SLAC–PUB–6023
June 1992
(T/E/A/I)

THE RISE OF COLLIDING BEAMS*

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1. Introduction

It is a particular pleasure for me to have this opportunity to review for you the rise of colliding beams as the standard technology for high-energy-physics accelerators. My own career in science has been intimately tied up in the transition from the old fixed-target technique to colliding-beam work. I have led a kind of double life both as a machine builder and as an experimenter, taking part in building and using the first of the colliding-beam machines, the Princeton-Stanford Electron-Electron Collider, and building the most recent advance in the technology, the Stanford Linear Collider. The beginning was in 1958, and in the 34 years since there has been a succession of both electron and proton colliders that have increased the available center-of-mass energy for hard collisions by more than a factor of 1000.

The history of that advance for both electron and proton colliders (constituent center-of-mass energy is plotted *versus* time of the first physics experiment) is shown in Fig. 1. The important number for the experimenter, the constituent center-of-mass energy, has increased by about a factor of ten every twelve years for both kinds of systems. On the electron line, one can see a kind of complete cycle in accelerator technology, from the birth of the colliding-beam storage ring to its culmination in LEP II and the beginning of the next technique for high-energy electron collisions, the linear collider. On the proton line, one has gone from the first bold initiative, the ISR at CERN which used conventional magnets, to the superconducting magnets that are used in all of the proton colliders built today.

For the historians here, I regret to say that very little of this story can be found in the conventional literature. Standard operating procedure for the accelerator physics community has been publication in conference proceedings, which can be obtained with some difficulty, but even more of the critical papers are in internal laboratory reports that were circulated informally and that may not even have been preserved. In this presentation I shall review what happened based on my personal experiences and what literature is available. I can speak from considerable experience on the electron colliders, for that is the topic in which I was most intimately involved. On proton colliders my perspective is more that of an observer than of a participant, but I have dug into the literature and have been close to many of the participants. There are others here at this symposium who can perhaps fill in any gaps that I leave.

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*Work supported by Department of Energy contract DE–AC03–76SF00515.

Presented at the Third International Symposium on the History of Particle Physics, Stanford, CA, June 24-27, 1992.

II. The beginnings

The earliest writing that I know of on the construction of machines based on the collision of two particle beams is by Rolf Wideröe, who obtained a German patent[1] on the technique in May, 1953. In it, he discussed collisions between the same kind of particles (proton-proton), different particles (proton-deuteron), and particles of opposite charge (electron-proton). Wideröe has said[2] that the idea came to him in 1943, when he realized that in the non-relativistic case (colliding automobiles is his example) two particles colliding at equal energy could dissipate four times as much energy as one particle of the same energy colliding with a similar particle at rest. In the colliding-beam case, two beams of equal energy have twice as much energy as a single beam colliding with a target at rest, but you achieve four times as much reaction energy.

Nothing came of Wideröe's idea. The patent was not circulated, and Wideröe was working in industry at the time with little contact with people in the physics community. Wideröe's patent was really conceptual in nature, and did not address any of the practical questions of how to bring these beams into collision, inject into the machines, etc.

The real beginning of colliding beams comes in a paper by Kerst *et al.*,[3] published in early 1956. Kerst was the leader of the Midwest Universities Research Association (MURA), which was the training ground for so many of the important accelerator physicists of the 1960s and '70s. The MURA group was working on the design of a new kind of synchrotron, the so-called Fixed-Field Alternating-Gradient (FFAG) Synchrotron. In this paper Kerst writes,

> The possibility of producing interactions in stationary coordinates by directing beams against each other has often been considered, but the intensities of beams so far available have made the idea impractical. Fixed-field alternating-gradient accelerators offer the possibility of obtaining sufficiently intense beams so that it may now be reasonable to reconsider directing two beams of approximately equal energy at each other. In this circumstance, two 21.6-BeV accelerators are equivalent to one machine of 1000-BeV.

Kerst and his colleagues had recognized in the relativistic case the enormous advantage of colliding beams over the fixed-target technique in attaining very high energy (far greater than the factor of four in Wideröe's non-relativistic example), and also analyzed the intensity requirements to get sufficient reaction rate to be able to use a colliding-beam machine as a useful physics tool. They also considered background generated from interactions on the residual gas in the vacuum chamber, circulating beam lifetime, and stacking many cycles to build up the necessary beam intensity.

Kerst and his colleagues' *Physical Review* letter was, of course, the culmination of discussions that had been going on at MURA for some time and that had excited considerable interest in a broader community. Activity built up very quickly, as can be seen in the Proceedings of the 1956 CERN Accelerator Conference.[4] Kerst, in his conference paper, expands considerably on the original MURA paper, looking at the complete injection cycle, phase-space limitations, space-charge effects, etc.

At this same symposium a new actor came on stage, G. K. O'Neill of Princeton University. He too was interested in proton-proton collisions at very high center-of-mass energies, and he introduced the notion of the accelerator-storage ring complex. Beams would be accelerated to some high energy in a synchrotron and then transferred into two storage rings with a common straight section where the beams would interact. Since the beams at high energy need much less space in an accelerator vacuum chamber than is required for beams at injection, the high-energy storage rings would have smaller-cross-section magnets and vacuum chambers, thus adding little to the cost of the complex, but at the same time enormously increasing the scientific potential. O'Neill noted, "If storage rings could be added to the 25 GeV machines now being built at Brookhaven and Geneva, these machines would have equivalent energy of 1340 GeV or 1.3 TeV." He also observed, "The use of storage rings on electron synchrotrons in the GeV range would allow the measurement of the electron-electron interaction at center-of-mass energies of about 100 times as great as are now available. The natural beam damping in such machines might make beam capture somewhat easier than in the case of protons." That observation was to have a profound effect on O'Neill's career (and mine), as well as on particle physics.

How to realize a colliding-beam machine was the question. The MURA FFAG accelerators discussed by Kerst were enormously complex, and none had ever been built at that time (nor has one been built since). There was considerable concern about whether FFAG machines would actually work as well as their proponents claimed. At the same time, the problem of injection into the proton-synchrotron storage-ring complex that O'Neill and others discussed was thought to be very difficult. Indeed, O'Neill's original idea of using a scattering foil for injection was soon proved to be impossible. On the other hand, injection and beam stacking in an electron storage ring looked easy because of synchrotron-radiation damping, as shown schematically in Fig. 2. An electron beam could be injected off-axis into a storage ring and would perform betatron oscillations around the equilibrium orbit. These oscillations would decrease exponentially over time in a properly designed magnet lattice because of the emission of synchrotron radiation. When these oscillations had damped sufficiently, another bunch could be injected into the storage ring and would damp down on top of the first one. Since phase-space was not conserved in the presence of radiation, there was, in principle, no stacking problem.

III. The Princeton-Stanford storage ring

In the mid-1950's the most powerful electron accelerator in the world with an external beam was the then 700-MeV linear accelerator at the Stanford University High Energy Physics Laboratory (HEPL). O'Neill visited HEPL in 1957 to discuss colliding beams with W.K.H. Panofsky, then the director of that laboratory, and to seek local collaborators. His goal was to develop the new colliding-beam technology as well as to demonstrate it by using the new technology for physics. The energy of the linac was such as to allow an experiment that would go far beyond anything that had ever been done before in testing the theory of quantum electrodynamics; and radiation damping made injection simple, allowing one to get on to confronting the more basic questions of beam stability, beam-beam interaction, etc., that O'Neill felt would be the limitations on large proton colliding-beam systems, which were really dearest to his heart.

