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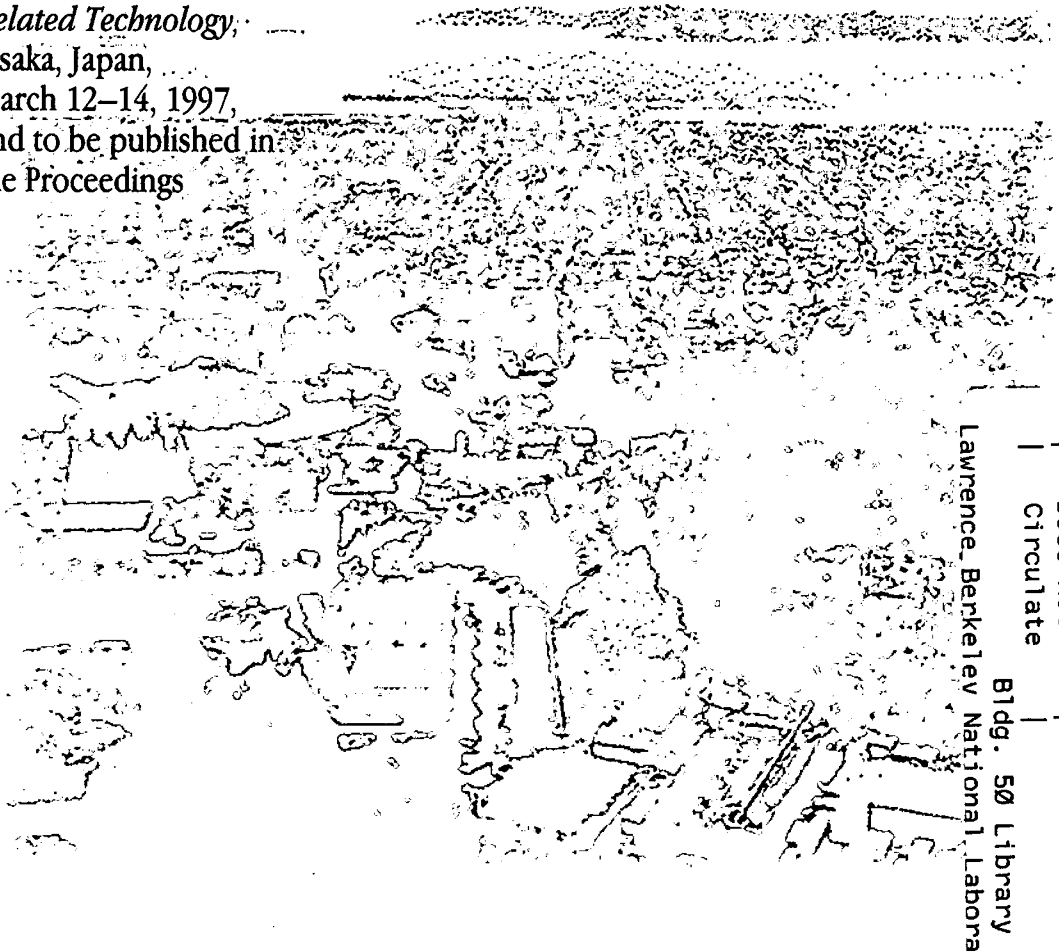

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The Development of Colliders*

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THE DEVELOPMENT OF COLLIDERS

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During the period of the '50s and the '60s colliders were developed. Prior to that time there were no colliders, and by 1965 a number of small devices had worked, good understanding had been achieved, and one could speculate, as Gersh Budker did, that in a few years 20% of high energy physics would come from colliders. His estimate was an under-estimate, for now essentially all of high energy physics come from colliders. I shall present a brief review of that history: sketching the development of the concepts, the experiments, and the technological advances which made it all possible.

Introduction

High energy colliders were developed in the 50's and 60's at only a few laboratories; namely, Stanford, MURA, the Cambridge Electron Accelerator, Orsay, Frascati, CERN, and Novosibirsk. Many hundreds of physicists contributed to the development of colliders (including some key people at each of the laboratories involved), but the men who started it, set it in the right direction, and forcefully made it happen, were Donald Kerst, Gersh Budker, and BrunoTouschek.

