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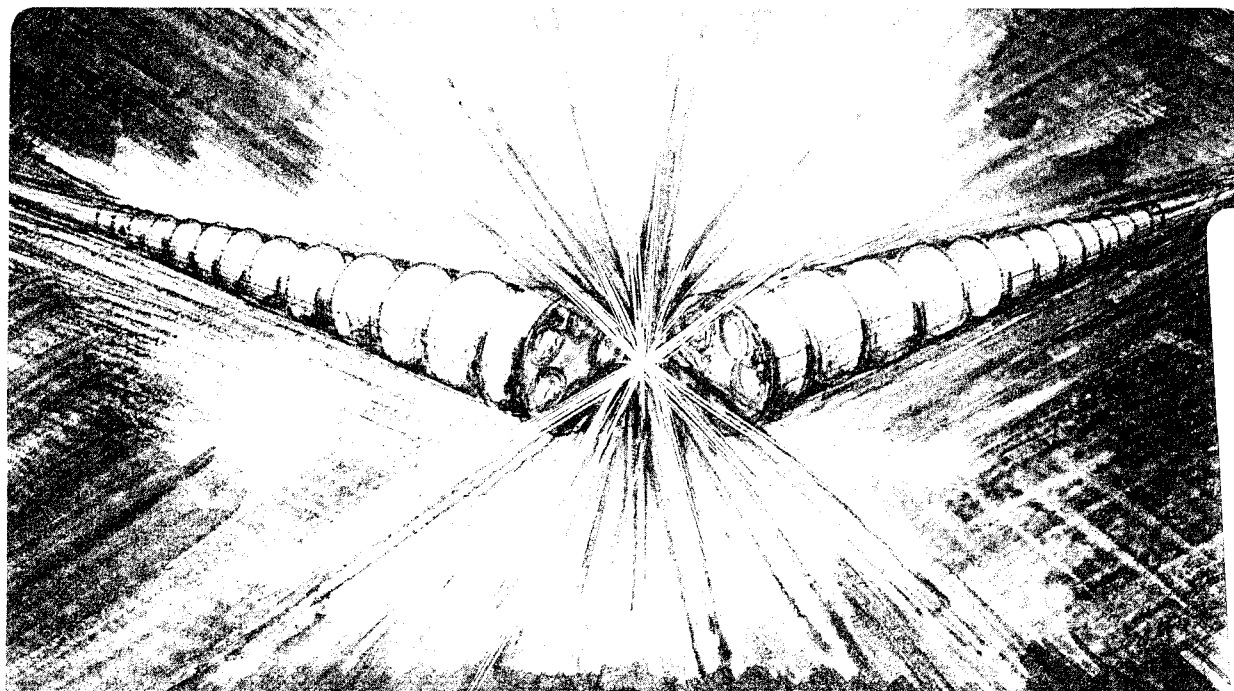
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THE DEVELOPMENT OF COLLIDERS*

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The Development of Colliders

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I. Introduction

Don Kerst, Gersh Budker, and Bruno Touschek were the individuals, and the motivating force, which brought about the development of colliders, while the laboratories at which it happened were Stanford, MURA, the Cambridge Electron Accelerator, Orsay, Frascati, CERN, and Novosibirsk. These laboratories supported, during many years, this rather speculative activity.

Of course, 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 Don, Gersh, and Bruno. Don was instrumental in the development of proton-proton colliders, while Bruno and Gersh spearheaded the development of electron-positron colliders.

In this brief review of the history, I will sketch the development of the concepts, the experiments, and the technological developments which made possible the development of colliders. It may look as if the emphasis is on theoretical concepts, but that is really not the case, for in this field--the physics of beams--the theory and experiment go hand in hand; theoretical understanding and advances are almost always motivated by the need to explain experimental results or the desire to construct better experimental devices.

It was during the period of the '50s and the '60s that colliders were developed. Prior to that time 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, and now--how sad it is that neither Budker nor Touschek are alive to see--essentially all high energy physics comes from colliders.

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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 the $E^{1/2}$, 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 am told that it was "well-known") this idea was patented by Wideröe. 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 now-a-days, enough luminosity to do interesting physics. All that changed in the '50s, and it is that story which I want to describe in this paper.

For those interested in the history, there are, of course, thousands of original papers. They make fascinating reading. Very instrumental was a conference which Budker called in Novosibirsk, in March 1965. Only about a dozen attended, and I don't think there are any Proceedings published, but it served to define, in a clear and precise manner, the problems which 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, an International Symposium on Electron and Positron Storage Rings was organized in Paris. The Proceedings make wonderful reading; the summary talks by Matt Sands, Bruno Touschek, and myself show that young as we were and young as the field was, we quite understood the subject.

II. FFAG and Non-Linear Phenomena

In the early '50s alternate gradient focusing was discovered, independently by Nick Christofilos and the team of Ernie Courant, Stan Livingston, and Hartland Snyder.¹ The concept really opened up a world of freedom for beam physicists. Prior to that time they 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 which allowed them to determine what was suitable for particle accelerators and what was not acceptable.