O'Neill and Panofsky quickly recruited W. C. Barber and myself to join the project. Barber was a senior scientist at HEPL who had built a 40-MeV linear accelerator that was used for nuclear structure studies. He knew the laboratory and, probably more important, he was very good at cost estimating and keeping the project moving. I was a post-doc who had come to HEPL in 1956 because I wanted to use the linac to test quantum electrodynamics. I was actually doing such an experiment at the time, studying large-angle electron-positron pair production which could test QED to 70 MeV/c. While this experiment would be the most sensitive test then done, the opportunity to do it at 1 GeV/c with the new colliding-beam system was too much for me to resist. O'Neill added another physicist, Bernard Gittelman, who was just finishing his Ph.D. at MIT, and we four set out on what we thought would be a great adventure of only a few years' duration. The adventure turned into something more like the voyages of Odysseus, for we were confronting the unknown and uncovered many problems that had to be solved.

The experiment (CBX) would have two 12-m circumference electron storage rings, with one common straight section. It would require the world's largest ultra-high vacuum system (two cubic meters at 10^{-9} torr). It needed injection kicker magnets faster than anything that existed at the time (80 ns pulse width, including a reasonable flat top). To do physics, it would require the storage of beam currents in the 100's of milliampere range. A photo of the partly completed machine is shown in Fig. 3.

In the design we thought through many issues. We had a model of the beam-beam interaction, which turned out to be wrong, but which gave us the right limit. We thought the limitation from beam-beam collision effects would come from a shift in the effective focusing strength of the magnet system (betatron tune) to the nearest integer or half-integer resonance. In designing the QED test we arbitrarily-derated that number by a factor of ten and used a tune-shift limit of 0.025, which turns out to be very close to the limit that modern electron colliders achieve. We worried about ion trapping in the circulating electron beams and designed in electrostatic clearing fields to remove the ions (they were needed). We worried about what are now called chromatic effects (the change in betatron frequency with energy within the stored beam) and designed in correctors to reduce the chomaticity to zero. Interestingly, this turned out to be vital, although we did not know it until subsequent machines without these corrections showed a beam instability.

The Office of Naval Research (ONR), a very imaginative organization that was then the principal supporter of fundamental research in physics, funded the project to the tune of $800,000 in December 1958, thanks to the persuasive powers of Panofsky. At the time this was the largest sum ever devoted by ONR to a single experiment. The first beam was stored on March 28, 1962; the first physics results testing QED were presented in 1963; and the facility was finally shut down in 1968. During that time we had to confront many new problems. For example, we found that synchrotron radiation desorbed enormous amounts of gas from the walls of our supposedly clean vacuum chamber, and we had to redesign the system to eliminate all oil diffusion pumps. We found what is called now the long-range-wake instability and learned to cure that with octupole magnets. We found a coherent coupled-beam transverse instability that we fixed by separating the tunes of the two rings. We found that the beam-beam interaction did, in fact, lead to significant beam degradation at tune shifts of 0.025 for head-on collisions and at the same limit applied for crossing-angle operations.

In those early years, CBX was a mecca for all who were interested in colliding-beam machines. We had a constant stream of visitors from laboratories in the U.S. and Europe, three of whom merit special mention because of their important contributions to understanding and solving the new problems that we faced. They were Ernest Courant, David Ritson and Andrew Sessler. The start of the CBX project encouraged others to think seriously about storage rings. With the storage of the first circulating beams in CBX in 1962, it was clear to all that colliding-beam machines could be built, and plans began to move ahead rapidly for machines at many places.

IV. The change to electron-positron colliders

Colliding-beam systems offered the potential for vast increases in the attainable center-of-mass energy that would allow the particle physicists to probe much more deeply into the ultimate structure of matter. While the Princeton-Stanford machine had the double goal of proving out the technology and doing particle-physics experiments, the physics potential of the machine was limited. Quantum electrodynamics could indeed be tested to much smaller distances than ever before, but only one or two other specialized experiments ($e^- + e^- \rightarrow \mu^- + \mu^-$, for example) could be done.

In the late '50s and early '60s it began to be realized that the electron-positron system offered a much richer vein from which to mine information about the elementary particles. Figure 4 shows the Feynmann diagram for electron-positron annihilation. In this reaction, the electron and positron form a virtual photon, and that virtual photon can produce any system that has either charge or magnetic moment. All such final states are accessible. Electron-positron annihilation not only had the potential for studying quantum electrodynamics via the elastic-scattering process or the two-photon annihilating process, but also had the potential to study hadronic final states as well. In those days, one talked about such things as form factors or structure functions of the hadrons, for example, and studies of different kinds of hadronic final states could reveal the relative structure functions of different kinds of particles.

In the CBX group, we had discussed conversion to a machine aimed at the electron-positron system. I had come to the realization of the benefits of this system in 1958,[5] and had discussed it with the group. We decided on discretion. Electron-positron colliding beams would be more difficult than electron-electron rings, for we would need such things as two beams circulating in one ring, faster kicker magnets, a positron source, etc. We felt that we had enough problems in developing this technology in the electron-electron system, where we at least had a very high-powered electron beam for injection, and had the flexibility of having the two beams in separate rings.

The first step in the electron-positron direction was taken in Italy, and the key personality was Bruno Touschek.[6] There is a seminal moment in this story that occurred at a seminar by Touschek at Frascati on March 7, 1960, in which Touschek outlined the scientific potential of electron-positron annihilation studies.

Giorgio Salvini, then director of the Frascati laboratory, and the high-energy physics community in Italy were immediately convinced by Touschek's arguments and began to work to bring e^+e^- colliders to life.

The first machine was called AdA, and it was brought into operation less than a year after Touschek's seminar. It was a very simple design with a torroidal vacuum chamber and magnet, and could be built rapidly. Injection was made by converting an in coming gamma-ray beam on a target that protruded slightly into the vacuum chamber. The synchrotron radiation process would allow a small fraction of the electrons and positrons pair-produced on the converter to be trapped in the vacuum chamber. Because of this, the machine had a very low injection efficiency, a very low circulating beam current, and a very low luminosity.[7] AdA was quite small, only about a meter or so in diameter. The entire machine was physically moved in front of the gamma-ray beam to inject the counter-circulating electron and positron beams (Fig. 5).

In my opinion, AdA was a scientific curiosity that contributed little of significance to the development of colliding beams. However, the project did keep interest at a high pitch in Italy while a much more important facility called ADONE was being designed. While ADONE was to be the first of the high-energy electron-positron colliders capable of getting into the region where many different kinds of hadrons could be produced, the first particle physics results actually came from two smaller machines that were completed earlier. I will digress briefly before getting back to the important story of ADONE.

The two smaller machines were ACO, a 450 by 450 MeV strong-focusing ring built at the Orsay laboratory in France, and VEPP II, a 500 by 500 MeV weak-focusing machine built at Novosibirsk in the USSR. Both machines were completed in 1966, and the first results of their high-energy physics experiments were submitted for publication around the end of 1967. Both experiments looked at π-pair production and studied the ρ-resonance with a precision never before attained. It is hard to know exactly when the Novosibirsk group started on electron-positron work. At the accelerator conference in 1961 there was no mention of any such work in Novosibirsk, while in 1963 the VEPP II project was well under way.[8] VEPP II was seriously damaged by a fire in 1968, and the reconstruction of the machine took about two years. By that time, the French group had explored the region around the ρ-resonance extensively, and Novosibirsk was never again a serious player in particle physics using these colliding beams, but they certainly have contributed and continue to contribute enormously to developing the technology.

V. ADONE

The ADONE project was the real goal of the Italian program that had been stimulated by Touschek's seminar. Serious design work began on this project in 1961 under the direction of Fernando Amman. The energy was set at 1.5 Gev per beam, high enough for multiple particle production, including meson and baryon resonances. The machine was to be strong-focusing with a radius of approximately 16.5 meters. Construction was started in 1965, and the project was completed in 1967.