A review of the history, and reprints of the key papers may be found in a reprint volume which recently appeared.[1] In this paper I will, briefly, review the major concepts—and the physical basis for them—that go into making a collider. Much more information can be found in Ref 1. Inevitably, since the subject is exactly the same, and the author is the same, this paper will follow very closely (even copy some sections verbatum) the Introduction of Ref 1.

Prior to 1950 there were no colliders, while by 1965 a number of small devices had worked, good understanding had been achieved, and one could speculate, as Gersh Budker did, that in a few years 20% of high energy physics would come from storage rings. Of course, further advances were made in the subsequent decades, but the period of rapid growth was during the two decades mentioned. Today, essentially all of high energy physics comes from colliders.

How did it happen? Prior to World War II it was already well known that relativistic collision theory showed that with fixed targets the "available energy" only scaled as $E^{1/2}$, where E is the particle energy, but with colliding particles, of energy E, the "available energy" varied as 2E. In fact, during World War II (although I have been told that it was "well known") this idea was patented by Wideroe. I think it is fair to say, however, that no one had the slightest idea as to how to make a sufficiently intense beam so as to achieve, as we would say nowadays, enough luminosity to do interesting physics.

For those interested in the history, there are, of course, thousands of original papers. They make fascinating reading. Very instrumental was a conference that Budker called in Novosibirsk in March 1965. Only about a dozen attended, and no Proceedings were published, but it served to define, in a clear and precise manner, the problems that had to be solved in order to achieve interesting colliders. Immediately following that, in the summer of 1965, there was a Storage Ring Summer Study at SLAC, which set the direction for solving many of the problems identified earlier that Spring. By September of 1966, the subject was sufficiently mature that an International Symposium on Electron and Positron Storage Rings was organized in Paris.

Alternate Gradient Focusing and Fixed-Field Alternating Gradient Accelerators

In the early 1950s alternate gradient focusing was discovered by the team of Ernest Courant, Stanley Livingston, and Hartland Snyder and, independently, by Nicholas Christofilos.[2] Prior to that time the physicists thought they had to make very uniform fields such as in cyclotrons; now many variations from uniformity were permitted and, more importantly, an understanding and method of calculation had been developed that allowed them to determine what was suitable for particle accelerators and what was not acceptable.

Knowing about the Brookhaven work, the group Midwestern Universities Research Association (MURA) began to widely explore focusing fields. In short order Keith Symon and Donald Kerst discovered fixed-field alternating gradient focusing (FFAG).[3] With FFAG, particles of all energies, from the injection energy to the final energy, were stable in the machine at the same time.

That immediately suggested to Donald Kerst the possibility of building up a sufficiently intense beam so as to make a realistic collider. Thus MURA grappled with the problems of (1) will non-linear behavior allow the stacked beam to last for a very long time (2) how to manipulate the RF so as to build up an intense beam without destroying the "stacked" beam at high energy?

We will turn to question number 2 in the next section; here we address number 1. Prior to this time beam physicists had only dealt with linear systems and short-time behavior, but the FFAG fields were very non-linear and interest was now in long-term stability. They tried tracking for a few turns (since computers weren't very powerful in those days), and developed mapping techniques for longer runs. Quickly they saw that if the map wasn't exactly dynamical, i.e., preserving Poincare Invariants (Liouville's theorem in 1D), in just a few iterations they obtained non-physical results (such as damping of phase space). Thus they made what we now call symplectic maps.

With these maps, and the most powerful computer of the time, they could apply the map 50,000 times. Then they ran it backwards to be sure they were free of truncation error. Thus they explored long-term stability, and learned that they could design highly nonlinear fields (but linear at small amplitudes) that gave stable motion at least for the length of runs they could study. They never published any of this work (the MURA Group considered the results uninteresting—no new phenomena were observed, and the runs were not long enough to make interesting statements about the long-term stability needed for colliders).

The MURA Group was well aware of the deep nature of the questions that were being explored, noting, for example, that the observed stability of the solar system provided little comfort, for they wished to store particles for much longer periods than the age of the solar system (measured in numbers of circulations). Jorgen Moser was interested in the subject of dynamical systems, which led to his subsequent work on the KAM Theorem.[4] Many years later, Boris Chirikov was able to develop a quantitative criterion[5] that was quite consistent with the early observations at MURA .

To summarize early work on long-term stability, although it couldn't be proven, it seemed probable that one could design systems that would store beams for very long times. Thus MURA had developed one very important ingredient necessary for colliders.

