Special issue:

History of Particle Colliders

January 2018
This one-day meeting on the 'History of Particle Colliders' took place at the H H Wills Physics Laboratory, University of Bristol, on 19 April 2017. It was a joint meeting between the IOP History of Physics Group, the High Energy Particle Physics Group and the Particle Accelerators and Beams Group, with additional sponsorship from the School of Physics, University of Bristol. The aim of the meeting was to look at the historical and political aspects of the design, construction and operation of the machines, and not so much on the physics results, which were perhaps better known.

The meeting opened with a talk by Dr Giulia Pancheri (INFN Frascati) on "The history of electron-positron colliders." She recalled the main events of how the Austrian born Bruno Touschek learned the art of making particle accelerators during WWII in Germany, building a 15 MeV betatron with the Norwegian Rolf Wideroe. This led to fundamental developments throughout Europe, including the building of high energy colliders in France with ACO, in the Soviet Union with VEPPII, in Italy with ADONE, in the USA with SPEAR, and in Germany with DORIS.

Next, Dr Philip Bryant (CERN) talked about "The CERN ISR and its Legacy" These were two intersecting synchrotron rings that were operational from 1971 to 1984. The ISR opened the door to 13 years of rapid advances in accelerator physics, technologies and techniques. Our concepts of accelerator design, engineering and diagnostics and the way experimental physics is conducted changed radically and came to look very much as they do today.

After lunch, Prof Peter Kalmus (Queen Mary, London) spoke on "The CERN Proton-Antiproton Collider Project", describing the evolution of this project and its pre-history. He illustrated stochastic cooling and the beam gymnastics of the collider. He gave details of the two large detectors, UA1 and UA2.

Vince Smith
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The path to high-energy electron-positron colliders: from Widerøe's betatron to Touschek's AdA and to LEP

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Fig. 1. Left: Rolf Widerøe, in middle age, photo from Emilio Segré visual Archives, AIP. Right: Bruno Touschek, 1955 (Courtesy of Touschek family).

Introduction

In what follows, we describe the road which led to the construction and exploitation of electron positron colliders. We sketch the sequence of events which brought together the Austrian born Bruno Touschek [1], shown in the right panel of Fig. 1 and the Norwegian Rolf Widerøe [2], shown in the left panel. Their encounter, during WWII, in Germany, ultimately led to the construction in Frascati, Italy, in 1960, of the first electron-positron collider, the storage ring AdA (in Italian ‘Anello di Accumulazione’), and to the first observation of electron-positron collisions in a laboratory, in Orsay, France, in 1963.
Following this proof of feasibility, new particle colliders were then planned and built, leading to milestone discoveries such as the 1974 observation of the \(J/\Psi\) particle, a new unstable state of matter, made of charm quarks. These early machines opened the way to the large particle accelerators of the 1980's, and the other discoveries that established the Standard Model of elementary particles as a well-tested physics theory.

2 The ante facts: in Europe before WWII

There are history-making events, such as the construction of the first electron-positron collider, AdA, which start by chance. In this section, we shall see how one of the last issues of the Physical Review to reach Norway, in 1941, led Bruno Touschek and Rolf Widerøe to meet and work together on the 15 MeV betatron project, commissioned by the Ministry of Aviation of the Third Reich, the Reichsluftfahrtministerium (RLM). This is how Touschek learnt from Widerøe the art of making accelerators, enabling him, many years later, to design, propose and carry through the construction of AdA.

2.1 Bruno Touschek came from Vienna

Bruno Touschek was born in Vienna, on February 3rd, 1921, and lived there until 1942, when he moved to Germany in order to pursue his studies in physics, which had been interrupted by the application of the Nuremberg Laws to Austrian citizens of Jewish origin. Bruno's maternal side was from a Jewish family active in the artistic circles of the Vienna Secession, the father a gentile and an officer in the Austrian Army. Because of his mixed parentage, Touschek's life and studies in Vienna were partly tolerated and he was allowed to attend the University of Vienna until June 1940, after which he was banned from classes and the University library. Studying and learning became extremely difficult. Between June 1940 and Fall 1941 Bruno's physics education progressed thanks to Paul Urban (later Professor at University of Graz, Austria), who would borrow various textbooks from the University library and help him study at his own home. Bruno thus was able to access Arnold Sommerfeld's fundamental treatise, Atombaum und Spektrallinien, summarizing all the development of the theory of atomic structure and of spectral lines, even finding small errors. Through Urban, Touschek had the chance to meet Sommerfeld in person in late 1941.

