Physics in Perspective

AdA: The First Electron-Positron Collider

Carlo Bernardini*

I review the origin of AdA, the first electron-positron collider at Frascati, Italy, in the early 1960s. I describe the problems that were tackled to produce the positron beam, the vacuum, and the injection system that were necessary to observe the electron-positron beam-beam collisions. Accidents and incidents occurred, such as the unpredicted "Touschek effect," and were surmounted. I discuss the roles of the physicists involved in this work and the state of physics at the time, and I sketch subsequent work on larger collider rings. My recollections are based on the original literature and unpublished documents, photographs, and drawings.

Key words: Edoardo Amaldi; Fernando Amman; Gilberto Bernardini; Henri Bruck; Gianfranco Corazza; Giorgio Ghigo; Jacques Haïssinski; Pierre Marin; Enrico Persico; Giorgio Salvini; Albert Silverman; Bruno Touschek; Cerenkov counter; Frascati National Laboratories; Orsay Laboratory; Touschek effect; electron synchrotron; electron linac; electron-positron collider; electron-positron storage ring; beam luminosity; beam lifetime.

I dedicate this paper to some friends, now regrettably deceased, who carried out the most brilliant part of this work: Bruno Touschek, the leader of the group, Giorgio Ghigo, and Pierre Marin. Their versions of the events would have been much more enlightening than mine.

Initial Conditions

In 1958 the Italian National Institute for Nuclear Physics (Istituto Nazionale di Fisica Nucleare, INFN) was close to completing the construction of a large particle accelerator, a 1100-MeV (million-electron-volt) electron synchrotron, at the Frascati National Laboratories (Laboratori Nazionali di Frascati, LNF), about 25 kilometers south of Rome. This constituted the realization of an old dream of Enrico Fermi and his via Panisperna group in Rome before its dispersal in the late 1930s. ¹ Its strongest sup-

^{*} Carlo Bernardini is Professor of Physics at the University of Rome "la Sapienza." He worked in Frascati with the electron synchrotron and AdA and Adone.



Fig. 1. Edoardo Amaldi (1908–1989), *left*, and Gilberto Bernardini (1906–1995) at a conference in 1960. Courtesy of Department of Physics, University of Rome "la Sapienza."

porter, however, actually was Gilberto Bernardini (1906–1995),* who had worked in Florence on cosmic rays and had not been closely associated with Fermi's group. Since 1956 he had held the chair of spectroscopy in the Physics Institute at the University of Rome. There he (figure 1) and Edoardo Amaldi (1908–1989),** as well as Antonio

^{*} Gilberto Bernardini received his doctoral degree in Pisa in 1928, held a chair in Bologna from 1938–1946, was at Columbia University and the University of Illinois from 1949 to 1956, then returned to Rome and also worked at CERN in Geneva. He again held a chair in Bologna from 1961–1964 and then became director of the Scuola Normale Superiore in Pisa until 1977. Along with Bruno Rossi and Giuseppe Occhialini, he was one of Italy's foremost cosmic-ray physicists. Gilberto was my unforgettable teacher at the University of Rome when I was a student there in 1947–1948, but I am not related to him. I say this because I still am frequently asked, "Was Gilberto your father?"

^{**} Edoardo Amaldi worked in Enrico Fermi's group and received his doctoral degree in Rome in 1929 and a chair there in 1937. He was primarily responsible for the reconstruction of Italian physics after the second world war, and he was a strong and influential supporter of CERN in Geneva. In addition to his many contribution to physics, he also wrote many outstanding works on the history of physics.

Rostagni (1930–1988) in Padua,* Piero Caldirola (1914–1984) in Milan,** and Gleb Wataghin (1899–1986) in Turin*** planned the organization of the INFN (which was founded in 1955) with the help of Felice Ippolito (1915–1997),**** then Secretary General of the Nuclear Research National Committee (Comitato Nazionale Ricerche Nucleari, CNRN), an Adam's rib of the National Research Council (Consiglio Nazionale delle Ricerche, CNR). The Director of the new Frascati National Laboratories was a very young physicist from the University of Milan, Giorgio Salvini (b. 1920),**** who decided to assemble a staff by recruiting a select group of recent graduates in physics and engineering from many Italian universities but mainly from those in Milan, Pisa, and Rome. Soon most Italian universities with a physics department joined the INFN.

The idea was to build a national accelerator laboratory at a central site that would be staffed by physicists and engineers who were dedicated to the machine and could rely on the various INFN groups that were dispersed throughout Italy for the preparation of experiments. The staff was greatly assisted – in some cases trained – by outstanding physicists from Italy and abroad, especially by Enrico Persico (1900–1969),****** a friend of the young Enrico Fermi, who was in charge of accelerator theory; Mario Ageno (1915–1992), Fermi's last student in Rome, who was in charge of the design of the injector; and Italo Federico Quercia (1921–1987), who was in charge of organizing the many services required, particularly the electronics. They were assisted by Matthew Sands (b. 1919) and Robert L. Walker (b. 1919) from the California Institute of Technology,² and later by Boyce McDaniel (1917–2002), Albert Silverman (b. 1919), and Robert R. Wilson (1914–2000) from Cornell University. The coordination of

^{*} Antonio Rostagni worked on the interaction of radiation and matter in Germany and the United States until 1938 when he returned to Padua, working on nuclear emulsions and cosmic rays. He had many students and helped in the reconstruction of Italian physics after the second world war.

^{**} Piero Caldirola was a theoretical physicist who worked particularly on electron theory and statistical mechanics.

^{***} Gleb Vasilievich Wataghin was born in Birsuka in the Ukraine and came to Italy just prior to the Russian Revolution in 1917. He received his doctoral degree in Turin, where he worked on cosmic rays and field theory for most of his life. He also was one of the founders of the University of Sao Paulo in Brazil.

^{****} Felice Ippolito was a geologist and engineer from Naples where he received his doctoral degree and a chair later in 1940. He obtained a position in nuclear physics in 1955 and collaborated closely with Edoardo Amaldi in promoting nuclear research and nuclear-power plants in Italy. He was victim of political persecution in the years 1963–1970 and then was rehabilitated and returned to Naples and the University of Rome.

^{*****} Giorgio Salvini received his doctoral degree in Milan and then directed the Frascati National Laboratories (LNF) where he oversaw the construction of the 1000-MeV electron synchrotron. He also served as president of the Italian National Institute for Nuclear Physics (INFN). He worked on cosmic rays, the photo-production of mesons, and the detection of intermediate bosons (W and Z). He also served as Minister of Research in the Italian government.

^{******} Enrico Persico was a theoretical physicist and friend of Enrico Fermi. He held chairs in Florence and Turin and then in Quebec, and finally in Rome. He was one of the most prominent university professors in Italy.

these people and their work was undertaken by the INFN; it was excellent, and the planning, building, and exploitation of the Frascati National Laboratories served as a wonderful test of the efficiency of the INFN. Its success must be entirely ascribed, in my opinion, to Edoardo Amaldi, its president at that time; his great administrative skills were reminiscent of those of Orso Mario Corbino (1876–1937),* Fermi's sponsor and protector in Rome. Note that the Italian physicists working at Frascati came from cities in all parts of the peninsula, in some cases separated by more than 1000 kilometers, from Catania to Padua, from Bari to Turin. Nevertheless, the INFN's Director's Council acted more or less like a body of a single institute.

