 regardless of the location of the nearest proper resonance. His conclusion was not surprising, since the actual dynamical problem is strongly non-linear, and the Amman-Ritson result follows from a linearized treatment. Courant's estimate remained for a long time the accepted statement of the Fundamental Limit.

Now I want to note before leaving this subject that, in the Amman-Ritson formula, the combination of the ν on top and the R and F_v on the bottom could have been combined together into a single function which I have already mentioned, the beta function. Had this been done, the formula would have appeared this way:

$$\frac{N}{A} \leq \frac{\Delta\nu_v \gamma}{r_e \beta_v}$$

We shall see in a few moments why it is important that it could have been written that way. I have talked to Dave Ritson to find out whether he or Amman had any notion that this alternative form of the equation held the key to getting higher luminosities than they thought in 1961 were in prospect, and he has told me, "No." They really were thinking of F_v as a periodic function which is intrinsically of order one and about which little could be done.

In 1962, construction began on the second generation of machines, *ACO* at Orsay, *ADONE* at Frascati and *VEPP-2* at Novosibirsk.

ACO was designed to operate at beam energies up to 500 MeV; it had a radius of 3.5 meters. It was a strong-focusing ring which reached luminosities up to $10^{29} \text{ cm}^{-2} \text{ sec}^{-1}$. *ADONE* was larger; a strong-focusing machine with a radius of 16.5 meters, it operated at energies up to 1.5 GeV.

ADONE was designed to take advantage of just about everything that had been learned in the first generation. *ACO* was somewhat too crowded to take advantage of everything; it was a rather small machine, no larger really than the Princeton-Stanford

machine. But *ADONE* was a big machine as its name implied. It was a machine with six periods. The vertical beta value at the interaction region, the thing I just mentioned, was 3.2 meters. Advantage had been taken of the knowledge that a small value of beta at the interaction region was desirable, and the smallest value of the vertical beta function anywhere in the machine occurred at the interaction region. The achievements of *ADONE* were that it reached $6 \times 10^{29} \text{ cm}^{-2} \text{ sec}^{-1}$ in luminosity and in fact achieved a tune-shift of 0.06 which is as good a value as has ever been achieved in any machine.