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1 Touschek's mother died young and his father later remarried.
2 Edoardo Amaldi [1], in citing Urban, gives the date November 24, 1942 for
Bruno's intelligence and drive in physics clearly impressed the great German theoretician. From the correspondence exchanged with Sommerfeld, which followed this encounter, there came the project for Touschek to move to Germany, where he could be able to unofficially attend classes and seminars held at the University of Hamburg and Berlin by Sommerfeld's former students and friends, such as Max von Laue, Paul Harteck, and Wilhelm Lenz.  

In February 1942 Touschek left Vienna, first for Munich and then for Hamburg, from where in August 1942 he was writing to his father amid hopes for his future studies, mixed with homesickness, and money worries. He moved between Berlin and Hamburg, also visiting his family in Vienna occasionally, and attending unofficial seminars held there by Hans Thirring. To earn his living, he took small jobs, often obtained through Sommerfeld's friends, or by chance, as it happened when, in the train from Hamburg to Berlin, he met a girl, half-Jewish like him, who suggested he work for an electronic valve firm Löwe-Opta in Berlin, furnishing electronic equipment to the war ministries, such as RLM. The firm was directed by Hans Thirring's former student, Karl A. Egerer, also editor and referee of scientific magazines such as the Chemisches Zentralblatt. In particular, and, most crucially, Egerer was the Editor-in-chief of the Archiv für Elektrotechnik, the journal where, in 1928, Widerøe had published his early article on the betatron [3]. Bruno, who was charged with the establishment of a radio-frequency laboratory at Löwe-Opta, became Egerer's editorial assistant and this is how, on February 10, 1943, he came across what, in a letter to his parents, he calls a “sehr dummen Artikel” [a very stupid article]. As we shall discuss below, the context of the letter, and the events which followed between February and June 1943, strongly suggest [4] that the article referred to was from the Norwegian engineer Rolf Widerøe.

Urban's seminar. While the month may be right, the year is actually 1941, as seen from letters exchanged between Urban, Sommerfeld and Touschek, and from Touschek's letters to his family in February 1942, already from Munich.  

The correspondence between Touschek and Sommerfeld is kept in the Archives of the Deutsches Museum in Munich, Germany.  

Touschek's life in Germany during the war is chronicled in a number of typewritten letters to his father and step-mother, to which GP and LB were given access by Bruno Touschek's late wife, Mrs. Elspeth Touschek. These letters, including a few before the war, and a number of them extending well into the post-war period, are presently in possession of their son Francis Touschek.
2.2 Rolf Widerøe was from Oslo

Rolf Widerøe, the author of a proposal for a 15 MeV betatron (submitted to the Archiv on September 15, 1942) which reached Egerer and Touschek's desk in February 1943, was born in Oslo, July 1902. Following his parents' suggestion, he went to study in Germany, first in Karlsruhe and then in Aachen, where he obtained his doctorate in engineering. Very early in his life, he had become fascinated [5] by the emerging field of particle accelerators. In 1928, he had described the successful construction of the first linear accelerator as part of his PhD thesis, based upon an earlier theoretical suggestion by Gustav Ising, and proposed a new technique to accelerate electrons and keep them in orbit, deriving what became known as the betatron equation [3]. His linear accelerator (which may be considered as a ‘rolled out’ cyclotron), and his first (unsuccessful) attempts to build a betatron, inspired Ernest Lawrence to build the first cyclotron and drew the attention of the American accelerator scientist Donald Kerst. Kerst succeeded in building the first betatron, and reported its operation in the Physical Review [6]. These papers have been pivotal in the history of e+e−-colliders; firstly, as we shall describe below, because they played a crucial role in bringing Widerøe back to Germany in 1943; secondly because, at around the same time, they became the subject of the graduation thesis of the Italian physicist Giorgio Salvini, the first director (1954-60) of the Frascati Laboratories. Here, as we shall also see, an electron synchrotron of 1100 MeV went into operation in 1959, and, a few months later, the construction of the first electron-positron collider, AdA, was approved.