Soon after commissioning of the machine was begun, a new beam-instability problem was discovered—the so-called head-tail instability. The instability limited both the positron and electron circulating beams to very low intensity. In 1968, Claudio Pellegrini of Frascati and Matthew Sands of SLAC analyzed the problem and solved it. The instability was driven by what the accelerator physicists called the "chromaticity," i.e., the variation of betatron oscillation frequency with momentum. Their analysis also indicated the cure, and the ADONE machine was soon equipped with sextupole magnets with which the chromaticity could be adjusted to the proper sign to cure the problem. It is interesting to note that the CBX collaboration had avoided this instability by building a correction into ends of the bending magnets in that machine. We had no real reason to do it—it seemed like the right thing to do at the time.

Experimental physics began on ADONE in 1968. The early results had a great impact on me and on some others in the high-energy-physics community, for the cross-section for multiple hadron production was much larger than expected. The early results are shown in Fig. 6, where the solid line shows what was expected by most at that time. The cross-section should have decreased very rapidly above the ρ-resonance and dropped to quite small values by the time one reached the maximum energy that ADONE was capable of, 3 GeV. It clearly did not, but unfortunately the experiments from the four groups working on the machine were inconsistent, and that inconsistency led to a certain skepticism about the validity of the results. I was not skeptical, for the results at high energy disagreed much more with theory than they disagreed with each other.

ADONE's impact on high-energy physics was dulled by the choice of experiments. To quote from Amaldi again:[6]

> Between the tendency to assign all, or almost all, the available resources to a single group that thus could have disposed of high-performance equipment and the opposite tendency of dividing the same funds between various groups, each by necessity endowed with an apparatus of limited performance, it was certainly not easy to find the right compromise! The solution finally adopted involved an excessive fragmentation of the financial means, with consequences not completely favorable from the scientific stand-point, and a certain disappointment to Bruno Touschek and Fernando Amman.

After the first results were in, the four groups working on ADONE began discussions with the management of the laboratory on follow-on detectors. These discussions went on for a very long time because of the reasons noted by Amaldi. By the time a detector of sufficient capability to do justice to the physics was ready, the science had passed ADONE by.

VI. SPEAR, CEA and DORIS—The Next Generation

If building CBX was like the voyages of Odysseus, then building SPEAR was more like the labor of Sisyphus. We rolled the boulder up the hill seven times (1964–1970) before pushing it over the top.

The project that came to be SPEAR was born in 1961. I mentioned earlier the discussions that the CBX group had had on conversion of the e^-e^- rings to an e^+e^- ring and the decision we made to keep on with our original course. However, I remained convinced of the importance of e^+e^- colliders for the study of hadron physics, and in 1961, before the first beam was stored in CBX, I, together with David Ritson (recently come to Stanford from MIT as a member of the Physics Department faculty), began serious discussions on the design of a high-energy e^+e^- collider. Our first problem was to define "high energy," for that would not only define the physics program but also set the scale of the project. With the help of the Stanford theoretical physicists, we soon settled on 3 GeV per beam, far enough above threshold (we hoped) to get into the "high energy" regime where structure could be compared free of threshold effects.

We continued our preliminary work on the machine design, and in 1962 Panofsky (by then the director of Project M: the design phase of what would become SLAC), invited me to set up a group at Project M to prepare a proposal to be submitted to the Atomic Energy Commission. Panofsky was and is a man of remarkable vision. He had immediately recognized the importance of O'Neill's proposal in 1957, and obtained the necessary funding to build it. He had remained the *éminence grise* behind the project, smoothing the fiscal and technical paths when needed. Now he was betting a great deal on a 31-year old assistant professor who wanted to look at hadron physics in a new way. I wonder if anyone could take such a risk now? We have now more committees, more detailed reviews, and more conservatism in our field. Even then, it could be done only under the umbrella of a large laboratory, where a small proportion of resources could be devoted to a very high-risk, high-payoff gamble.

In 1963, a preliminary proposal was sent to the AEC justifying the proposal because, "... it is in the field of strong interactions that we believe the storage ring can make its main contributions to physics." The proposal already included a full-solid-angle coverage (4π) magnetic detector. In 1964, the formal proposal was submitted.

However, physicists at the Cambridge Electron Accelerator (CEA) also submitted a proposal for an e^+e^- colliding-beam project. The AEC now had to deal with two proposals, and they set up a review committee chaired by Jackson Laslett of LBL to conduct a comparative review. The committee recommended proceeding with the SLAC proposal, but expressed concern about potential problems from the beam-beam instability that had been observed at CBX. The committee felt that more data from CBX was needed. In 1965, the Laslett committee reviewed new data from CBX and recommended that the AEC proceed with the SLAC project.

Then followed a saga of proposal submissions and dashed hopes; redesigns to simplify and lower costs; and modifications to incorporate all of the new ideas generated by colliding-beam studies around the world. John Rees, an accelerator physicist who had worked on the CEA synchrotron, joined me in 1965, and the two of us kept the group together through the long wait. Rees' contribution was essential to the success of the project.

In 1965, the remarkable increases in Federal funding for the physical sciences, triggered in the Eisenhower years by the Soviet Sputnik spacecraft, came to an end. Our project was not included in the budget. In 1966, the proposal was submitted for the third time and, in spite of the strong recommendation of an advisory committee chaired by George Pake, it was not funded. Similarly in 1967, 1968, 1969, and 1970, in spite of increasingly strong endorsements by the High Energy Physics Advisory Panel, no construction funds were available.

Finally, in 1970, a breakthrough occurred because of an intervention by the then Controller of the AEC, John P. Abbadessa. Abbadessa was interested in science as well as in the financial management of the AEC. He became fascinated by the concept of an electron and positron annihilating and turning into other kinds of particles and did what only a great bureaucrat can do—he advised us on how to present the project so that no specific high-level approval was required. A construction project was turned into an experiment, and, with the enthusiastic support of the high-energy-physics program people of the Atomic Energy Commission, SLAC proceeded to build the project out of its on-going budget. The SPEAR project is illustration in Fig. 7.

Construction started in October of 1970, and the first beam was stored in April of 1972. Thanks to the early Frascati results, the project still had its 4π magnetic detector, [9] which was so essential to the experimental program that lead to the "November Revolution." Those results will be described by Gerson Goldhaber at this conference.

During all this time, the CEA group had not dropped out of the colliding-beam business. Robinson and Voss of CEA had invented the "low-β" interaction region[10]—a vital contribution to the scientific productivity of colliding beams. Low-β allowed much higher luminosity than the previous system within the constraints on beam stability imposed by the beam-beam interaction. Experimenters are always looking for higher yield in any given process to allow them to study more subtle effects, and low-β allowed an increase of between a factor of 10 and 100 in the yield of a given process. All of the modern colliding-beam machines incorporate this idea.

The CEA group, while not funded for a major colliding-beam project, came up with an idea on how to modify their synchrotron to allow the storage of electrons and positrons and carry out some limited colliding-beam studies. They designed a "by-pass" that switched the low-intensity circulating beams that could be accelerated into a section of the synchrotron on a parallel track to the synchrotron itself that had a low-β interaction region and room for an experiment.

John Rees, in a 1986 article on colliding beams,[11] summed up the by-pass project very well:

> 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.

The DESY laboratory, which became such an important player in the colliding-beam business in the 1980s, was not involved in the early developments. In the mid-1960s, the laboratory was discussing the appropriate next step beyond their existing 6-GeV electron synchrotron. There were two camps at DESY: one wanted to increase the energy of the synchrotron, while the other wanted to build an e^+-e^- collider, and they were at an impasse.