Given this historical understanding, it is not surprising that the midwestern group, MURA, began to widely explore focusing fields. In short order Keith Symon and Don Kerst discovered fixed-field alternating gradient focusing (FFAG).² 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 Don Kerst, the possibility of building up a sufficiently intense beam so as to make a realistic collider. Thus when I joined MURA in 1955 the very first questions that Don asked me were: (1) How to manipulate the RF so as to build up an intense beam without destroying the "stacked" beam at high energy? and (2) Will non-linear behavior allow the stacked beam to last for a very long time?

I will turn to RF questions in the next section, let me here focus upon long-time behavior. Many of us at MURA started to ask questions about the long-term behavior of non-linear dynamical systems. You see, 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 upon long-term stability. We tried tracking for a few turns (since computers weren't very powerful in those days), and we developed mapping techniques for longer runs. Quickly we saw that if the map wasn't exactly dynamical; i.e., preserving Poincare Invariants (Liouville's theorem in 1 D), in just a few iterations we obtained non-physical results (such as damping of phase space). Thus we made--what we now call--symplectic maps.

With these maps, and the most powerful computer of the time (the Illiac) we could apply the map 50,000 times. Then we ran it backwards to be sure we were free of truncation error. Thus we explored long-term stability, and we learned that we could design highly non-linear fields (but linear at small amplitudes) that gave stable motion at least for the length of run we could study. We never published any of this work (for we considered the results--no new phenomena were observed--uninteresting and we didn't consider the runs long enough to make interesting statements about the long-term stability needed for colliders, and, finally, in those days we didn't publish developments of "techniques and methods").

We were 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 we wished to store particles for much longer periods than the age of the solar system. Ernie Courant said he had a brother-in-law that might be able to help us, and thus we invited Jorgen Moser to MURA. We learned much from him, such as the speculations of Kolmogorov, which boded well for us. I believe we were instrumental in getting Moser interested in dynamical systems; his subsequent work on the KAM Theorem is known to all.³ Many years later, Chirikov was able to develop a quantitative criterion⁴ which was quite consistent with our early observations at MURA (especially with RF).

To summarize our work on long-term stability, although we couldn't prove it was okay, it seemed probable that one could design systems that would store beams for very long times. Thus we had one very important ingredient for collider development.

III. RF Stacking

The other question that Don Kerst asked me that first day, had to do with RF manipulation. I remember that I achieved a good deal of understanding, after about a month at MURA, by the morning of July 4, 1955. What I appreciated was that it was possible to achieve what Don Kerst desired; namely to build up a stacked beam; that is, to accelerate particles with RF, while not having the RF destroy the stacked particles. Of course that isn't fully true, but it is more true than false.

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 we needed to know about the influence on particles outside of buckets), while I worked on developing a computer program to study particle motion.

When we turned on the computer program, we used 11 particles at first--spread over all phases, so some were inside, but most were outside a bucket--and discovered that 8 of them went down in energy. Thus we discovered phase displacement, and with a remark from Wigner about the importance of Liouville's theorem, and Keith's Hamiltonian, it didn't take very long to establish a complete understanding of stacking.⁵

With some assurance of long-term stability, and with some understanding of stacking, MURA could now, for the first time, in 1956, seriously propose a proton collider.⁶

IV. Model Work for Proton Colliders (MURA and CERN)

MURA started to develop electron models of FFAG in the mid 50's. The first model was a radial sector machine built in Michigan, see Fig 1, and then soon-followed by a spiral sector model built in Wisconsin, see Fig 2. These machines⁷ confirmed the validity of FFAG. Out of this work came the whole field of spiral ridged cyclotrons. The first models used betatron acceleration, and were built to confirm FFAG, which they did. Later, RF was employed and so study could be made of the RF manipulation of particles. See Fig 3 for 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, see Fig 4.⁸ 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, but, nevertheless, MURA kept proposing large FFAG machines, but not receiving support for them from the government. The authorization of the ZGS at the Argonne served as a death-blow for high energy physics at MURA (although other physics, such as synchrotron radiation sources continued at MURA).

V. The ISR

The ISR was an adventurous machine to build. It really was most unclear that it would work. After all, 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 conventional regards.⁹ Thus one had great tools available, if necessary, to handle any untoward effects.

And there was one; namely an unexpected dependence on gas pressure, of course 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.

VI. Early Electron Experiments (Stanford, Orsay, Frascati, Novosibirsk)

Electron-positron storage rings were developed in Europe and the Soviet Union, while electron-electron storage rings were developed in the US and the Soviet Union. In contrast with proton storage rings, the problems were collective instabilities, radiation damping, gas scattering, intra-beam scattering, and beam-beam interaction. While theoretical work went forward, experimental construction of electron-positron machines proceeded apace. The early machines didn't work, but no sooner were they built than the problem was understood and success was achieved in the second generation.¹⁰ In particular, the first rings AdA, see Fig 5, and VEP II suffered from an intra-beam scattering limit, while the second generation, ACO and ADONE, were specifically designed to get around that limit.