While in the United States advances in accelerator physics had been making history, Rolf Widerøe, not having succeeded in building the betatron he was proposing, had left the active research field, and, in 1940, had joined the technical firm Norsk Elektrisk Brown Boveri in Oslo. However, he remained always interested in the field, and thus it happened that in late autumn 1941, he attended a Conference on particle accelerators, held in Oslo, at the Physics Association. Norway was occupied by Germany at the time and scientific communications with the English and American publications were dwindling, stopping altogether after the United States, in December 1941, entered the war. But one last copy of the Physical Review had reached the University of Trondheim by ordinary mail a few months earlier, with the article by D. Kerst on the first operational betatron.5

5 Since the 1941 Physical Review articles were published in the July 1st 1941 issue, and reaching Norway by ordinary mail would have taken 3 months, the journal arrived not before October.
Roald Tangen, Professor at the University, decided to make it the subject of a talk he was preparing for the Oslo Physics Association. When he gave his talk, he mentioned being intrigued by the reference to an article from an author with a Norwegian sounding name, Widerøe [2]. Rolf was in the audience. Rushing back to his old passion, and clearly engaged anew seeing Kerst's success, in a few months Widerøe prepared a proposal for a 15 MeV and even a 200 MeV betatron, and in September 1942 submitted it to the Archiv, where his original 1928 paper had appeared.

2.3 Touschek and Widerøe: the article which was never published and the 15 MeV betatron

Between February and June 1943 there unfolded the events which led to the approval of Widerøe's proposed project. Two of Touschek's letters to his family [4], respectively dated February 15 and June 17, 1943, give a clue of what happened. From the first of such letters, one sees that Touschek discussed the physics of the project with his boss Karl Egerer. It also appears that he was critical of the treatment of relativistic effects, probably not correctly included at the time of the submitted article. Of special note, in this letter, is Egerer’s mention of Heisenberg's possible interest. At the time, in Germany, there were other groups working on betatrons, such as Max Steenbeck at Siemens and the small company owned by Heinz Paul Schmellenmeier in Berlin. In Spring 1943, while the above-mentioned machines were being developed, a project with a clear war potential was being considered by the Luftwaffe, a X-ray gun, proposed by Ernst Schiebold, a well-known mineralogist and expert in X-ray technology. Widerøe's higher energy betatrons, being a source of more powerful X rays, became connected with Schiebold's project as a possible realization of a death-ray weapon [7].

Widerøe's article was brought to the attention of the military and soon taken into active consideration, to the extent that a few months later, March, or

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6 Notice that in Kerst's 1940 article, the reference to Widerøe 's paper [3] has the wrong year. This was later corrected in the 1941 papers, but still with the wrong page number.

7 At that time, Werner Heisenberg had become the director of the Kaiser Wilhelm Institute for Physics in Berlin and was leading the group of physicists working on the German nuclear project, the Uranverein. His auspicated interest provides an important premise towards the future involvement of the RLM in Widerøe's proposal.
April, in Spring as Widerøe says, he was approached in Oslo by “several German Air Force officers”, who invited him to come to Berlin to discuss his project with the German military authorities. Widerøe accepted and two days later [2] was flown to Berlin. In the months that followed, Widerøe refined his project, and a correspondence with Touschek started, about relativistic effects which needed to be taken into account. Touschek seems also to have joined the proposed project. In his letter of June 17th, 1943, Bruno Touschek writes:

“. . . slowly I begin even to be creditworthy. Based on my past work I was promised by the RLM, a monthly remuneration of about 100 marks. I have not seen anything yet . . . My Norwegian has been beaten on the whole line by me. Today there was a meeting in RLM, in which he had to agree with me point-by-point... A work in-print on the theory of a specific device will be pretty much changed based on my remarks.”