A critical meeting in the history of DESY took place at SLAC in 1966. Willibald Jentske, then director of the DESY laboratory, brought the senior staff members who were the strongest advocates of the synchrotron approach to a four-hour meeting with Sidney Drell, Panofsky and me on colliding-beam physics, technology, etc. Jentske was clearly using us as the sales force to convince his staff to buy into colliding beams.

DESY soon decided to proceed with DORIS, a 3-GeV two-ring e^+e^- machine. The double-ring configuration that they chose gave rise to beam instabilities that are understood now, but that seriously limited the performance of the DORIS facility then. With the return of Voss from CEA to DESY, the colliding-beam program at that laboratory began to make great strides. The PETRA e^+e^- machine and the HERA *ep* machine have and will make great contributions to physics, but those are stories for the next conference in this series.

The development of colliding-beam storage rings for electron-positron collisions reached a plateau with the completion of SPEAR that has lasted to the present day. The subsequent machines,

Project	Beam Energy (GeV)	Location
CESR	8	Cornell
PEP	17	SLAC
PETRA	22	DESY
TRISTAN	35	KEK
LEP	100	CERN

all are scale-ups of SPEAR. There was nothing new until the development of the linear collider, which is generally acknowledged to be the replacement for storage rings for very-high-energy electron-positron collisions, and the return of the two-ring machine with the design of the various "factory" machines (*B*-Factory, Tau-Charm Factory, Phi-Factory). These new developments are not coupled to the rise of the Standard Model, and so their stories too can wait until the next conference.

VII. Electron-Proton Colliders

I want to mention this topic briefly for, although the commissioning of HERA, the first electron-proton colliding beam facility, happened only in 1992, the story starts a long time ago. It began with a meeting in 1971 at SLAC involving Dieter Möhl (CERN), Claudio Pellegrini (Frascati), Andrew Sessler (LBL), and John Rees, Mel Schwartz and myself of SLAC. A paper was presented at the 1971 accelerator conference by Rees,[12] which aroused great interest. Four proposals soon appeared: from the Rutherford Laboratory (EPIC), from Frascati (Super ADONE), from SLAC/LBL (PEP) and from KEK (TRISTAN). The first two were never built, while the second two turned into e^+e^- colliders when funding limits and lack of experience with the required superconducting magnets forced the elimination of the proton rings.

Electron-proton colliders were proposed again at CERN in the mid-1970s as an upgrade to the SPS, but that project lost out in a competition with the proton-antiproton collider that I will discuss later.

Now the HERA project at DESY is operating and the experimental program has begun making high-energy electron-proton colliders a reality.

VII. Proton-Proton and Proton-Antiproton Colliders

As I mentioned earlier, the first studies on colliding beams were aimed at proton colliders. Injection, stacking, the effects of non-linear resonances, etc., were not well understood, however, and so the actual realization of colliding-beam machines began with the electron colliders. The proton machines were not forgotten, however, and serious studies continued in the early 1960s at both Brookhaven National Laboratory in the U.S. and at CERN in Europe.

At Brookhaven there were two options: one was to build storage rings to go with the AGS synchrotron, and the other was a major program to upgrade the AGS and greatly increase its intensity as a fixed-target machine. I was not privy to any of the discussions, nor have I had access to any of the minutes of relevant meetings at Brookhaven. The laboratory decided to drop the colliding-beam project and proceed with the AGS upgrade project. It would be interesting to understand why.

CERN took the opposite course and decided to proceed with the construction of what would become the ISR. Serious study of the possibility began in 1960, the CERN council approved the project in 1965, construction began in 1966, and the first collisions were achieved in 1971. Kjell Johnsen elsewhere in these proceedings tells the ISR story, and so I will not go into any details here. It was a brilliantly conceived and executed project that should have contributed much more than it actually did to the rise of the Standard Model. The problem was with the choice of experiments, which mainly emphasized small-transverse-momentum phenomena that turned out not to be very relevant to the Standard Model.

One can say that the discovery of W and Z bosons at CERN was the final step in the confirmation of the Standard Model, and so I will go into much more detail on this story. Antiproton-proton ($\bar{p}p$) colliders first come into focus in a talk by G. I. Budker (of the Institute of Nuclear Physics at Novosibirsk) at the 1966 Saclay conference on storage rings.[13] Budker's talk (presented in the Proceedings only in summary form) contains all the key elements of a workable proton-antiproton collider system. He included an outline of the machine design and a brief description of a damping technique that would allow the accumulation of a large number of antiprotons in a small enough phase space to make sufficient luminosity for experimental work. Budker's talk also discussed the physics potential of such machines.

The damping mechanism described by Budker was the so-called "electron cooling" technique in which a beam of electrons with small transverse and longitudinal velocity spreads would co-stream with a proton beam of much larger velocity spread, exchanging momentum with the protons through the coulomb interaction and thus decreasing the velocity spread in the proton beam (cooling) and increasing the velocity spread in the electron beam (heating). A more detailed paper was presented in "Atomic Energy" **22**, 346 (1967), and a complete description of the project they began to construct in Novosibirsk was given in a paper by Skrinsky in the Proceedings of the 1971 International Accelerator Conference. A demonstration of electron cooling was made, but the project was never completed as an antiproton-proton collider both because it went slowly for financial reasons and because it proved difficult in practice to get fast enough cooling rates with this co-streaming electron technique. The project was eventually converted to an electron-positron colliding beam ring (VEPP IV) and has been running for several years.

The next step was the invention in 1968 by Simon Van der Meer of CERN of an alternative technique of cooling called stochastic cooling. In essence this technique senses density fluctuations in a beam and damps them out by an active feedback system. The first formal report on stochastic cooling was issued in 1972 (CERN ISR/PO/72-31), although the discovery was known throughout the accelerator community soon after it was made and there probably exist internal reports of the ISR group that describe it. Stochastic cooling had a great potential advantage over electron cooling in that the cooling rate was independent of energy, while in the case of electron cooling the rate decreased as the 5th power of the energy. The optimum energy for antiproton production is much higher than the best energy for electron cooling, and thus, to use the electron technique, complex beam manipulations were required to decelerate the antiproton beams to an appropriate energy—typically a few hundred MeV. Stochastic cooling could be applied at the energy where the antiprotons were optimally produced. Experiments were carried out at the ISR in the first half of the 1970s that showed that stochastic cooling worked as Van der Meer has predicted. Indeed there were informal discussions at CERN about possible antiproton-proton collisions in the ISR, but there was insufficient interest on the part of the experimental community because there was no energy advantage in the antiproton-proton system and the luminosity of the proton-proton collider was much higher.

The next step came from the decision by R. R. Wilson, then director of Fermilab, to build the energy doubler. Initial discussions centered on the possibility of making a proton-proton collider by making a beam circulating in the FNAL conventional ring collide with a beam circulating in the superconducting ring. The first suggestion of this possibility is, I believe, in a letter from Cline and myself to Wilson in 1974 or early 1975.

By the time of the Fermilab program committee meeting in June of 1976, three very different proposals were in hand. One proposed a 25-GeV high-current proton ring whose beam would collide with the beam in the existing main ring;[14] a second proposed proton-proton collisions between a 1-TeV beam in the new doubler ring and a 150-GeV beam in the old main ring;[15] and a third proposed antiproton-proton collision at energies up to 1 TeV per beam in the new main ring.[16] This last proposal evolved into both the CERN $Sp\bar{p}S$ collider and the Tevatron collider.

The $\bar{p}p$ concept was detailed in a paper by Cline, McIntyre and Rubbia in the Proceedings of the 1976 Neutrino Conference at Aachen, describing the possibility of making a very-high-energy antiproton-proton colliding-beam facility using one ring of an existing machine. This paper described the full system, including the requirements for the antiproton source, the specification for the cooling technique (either electron or stochastic cooling), antiproton yield estimates, accumulation time, etc. It also described the physics motivation, emphasizing the search for the W and Z.