Radio Frequency Manipulation

The second question that MURA was concerned with had to do with RF manipulation. In order to make progress, Keith Symon developed a Hamiltonian formulation of the effect of RF on particles (prior to this time only small amplitude motion in buckets was considered, but one needed to know about the influence on particles outside of buckets); simultaneously there was developed a computer program to study particle motion.

When MURA turned on the computer program, they discovered phase displacement, and with a remark from Wigner about the importance of Liouville's theorem, and Hamiltonian formulation, it didn't take very long to establish a complete understanding of stacking.[6]

Thus it was possible to achieve what was desired; namely, to build up a stacked beam; that is, to accelerate particles with RF while not having the RF destroy the stacked particles (to a considerable degree). With some assurance of long-term stability, and with some understanding of stacking, MURA could now (in 1956) for the first time seriously propose a proton collider. [7]

The Colliding Beam Concept/ The Storage Ring Concept

MURA started to develop electron models of FFAG in the mid 1950s. The first model was a radial sector machine built in Michigan, shown in Fig. 1, and was soon followed by a spiral sector model built in Wisconsin, shown in Fig. 2. These machines confirmed the validity of FFAG. Out of this work came the whole field of spiral ridged cyclotrons. The first models used betatron acceleration; later, RF was employed so study could be made of the RF manipulation of particles. Fig. 3 shows a model which had this capability; namely, the Two-Way Model. This was a storage ring, but of rather low intensity.

The work at MURA attracted the attention of the CERN Group, and in 1960 the CERN Group decided not to build an FFAG model, but rather to design and build a storage ring electron model called CESAR[8], shown in Fig. 4. The energy was taken to be low so that radiation damping was negligible, and therefore the ring was a good model of proton behavior. Out of this work came the first "real" proton storage ring, the ISR. The idea of storage rings, in contrast with a purely FFAG machine, was a MURA idea. Nevertheless, MURA kept proposing large FFAG machines, and did not receive support for any of them.

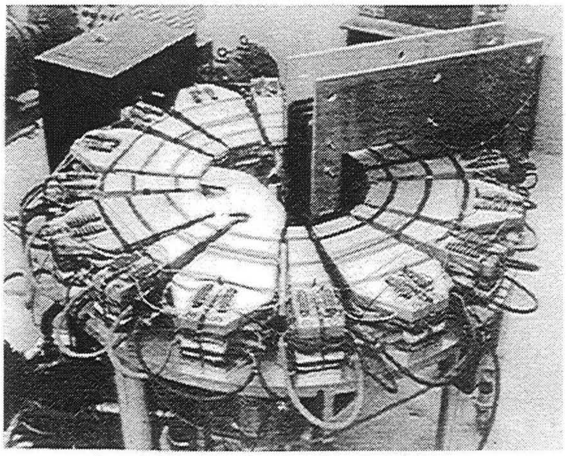

Figure 1. The Mark 1 Model built by the MURA Group in Michigan in 1955. There are 8 sectors, and electrons of 30 keV were injected at a radius of 34 cm and accelerated, by betatron action (note the large core), to 400 keV at a radius of 50 cm. (Photograph from the personal collection of Andrew M. Sessler.)

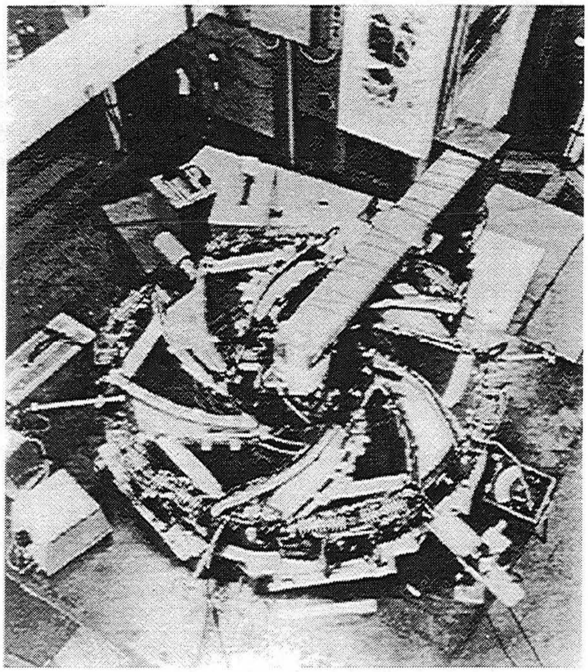

Figure 2. The Mark 2 (Spiral Sector) Model built by the MURA Group in Wisconsin from 1956 to 1959. It had 6 sectors and accelerated electrons, by betatron action from 35 keV, at an injection radius of 31 cm, to 180 keV, at 52 cm radius. (Photograph from personal collection of Andrew M. Sessler.)