Significant events in physics followed one after another rapidly at this time: the discovery of the antiproton,³ developments in K^o physics,⁴ studies of the form factor of the proton,⁵ the discovery of non-conservation of parity⁶ – just to mention some of the most important ones. At the same time, theorists were producing phenomenological models that fit the difficult-to-interpret "low-energy data" by means of dispersion relations, the Pomeranchuk theorem and pomerons, Regge poles, bootstraps, and the like.⁷ There were some theorists at Frascati, since Salvini recognized the importance of having theorists working at the same place where experiments were being performed. At first, Giacomo Morpurgo worked there but did not communicate very much with the experimentalists; then Raul Gatto spent some time there, discussing various problems with the experimentalists and distributing calculations on topics on which his students were working. Meanwhile, Edoardo Amaldi, a true "talent scout," had remembered someone who had visited Rome in 1938: Bruno Touschek (1920–1978), a brilliant Austrian theoretician who had survived a difficult time under the Nazis. Amaldi now offered him a position in Rome, which Touschek (figure 2) accepted. At first, Bruno collaborated with Luigi Radicati and Giacomo Morpurgo on fundamental problems, particularly on time reversal and weak interactions, then with Marcello Cini on weak interactions. Bruno did not like much of the above theoretical machinery (dispersion relations, Regge poles, and so on), but he regarded as quite important the problem of analyticity of form factors and their analytical continuation to time-like values of squared-momentum transfer.

In any case, electron accelerators were not highly regarded because of the "softness" of the electromagnetic interactions, although quite a few electron synchrotrons and electron linear accelerators had been built. Proton accelerators were the stars of nuclear laboratories, and for these new concepts were entering the arena: The strong-focusing method was attracting designers, so that new proton synchrotrons were being proposed. In Europe, CERN in Geneva was attracting high-energy physicists, including many Italians. Nevertheless, thanks to Salvini's determination, he wisely continued to develop its weak-focusing electron synchrotron at Frascati, just changing it so that electrons were injected not with the 1.5-MeV Cockcroft-Walton accelerator then in use but at a higher energy and safer magnetic field (smaller field errors) with a 2.5-MeV Van de Graaff accelerator. All together, the international community of physicists

^{*} Orso Mario Corbino also was a prominent politician during the Fascist period, but not a member of the Fascist Party.



Fig. 2. Bruno Touschek (1920–1978), *left*, and Edoardo Amaldi (1908–1989) at a meeting around 1960. Courtesy of Department of Physics, University of Rome "la Sapienza."

working with high-energy electron accelerators was formed by groups using electron linear accelerators at Stanford in the United States and at Orsay in France, and those using electron synchrotrons at Caltech and Cornell in the United States, at Bonn in Germany, at Lund in Sweden, and at Frascati in Italy. The Cornell group was the only one that was tempted by the strong-focusing method and decided to convert its machine to use it; they succeeded after overcoming some minor difficulties. At Frascati our attitude was much more conservative; we were afraid to waste time and money.

Theory was greatly influenced by the development of quantum electrodynamics (QED), a highly successful theory that had served as a prototype for Fermi's theory of weak interactions and worked in the lowest-order perturbative approximation because of the smallness of the relevant coupling constant, the fine-structure constant. Some people speculated on the possible breakdown of QED¹⁰ and looked for it in high-precision measurements, such as those of the gyromagnetic ratio *g*-2,¹¹ and in electron-collision experiments. In the latter case, physicists generally believed that the breakdown might occur at some very high energy (or better, momentum-squared transfer) characterized by a length (or mass) cutoff occurring in the modified electron-photon vertex or in the electron or photon propagators. The most naïve proposal was that of the so-called "heavy-electron," *e**, which was supposed to decay into an electron and gamma

ray, a decay mode that already had been shown by Giuseppe Fidecaro and his collaborators to be forbidden for the mu meson. 12

A New Concept in Accelerator Physics

The idea of exploring collisions in the center-of-mass system to fully exploit the energy of the accelerated particles had been given serious consideration by the Norwegian engineer and inventor Rolf Wideröe (1902-1992), who had constructed a 15-MeV betatron in Oslo and had patented the idea in 1943 after considering the kinematic advantage of keeping the center of mass at rest to produce larger momentum transfers.* This idea was also taken seriously by a Princeton-Stanford group that included William C. Barber, Bernard Gittelman, Gerry O'Neill, and Burton Richter, who in 1959, following a suggestion of Gerry O'Neill in 1956, proposed to build a couple of tangent rings to study Møller scattering.¹³ Andrei Mihailovich Budker (1918–1977) initiated a somewhat similar project at Novosibirsk, where the VEP-1 electron-electron collider** was under construction in 1961.¹⁴ Donald W. Kerst (1911–1993), who had constructed the first successful betatron at the University of Illinois in 1940, also was considering colliders, particularly for protons, in 1959 using the Fixed-Field Alternating-Gradient (FFAG) concept;¹⁵ his contributions to accelerator physics and technology as technical director of the Midwest Universities Research Association (MURA) while at the University of Wisconsin in Madison during this period were of much interest to the accelerator community.¹⁶

The attention of these people was apparently focused more on the kinematic advantage of colliding beams than on the new physics to be learned from them. To achieve head-on collisions between accelerated particles in flight required storing them in magnetic devices (storage rings) to allow them to collide repeatedly as they crossed at various points in their circular orbits. I find in my notes, dated February 24, 1958, a sketch of a circular magnetic bottle I called the "Storion," which means that we had discussed the storage problem then (I now cannot remember precisely how or why). We had given no deep consideration, however, to the most convenient experiments to carry out with colliding beams.

That was precisely Bruno Touschek's starting point at Frascati. He considered the kinematics as rather obvious; to him the possible physics to be learned from colliding particles was far more significant. He had a very strong picture of the microscopic world in his mind. He conceived the vacuum as a reactive dielectric resonating at frequencies $v = mc^2/h$, where c is the speed of light, h is Planck's constant, and m in this case is the mass of a boson homologous to the photon, that is, a neutral vector meson.

^{*} The kinematic advantage is that no kinetic energy is wasted by the motion of the center of mass of the system; all of the energy imparted to the accelerated particles goes into the reaction products in the center-of-mass system. Thus, the energy $E_{\rm L}$ required by a particle in the laboratory system to produce a given reaction by a collision with a particle of energy E and mass m at rest in the center-of-mass system is, in the extreme relativistic case, $E_{\rm L} = 2E^2/m$.

^{**} VEP is the Russian acronym for electron-electron collider.

Some people at this time were speculating on the existence of such mesons. I was corresponding, for instance, with Yoichiro Nambu in Chicago, who had suggested searching for such particles as plausible intermediaries of the strong interactions;¹⁷ the concept of "vector dominance" was close at hand. We eventually performed an experiment – the first one carried out with the Frascati electron synchrotron – searching for neutral rho mesons, following a simple brilliant suggestion by Albert Silverman.¹⁸ We found an upper limit of the cross section for the photo-production of a rho meson for a range of masses lower (as it turned out) than its actual mass.

Bruno's view, as he put it, was that a physical system can be characterized appropriately by investigating its "geometry" and its "dynamics." Its geometry, its size and shape, is observable by employing space-like photons as in electron-proton scattering experiments ("diffraction of electron waves"); this was precisely what Robert Hofstadter was doing at Stanford using the Mark III Linac to measure form factors of nuclear particles.¹⁹ No one, however, had as yet observed the dynamics; for this one needed to produce time-like photons of sufficiently large energy to excite resonant modes of the vacuum corresponding to the masses of the vector mesons. Thus, Touschek concluded, we should make electrons and positrons collide and annihilate in the center-of-mass system to produce time-like photons (in the dominant one-photon channel where the small value of the fine-structure constant helps). Bruno gave a seminar on March 7, 1960, at Frascati in which he guaranteed that an electron and a positron necessarily meet in a single orbit because QED is CP (charge-parity) invariant. His skeptical colleagues did not have the courage to doubt him. His seminar was attended by many people, among them Salvini, Fernando Amman (who a couple of years later would be in charge of the Adone collider), and Raul Gatto, who with Nicola Cabibbo immediately began to investigate all possible electron-positron reaction cross sections and derived formulas describing the relevant parameters,²⁰ particularly in hadronic physics. The coupling constants and form factors in the time-like region were unknown, but the question was asked: Will many or few hadrons be produced? This all fit quite naturally into Touschek's picture of the dielectric; he even expected to observe not only the absorptive part of the dielectric constant but also the dispersive part, thanks to an interference term at resonance, as was actually observed later at Orsay. 21