Although first observation of e^+e^- collisions were made on AdA; further physics awaited the next generation. That machine, ADONE ("big AdA"), was very conservatively built (in all but its concept) by Fernando Amman and it was successful, as were ACO and VEP II, in producing significant particle physics.

The first electron-electron storage rings were VEP I (160 MeV \times 160 MeV) and the Stanford rings (500 MeV \times 500 MeV). The rings at Stanford, shown in Fig 6, took many years to achieve success.¹¹ During the course of making the storage ring work, many diverse physical phenomena were discovered, understood, and circumvented. These include such (now) well-known effects as the resistive wall instability, beam-beam interaction, and the degradation of vacuum due to beam radiation.

Incoherent beam-beam phenomena were first observed on the Stanford rings. Ernie Courant and I were involved in "understanding" the effect. I remember building a computer code, studying the effect computationally, and learning that a simple 1D model would not give an adequately low Δv (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. I don't think either of us published his work. Experimental observations of the effect were soon made in colliders around the world. Now-a-days study of the beam-beam effect is a large industry, as the incoherent beam-beam effect is *the* limit in modern colliders.

VII. 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 Ken Robinson and Matt Sands.¹² It was essential to have that understanding.

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.

And, as a second example, the understanding of radiation, and the freedom it allowed, led to the concept of separated function structures (now used in all machines and first incorporated into the second generation machine ADONE).

VIII. Low Beta

Motivated by the desire to make the Cambridge Electron Accelerator (CEA) into a high luminosity storage ring, Ken Robinson and Gus Voss invented the concept of low beta.¹³ The idea of squeezing the beam is obvious, but that one can do it at one point, and still have the storage ring give stable motion, was most un-obvious at the time. The concept was demonstrated on the CEA, and has by now become a vital part of all storage rings.

IX. Collective Behavior

Prior to the MURA work, no one thought that a stored beam could undergo collective motion. The concept of equilibrium conditions--space charge limits--were well-understood, but it was thought that these were the 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 publication⁶, to achieving stored beams of adequate intensity !

At first, none of us took Carl seriously, but soon we realized that there must be some intensity below which the instability does not happen and above which it does happen. I set to work to find that limit and it took me some months to do it, but on New Year's Day in 1959 (the very day Castro

took over in Cuba, I remember listening to the news as I worked) I derived the criteria for stable behavior of the negative mass instability (now generalized, and known as the Schnell-Keil criteria for the--renamed--microwave instability). Very similar work was done, independently, by Kolomenskij and Lebedev.¹⁴

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. In fact, it was a widely circulated joke that of all the instabilities, only the negative mass instability was predicted, all others were first observed and then "explained" by the theorists. The instabilities included the resistive wall, the head-tail effect, and coupled bunch phenomena.

Life was somewhat leisurely, in those days. For example, Kelvin Neil and I worked for about three years on the resistive wall instability. We didn't start out to analyze a resistive wall, but started with some observed behavior of the Stanford Rings, and we tried one thing after another, finally arriving at an effect that, at first, we thought would be negligible, but was, in fact, the correct explanation of the data. We were sufficiently interested in the work, even though it was of no particular interest to either Livermore or Berkeley, that we worked in the evenings at each other's homes.

In Frascati, when ADONE was first turned on, it was only able to store somewhat less than one percent of its design current. It was realized by Pellegrini and Amman that the effect was coherent and, at the same time, not the resistive wall instability. The instability was soon analyzed by Matt Sands and Claudio Pellegrini, and named by them as the "head-tail" instability for information is passed longitudinally through the bunch, although the coherent motion is primarily transverse. Removal of the improperly terminated clearing electrodes, in ADONE, soon brought the machine up to design.

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.¹⁵ Collective instabilities can be cured in principle, but in practice that is very difficult and usually not attempted. They tend to 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.

X. SPEAR and Doris, then PEP and PETRA, then LEP and HERA

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 now

be constructed with confidence. Thus one saw the progression of SPEAR, Doris, PEP, PETRA, and other rings. The physics that was done with these rings is much of the subject of this Conference.

The most recent stage of development is, of course, LEP and the electron-proton machine HERA. An electron-proton machine had been suggested, years ago, by the Americans (PEP originally stood for Positron-Electron-Proton), but no support for it was forthcoming in the US.

XI. Scattering Phenomena and Electron Cooling

Through the years quite an extensive study was made of background gas scattering of a stored beam.¹⁶ Of much more importance, and not predicted ahead of time, was intra-beam scattering.¹⁷ The so-called "Touschek Effect", which is intra-beam scattering leading to longitudinal loss of particles as they jump out of the RF bucket, prevented the earliest storage rings, AdA and ACO, from working very well. It provides a limit which must be carefully observed in all colliders.