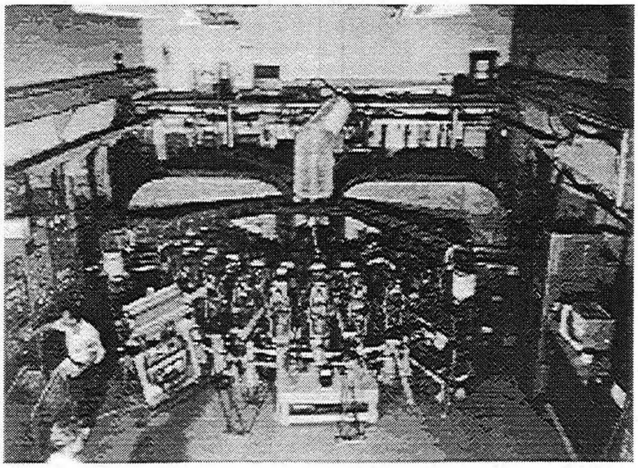

Figure 3. The MURA Two-Way Model, completed by the end of 1959. It had 16 sectors and accelerated electrons from 100 keV, at a radius of 123 cm, to 50.7 MeV, at a radius of 200 cm. An RF system was installed and 10A of electron at 50 MeV were stacked. (Photograph from the personal collection of Andrew M. Sessler.)

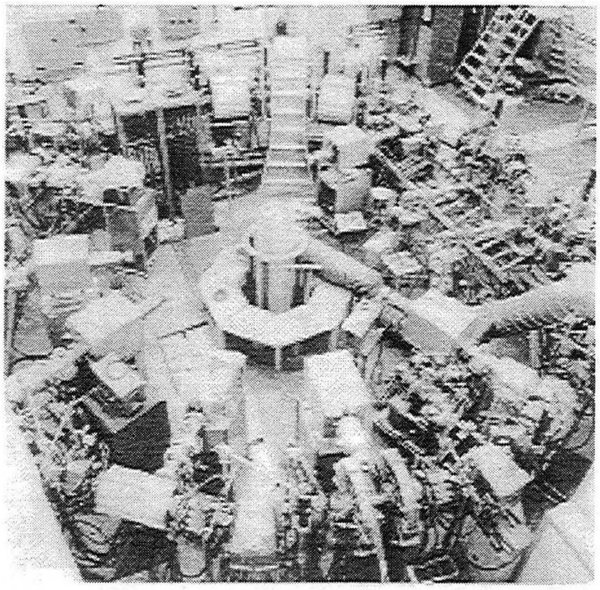

Figure 4. The CERN Electron Model for the ISR, initiated in 1960. The circumference is 24m, including 12 straight sections each 1m long. The electron energy could be 100 MeV, but 2 MeV was used for most studies. (Photograph courtesy of CERN.)

The ISR, which, as I said, grew out of the CERN model work on CESAR, was an adventurous machine to build for it was most unclear whether it would work. Single particle stability might not be as was thought (it had never really been tested), and various other effects-too horrible to mention might occur. The machine, thanks to Kjell Johnsen's insistence, was conservatively built in all its conventional regards and thus one was able, if necessary, to handle any untoward effects.[9]

There was such an effect; namely, an unexpected dependence on gas pressure; explained after the fact as a pressure bump caused by the ions produced by the beam, accelerated to the walls by the beam's electrostatic potential, and there liberating even more molecules. Because of the conservative design the walls could readily be cleaned, and the vacuum could be increased by two orders of magnitude over the design value (to 10^{-11} Torr), and the ISR performed as predicted; in fact, eventually, much better than predicted.

The first electron-electron storage ring, at Stanford is shown in Fig. 5, and took many years to achieve success.[10] During the course of making the storage ring work, many diverse physical phenomena were discovered, understood, and circumvented. These include such well-known effects as the resistive wall instability, beam-beam interaction, and the degradation of vacuum due to beam radiation, all first discovered on this ring. At the same time, Budker was developing storage rings in the Soviet Union. Their first collider, also an e^-- e^- machine, was VEP-I is shown in Fig. 6.