The Birth of AdA

Touschek, in his peculiar style, tried to convince Salvini to immediately convert the Frascati electron synchrotron into a collider ring (as actually was done ten years later for the Cambridge Electron Synchrotron). Salvini (figure 3) wisely refused to do so: The Frascati electron synchrotron was unfit for this purpose, and many experiments had already been performed or scheduled for it. Salvini however warmly agreed with the proposal to prepare a new machine. We therefore immediately constituted a small group of people to investigate the most pressing problems that would have to be addressed to build an electron-positron collider ring *ex novo*. We were terribly excited; we had the impression that a new era in accelerator physics was beginning, although we did not dream that colliders would become a unique tool in many high-energy lab-



Fig. 3. Giorgio Salvini (b. 1920) making music around 1970. Courtesy of Giorgio Salvini.

oratories in the near future. We immediately baptized our collider as AdA, the acronym for the Italian *Anello di Accumulazione*,* since we considered the beam-storage (*accumulazione*) problem to be the most serious one. The original group consisted of Gianfranco Corazza (b. 1924, figure 4), Giorgio Ghigo (1929–1968, figure 5), Bruno Touschek, and myself (figure 6). We had the complete support of the LNF service staff, particularly of Mario Puglisi for making the RF (radiofrequency) instrumentation and of Giancarlo Sacerdoti for making the magnet. We agreed that the energy of the electron and positron beam should be 250 MeV, which was a reasonable amount higher than the threshold for producing a positive and negative pion pair.

We then had to fix our ideas on everything that was required to get acceptable physical results. Our list of priorities included:

^{*} Bruno told me that he had an aunt named Ada who was living in Rome when he visited Rome for the first time in 1938 and for a short time studied mathematics there. On her death, she had left Bruno a little Austro-Italian firm, Garvens, that produced water pumps. Bruno then personally administered Garvens, not with great success. When we got the first stored electrons and positrons with AdA, on February 27, 1961, this was just on the anniversary of his aunt Ada's death. Bruno told everyone about this coincidence.



Fig. 4. Gianfranco Corazza (b. 1924) working on the electron synchrotron donut around 1958. Courtesy of Gianfranco Corazza.

- The evaluation of the "source factor," which from then on was called the "luminosity." (I believe, but am not certain, that Bruno was the first to use this term. We have a little notebook in which, under the date of February 1960, there is a luminosity formula handwritten by Bruno on its very first page.) This is the factor multiplying the electron-positron annihilation cross section that relates the number of stored particles, beam geometry, and time characteristics to the number of annihilation events.
- Analysis of the beam lifetime and of the processes that might influence it. We recognized immediately that the scattering of the electrons and positrons by the residual gas in the collider ring was the main factor responsible for beam particle losses, so that we had to devise means for producing a high vacuum.
- Injection of electrons and positrons. This was a serious problem that we had to consider (see below).
- Design of the magnet to achieve compactness and to leave enough room to allow access to the electron and positron beams, and design of the RF cavity to compensate for the synchrotron-radiation losses.

Giorgio Ghigo attacked this last problem in a couple of days with the help of the laboratory shops. The magnet had a sandwich structure with grooved iron discs above and



Fig. 5. Giorgio Ghigo (1929–1968) in 1964. Courtesy of Giorgio Ghigo's family.

below the evacuated donut in which the electrons and positrons orbited. There also were two large gaps ("quasi-free sections") between the discs to house the RF cavity in one and the detector of the annihilation products (if any) in the other one. The current-carrying coils were wound around the middle of the magnet, giving it the appearance of a toy yo-yo. The RF cavity was silvered on the inside to reduce resistive losses and was asymmetric, having a race-track section with the evacuated donut traversing it sideways. Mario Puglisi, Antonio Massarotti, and Dino Fabiani helped to design the RF cavity with the correct frequency (147.2 ± 0.1 megahertz). The general design was ready in just a few days, and an order for the magnet was placed with Ansaldo, a well-known firm in Genoa – this was expedited by the joint action of Edoardo Amaldi, Felice Ippolito, and Giorgio Salvini on the CNRN administrative authorities. (Today, I think that to get the same response from the bureaucracy would take around two years.)

The problem of how to inject electrons and positrons was addressed somewhat less brilliantly – by Bruno Touschek and myself. We excluded the use of an electromagnetic injector both because there was no room for it (the vacuum in the donut might become worse if complex devices were inserted into it), and because no one in the world had as yet produced an optically sharp positron beam. The injection problem actually was soon addressed better elsewhere, because it was placed in the hands of



Fig. 6. The author (b. 1930) in the early 1960s.

"professional accelerator people," whereas we were just "physicists in a hurry" who were anxious to test the scientific feasibility of an electron-positron storage ring. We therefore decided to opt for a simple metallic converter (a thin tantalum target) of bremsstrahlung gamma rays into electron-positron pairs, placing the converter inside the evacuated donut (figure 7); we assumed that dissipative processes, such as synchrotron-radiation losses, would help a small fraction of the electrons and positrons to spiral away from the converter into their circular orbits. We knew that the useful phase space available to them was rather exiguous; nevertheless, we decided to try this technique. Many discussions occurred as to which were the electrons and which were the positrons; they ended when Bruno drew a famous cartoon underlining the inconclusive debate (figure 8).

The first injection trials were carried out with a single beam in the collider ring by irradiating the internal-converter target when the ring was installed far away from the electron synchrotron on a tower (tripod) of the proper height on rails. Later, the problem of "inverting the magnetic field" to switch from electrons to positrons, and *vice versa*, was solved naïvely at first by mounting the magnet on a support that rotated around a horizontal axis – we called it the "roasting spit." This was a unique feature of our collider ring, which we later abandoned (see below). We called this rudimentary

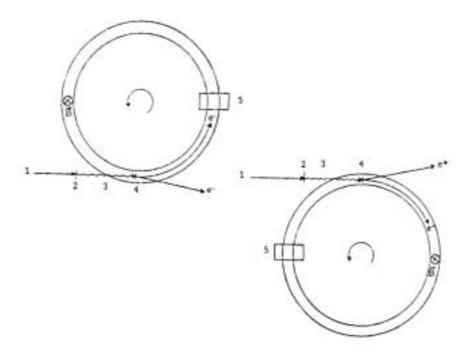


Fig. 7. The AdA collider ring. With the magnetic field (B) directed downward (into the page) as indicated, electrons are injected into the ring when it is in the position shown at the left. Thus, the electron beam from the electron synchrotron (1) strikes an external target (2), producing bremsstrahlung gamma-rays that enter the collider ring (3) and strike the tantalum internal-converter (4), producing electrons that orbit counter-clockwise and pass through the RF cavity (5). The ring is then translated and rotated 180° as shown on the right. Positrons are then produced as above and orbit clockwise as shown, colliding with the oppositely orbiting electrons in the ring. Drawing courtesy of Jacques Haïssinski.

injection device, which as I said produced electron-positron pairs in an internal-converter target, a "stochastic injector," since it worked mainly because the betatron oscillations helped the spiraling away of the electrons and positrons from the converter owing to radiation-energy losses by increasing the time that elapsed before they returned in their orbits to it. We did a lot of calculations trying to estimate the injection efficiency; most of them, however, were quite unreliable because of their sensitivity to imperfectly-known magnetic parameters.