The formalism for beam scattering was employed to analyze electron cooling, which was invented by Budker in 1966.¹⁸ We at MURA had long tried to think of ways in which to "beat Liouville", but our attempts all 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 moving electrons of very cold temperature, so that the interaction between protons and the cooled electrons would lead to a cooling of the protons. Subsequent experiments confirmed this fine idea, and although electron cooling never made a big impact on colliders, although it has been used, rather extensively, and very effectively, to make "cooler rings" for nuclear physics studies.¹⁹

XII. Stochastic Cooling

Early in the 70's Simon van der Meer realized that it was practical to "beat Liouville" by developing a Maxwell Demon. He proposed operating (with pickups and kickers) on individual particles (or a rather small number of particles, where the finiteness of the number is vital). Thus he invented stochastic cooling.²⁰ The main difficulty was technological; that is, the development of sufficiently sensitive pickups, good amplifiers, and excellent filters.

With development, stochastic cooling proved to be remarkably effective and thus allowed for the construction of proton-antiproton colliders. For these colliders cooling was essential, for the antiprotons are produced in a very warm state; i.e., with a density which was completely inadequate to give the desired luminosity. With cooling the energy spread was reduced by factor of

10^4 , while the transverse emittance was also reduced by large factors. It was this very powerful cooling that made proton-antiproton colliders possible.

XIII. The SPS, and then Fermilab and the SSC

With practical stochastic cooling in hand, and the background of knowing 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. The physics done with these machines is very much the central purpose of this Conference.

The next generation is back to proton-proton colliders (so as to obtain lots of luminosity, which can only be achieved in two rings), thus the SSC is under construction and the LHC is being seriously considered by CERN. Neither of these machines require cooling for their operation.

XIV. Conclusion

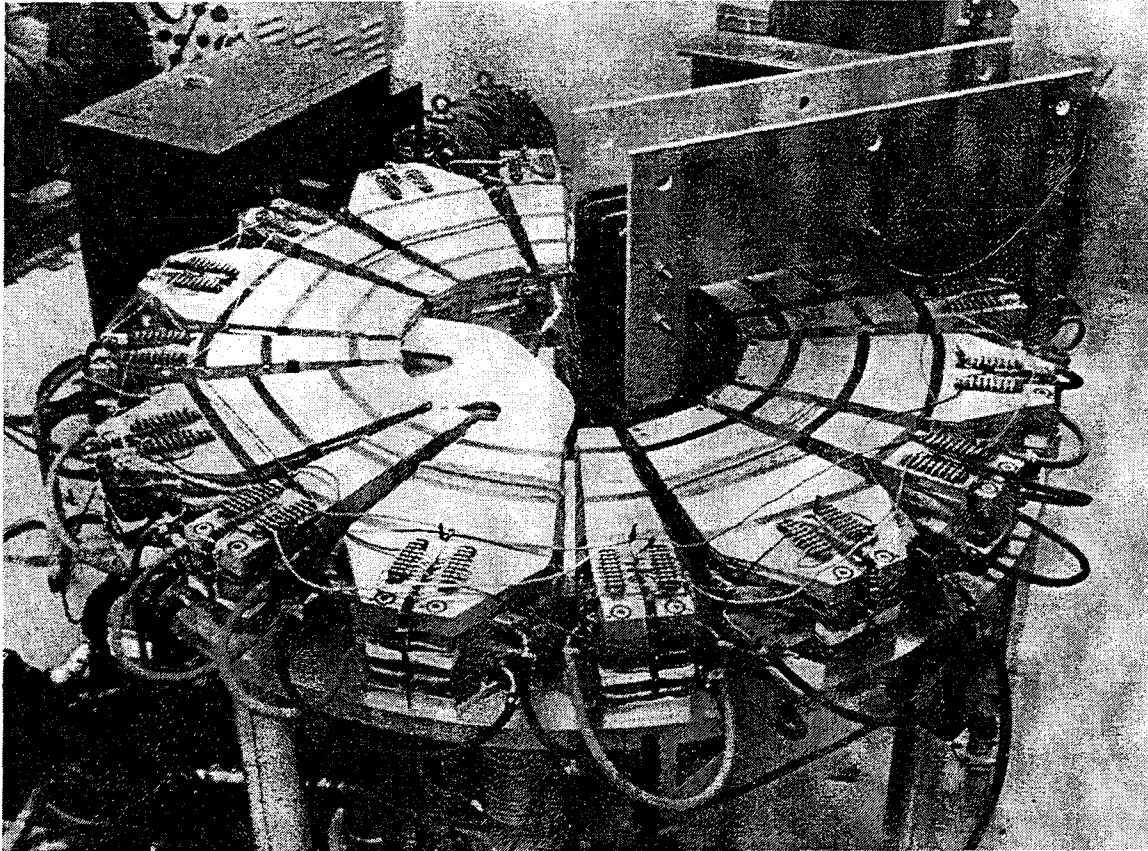
I hope there isn't a "conclusion" to the history of colliders. Surely linear colliders are in the future for electrons, but no-one can see far enough to even guess, for protons, what will come after the SSC.