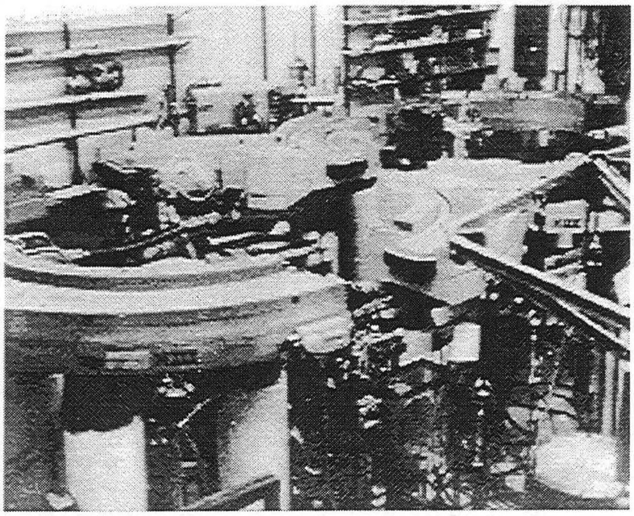

Figure 5. The Stanford electron-electron collider, which was started in 1959, although a paper describing electrodynamically interesting results was not published until 1966. The two rings can be seen; the electron energy was 500 MeV, the orbit radius 56 in. Although up to 1A of a single beam could be stored, typical operation with colliding beams was with about 50 mA in each beam. (Photograph from Stanford News and Publications.)

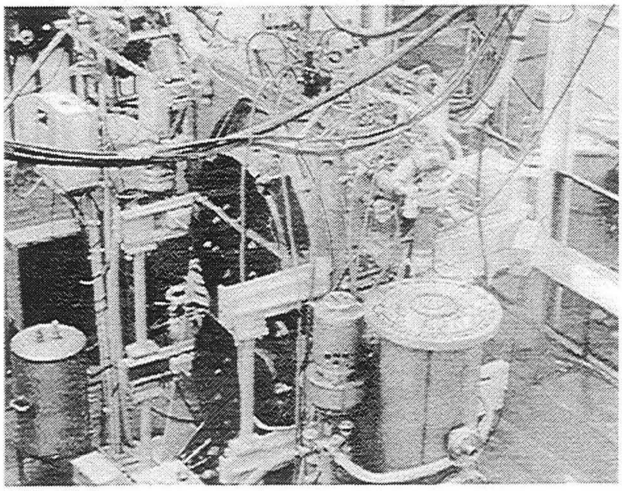

Figure 6. The first Soviet e^-- e^- storage ring, VEP I, upon which construction was started in Moscow, moved to Novosibirsk in 1962, and by only 1965 was giving results on e^-- e^- scattering. There are two rings, each of radius 43 cm, and the "equivalent energy" was 100 GeV. (Photograph from the personal collection of Andrew M. Sessler.)

Electron-positron storage rings were developed in Europe and the Soviet Union. Electron-positron colliders were proposed by Touschek at Frascati. Touschek was interested in studying the properties of vacuum and vacuum fluctuations, and saw in e^+- e^- reactions a clean way of doing it. The proposal to venture into this new area was approved in a week, and the collider, AdA, was built in one year.[11] The first machine didn't work, but no sooner was it built than the problem was understood. Touschek pushed on, and success was achieved in the second generation.

In particular, the first ring, Anello di Accumalazione (AdA), started in 1961 as shown in Fig. 7, suffered from an intrabeam scattering limit, while the second generation machines, ACO and ADONE, were specifically designed to get around that limit.