Regarding the vacuum, Gianfranco Corazza (a true leader in the field of vacuum technology) was already working with the wonderful titanium ionization pumps. Thus, the prototype of the evacuated donut already reached a pressure of 10^{-7} torr with no special treatment of its internal walls. Anticipating that the first trials of the assembled machine would take place as early as February 1961 – just a little less than a year after Touschek's seminar – we found that the first electrons and positrons stored in the ring

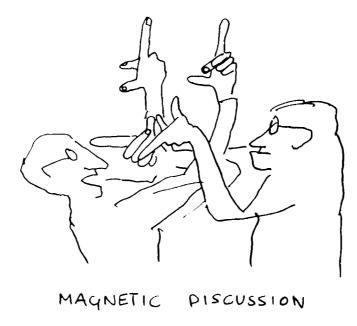


Fig. 8. "Nobody was able to say which were the electrons and which were the positrons." Cartoon by Bruno Touschek. Courtesy of Department of Physics, University of Rome "la Sapienza."

lived for tens of seconds. After that, their lifetime increased continuously. We installed in the gap between the upper and lower discs of the magnet a much better inox steel donut (1.2 millimeters thick) – a torus with an elliptical cross section of 9×3.8 centimeters whose long axis was horizontal and which had two portholes in it to view the visible portion of the radiation spectrum. We also installed in the gap a titanium vacuum pump (which pumped at the rate of 75 liters per second) with an Alpert gauge; the internal converter for the injection of electrons and positrons; and the RF cavity. With this final configuration, the vacuum easily reached a pressure of 10^{-9} torr; actually, when we sometimes used a special, thoroughly cleaned donut chamber, the pressure went down to less than 10^{-10} torr, perhaps 10^{-11} torr, thus reaching the limit of sensitivity of the Alpert gauge. (This prompted Corazza and myself to have some fun by inventing a crazy vacuometer, a Müller microscope with an inverted polarity;* unfor-

^{*} The Müller microscope consisted of a metal tip that was maintained at a potential of around 5000 volts with respect to a flourescent screen, so that electrons extracted from the tip followed divergent trajectories to the screen and thus amplified the images of the (usually organic) molecules deposited on the tip. We thought that by inverting the polarity, neutral atoms in the residual gas would be attracted by a polarization force to the tip, thus amplifying the electron current and providing a measure of the pressure of the residual gas. For a description of this microscope, see Erwin W. Müller, "Field Ionization and Field Ion Microscopy," *Advances in Electronics and Electron Physics* 13 (1960), 83–179, especially 114–143.



Fig. 9. AdA on the tripod tower at Frascati. Courtesy of Frascati National Laboratories.

tunately, the electrical stresses that were set up by the high electric field (5 kilovolts per micron or 5 gigavolts per meter!) at its metal tip – a needle from my old gramophone – caused it to explode too frequently.) We eventually stored feeble beams of electrons and positrons that lived up to 40 hours. This was the first important success and the main result we obtained at Frascati with AdA using the electron synchrotron to produce bremsstrahlung gamma rays for AdA's electron-positron injector. It took a little more than a year to achieve this result, and it helped to silence some of our remaining critics.

Actually, the injection problem was our sore point. In our first tests we used the tripod device noted above, but we soon abandoned this in favor of the roasting spit because we could position it closer to the electron synchrotron, so the geometry was better. The bremsstrahlung gamma-ray beam produced with electrons from the synchrotron now was sent to the collider ring installed about 4 meters above the floor on a tower that moved on rails (figure 9). Because of overcrowding in the laboratory, how-

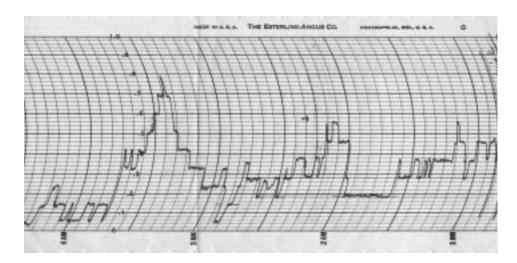


Fig. 10. The phototube record of February 1961 showing steps that correspond to single electrons entering or leaving AdA. Personal collection of the author.

ever, the electron synchrotron was still quite far away from the internal electron-positron converter in AdA (nearly 10 meters, so far as I can remember), so that the synchrotron gamma rays were spread out over a couple of milliradians, producing a spot on the internal electron-positron converter in AdA of more than 1 centimeter in radius. Its efficient part was only on the order of few microns in radius, however, because the pitch of the electrons and positrons spiraling away from it, owing to radiation losses, was on this order. We therefore were able to inject only a few tens of electrons per second in this configuration. Nevertheless, this was sufficient to monitor the intensity of the stored beam: We saw the light signal from a single electron after its capture in the ring as a pulse in the phototube output (figure 10), which we used to calibrate it as a function of the number of electrons or positrons, that is, of the intensity of the stored beam. We then attenuated the light signal either by using opaque filters (a technique we abandoned because radiation damage was darkening the filters in an unpredictable way) or by reducing the voltage on the phototube. We recorded up to 108 stored particles. Our calibration procedure thus worked very well indeed.

Touschek kept himself fully informed on all details of these technical aspects of our work-in-progress with AdA, but his main concern, as always, was the physics. To get physical results, the oppositely orbiting electron and positron beams had to meet and overlap completely. Touschek thus was fascinated with the luminosity formula, which actually followed from a classical calculation of the "quality" of the operating ring, something like the duty cycle of an engine. To the question: "How can you be sure that electrons and positrons will meet?" he answered: "Obviously, TCP [time-charge-parity] theorem! Actually, CP is enough!" Another question sometimes was: "Will electromagnetic interactions with the walls of the donut separate the beams?" Bruno's answer: "Scheisse!" And so on.

AdA Moves to Orsay

On February 27, 1961, just less than a year after Touschek's seminar, we got the first stored electrons and positrons. The phototube registered pulses, and to our surprise even a single electron was visible to the naked eye through one of the portholes. A common joke was to store a few electrons and astonish distinguished visitors, among whom were Edoardo Amaldi, Philip Ivor Dee from Glasgow (a former student of Ernest Rutherford and a good friend of Bruno), Wolfgang Paul from Bonn, Guy von Dardel from Lund, Boyce McDaniel and Albert Silverman from Cornell, and Matthew Sands and Robert Walker from Caltech. There was enough light to take a Polaroid photograph of a single electron: Although its intensity per orbit was quite small, it passed the porthole 75 million times per second! We estimated that the small blue-white spot we saw was nearly equivalent to the light from a star like the Sun five light-years away from us.

More problems with injection occurred after the first trials. Both the geometry of the configuration and the duty cycle of the synchrotron were unfavorable. As in our second generation of trials, to help the geometry we installed the roasting spit as close as possible to the synchrotron. Then a new and unpredicted phenomenon occurred. When we switched from electrons to positrons by rotating the magnet about its horizontal axis, the first stored beam sometimes was suddenly lost. This inconvenience did not occur frequently, but it was annoying and difficult to interpret. We solved the puzzle after we moved AdA to Orsay, France, in early 1963. There, by sheer chance, Bruno and Pierre Marin (1927-2002), who had been in Frascati as a visitor when the first injection trials were made (and who soon became one of the most far-seeing supporters of collider development), were looking through the porthole in the donut searching for the malefic elfs who were destroying the beam – and found them in the form of fluttering diamagnetic dust left over from the welding of the donut and moving under gravity along the magnetic field lines and passing through the beam. Corazza immediately invented a new support, a turntable on rails (figure 11), for positioning AdA close to the Orsay electron linac, and built it with the help of Antonio Marra. The roasting spit thus was replaced by a mechanical device for producing two orthogonal motions, one about a vertical axis, the other a horizontal translation on the rails. It worked, Bruno told visitors, for "obvious group-theoretic reasons."

The difficulties in using a well-collimated bremsstrahlung gamma-ray beam to produce electrons and positrons in AdA was the subject of widespread bad humor at Frascati, but they favored the rapid acceptance of Pierre Marin's proposal, on behalf of André Blanc Lapierre, the Director of the Orsay Laboratory, to move Ada to Orsay, where their electron linac could be positioned very close to AdA's evacuated donut and internal converter. Bruno and I agreed with Marin's proposal, and we easily convinced Salvini and Amaldi to accept it. We therefore immediately organized the transportation of AdA to Orsay on a truck in the first half of June 1962.