But most important is the realization that just a few individuals, located at only seven institutions (who gave them financial and emotional support, and backed them for many hard years), by their efforts completely changed high-energy physics; we need to be sure that government agencies and laboratories, of the present and future, maintain this ability.

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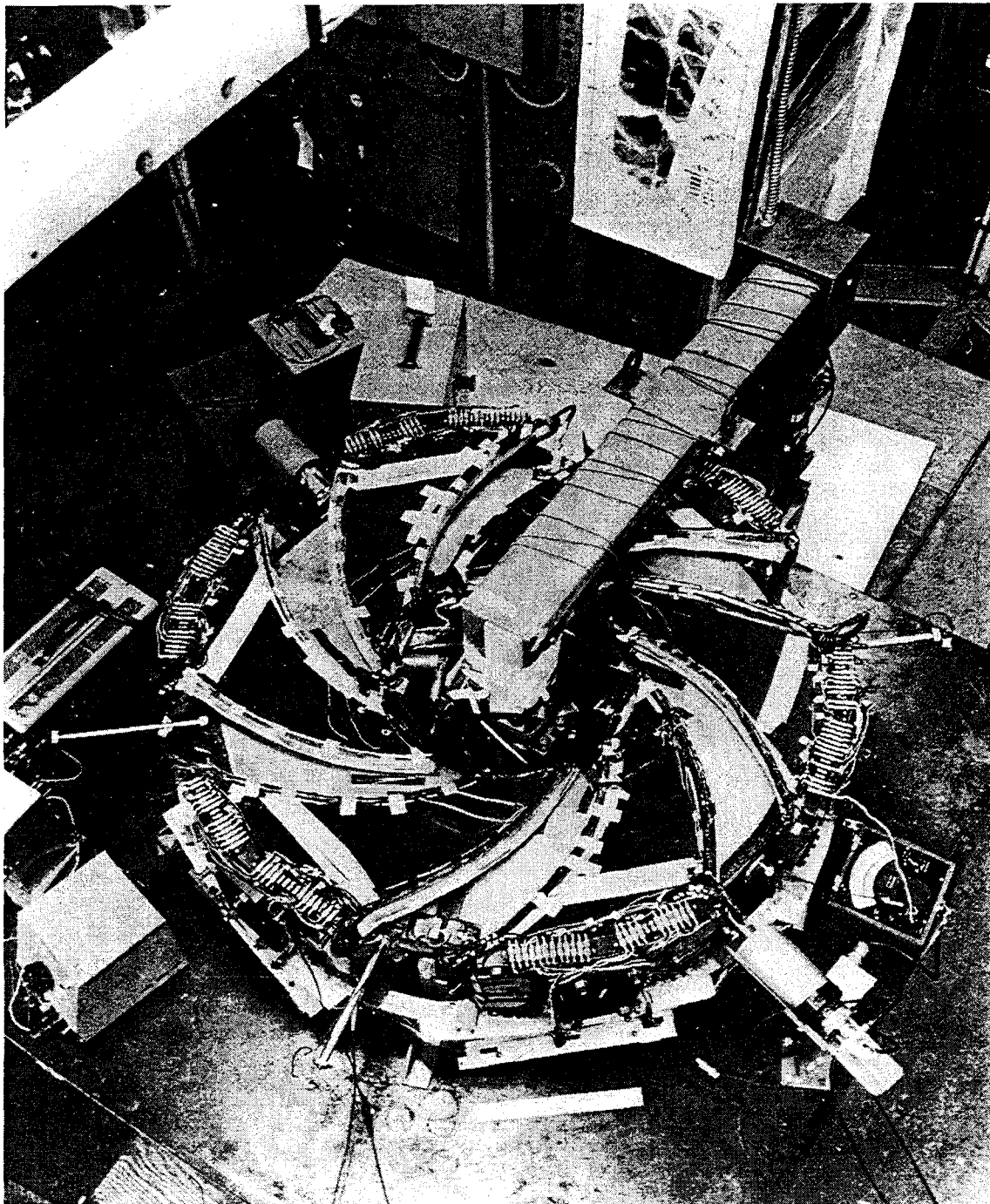