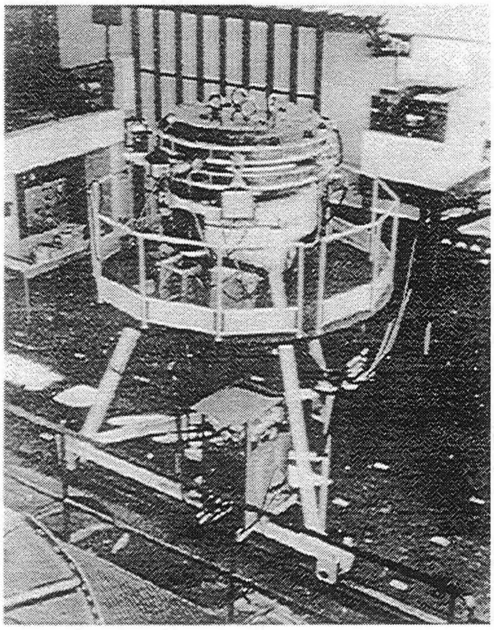

Figure 7. The early electron-positron collider AdA when it first started operation in March 1961 in Frascati (later it was moved to Orsay for there was a more powerful injector there). The machine was equipped with RF and stored beams of energy up to 250 MeV at a radius of 58 cm. Injection involved moving the apparatus on the rails. Beam lifetime was very short, but electron-positron annihilations were observed. (Photograph from the personal collection of Andrew M. Sessler.)

The first Soviet e^- - e^+ storage ring, VEPP II is shown in Fig. 8, being at a higher energy suffered less from intra-beam scattering. ADONE ("big AdA") is shown in Fig. 9, and was very conservatively built (in all but its concept) by Fernando Amman and it was successful, as were ACO and VEPP II, in producing significant particle physics.

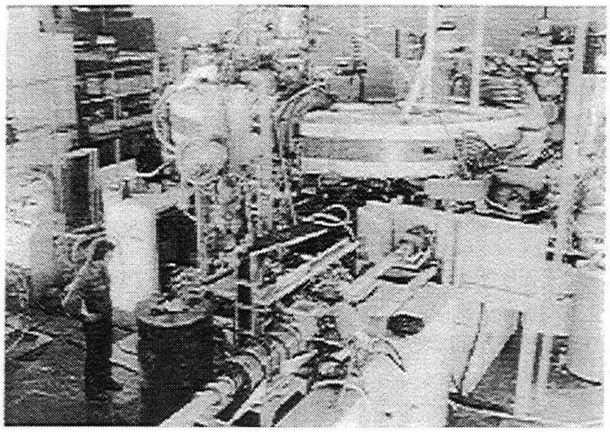

Figure 9. ADONE, the first of the large e^+- e^- storage rings. One can see that significant space had been provided for particle experiments. The figure shows the ring during construction; operation commenced in 1969. (Photograph from the personal collection of Andrew M. Sessler.)

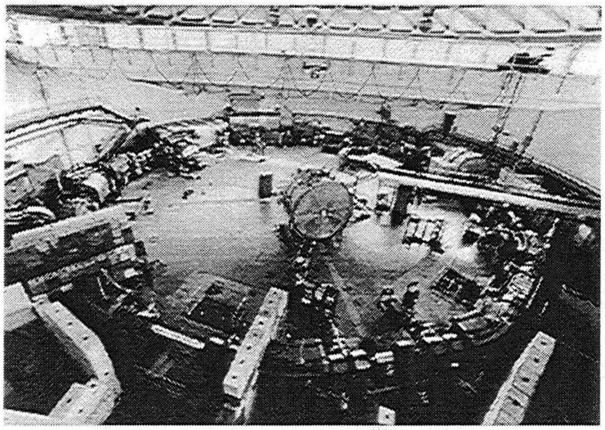

Figure 8. The first Soviet e^-- e^+ storage ring, VEPP II. Each beam had 700 MeV and in 1967 it was used to study electron-positron annihilation into pions at the rho resonance. This was the first experiment ever using an e^- - e^+ storage ring. (Photograph from the personal collection of Andrew M. Sessler.)

Collective Instabilities

No one thought, prior to MURA work, that a stored beam could undergo collective motion. The concept of equilibrium conditions--space charge limits--was well understood, but it was thought that there were only static space charge phenomena. It was Carl Nielsen who first realized that an azimuthally perfectly uniform beam was unstable against bunching; i.e., behaved as if the particles had a "negative mass." So here was a possible impediment, not realized by the MURA Group in its previous publication,[7] to achieving stored beams of adequate intensity!

Soon the MURA Group derived the criteria for stable behavior of the negative mass instability (now generalized and known as the Schnell-Keil criteria for the re-named microwave instability). Very similar work was done, independently, by Andrei Kolomenskij and Andrei Lebedev.[12]

The high beam intensities, first being explored at that time by the experimentalists, brought them into a new regime and instabilities were now observed everywhere. The instabilities included the resistive wall, the head-tail effect, and coupled bunch phenomena.