This was an amazing undertaking. We wanted to arrive at Orsay with the donut under vacuum so that we would not have to repeat the long cleaning procedure of its internal walls and thus save nearly three weeks of pumping time to reach a pressure of 10^{-9} torr. The titanium vacuum pump therefore had to be powered throughout the

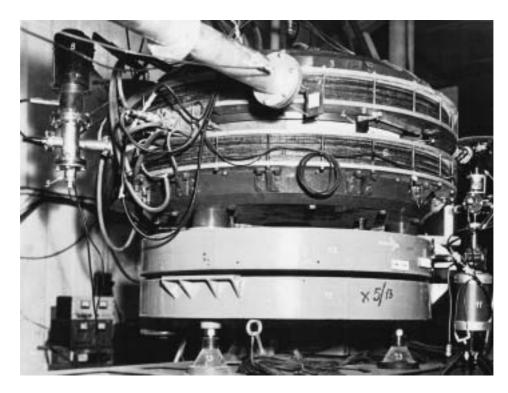


Fig. 11. AdA on the rotating and translating platform at Orsay. The injector beam channel is visible on the left. Courtesy of Jacques Haïssinski.

1500-kilometer trip. We used batteries to feed its power supply, which worked well – the pressure never rose above 10⁻⁸ torr. But we had one accident and one incident, luckily both with happy endings. Bruno wanted to "personally" test the stability of the truck carrying AdA – which weighed nearly ten tons – and he knocked down a street lamp because of his inexperience in driving such a large vehicle. Then, when the truck arrived at the Italian-French border, the driver phoned us very excitedly because the customs officers wanted "to inspect the inside of the donut." Thanks to the Italian Minister of Foreign Affairs, who became alarmed by a call from Felice Ippolito, we convinced the customs officers that the donut contained an unprecedented high vacuum. Actually, we were afraid that the batteries for the power supply might run down or fail if the truck were forced to stop at the border while prolonged and laborious official negotiations took place.

Meanwhile, since 1961 new colleagues had joined the Frascati group working on AdA: first Ruggero (Bibi) Querzoli, then Giuseppe Di Giugno, and for a short time, Ubaldo Bizzarri. French colleagues also joined the group in the summer of 1962: Pierre Marin, of course, and François Lacoste (the son of the tennis champion and manufacturer of the famous shirts). Lacoste was a talented guy but decided to leave the group in less than a year to work on satellites. He was replaced, very well indeed, by Jacques



Fig. 12. Jacques Haïssinski (b. 1940), *left*, and Pierre Marin (1927–2002) talking around 1970. Courtesy of Jacques Haïssinski.

Haïssinski (b. 1940, figure 12). Then, after our move to Orsay, we also drew on the expertise of other people, including Henri Bruck, an accelerator theorist from Saclay who was excited about AdA, and Boris Millman, a former nuclear physicist who was the deputy director of the Orsay laboratory. Roger Belbeoch, Henri Zyngier, Jean-Eudes Augustin, Gabriel Gendreau, and Gerard Leleux, most of whom were Bruck's collaborators, were frequently with us later but were already working on the Orsay collider ring (*Anneau de Collision Orsay*, ACO). Louis Burnod also helped us very much in operating the electron linac at Orsay. Our French friends were rather different in character from us; they had many important virtues that we lacked. They were very systematic, orderly, and precise, whereas we were not, following somewhat in the "creative" style of Touschek. Nevertheless, there was widespread and mutual appreciation of the benefits of both styles. Our collaboration was nearly ideal because we complemented each other.

The duty cycle of the Orsay electron linac corresponded to nanosecond pulses of electrons at its operating frequency, which was very inconvenient for coincidence experiments, but was excellent for our purposes with AdA. Its electron current was enormous – millicoulombs per hour – compared to that of the Frascati electron syn-

chrotron. I had the idea to modulate the peak voltage of the RF cavity in AdA – I argued that reducing it for a short time would not affect the stored beam but would help the electrons and positrons entering the donut to spiral away from the internal converter. This idea, which we implemented with the help of Puglisi's group in Frascati, resulted in an increase in the injection rate by a factor of 25.

To indicate how "unnatural" such a modulation process was, I mention that I had an intense discussion with Maury Tigner during a seminar I gave at Cornell in 1963, because he considered it to be much more "natural" to increase the peak voltage during injection, thinking that this would "help capture" the electrons and positrons; his argument was based simply on "brute force." Actually, the electrons and positrons spiral away from the internal converter faster when a smaller energy is supplied to them by the RF cavity. This was evident to me from the comet-shaped phase-space plot of radial-longitudinal motion I had seen when Enrico Persico had presented it in Varenna in 1954 during Enrico Fermi's last visit to Italy. Persico very much liked the intrinsic elegance of analytical mechanics and this had influenced me. The point I want to emphasize is that we had a mental picture of the processes taking place in the collider ring that was very close to reality – this would reveal its full power later, when the "Touschek effect" was detected, correctly interpreted, and cured – all in a single night in 1963.

We eventually were able to inject a non-negligible electron-positron current, which was extremely satisfactory to us. I already had reconsidered the calculation of the transverse size of the beam – its horizontal and vertical dimensions – and I now realized that its vertical dimension was much smaller than we had believed. This is an amazing point and deserves comment. No one had doubted (was it just a prejudice?) that the size of the beam was determined by multiple processes in both the horizontal and vertical directions when producing the betatron oscillations. These processes had a finite asymptotic limit owing to radiation damping, resulting in an equilibrium size of the beam. In the horizontal direction, the processes are radiation fluctuations and gas scattering, while in the vertical direction only gas scattering occurs. Now, single-photon emission during radiation damping takes place very frequently, so that in the horizontal direction there is a buildup of amplitude (the vector sum of successive root-mean-square partial amplitudes) owing to these discrete energy losses by the radiation, while in the vertical direction this process is absent and gas scattering is much smaller on average – by a factor of 100 – than in the horizontal direction.

I was confident of this result, since I had just worked on radiation fluctuations with Claudio Pellegrini (who had done the most important work on radiation problems at Frascati). I therefore announced to Bruno and everyone else that, in my opinion, the beam-size parameters promised a much higher luminosity than we had believed earlier – an increase by the above factor of 100. Bruno became terribly excited: This would make possible the observation of coincidences between annihilation events taking place in the forward and backward directions – if not electron-positron annihilation events producing a positive and negative muon, then ones producing two gamma rays. We calculated that at an energy of 200 MeV and a pressure of 10^{-9} torr the root-mean-square dimensions of the electron and positron bunches were 1.8 millimeters in the horizontal direction, 1.5 microns in the vertical direction, and 255 millimeters in the

longitudinal direction, where only the transverse (horizontal and vertical) dimensions matter for the beam's luminosity. Our enthusiasm, however, was short-lived.

Large and Small Technical Troubles

That night in March of 1963 we were steadily filling the collider ring as usual; the vacuum was excellent, as was the electron-positron injection current. Everything gave the impression that we were reaching higher stored currents than ever before. Then, at a certain point, we noticed that the injection rate was decreasing, and sometime later the stored current increased no further – it had reached saturation. The vacuum pressure gauge showed no change; a single beam was in. Touschek went crazy. It was about 2 or 3 o'clock in the morning – a time when we usually were at work at Orsay. Touschek left the laboratory and went to the Café de la Gare, which was open to serve passengers leaving and boarding the night trains. We continued to try to inject; clearly, saturation was occurring, depending on the number of particles in a single beam. Suddenly, Bruno reappeared (I cannot claim that he was exactly sober at the moment) announcing: "I got it! It is Møller scattering in the bunch!" He then exhibited a formula, explaining that he had calculated that saturation should occur at the beam intensities we had reached because electron-electron scattering in the beam's bunches was transferring energy from the betatron oscillations in the traverse directions into the longitudinal stability zone, which was limited in the amount of energy it could accept. Bruno's formula was excellent,²³ even though he had obtained it by introducing rough but wise approximations. The following morning, I made a complete calculation showing the full dependence of saturation on beam energy. Henri Bruck then checked my calculation and agreed that it was correct – after a long discussion about a factor of 2 in it. The electron-electron scattering probability obviously depends on the particle density in the beam, so that its small vertical size that I had found earlier now was considered to be a calamity. I felt guilty, but only for a moment.