Widerøe worked on the article for the final publication expecting it to appear in the November issue of the Archiv für Elektrotechnik (vol. 37, pp. 542-555), according to the reference mentioned in his autobiography. But the article, whose proofs are preserved in Widerøe’s archives in Zurich, is nowhere to be found in the 1943, nor in the 1944 issues of the journal. In fact, it was never published, and a different article actually appears, exactly filling the range of pages corresponding to Widerøe’s article. Instead, in June, the project was approved by the German Aviation Ministry, due to start as early as possible, and classified as of military interest. In late summer (July or August?), Widerøe went on vacation with his wife and had one of his inspirations: positive particles colliding against negative ones and releasing greater amount of energy than if hitting a stationary target. As Touschek later recalled about center of mass collisions:

“The first suggestion to use crossed beams I have heard during war [was] from Widerøe, the obvious reason for thinking about them being that one throws away a considerable amount of energy by using ‘sitting' targets' most of the energy being wasted to pay for the motion of the center of mass.”

Upon returning from his vacation Widerøe mentioned his idea to Touschek and applied for a patent, dated as submitted on September 8th, 1943, but officially recorded only after the war, in 1953. Touschek scoffed at the idea, which he considered “obvious”, but the seed had been planted in his mind,

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8 A preliminary copy of Widerøe's article was sent to us by A. Sørheim [5]. Copy of the article is actually preserved at the Archive of the Max Planck Society in Berlin, together with the undated typed draft of the second part describing a more powerful 200 MeV machine (Der Strahlentransformator II).

9 Undated note, presumably written in February or March 1960.
to bear fruit when the time came, after the war and when both particle physics and the science of accelerators had progressed enough. The betatron project was housed near Hamburg, and in the months to follow, the project went ahead, being completed in late 1944. In March 1945, the war was at its end. The military authorities ordered the betatron be moved to relative safety to a deserted factory house north of Hamburg, and as soon as this task was completed, on March 15 Touschek was arrested by the Gestapo and held prisoner in the infamous Fulsbüttel prison. But, as the Allied troops were approaching, orders came for the prisoners to be moved away, to the Kiel concentration camp 30 Kilometers North of Hamburg. As Touschek says in a postwar letter to his parents [4]: “There were SS soldiers behind us, in front, and on both sides.”

He was lucky: as it is recalled in many slightly differing versions, he narrowly escaped death, and, after the war, went for his formal studies in physics, first to Göttingen for his diploma, and then Glasgow for the DPhil. It is not clear why Touschek, who was part of Heisenberg's group, left and went to Scotland. Later Touschek regretted leaving Göttingen, but in Glasgow a synchrotron was being built and Touschek was also involved in the project, thus furthering his knowledge on the art of making accelerators. After the doctorate and being awarded the Nuffield fellowship at University of Glasgow, Bruno was offered a position at University of Rome, and, in 1953, moved to Italy.

In Norway, when the war ended in 1945, Widerøe's decision to work for the occupying forces and the successful completion in Germany of the 15 MeV betatron by late 1944, were subjected to an inquiry by a Norwegian Commission, who accused Widerøe of complicity with the occupying regime and even of participating in the construction of the V2 rockets which struck England during the war. Of this he was cleared, but his work in Germany was not condoned. Although he justified his collaboration with the Germans as a way to help reduce the pressure on his brother Viggo Widerøe, a hero of the Norwegian resistance movement held prisoner in Germany at the time, he had to pay a heavy fine, which left him and his family in financial difficulties. Unlike Touschek, he did not turn to fundamental research, but instead went on to develop the field of radiation medicine in Europe, from Switzerland, where he moved in 1946 with his family, and at the Oslo Radium Hospital.