I have asked at Fermilab about the origins of the $\bar{p}p$ concept, and I have been told by R. R. Wilson that the first "bare bones" suggestion came from McIntyre, and that Cline, McIntyre, and Rubbia took it from there. The proposal included R. R. Reeder and L. Sulak.

Fermilab was not enthusiastic about proceeding rapidly with any of the proposals. Proposal P-478 required a new 25 GeV ring and would divert resources from the Tevatron program. Proposals P-491 and 492 required the completion of the Tevatron, and Wilson felt that not enough was known about superconducting magnets to make a firm schedule at that time.

Rubbia was not content with what he regarded as an excessively conservative and slow approach at Fermilab. He returned to CERN and worked with Van der Meer and others in the accelerator physics groups at CERN to produce a detailed design. Leon van Hove, then Co-Director General, had the vision to recognize the importance of a high-energy antiproton-proton collider to physics and to CERN, and he overcame the inertia of the CERN system, gaining formal approval of a two-stage process. The two stages were to include a large-scale test of the cooling schemes and then, if that were successful, the building of a full-scale project.

The role of van Hove is not well known. The SPS proton accelerator had been operating only for a few years. John Adams, who had brilliantly led that project, was Executive Director General of the entire lab and van Hove was Scientific Director General (an experiment in divided leadership that CERN tried only this one time). CERN had been operating the costly SPS proton synchrotron for only a few years, and discussions were in full swing on the possibility of the LEP project, a 27 km-circumference electron-positron collider that dwarfed the SPS. Adams was concerned about the possible reaction of the CERN member states to an expensive SPS, and to an even more costly LEP, and still another new, although relatively small, $\bar{p}p$ project tucked in between. Van Hove felt very strongly that the scientific potential of the $\bar{p}p$ was such that CERN must move ahead with it if it were feasible.

They had argued about it several times without coming to an agreement. I was present at a meeting with both of them that started as a discussion of LEP and drifted on to the $\bar{p}p$ collider topic. I was the sole audience and the discussion grew quite heated. It reached the point where van Hove reminded Adams that he, van Hove, was the Scientific Director General, that in his opinion the case for the $\bar{p}p$ collider was overwhelming, and that if Adams did not back the project in the Council van Hove would resign! They then abruptly realized that I was still there, and the meeting ended with embarrassed mumbles.

I have never mentioned this incident except once or twice to van Hove. When the Rubbia-Van der Meer Nobel Prize was announced, I not only wrote to congratulate the Laureates but also wrote to van Hove telling him that at least one person in the physics community knew that without him there would have been no $\bar{p}p$ collider. In a conference devoted to the Rise of the Standard Model, it seems to be appropriate to break my 15-year silence.

The rest of the story of the CERN $\bar{p}p$ collider is well known. The cooling experiment worked as predicted by Van der Meer. Roy Billinge and Van der Meer led the construction of the antiproton source at CERN. Fermilab, under the leadership of Leon Lederman, decided to stay out of a race with CERN for the W and Z and stick to the Tevatron program and the superconducting-magnet technology development that is so important to the proton machines of today. The CERN $\bar{p}p$ collider worked well, culminating with the discovery of the W and Z by experiments UA-1 and UA-2, and an essential confirmation of the Standard Model. Van der Meer's invention made it possible, and Rubbia's drive and determination brought it about.

9. Conclusion

From the start of the first collider, CBX, to the time of this conference is 34 years, and the colliders have taken over the world of high-energy physics. This paper traces the main threads in the evolution of the technology. It is not a complete history of colliding beams and leaves out important contributions from Orsay, BNL, Cornell, Fermilab, KEK and Novosibirsk, that advanced the art but are not clearly related to the topic of this meeting.

Looking back from now to then, we see that the electron colliders came first because the technology was easier, and relatively small facilities could and did make great contributions to physics. The evolution of the electron machines was very rapid, reaching a plateau with SPEAR wherein all of the elements of all the storage rings that have been built since were in place. LEP marks the culmination of the storage-ring technology, for electron machines have a scaling law of costs with energy that is quadratic and makes it to costly to go much further with the storage-ring technique. Fortunately, the linear collider, first realized with the SLC at SLAC, has come along to replace the storage ring, and an active international R&D program is in progress aimed at the next step in very-high-energy electron colliders.

The fact that the early electron machines were low-cost and extremely productive created a climate where, for larger and more costly machines, technological and scientific "success" was the expected norm, and it was relatively easy, post-SPEAR, to obtain funding for larger projects.

The proton colliders, on the other hand, came more slowly because the technology was more difficult and because, if a collider were to make major advances in physics, the machine had to be large and costly from the beginning. The ISR was a brilliant success as an accelerator project, but the choice of initial experiments virtually precluded the discovery of the new particles and the large-transverse-momentum phenomena that are the stuff of the Standard Model. Thus there was no "demand pull," as the economists would say, from the physicists until the Standard Model itself began to unfold. The CERN $\bar{p}p$ collider was the first result of this demand pull, and that same demand is driving the programs to realize the SSC and the LHC.

I think we all hope that the next conference in this series is entitled "Beyond the Standard Model," and, if so, it is certain that the high-energy high-luminosity proton machines now being built; the low-energy, high-luminosity electron factories; and the high-energy linear electron colliders, all will have made an essential contribution to whatever unfolds.

References

[1]Deutsches Patentamt , Patentschrift Nr 876279 Klass 21g Gruppe 36 Ausgegeben am 11 Mai 1953.

[2]This story is from a letter from Wideröe to Eduardo Amaldi. It is quoted in Amaldi's biography of Bruno Touschek (Ref. 6).

[3]D. W. Kerst et al., Phys. Rev. **102**, 590 (1956).

[4]*CERN Symposium on High Energy Accelerators and Pion Physics* (Geneva: CERN, 1956), p. 36.

[5]A group of post-docs including me had asked J. D. Bjorken to lead a seminar on how to calculate with Quantum Electrodynamics. He gave us an exercise to calculate the annihilation cross section of an electron and a positron into a pair of spin-zero charged particles. I solved the problem, and suddenly realized that π-mesons and K-mesons were such particles, that any structure that they had would modify the cross section, and that the colliding-beam technique, if it worked, could be modified to do the experiment.

[6]An excellent scientific biography of Touschek is *The Bruno Touschek Legacy* by Eduardo Amaldi CERN 81-19 (Geneva: CERN, 1981).

[7] The effective intensity of a colliding-beam machine is measured by its luminosity, which is the reaction rate that would be observed for a process with a cross section of one. It is proportional to the product of currents in the colliding beams, divided by the effective overlap area.

[8]*Proceedings of the International Conference on Hign-Energy Accelerators* (Dubna, 1963).

[9]Rees and I were under great pressure to reduce the cost of the project. One possibility was to eliminate the magnetic detector in favor of a much less costly detector with no magnetic field. We were traveling home from a meeting at Frascati in '68 or '69, where I had my first opportunity to see the ADONE data and talk in detail with the experimenters. We spent much of the time talking about the results, and I came to the conclusion that the "4π" magnetic detector was essential to understanding the physics. No matter what the implications on the SPEAR schedule, we had to preserve it.

[10]K. W. Robinson and G. A. Voss, CEA Technical Note, 1965 or 1966. I have been unable to find a copy of this note, and so cannot give the exact citation. It is, however, mentioned, but not referenced, in a CEA report on the bypass project that was issued in late 1966.

[11] J. Rees, "Colliding Beam Storage Rings—A Brief History," in *SLAC Beam Line,* March 1986.

[12] C. Pellegrini et al., in *Proceedings of the 8th International Conference on High-Energy Accelerators,* M. N. Blewett, ed., (Geneva: CERN, 1971).