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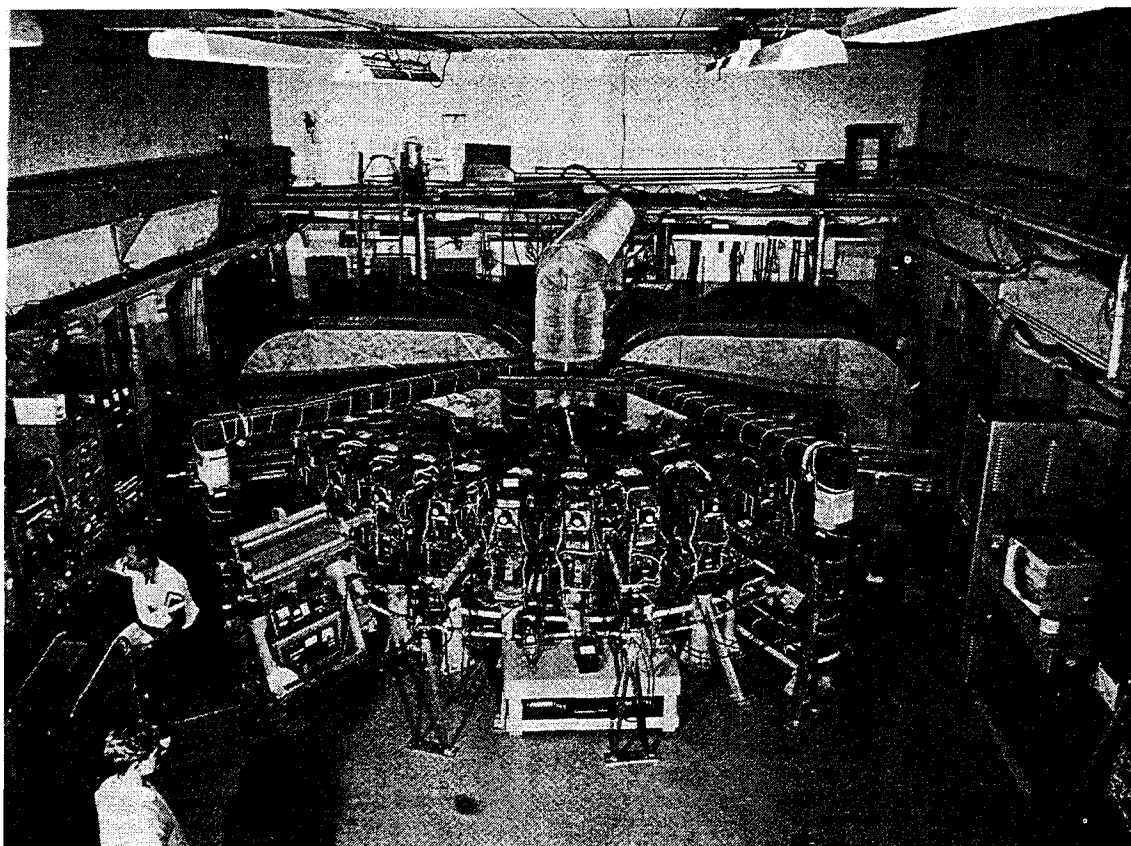
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Fig 1. The Mark 1 Model built by the MURA Group in Michigan in 1955. There are 8 sectors, and electrons of 30 keV were injected at a radius of 34 cm and accelerated, by betatron action (note the large core), to 400 keV at a radius of 50 cm. (Reprinted from L.W. Jones and K. Terwilliger, p. 359, Proc. of the CERN Symposium (1956).)