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10 Chronology and details of the events concerning Touschek's imprisonment and release are described by Touschek himself in two letters to his parents, dated June and October 1945 [4], and do not coincide with Widerøe's later recollections in [2].
As for the betatron, it was appropriated as booty of war by the Allied Forces after the end of the war and brought to England, where it disappeared, after being used to X-ray iron plates [2].

3 AdA: from Frascati to Orsay

After the war, the reconstruction of science in Europe developed alongside the creation of CERN and the decision to build particle accelerators as the tool for technological advances and for fundamental research in the laws of matter.

The decision to create CERN and build a proton-synchrotron at the Geneva site went together with the establishment of national laboratories, equipped with accelerators, where scientists, technicians and engineers would be trained and become part of the national resources, on which CERN would draw. In France, it was decided to build a proton synchrotron, SATURNE, and a linear accelerator (LINAC) of electrons, in Orsay, south of Paris. In Italy a circular machine, an electron synchrotron, was planned. The site was chosen to be near the town of Frascati, South-East of Rome. And so it happened that, at the end of 1959, in addition to SATURNE, which had started operating in 1958, three powerful particle accelerators started functioning in Europe: at CERN it was the proton synchrotron, in Frascati the electron synchrotron; the LINAC in Orsay. With these accomplishments, new kinds of scientists, technicians and engineers, then called machine builders, prepared the field for the arrival of new accelerators, the particle colliders. In all the existing machines, particles, protons or electrons, were accelerated to hit stationary protons. But, at this time, the concept of stacking beams of particles and making them collide head-on had begun to be investigated [8, 9]. Projects for electron-electron storage rings were started in the US [10], as well as in the Soviet Union under the direction of Gersh Budker, first in Moskow and then in the new laboratory of Novosibirsk, in Siberia [11].

In September 1959, a conference about the future of high-energy accelerators in physics was held at CERN. Both American and Russian scientists attended it. Electron-positron collisions were also mentioned [12], but no one knew how to do it. As Touschek later wrote:

“The challenge of course consist[ed] in having the first machine in which particles which do not naturally live in the world which surrounds us can be kept and conserved.”

11 B. Touschek. Excerpts from a talk at Accademia dei Lincei, 24.5.74.
A few weeks after the CERN conference, Wolfgang K.H. Panofsky, from Stanford University, was in Rome giving a seminar about the electron-electron rings being built in the USA, and the question of using electrons and positrons was raised again, by Bruno Touschek. Touschek clearly stressed that electron-positron annihilations, reactions proceeding through a state of well-defined quantum numbers, would be the pathway to new physics.

3.1 Frascati, Italy, 1960

Since arriving in Rome in 1953, Bruno had engaged in theoretical work with colleagues at University of Rome, and had also become actively interested in Frascati Laboratories, where the synchrotron had been built and, since December 1959, was now functioning. Thus, in the months following Panofsky's seminar, the idea of AdA started taking place and, on March 7th, 1960, during an epoch-making seminar at Frascati Laboratories, Bruno Touschek outlined his proposal to explore the physics of $e^+e^-$ annihilation processes and to build, as a first step, a small storage ring in which to store bunches of electrons and positrons to make them collide at a center of mass energy of 500 MeV.

Before the seminar, Touschek had thoroughly explored the feasibility of what he was calling “an experiment”. In emphasizing the possibility of a complete transformation of the collision energy in the creation of new particles through a channel with the well-defined quantum numbers of the photon, Touschek saw in the inherent simplicity of the annihilation process,

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12 The precise date of this seminar is not known, however a seminar by Panofsky at the Frascati National Laboratories on the subject of the Two-mile linear accelerator in Stanford, is recorded as October 27, 1959. It can be expected that the seminar in Rome would have taken place around the same date.

13 N. Cabibbo in Bruno Touschek and the Art of Physics, docu-film by E. Agapito and L. Bonolis, INFN 2003. Raul Gatto recalled that, “Bruno kept insisting on CPT invariance, which would grant the same orbit for electrons and positrons inside the ring” (personal communication to LB, January 15, 2004).