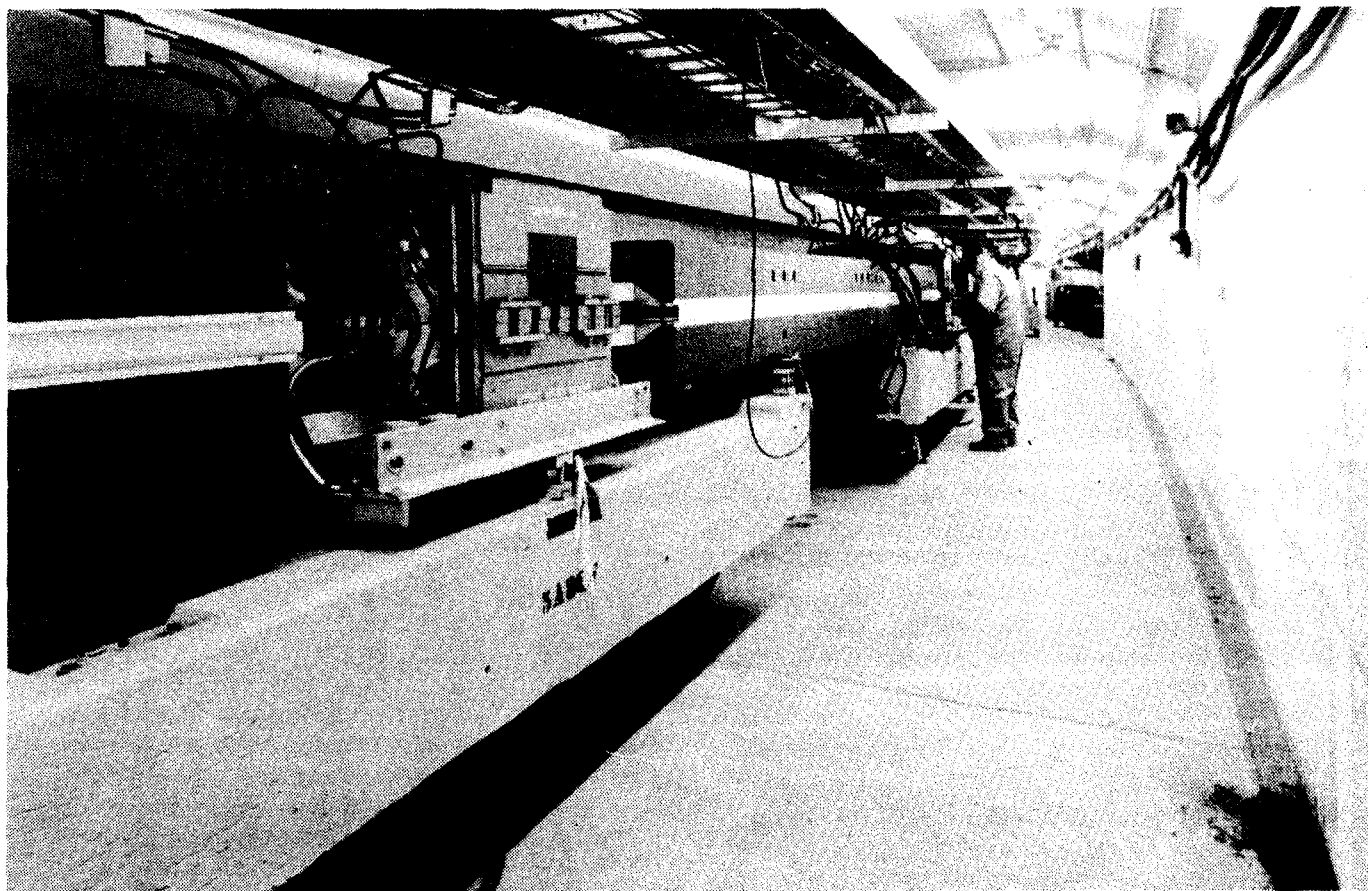
A new and unexpected feature arose in *ADONE*: the head-tail instability. This single-beam instability curtails the current that can be stored in a single bunch. Its nature was elucidated by Claudio Pellegrini of Frascati and Matt Sands of *SLAC* who was visiting Frascati at that time. It can be greatly ameliorated by the use of sextupole magnets, and it has been avoided as a performance limitation in existing machines. Nevertheless it has been a design influence on all of them.

Then came *CEA* and 'low beta.' The Cambridge Electron Accelerator physicists had a synchrotron and wanted to get into the colliding-beam business.

They were prevented from doing what they first proposed: building a new storage ring and filling it from the synchrotron.

Then two of them, Ken Robinson and Gus Voss, had a brilliant insight. They looked at the beam-beam limit in the form of the second equation above, and they recognized that the beta function could be made extraordinarily small — not just somewhat smaller than it was in other places in the machine, but extraordinarily small — in one or a few special places around the machine. And if they made it tiny, they then had the wherewithal to get high luminosity even with a machine that was limited by its complexity to rather small currents. (In terms of the beat function, they realized that it did not have to be of order one everywhere; it could much smaller locally.)

Using this idea, the *CEA* synchrotron itself was converted into a special 'bypass' storage ring. The result was — and I can't think of a better way of saying this — a machine of staggering complexity. It was made to work and used to do colliding-beam physics in 1973 at a maximum energy of 2.5 GeV per beam, 5 GeV in the center of mass. They succeeded in making the beta function as small as 5 centimeters, sixty-five times smaller than in *ADONE*.



PEP — 1980. The most recent storage ring at *SLAC* fills an underground tunnel nearly 1.4 miles in circumference.