[13] *Proceedings of the International Syumposium on Electron and Positron Storage Rings* (Saclay: Presses Universitaires de France, 1966).

[14] R. Huson et al., *Proposal to Search for Intermediate Boson Production at 200 GeV in the Center-of-Mass,* Fermilab Proposal 478. (1976)

[15] C. M. Akeubraut et al., *Clashing Gigantic Synchrotrons,* Fermilab Proposal 491. (1976)

[16] D. Cline et al., *Proposal to Construct an Antiproton Source for the Fermilab Accelerators,* Fermilab Proposal 492. (1976)

Figure Captions

1. Energy available in the constituent center-of-mass system *versus* time for the electron and proton colliding-beam machines. The open circles and squares represent machines under construction or in the planning phase.

2. Radiation damping in an electron storage ring with an appropriate magnet configuration leads to a decrease in oscillation amplitude, allowing another injected pulse to damp down on top of the previously injected ones.

3. The partially assembled Princeton–Stanford storage ring. The lower halves of the magnets are in place and the vacuum chamber is installed. Radio-frequency cavities and beam transport to the rings are yet to be installed. The picture is from some time in 1962.

4. Feynman diagram for e^+e^- annihilation. The electron and positron form a virtual photon which can then produce any final-state coupling to the electromagnetic field constrained only by the conservation of energy, angular momentum, etc.

5. AdA, the first electron-positron storage ring.

6. The ratio R of the inclusive cross section for hadron production to the cross section for μ-meson pair production *versus* center-of-mass energy. The results from the three ADONE experiments differed widely, but all of them were very large compared to the theoretical expectations of that time, shown by the solid line.

7. SPEAR as it was in 1972. The housing is movable shielding blocks, and the buildings are portable. It was the absence of permanent civil construction that allowed the project to be dubbed "an experiment."