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Fig 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--one can see the large core, from 35 keV, at an injection radius of 31 cm, to 180 keV, at 52 cm radius. (Reprinted from R.O. Haxby et al, p. 75, Intern. Conf. on High-Energy Accelerators, CERN (1959).)



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Fig 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 10 A of electron were stacked. (Reprinted from *The MURA Staff*, p. 344, Intern. Conf. on High-Energy Accelerators, Brookhaven (1961).)

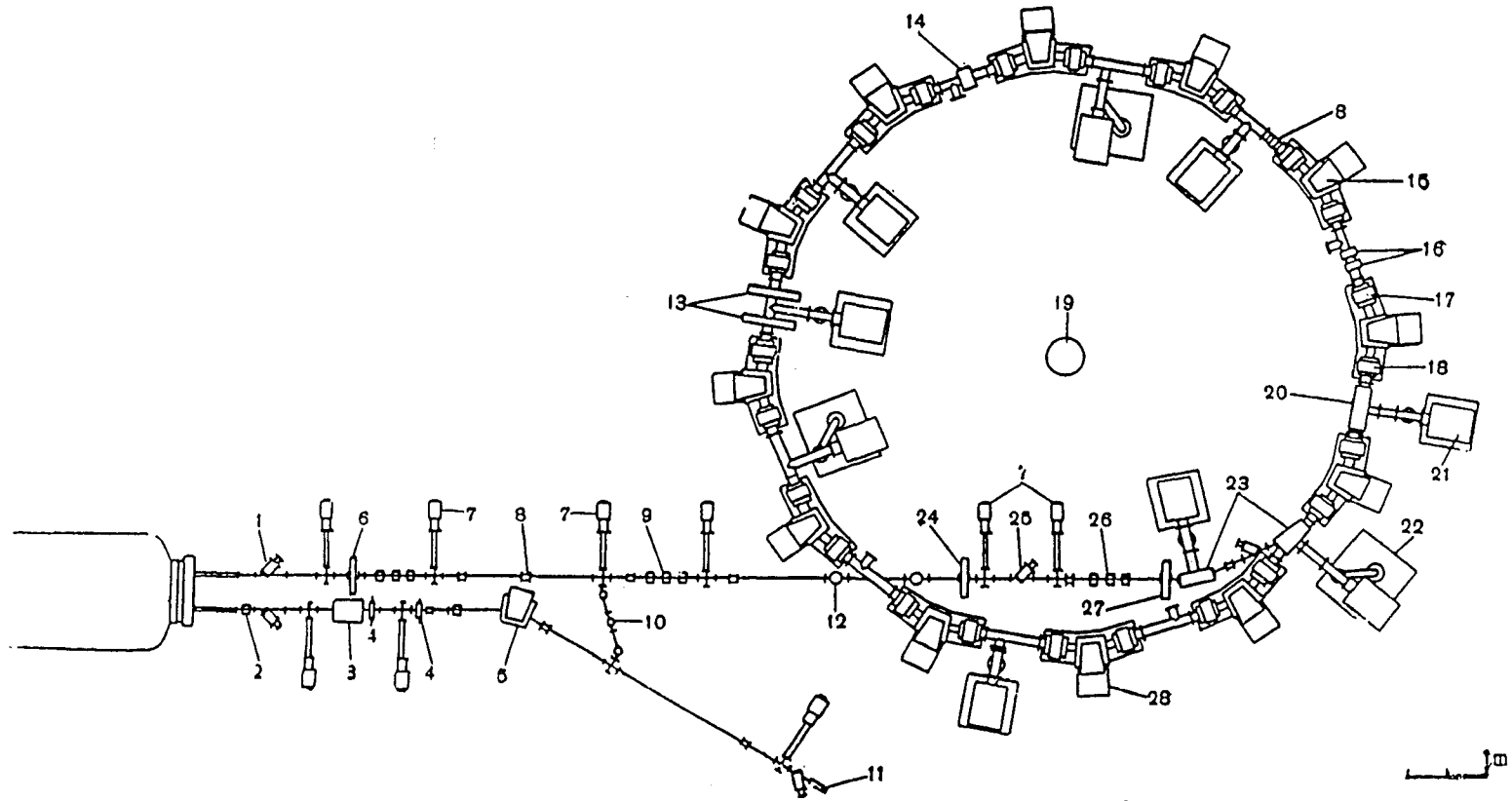
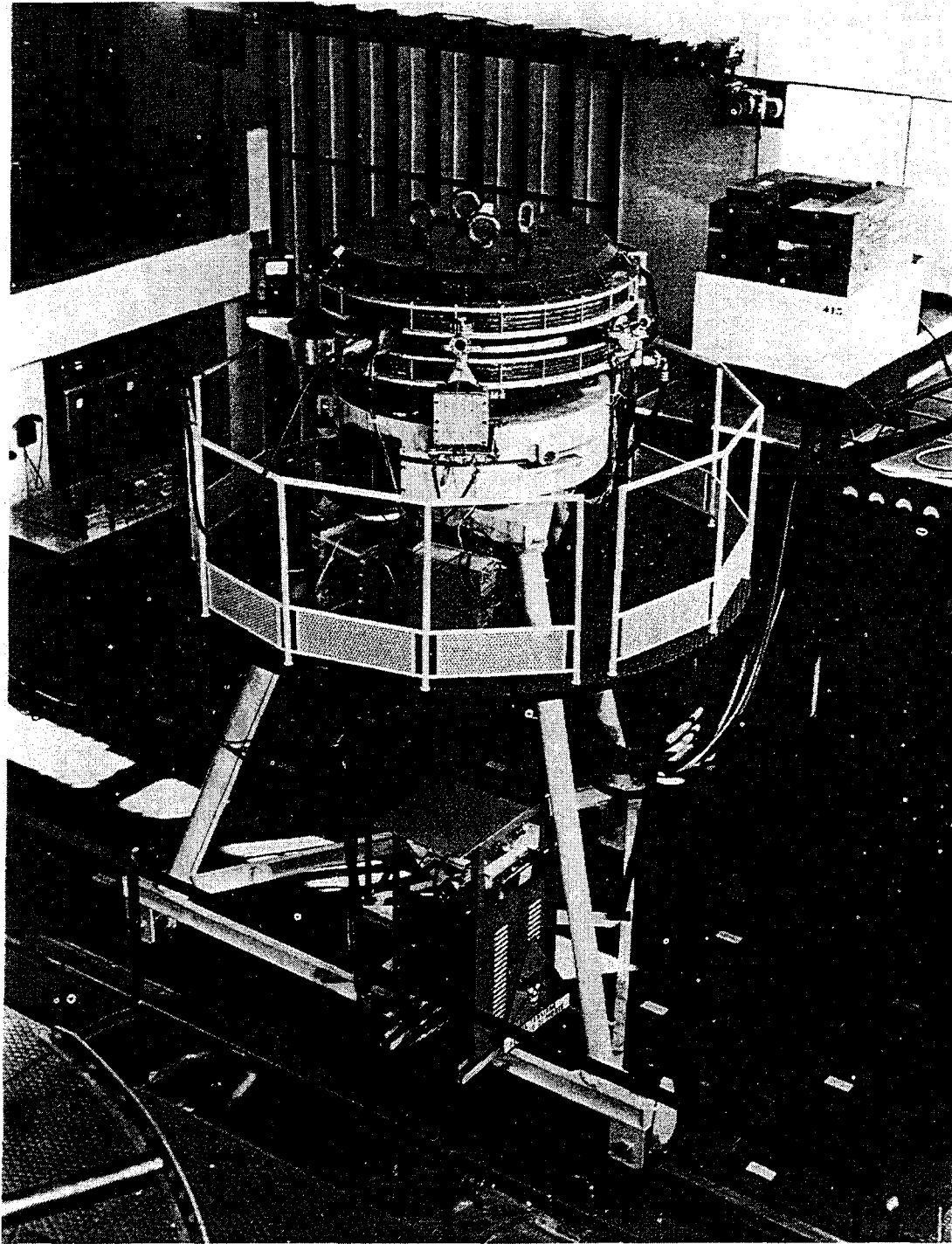
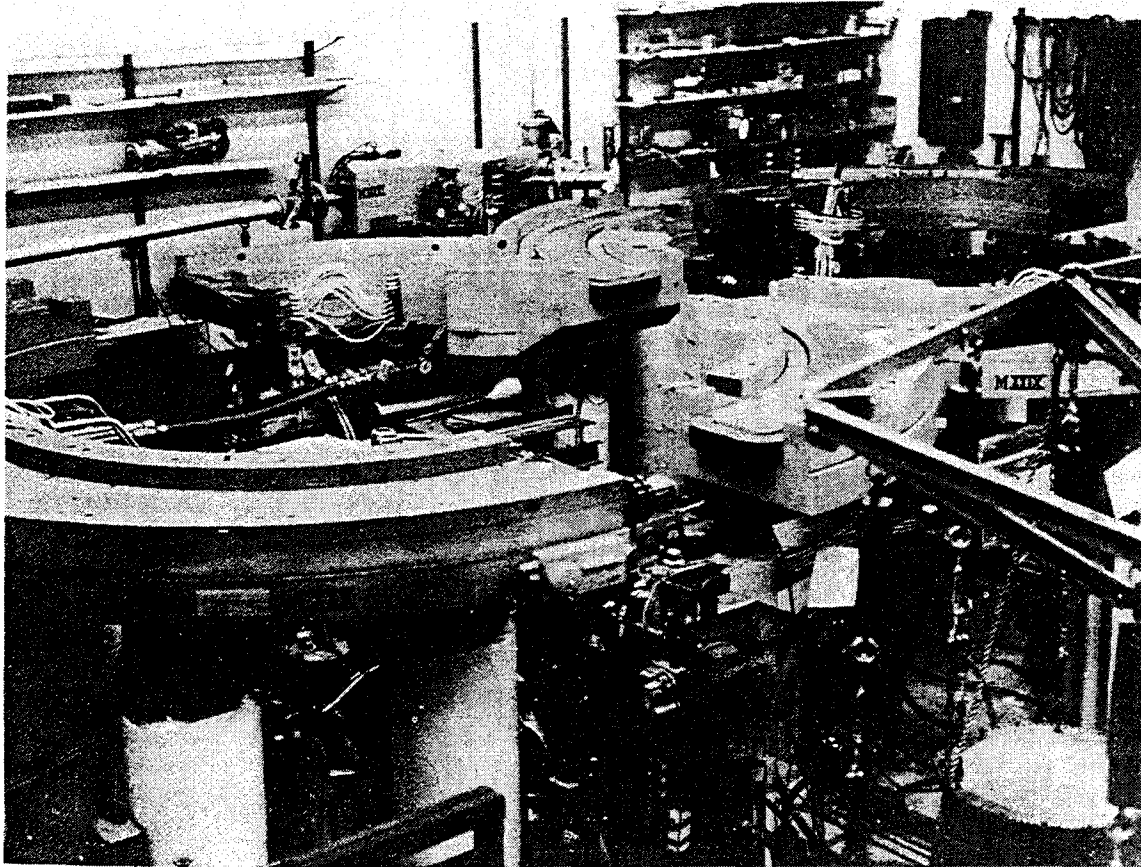


Fig 4. The CERN Electron Model for the ISR, initiated in 1960. The circumference is 24 m, including 12 straight sections each 1 m long. The electron energy could be 100 MeV, but 2 MeV was used for most studies. (Reprinted from F.A. Ferger et al, p. 417, Intern. Conf. on High-Energy Accelerators, DUBNA (1963).)



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Fig 5. 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. (Reprinted from C. Bernardini et al, p. 256, Intern. Conf. on High-Energy Accelerators, Brookhaven (1961).)



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Fig 6. 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 top electron energy was 500 MeV, the orbit radius 56 in. Although up to 1 A of a single beam could be stored, typical operation with colliding beams was with about 50 mA in each beam. (Reprinted from G.K. O'Neill, p.247, Intern. Conf. on High-Energy Accelerators, Brookhaven (1961).)