Bruno, however, was desperate. In thinking about the complete calculation, we understood that this disadvantage applied especially to small colliders. AdA thus looked like a flop. I began to reconsider the situation, trying to be optimistic. For some unknown reason, I pictured the beam in my mind as a strap because of the "different" mechanisms that resulted in forming its horizontal and vertical sizes. "Different" means "uncoupled." Suppose, then, that you introduce a coupling. I telephoned Frascati: "Please prepare a small quadrupole magnet, of such and such dimensions, to be inserted into the quasi-straight section of AdA. I will fly tonight, come to the lab tomorrow morning, and bring the machined magnet back to Orsay in 48 hours. Thank you, guys."

At this time, Frascati was the best place in the world to get such a rapid response. So I got the quadrupole magnet, inserted it into AdA, rotated its axes, initiated electron-positron storage, and – helas! – the current went up again. Jacques Haissïnski later played a lot with this device to produce images of the light from the beam as variously shaped by the coupling, particularly near betatron resonances. But we had won the battle and not the war. When the coupling was turned off, the beam lifetime decreased

drastically for the first hour, so that the number of annihilation events recorded by our Cerenkov counter was low. Bruno was disappointed, but not entirely: At higher energies, which for instance we later had with the Adone collider, we knew that this saturation effect, now known as the "Touschek effect," was not a disaster.

Soon after its discovery, we felt in a hurry to show that genuine electron-positron annihilation events were occurring with their predicted luminosity at the crossing points of the electron and positron beams. A single electron-positron annihilation event producing two gamma rays was by far the most frequent reaction in which gamma-ray photons left the donut (mostly in the direction of the beam). It thus was no accident that this reaction was chosen subsequently as the typical "luminosity monitor" for electron-positron colliders. Obviously, to observe single gamma-ray-producing events in the presence of scattering by residual gas atoms in the larger collider rings, such as ACO (Orsay), Adone (Frascati), DORIS (Hamburg), and SPEAR (Stanford), is not as delicate as it was with AdA, where as in the others the gas pressure was low but the luminosity was small. (The Novosibirsk electron-positron collider ring VEPP-2 had a much higher gas pressure because the lack of commercial relations prevented the physicists there from buying titanium vacuum pumps;* they had a double vacuum-chamber system with differential pumping, which caused some delay in their work.)

In any case, our goal was to prove that electron-positron beam-beam collisions were indeed occurring at the predicted rate. We therefore had to devise a good method for subtracting the radiation background owing to scattering by the residual gas in the ring. Bruno noticed that the rate of gamma rays observed in the direction of beam 1 is proportional to the number of particles N_1 in it, while for beam-beam events the rate is proportional to the number of particles N_1N_2 in both beams 1 and 2. Thus, the observed gamma-ray rate divided by N_1 depends linearly on N_2 , and the slope of the line is a measure of the rate recorded by the detector monitoring the reaction, that is, of the luminosity of the beam. That prompted Querzoli, Di Giugno, and Marin to demonstrate their great expertise with the lead-glass Cerenkov counter, pulse analysis, and all that. Bruno and I took charge of the data analysis. We used his formula to predict the beam size and calculate its luminosity, finding a luminosity of 10^{25} particles per square centimeter per second – small but not negligible.

The data-taking time passed quickly, despite some accidents. The tower supporting the Cerenkov counter (a 150-kilogram lead-glass cylinder 2 meters above the ground) fell on Pierre Marin's leg, seriously injuring him (he recovered in a couple of months). The pulses from the Cerenkov counter also faded steadily over time; we discovered that it had been blackened by puffs of electrons from the linac beam that were striking various obstacles in the target room, the *salle de cible 500 MeV*, and had produced color centers in the Cerenkov counter, giving it a diffuse opacity. Corazza produced a near miracle by cleaning it in two days by baking it slowly, thus avoiding its melting under its own weight. We sent a report on this to *Nuovo Cimento*²⁴ and toasted the success with Chianti Straccali, the Chianti that Bruno (figure 13) loved best.

^{*} VEPP is the Russian acronym for electron-positron collider.



Fig. 13. Bruno Touschek (1920–1978) and his dog Lola in 1965. Courtesy of Bruno Touschek's family.

The adventure that was AdA thus came to its happy end. I particularly want to emphasize not only our scientific achievements with it, but also the exceptional – I would say unique – atmosphere of collaboration and friendship that we experienced during those four years, 1960 to 1964.

Final Remarks

We certainly made mistakes. At the very beginning, we had dreamed of seeing muon pairs without carefully considering the electron-positron beam luminosity necessary to achieve that goal; it is many orders of magnitude higher than what we were capable of obtaining. We also did not publish enough papers emphasizing the contributions of everyone. The best recollection of our work remains Jacques

Haïssinski's doctoral thesis of 1965,²⁵ which is very hard to find today in libraries. Colleagues too were ambivalent toward us: Accelerator engineers regarded us as physicists invading their domain; physicists regarded us as accelerator engineers (I will comment more on this below). This is not surprising, since we made no effort to explain that AdA was both a new experimental tool and a device that we conceived for carrying out a new class of experiments. Also regrettably, we dispersed almost all of the laboratory notebooks that recorded our day-to-day progress, and we never took a group photograph, which is particularly lamentable since Ghigo, Querzoli, and Marin are now deceased.

Nonetheless, AdA succeeded in convincing skeptical physicists that the problems associated with electron-positron colliders were not insurmountable. Western Europeans and Russians were the first to recognize the importance of this new tool: The Adone collider at Frascati, the ACO collider at Orsay, and the VEPP-2 collider at Novosibirsk were soon approved for construction, with a more reasonable mix of physicists and accelerator engineers being involved in designing them. The Princeton-Stanford group in America persisted for some time with its electron-electron double-tangent ring, which appeared to us to be a crazy idea without much relevance for physics. (Bruno usually referred to their machine to underline his view that accelerators must be conceived to advance physical knowledge and not accelerator technology.) In any case, their double-tangent ring came into operation more than a year after AdA was dismantled, with their first results being published only in 1966. (The Russians with their smaller VEP-1 double-ring collider at Novosibirsk had observed electron-electron scattering already in 1965.)

The case of AdA is a peculiar singularity in the lifestyle of the high-energy physics community. The building of accelerators usually was and is an undertaking of large laboratories with a dedicated staff that aims to design and build a machine that competes with those in other similar laboratories but possibly serves as a domestic household appliance. When new concepts in accelerator building are explored, as with the above colliders in the 1960s, machine people begin by designing scale-model prototypes, trying to incorporate the new conceptual improvements already available, such as strong focusing, separation of bending and focusing, and sophisticated injection optics. In the list compiled by Mark Q. Burton in 1961 at Brookhaven National Laboratory (BNL 683; T-230) we find, besides AdA and the Princeton-Stanford double-ring machine, a quite expensive 2-MeV electron-positron collider ring at CERN in Geneva, a 4-MeV spiral-ridge proton cyclotron at Harwell in the United Kingdom, a 45-MeV electron FFAG radial-sector electron accelerator at MURA in Madison, Wisconsin, and a 450keV electron cyclotron at Oak Ridge, Tennessee. All of these machines, including the Princeton-Stanford double ring, were of no interest to high-energy physicists. They responded to the demands of machine people (mostly engineers), who are a good example of people engaged in "separated functions," unlike at a former time when wise experimentalists were managing every detail in their laboratories.