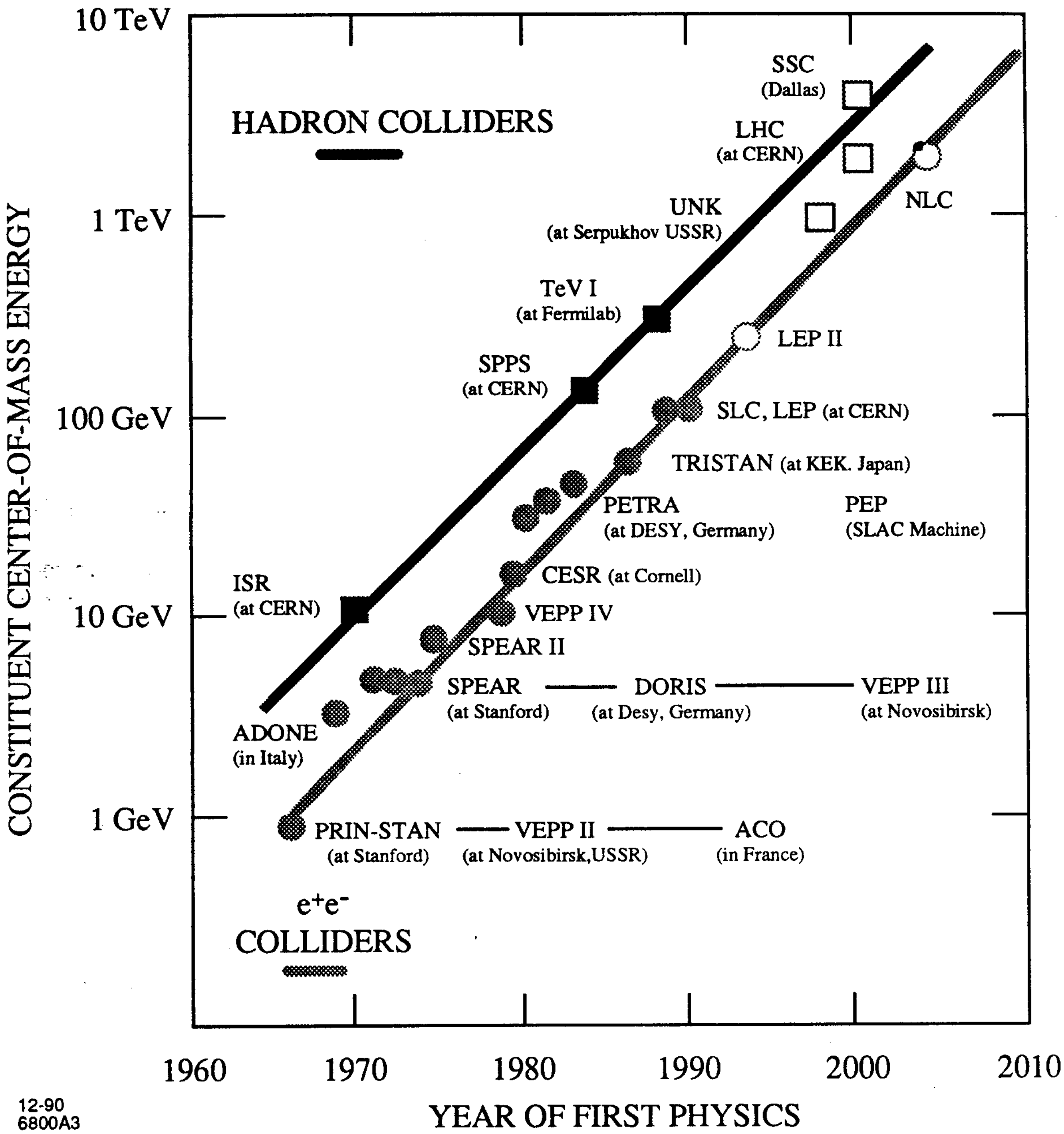

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Fig. 1

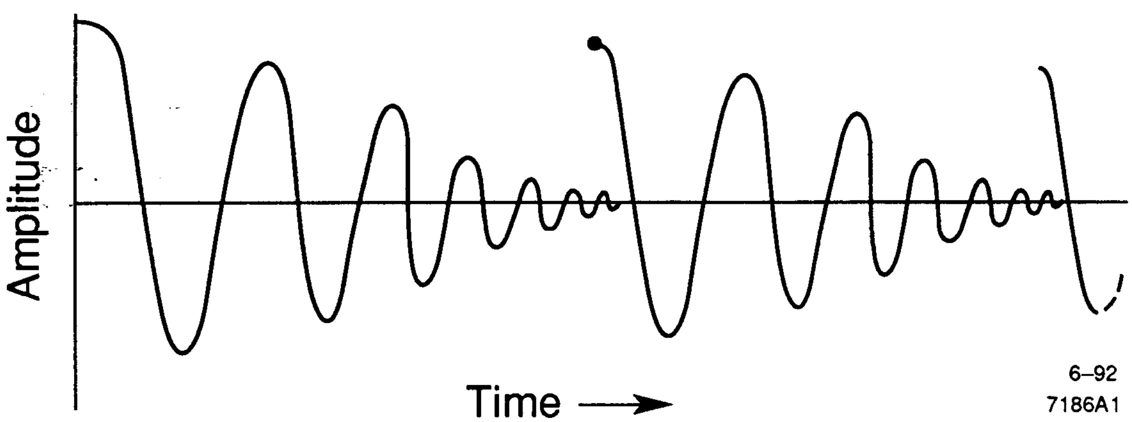

Fig. 2

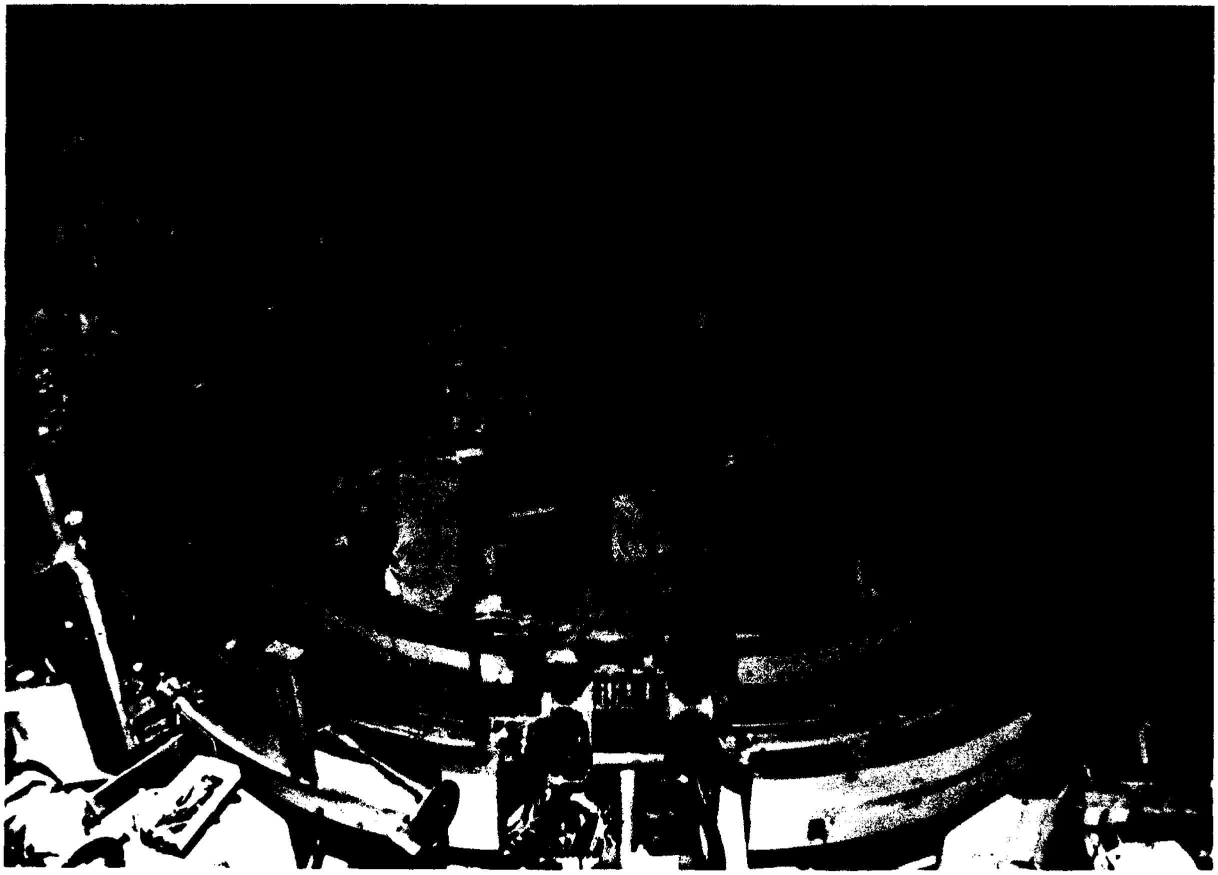

Fig. 3

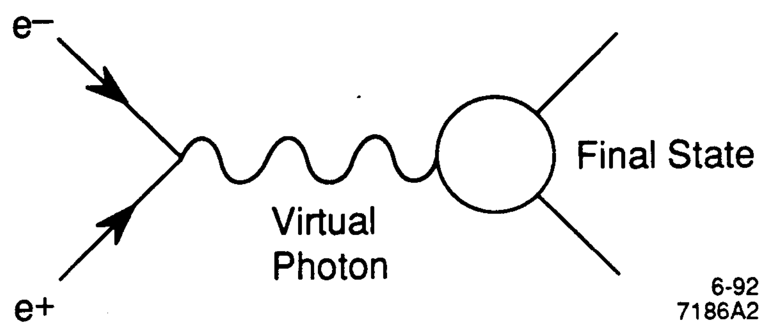