It is fair to say that many different collective instabilities had to be understood before colliders could be achieved. Many workers, both theoretical and experimental, were involved in that process.[13] Collective instabilities can be cured in principle, but often in practice it is very difficult. Feedback systems, to handle some collective instabilities, are employed on essentially all storage rings. As a result of careful design and feedback systems, collective instabilities put limits on the stored current of a single beam, but that limit can be made above what is allowed by the incoherent beam-beam effect for colliding beams. Thus the limit on collider operation almost always comes from the incoherent beam-beam interaction.

Radiation Damping

Successful electron storage rings required that one understand the radiation process and its reaction on the electrons. That understanding had been pioneered by Kenneth Robinson and Matthew Sands.[14]

For example, the Cambridge Electron Accelerator (CEA) had been constructed so that it didn't damp in all three directions (because that was of no importance in a synchrotron), but complete damping was essential for a storage ring. In the conversion of the CEA, special magnets were installed so as to make the ring damp in all three directions.

A second example of the consequences resulting from the understanding of radiation was an appreciation of the freedom it allowed in the design of lattices. This led to the concept of separated function structures (now used in all machines, but first incorporated into the machine ADONE).

Low Beta

Motivated by the desire to make the Cambridge Electron Accelerator (CEA) into a high luminosity storage ring, Kenneth Robinson and Gus Voss invented the concept of low beta.[15] That one can squeeze the beam at one point and still have stable motion in the storage ring was not at all obvious at the time. The concept was demonstrated on the CEA and has by now become a vital part of all storage rings.

With the successful operation of the Stanford electron-electron rings, and the success in Europe with electron-positron rings, understanding could now be codified.[16] More importantly, large electron storage rings could be constructed with confidence. Thus one saw the progression of SPEAR, DORIS, PEP, PETRA, and LEP (as well as and other rings).

The most recent stage of development is, of course, LEP, the very large e^+- e^- storage ring at CERN, the electron-proton machine HERA at DESY, and the B-Factories PEP-II at SLAC and at KEK. The B-Factories requires two rings, so as to avoid crossing of the very many thousands of bunches, as a result of the desire to achieve a very large luminosity (of the order of 10^{33} cm^{-2} sec^{-1}).

Beam-Beam Interaction

The interaction of the two beams at the collision point introduces an additional force on the particles, which can alter their motion. The importance of taking this effect into account was recognized as early as 1961 by Fernando Amman and David Ritson, who pointed out that the nonlinear character and the time dependence of the force between the beams can produce excitation of many resonances.[17] Their initial estimate was that to maintain the beam stability, the tune shift produced by the interaction should be less than the typical distance from the nearest resonance, or about 0.1.

Incoherent beam-beam phenomena were first observed on the Stanford rings and at a value of beam-beam interaction induced tune shift of about 0.02. Immediately, in Brookhaven and at Lawrence Berkeley, computer codes were constructed to study the effect computationally. It was learned that a simple ID model would not give an adequately low threshold (as was observed), but that one had to include longitudinal motion to "explain" the experiments, a result that has been substantiated by work in the subsequent decades. Experimental observations of the effect were soon made in colliders around the world.

Scattering Phenomena and Electron Cooling

Extensive study has been made of background gas scattering of a stored beam. An aspect of scattering that was not predicted ahead of time, was the "Touschek Effect," which is the scattering of particles within the same bunch (intra-beam scattering) leading to longitudinal loss of particles longitudinally as they jump out of the RF bucket.[18] It was observed that multiple small angle intra-beam scattering can produce diffusion and change of the bunch volume. This effect prevented the earliest storage rings, AdA and ACO, from working very well, and it provides a limit which must be carefully observed in all colliders.