Adone, a 3000-MeV electron-positron collider at Frascati (each beam had an energy of 1500 MeV), was Fernando Amman's masterpiece. Amman (b. 1930, figure 14) conceived its ring in 1961–1963 using the most advanced concepts, a powerful electron linac to use with the electron-positron converter, and a hall suited for experiments.

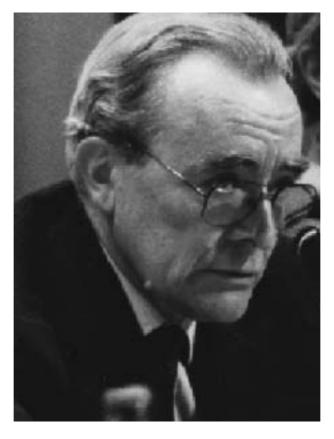


Fig. 14. Fernando Amman (b. 1930) in 1998. Courtesy of Fernando Amman.

Large luminosity was one of his main goals from the very beginning. Of the old AdA staff, only Corazza, because of his expertise in vacuum technology and in solving problems with the donut, was heavily involved in Adone. Touschek hesitated to get involved with it: There were too many problems of a "nonphysical" nature (placing orders, carrying out other duties, drawing up plans, lack of improvisation, and the like). Still, he was anxious to see the machine operating, and he participated in the meetings preparing experiments for it. A new concept in accelerator theory, the so-called Amman-Ritson effect, ²⁶ also attracted his attention for some time.

When Adone (figure 15) was nearly completed, I was placed in charge of coordinating, on behalf of the laboratory, the allocation of time for experiments being proposed by Italian physicists from various institutions. This was a difficult job because of the VIPs involved. Adone was a unique new tool, and prominent Italian physicists wanted to measure something with it. My personal feeling (which I still maintain was right, after so many years) was that one should first explore, with unsophisticated multi-purpose experimental devices (counters and spark chambers covering a wide



Fig. 15. An overview of the Adone collider. Courtesy of Frascati National Laboratories.

solid angle), if and how hadrons are produced at significant energies, particularly in the form of narrow resonances, to profit from the very precise energy definition of the colliding beams.²⁷ We missed discovering the J/Ψ particle only because it was found at 50 MeV above Adone's maximum beam energy! I do not wish to discuss here the experiments that were done with Adone, mainly because of the bitter memories I have of some unfriendly encounters with overbearing colleagues, but also because Amman has carefully reviewed the period in which Adone was built. ²⁸ The successes at intermediate energies of ACO at Orsay and VEPP-2 at Novosibirsk also have been reviewed elsewhere. ²⁹ Noteworthy too was the work of Karl Strauch and his staff at the Cambridge Electron Accelerator.* Nevertheless, historians have often ignored the developments associated with electron-positron colliders in the decade from 1960 to 1970, and this is my modest effort to contribute to this history.

^{*} The so-called CEA-bypass was operated for a short time at 2.5 GeV in the first years of the 1970s and then was shut down in 1973 because of budget problems.

Acknowledgments

I warmly thank Fernando Amman, Luisa Bonolis, Paul Forman, Jacques Haïssinski, Giorgio Salvini, and Albert Silverman for reading and commenting on a draft of my paper. I also thank Roger H. Stuewer for his careful editorial work on it. But my greatest thanks are posthumous and go to Edoardo Amaldi, who transmitted to me his love of the history of physics, and who wrote a fascinating biography of Bruno Touschek.³⁰

References

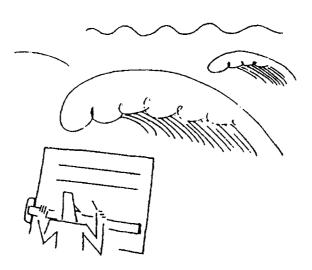
- 1 G. Battimelli and I. Gambaro, "Un laboratorio per le alte energie alla vigilia della seconda guerra mondiale," in Arcangelo Rossi, ed., Atti del XIV del XV Congresso Nazionale di Storia della Fisica, Udine 1993/Lecce 1994 (Gruppo Nazionale di Coordinamento per la Storia della Fisica del CNR, 1995), pp. 475–487; G. Battimelli and I. Gambaro, "Da via Panisperna a Frascati: gli acceleratori mai realizzati," Quaderni di Storia della Fisica 1 (1997), 319–333.
- 2 For a personal account, see Matthew Sands, "The making of an accelerator physicist," in Laurie M. Brown, Max Dresden, and Lillian Hoddeson, ed., *Pions to quarks: Particle physics in the 1950s. Based on a Fermilab symposium* (Cambridge: Cambridge University Press, 1989), pp. 149–161, especially pp. 151–153.
- 3 Owen Chamberlain, Emilio Segrè, Clyde Wiegand, and Thomas Ypsilantis, "Observation of Antiprotons," *Physical Review* **100** (1955), 947–950; Bruce Cook, Glen R. Lambertson, Oreste Piccioni, and William A.Wentzel, "Antineutrons Produced from Antiprotons in Charge-Exchange Collisions," *ibid.* **104** (1956), 1193–1197. For personal accounts, see Owen Chamberlain, "The discovery of the antiproton," in Brown, Dresden, and Hoddeson, *Pions to quarks* (ref. 2), pp. 285–295, and Oreste Piccioni, "On the antiproton discovery," in *ibid.*, pp. 285–295.
- 4 M. Gell-Mann and A. Pais, "Behavior of Neutral Particles under Charge Conjugation," *ibid.* 97 (1955), 1387–1389; A. Pais and O. Piccioni, "Note on the decay and Absorption of theta^o," *ibid.* 100 (1955), 1487–1489.
- 5 For a personal account, see Robert Hofstadter, "A personal view of nucleon structure as revealed by electron scattering," in Brown, Dresden, and Hoddeson, *Pions to quarks* (ref. 2), pp. 126–143. Also see the reprints of his papers and those of others in his book, *Electron Scattering and Nuclear and Nucleon Structure: A Collection of Reprints with an Introduction* (New York: Benjamin, 1963).
- 6 T.D. Lee and C.N. Yang, "Question of Parity Conservation in Weak Interactions," *Phys. Rev.* 104 (1956), 254–259; C.S. Wu, E. Ambler, R.W. Hayward, D.D. Hoppes, and R.P. Hudson, "Experimental Test of Parity Conservation in Beta Decay," *ibid.* 105 (1957), 1413–1415. For personal accounts, see T.D. Lee, "Reminiscences," in Robert Novick, *Thirty Years Since Parity Nonconservation: A Symposium for T.D. Lee* (Boston: Birkhäuser, 1988), pp. 153–165, and Chen Ning Yang, "Particle physics in the early 1950s," in Brown, Dresden, and Hoddeson, *Pions to quarks* (ref. 2), pp. 40–46. See also C.S. Wu, "The Neutrino," in M. Fierz and V.F. Weisskopf, ed., *Theoretical Physics in the Twentieth Century: A Memorial Volume to Wolfgang Pauli* (New York: Interscience, 1960), pp. 249–303, especially pp. 263–268, and Allan Franklin, *The neglect of experiment* (Cambridge: Cambridge University Press, 1986), especially pp. 7–38.
- 7 For discussions, see the various papers in R.G. Moorhouse, ed., Strong Interactions and High Energy Physics: Scottish Universities' Summer School 1963 (New York: Plenum Press, 1964).
- 8 Edoardo Amaldi, *The Bruno Touschek Legacy (Vienna 1921–Innsbruck 1978)* (Geneva: CERN [Report 81–19], 1981); "L'eredità di Bruno Touschek (Vienna 1921–Innsbruck 1978)," *Quaderni del Giornale di Fisica* 5, no. 7 (1982), 1–94. The former is reprinted in Giovanni Battimelli and Giovanni Paoloni, ed., *20th Century Physics: Essays and Recollections. A Selection of Historical Writings by Edoardo Amaldi* (Singapore: World Scientific, 1998), pp. 505–591.
- 9 E.D. Courant, M.S. Livingston, H.S. Snyder, and J.P. Blewett, "Origin of the 'Strong Focusing' Principle," *Phys. Rev.* 91 (1953), 202–203; E.D. Courant and H.S. Snyder, "Theory of the Alternating-Gradient Synchrotron," *Annals of Physics* 3 (1958), 1–48. See also, Ernest D. Courant, "Early history of the Cosmotron and AGS at Brookhaven," in Brown, Dresden, and Hoddeson, *Pions to quarks* (ref. 2), pp. 180–184.