Fig. 4

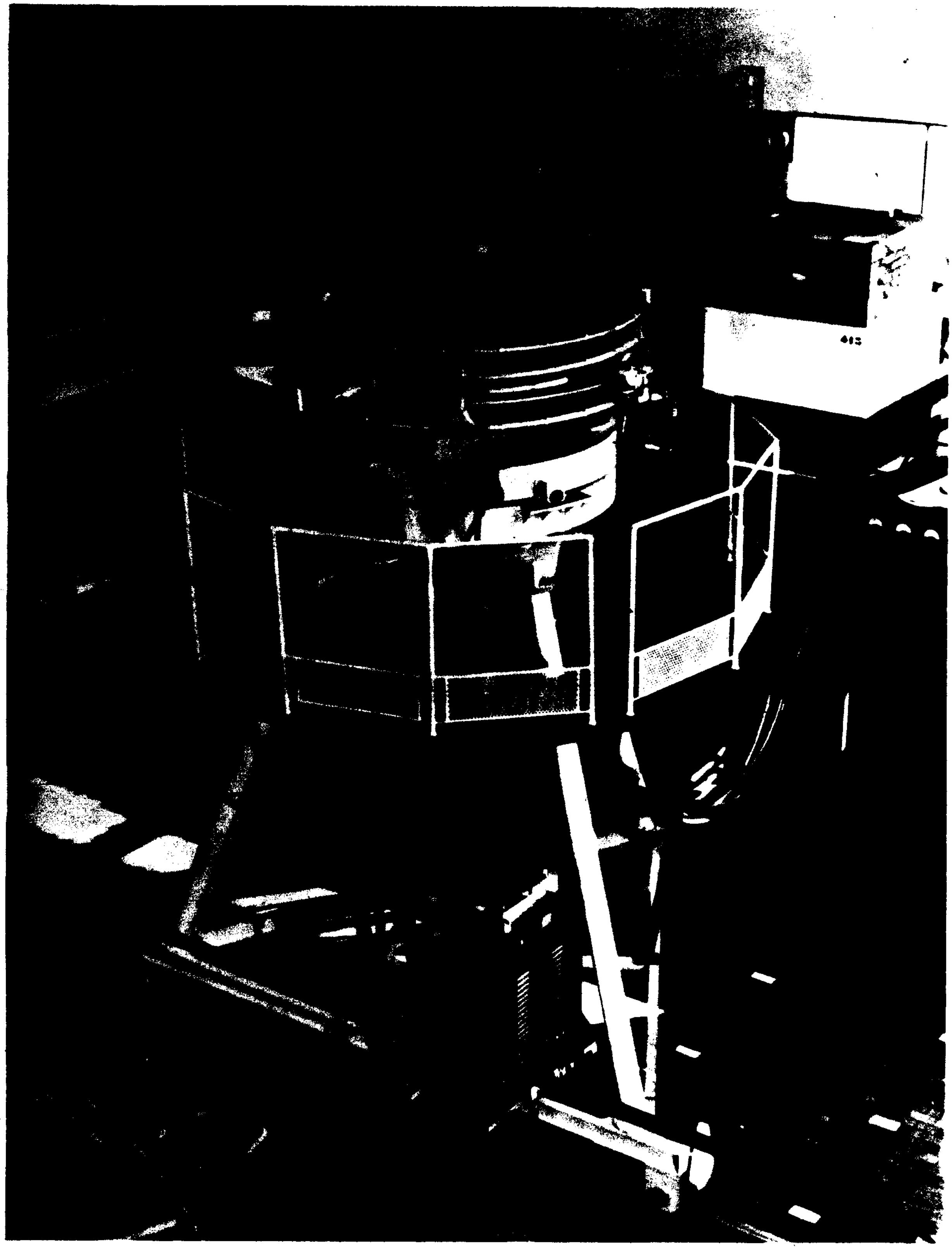

Fig. 5

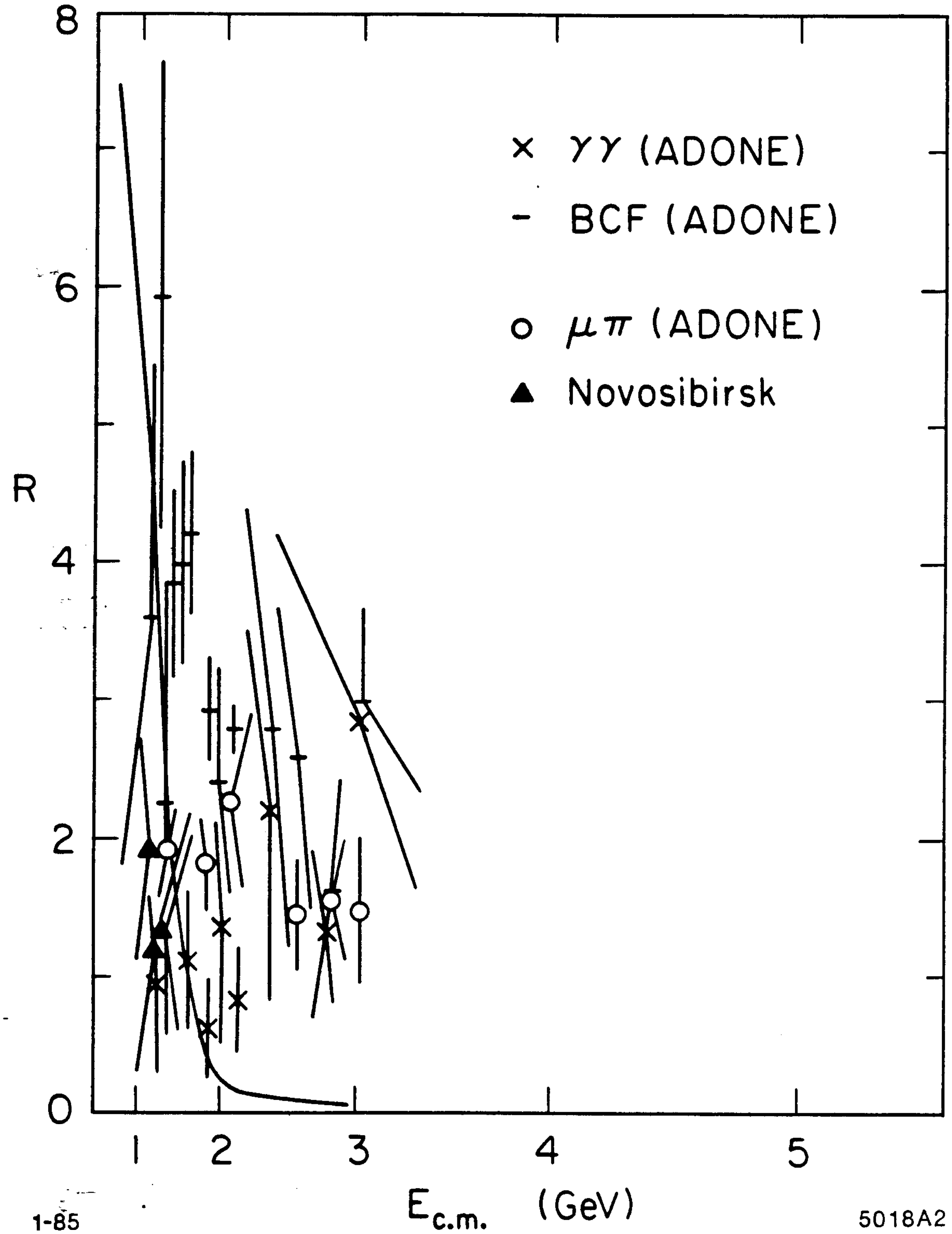

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Fig. 6

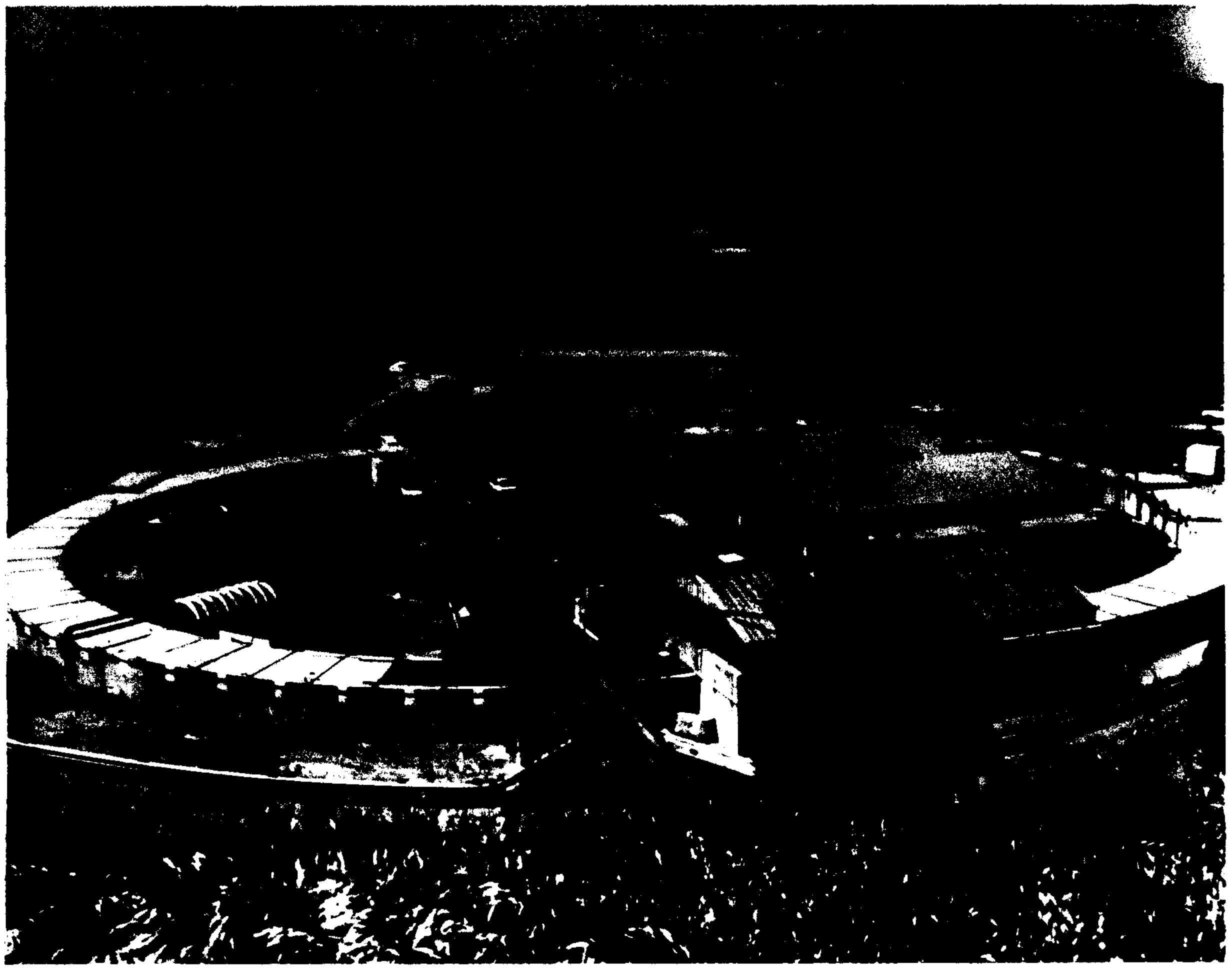

Fig. 7