The idea of electron cooling was invented by Budker in 1966.[19] Physicists at MURA had long tried to think of ways to "beat Liouville," but all their attempts failed. Some failed in principle, while other ideas (such as tapered foils) worked in principle, but not in practice (because of too much scattering in the foil). But Budker's idea replaced a fixed foil with electrons so there was little scattering. Furthermore, he proposed using moving electrons of very cold temperature, so that the interaction between protons and the cooled electrons would lead to a cooling of the protons. The formalism for beam scattering was employed to analyze electron cooling. Subsequent experiments, on a machine called NAP, and shown in Fig. 10, confirmed the cooling idea, and although electron cooling never made a big impact on colliders, it has been used rather extensively, and very effectively, to make "cooler rings" for nuclear physics studies.[20]

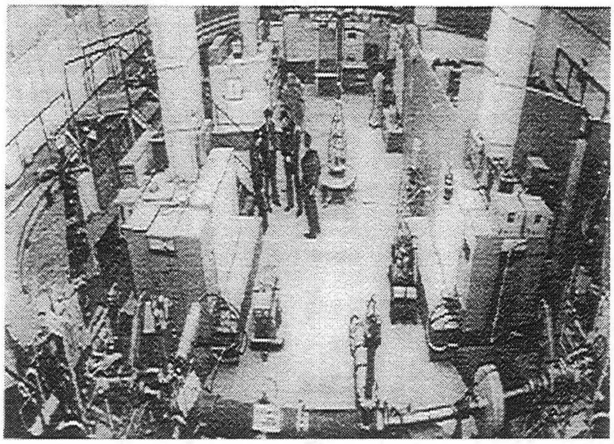

Figure 10. The NAP device (Novosibirsk) in which electron cooling of a proton beam was first achieved. (Reprint from G.I. Budker: Reflections and Remembrances.)

Stochastic Cooling

Early in the 1970s Simon van der Meer realized that it was practical to "beat Liouville" by means of a device that works on the fluctuations from equilibrium. He proposed operating on individual particles (or a rather small number of particles, where the finiteness of the number is vital) with pickups and kickers. Thus he invented stochastic cooling.[21] The main difficulty was technological; that is, the development of sufficiently sensitive pickups, good amplifiers, and excellent filters. A model was built to study stochastic cooling, called ICE, and shown in Fig. 11.

Stochastic cooling has proved to be remarkably effective and made possible the construction of proton-antiproton colliders. The antiprotons are produced in a very warm state; i.e., with a density which is completely inadequate to give the desired luminosity. With cooling, the energy spread has been reduced by a factor of about 10^4, while the transverse emittance has also been reduced by large factors.

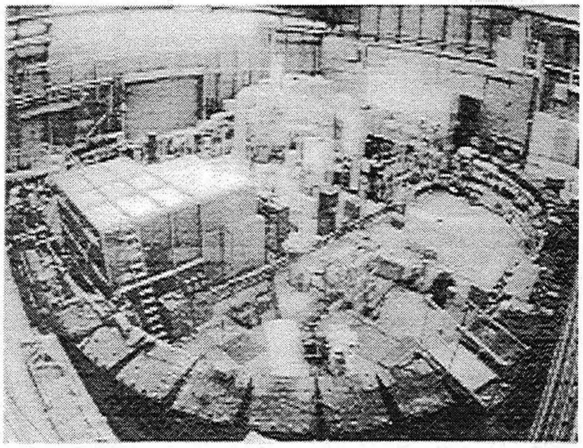

Figure 11. The storage ring, ICE, at CERN, on which stochastic cooling was developed. (Photograph courtesy of CERN.)

With practical stochastic cooling in hand, and knowing that one could make proton-proton colliders, as evidenced by the ISR, CERN built the first proton-antiproton collider by converting the SPS for this purpose. Subsequently, Fermilab converted its Tevatron to colliding beam operation.

The next generation of hadron colliders, the LHC, which is being built by CERN, is going back to proton-proton colliders (so as to obtain lots of luminosity, which can only be achieved in two rings). This machine does not require cooling for its operation. On the other hand, the heavy-ion collider being constructed at Brookhaven, RHIC, can operate without cooling, but cooling of the bunched beams is being seriously contemplated so as to improve performance.

Conclusion

The beam research done in designing and operating colliders is now so commonplace that there are textbooks on the subject. Some of the more prominent ones are included in the references.[22-34]

In conclusion, I hope there isn't a "conclusion" to the history of colliders. Most important is the realization that all this was done by just a few individuals. They were located at only seven institutions which gave them financial and emotional support, and backed them for many hard years. Their efforts completely changed the way we explore the structure of matter at the subnuclear level; we need to be sure that government agencies and laboratories, of the present and future, will encourage innovative work so as to continue the search to unfold the details of particle physics at even smaller dimensions.

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