And even then the luminosity of *CEA* was not limited by the beam-beam limit; it was limited by the incredible complexity and difficulty of the *CEA* operating cycle. I think that the saga of *CEA* is the Book of Job of the accelerator builders. They were afflicted by every handicap that could have been visited upon them, yet they persevered, and in the end the Lord loved them and they got the right value of *R*. Of course nobody believed it. The machine was too hard to operate.

In the period we have reached now in the history of colliding-beam storage rings, the first — and to date the only — proton-proton colliding-beam system was built, the Intersecting Storage Rings of *CERN*. Construction took place between 1966 and 1971. The *ISR* was a great success in accelerator-physics terms. It exceeded its design goals in terms of energy and luminosity, the two most important parameters. Designed for a maximum energy of 28 GeV, it reached 31.4 GeV; and designed to produce a luminosity of $4 \times 10^{30} \text{ cm}^{-2} \text{ sec}^{-1}$, it attained $10^{32} \text{ cm}^{-2} \text{ sec}^{-1}$, the highest luminosity yet reached by any colliding-beam system.

Now we shall turn to the third generation of electron storage rings. (I have treated the *CEA* not as a generation, but as a special event.) The third-generation machines were *SPEAR* at Stanford, *DORIS* at *DESY* and *VEPP-2M* at Novosibirsk. We have been through the first and second generations, and I shall not dwell on the characteristics of these third-generation machines. I shall merely remark that, historically speaking, the third generation took full advantage of what had been learned at such great expense of effort in the first two generations. The third generation used good high-vacuum technique; it used low-beta interaction regions; and it had head-tail-instability control built in. These advances paid off — especially in *SPEAR* where a luminosity of $10^{31} \text{ cm}^{-2} \text{ sec}^{-1}$ was achieved at 3.7 GeV, the highest luminosity achieved up to that time in any machine (the *ISR* had not yet bettered that figure). The tune shift reached by *SPEAR* was approximately equal to that which had been achieved at Frascati, but with a much lower beta at the interaction region, *SPEAR* produced higher luminosity.

In my view, this third generation of storage rings indicated that the storage ring technique had matured. There were no profound new difficulties encountered with these machines. There was detailed understanding of the problems that had been faced before.

The fourth generation further showed the maturity of the technology. It could be characterized as a scaling up by an order of magnitude in energy of already successful techniques, and as the table shows, the luminosities that were achieved (which I state to

Machine	Beam Energy	Maximum Luminosity
<i>CESR</i>	6 GeV	$2 \times 10^{31} \text{ cm}^{-2} \text{ sec}^{-1}$
<i>PEP</i>	15 GeV	$3 \times 10^{31} \text{ cm}^{-2} \text{ sec}^{-1}$
<i>PETRA</i>	20 GeV	$2 \times 10^{31} \text{ cm}^{-2} \text{ sec}^{-1}$

THE FOURTH GENERATION — Scaling up a successful technology.

only one significant figure) are a little bit better than the best that were achieved in the preceding generation. These machines worked reasonably well when we got them working.

There are a couple of other things that should be mentioned before I close. One of them is the idea of building a system of rings to collide protons with electrons. That possibility was studied as early as 1971 by an international group which assembled here at *SLAC*. It consisted of Dieter Möhl from *CERN*, Claudio Pellegrini from Frascati, Andy Sessler from *LBL*, and the *SLAC* bunch of Burt Richter, Mel Schwartz and me. Our paper, which I presented at the 1971 International Accelerator Conference, excited the formation of several national projects: *Epic* in England, *SuperAdone* in Italy, and *PEP* in the United States. none of these ever got funded, built or even carried very far, but *PEP* metamorphosed into the electron-positron project I have already mentioned. Now, finally, that dream is being pursued at *DESY* in the *Hera* project.

Now, of course, the largest of all the colliding-beam storage rings, *LEP*, is being built at *CERN*. At the same time linear colliders are being touted as the wave of the future for economic reasons. Is *LEP* the last of the rings? If it is, then the whole history of designing and building colliding-beam storage rings will have spanned just over three decades.

(The author gratefully acknowledges the helpful comments of W. Kirk and V. Kistiakowsky in preparing this talk for publication.)

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