S.D. Drell, "Quantum Electrodynamics at Small Distances," Annals of Physics 4 (1958), 75–86; C. Bernardini, "High Energy Experiments in QED," in K.T. Mahantappa, W.E. Brittin, and A.O. Barut, ed., Lectures in Theoretical Physics, Part II. Elementary Particle Physics, University of Colorado, Summer 1968 (New York: Gordon and Breach, 1968), pp. 465–491.

- 11 See for example H. Richard Crane, "How We Happened to Measure *g-2*: A Tale of Serendipity," *Physics in Perspective* **2** (2000), 135–140.
- 12 J. Ashkin, T. Fazzini, G. Fidecaro, N.H. Lipman, A.W. Merrison, and H. Paul, "Search for the Decay $\mu \rightarrow e + \gamma$ and observation of the Decay $\mu \rightarrow e + \nu + \bar{\nu} + \gamma$," Il Nuovo Cimento 14 (1959), 1266–1281.
- 13 G.K. O'Neill, "The Storage-Ring Synchrotron," in CERN Symposium on High Energy Accelerators and Pion Physics, Geneva, 11th–23rd June 1956, Proceedings (Geneva: CERN, 1956), pp. 64–65; W.C. Barber, B. Gittelman, G.K. O'Neill, and B. Richter, "Test of Quantum Electrodynamics by Electron-Electron Scattering," Physical Review Letters 16 (1966), 1127–1130.
- 14 A.P. Onuchin, "The Organizer of the Institute," in Boris N. Breizman and James W. Van Dam, ed., G.I. Budker: Reflections and Remembrances (New York: American Institute of Physics Press, 1994), p. 184.
- 15 K.R. Symon, D.W. Kerst, L.W. Jones, L.J. Laslett, and K.M. Terwilliger, "Fixed-Field Alternating-Gradient Particle Accelerators," *Phys. Rev.* 103 (1956), 1837–1859.
- 16 Andrew M. Sessler and Keith R. Symon, "Donald William Kerst November 1, 1911–August 19, 1993," National Academy of Sciences Biographical Memoirs 72 (1997), 235–244; especially 236–239. See also Donald W. Kerst, "Accelerators and the Midwestern Universities Research Association in the 1950s," in Brown, Dresden, and Hoddeson, Pions to quarks (ref. 2), pp. 202–212.
- 17 Yoichiro Nambu, "Possible existence of a Heavy Neutral Meson," *Phys. Rev.* **106** (1957), 1366–1367. See also his recollections, "Gauge principle, vector-meson dominance, and spontaneous symmetry breaking," in Brown, Dresden, and Hoddeson, *Pions to quarks* (ref. 2), pp. 639–642.
- 18 C. Bernardini, R. Querzoli, G. Salvini, A. Silverman, and G. Stoppini, "Search for New Neutral Mesons (the ρ°- Mesons)," *Il Nuovo Cimento* **14** (1959), 268–271.
- 19 For a personal account, see Hofstadter," A personal view" (ref. 5).
- N. Cabibbo and R. Gatto, "Electron-Positron Colliding Beam Experiments," Phys. Rev. 124 (1961), 1577–1595.
- 21 J. Lefrançois, "Results of the Orsay Storage Ring A.C.O.," in N.B. Mistry, ed., Proceedings 1971 International Symposium on Electron and Photon Interactions at High Energies, Cornell University, Ithaca, N.Y., August 23–27, 1971 (Ithaca, N.Y.: Laboratory of Nuclear Studies, Cornell University, 1972), pp. 52–63.
- 22 Carlo Bernardini, "Storia di AdA," "The Story of AdA," Scientia 113 (1978), 27–38, 39–44; "Storia dell'anello AdA," Il Nuovo Saggiatore 27, no. 6 (1986), 23–33; "From the Frascati Electron Synchrotron to Adone," in C. Bacci, C. Bernardini, G. Diambrini Palazzi, and B. Pellizzoni, ed., Present and Future of Collider Physics: Conference in honour of Giorgio Salvini's 70th birthday (Bologna: Italian Physical Society, 1991), pp. 3–15.
- 23 C. Bernardini, G.F. Corazza, G. Di Giugno, G. Ghigo, J. Haïssinski, P. Marin, R. Querzoli, and B.Touschek, "Lifetime and Beam Size in a Storage Ring," *Phys. Rev. Lett.* 10 (1963), 407–409.
- 24 C. Bernardini, G.F. Corazza, G. Di Giugno, J. Haïssinski, P. Marin, R. Querzoli, and B.Touschek, "Measurements of the Rate of Interaction between Stored Electrons and Positrons," *Il Nuovo Cimento* 34 (1964), 1473–1493.
- 25 Jacques Haïssinski, "Thése pour obtenir le grade de docteur ès-sciences." Orsay, série A, no. 81, February 5, 1965.
- 26 F. Amman and D. Ritson, "Design of Electron-Positron Colliding Beam Rings," in M. Hildred Blewett, ed., *International Conference on High Energy Accelerators, September 6–12, 1961* [Brookhaven National Laboratory] (Washington, D.C.: U.S. Government Printing Office, 1961), pp. 262–264; for other developments, see Claudio Pellegrini and Andrew M. Sessler, ed., *The Development of Colliders* (New York: American Institute of Physics Press, 1995).
- 27 B. Bartoli, C. Bernardini, F. Felicetti, V. Silvestrini, A. Goggi, D. Scannicchio, F. Vanoli, and S. Vitale, "Reactions dans Adone produisant un seul boson," in R. Beck and E. Cremieu-Alcan, ed., Symposium International sur les Anneaux de Collisions a Electrons et Positrons tenu à l'Institut National des Sciences et Techniques Nucléaires, Saclay (Orsay: Laboratoire de l'Accélérateur Linéaire and Saclay: Institut National des Sciences et Techniques Nucléaires, 1966), pp. VIIa-1-1-VIIa-1-6;

- C. Bernardini, "Results on e⁺e⁻ reactions at Adone," in Mistry, 1971 International Symposium (ref. 21), pp. 38–49.
- 28 Fernando Amman, "The Early Times of Electron Colliders," *Rivista di Storia della Scienza* **2** (1985), 130–151. For the physics, see G. Salvini and A. Silverman, "Physics with matter-antimatter colliders," *Physics Reports* **171**, nos. 5 and 6 (1988), 231–424.
- 29 See for example P.C. Marin, et al., "The Orsay Storage Ring Group," reprinted in Pellegrini and Sessler, Development of Colliders (ref. 26), pp. 139–143 and Fig. 8, in ibid., p. 10. See also the papers presented in the IV Plenary Session, "Colliding Beams and Storage Systems," in A.A. Kolomensky, A.B. Kusnetsov, and A.N. Lebedev, ed., Proceedings of the International Conference on High Energy Accelerators, Dubna, August 21–27, 1963, Vol. 1 (Reproduced Washington, D.C.: U.S. Atomic Energy Commission, 1964), pp. 309–431.
- 30 Amaldi, Bruno Touschek Legacy; "L'eredità di Bruno Touschek" (ref. 8).

Department of Physics University of Rome "La Sapienza" Piazzale Aldo Moro, 5 I-00185 Rome, Italy e-mail: carlo.bernardini@roma1.infn.it



PRIMOGENITURE

Great strength is in a principle, but praxis is invincible.

Piet Hein

Copyright © Piet Hein Illustration & Grook Reprinted with kind permission from Piet Hein a/s, Middelfart, Denmark