14 Early notes by Touschek about the possibility of electron-positron collisions dated 18.2.60, follow a laboratory discussion about the future of the Frascati Laboratories on February 17th. Discussions about the physics to be explored had also been taking place between Rome and Frascati, as seen by the paper by N. Cabibbo and R. Gatto, “Factors from Possible High-Energy Electron-Positron Experiments” received by the Physical Review Letters on February 17th, published on March 15.
a conceptually different road to gain deep insight in fundamental particle physics and expressed his belief that this experiment should be ‘the future goal’ of Frascati Laboratories.

Only one week after the seminar, on March 14 the decision to build AdA was taken. Work to build AdA immediately started. Such was the optimism and the strong belief in this challenging enterprise that on November 9, 1960, Touschek prepared a note for a collider with an electron beam energy of 1500 MeV:

“. . . if the work now in progress on ADA shows real promise (we will know more about this in February 1961) the development of ADONE should be considered as compulsory. . . ”.15

Indeed, on February 27, 1961, exactly one year after Touschek's first proposal, the photomultiplier's current, which was recording the synchrotron light emitted by the circulating particles, showed that electrons were accumulated in AdA. In early June 1961, Touschek presented the first results at the International Conference on Theoretical Aspects of Very High-Energy Phenomena held at CERN in Geneva, announcing the development of two storage rings in Frascati, AdA and ADONE [13].

3.2 AdA: Orsay 1961-64

The announcement at the CERN conference had an immediate echo.16 In the month of August, two French physicists, Pierre Marin from Orsay and Georges Charpak from CERN, went to Frascati to see the “intriguing things” happening there [14]. Marin writes:

“In Frascati, a small team of top class physicists showed us with great pride a small machine, in a hall just next to the Frascati electron synchrotron. This was AdA, un vrai bijou, a real jewel. The team, Bruno Touschek, Carlo Bernardini, Giorgio Ghigo, Gianfranco Corazza, Mario Puglisi, Ruggero Querzoli and Giuseppe di Giugno had succeeded in storing in the ring a few thousand electrons and positrons in turn, keeping them circulating for a few hours, until the beams decayed, observing them through the radiation they emitted. An enormous step for storage rings . . .”.

15 ADONE a Draft proposal for a colliding beam experiment, B. Touschek Archive, Sapienza University, Rome, Box 12, Folder 3.95.3.

16 In addition to the French interest described in this section, we note the impact of Touschek's announcement in V.N. Baier, who mentions it as “proof that we were not alone” in his reminiscences about Forty years of Electron Positron Colliders. The announcement is also mentioned in Baier's early extensive paper on electron-positron physics, submitted in December 1962, Sov. Phys. Usp. 5, 976.
However, the intensity of the gamma ray beam entering AdA from the synchrotron to produce electrons and positrons within the ring was too low and the feasibility of the Frascati set-up for physics studies could not be demonstrated. It was then envisioned, among Marin, Touschek and Bernardini, that the machine could be moved to the Laboratoire de l'Accélérateur Linéaire, in Orsay, near Paris, in order to use the Orsay LINAC as a more efficient injector.

Fig. 2. Left: AdA installed in Salle 500 MeV, Laboratoire de l'Accélérateur Linéaire, Orsay, France, 1963. Right: a drawing by Bruno Touschek reflecting frequent discussions about positrons or electrons entering AdA.

In the year to follow between September 1961 and July 1962, an agreement between France and Italy was reached and on July 4th 1962 Ada was moved from Frascati to Orsay, where it was placed in the 500 MeV experimental hall (Salle 500 MeV), as shown in the left panel of Fig. 2. In France, thanks to the Orsay LINAC, the injection rate increased by no less than two orders of magnitude. Pierre Marin and François Lacoste from Orsay joined the Italian group. When later Lacoste left, Jacques Haïssinski joined the Franco-Italian team. They were given the use of the LINAC during the weekends,

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17 Haïssinki’s 1965 *Thèse d'Etat* is the most comprehensive presentation of all that was known at the time in electron storage ring physics.
which meant that sometimes they would work as much as 60 hours in a row [15]. The turning point of the experimentation in Orsay came one night, in late winter 1963. Carlo Bernardini remembers [16]:

“The Linear accelerator was running like a dream, the injection of particles in the first beam worked like wonder (were they electrons or positrons?).” Touschek used to sketch their discussions in the drawing shown in Fig. 2.[18]

And then something happened: the speed at which particles were being stored in the ring started decreasing, as if the beam lifetime were shortening. The team panicked: was there a limit to the luminosity which could be achieved? Would they have to abandon the dream of higher energies, and forgo the future of this type of machines? Bruno decided to take a break, and left. It was about 3 a.m. in the morning. Then, on the paper covering a table at the Café de la Gare d’Orsay open all night, he sketched the solution of the problem, which he called the AdA effect. Touschek understood that there was a large momentum transfer from the radial to the longitudinal motion in a particle bunch due to the scattering of these particles within the same bunch. Many particles were lost when the density in the beam had reached a relevant value. This effect could have been devastating for the operation of the larger machine, such as ADONE or ACO, the Anneau de Collisions d’Orsay, which was being envisioned by Pierre Marin, but Touschek had also calculated that this phenomenon was decreasing rapidly with energy. Still, due to the AdA effect (later the Touschek effect) [17], AdA would suffer from an intra-beam scattering limit that gave a density-dependent lifetime generally significant at low energies (below 1 GeV, or so).

Although the machine never reached the hoped-for collision rates for observing annihilation into new particles, AdA was however able to shed light on the actual size of the particle bunches, which had never been previously experimentally tested as far as the behavior of stored beams was concerned. Most important, by means of single bremsstrahlung as a monitoring reaction, AdA was able to give definite proof for the existence of conditions in which the two beams could be made to collide [18]. The process \( e^+e^- \rightarrow e^+e^- + \gamma \) was observed with AdA in Orsay, the interaction rate was measured and found to be in good agreement with the hypothesis

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18 Bruno Touschek's drawings constitute an important chronicle of his life during the war, being included in his letters to his family, but also about academic life in Italy and, in particular about the student protests at University of Rome from 1968 onwards. Some of the drawings have been published in [1], others in [4].
that there was a complete overlap between the two beams and that the
dimensions of the beams were those calculated from the lifetime effect.\(^{19}\)

In [15] Jacques Haïssinski says:
“C'était la première fois au monde que l'on montrait que les particules,
effectivement, intérègissaient et entraînaient en collision les unes avec les
autres et donc, ça montrait, on peut dire, que ces machines étaient utilisables
pour faire de la physique des très hautes energies. . . ”.\(^{20}\)


As word spread that it was possible to accumulate enough particles in the
same ring and observe their collisions, projects for building higher energy
colliders based on these same principles, were accelerated, in the Soviet
Union, in the United States, in Europe. In Europe, France started building
ACO, an electron positron collider with a center of mass (c.m.) energy of
2x550 MeV, and Italy went ahead with the construction of a bigger and
better AdA, namely ADONE with a c.m. energy of 3000 MeV. ADONE had
been proposed by Touschek as early as November 1960, when he realized
that AdA would soon be working.

In the proposal to the Frascati Laboratory, there lies the decision which
doomed Frascati not to observe the J/\(\Psi\) at the same time as the Americans.
Touschek's 1960 early proposal argued for a 3.0 GeV c.m. energy as being
the highest energy to study pair production of all the elementary particles
known to exists at the time: pions and kaons, including studies of the
nucleon-antinucleon threshold around 2.0 GeV. In the Soviet Union [11, 19]
the construction of VEPP-2, a 2000 MeV electron-positron collider, was
accelerated in earnest; in the USA, at Stanford, an e+e- ring with the highest
energy of them all, the Stanford Positron Electron Accelerating Ring
(SPEAR), with c.m. of 6.0 GeV, was approved with University funding, and
built in a vacant lot owned by the University [20].

VEPP-2 and ACO were the first to operate electron-positron storage rings

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\(^{19}\) The theoretical calculation for the cross-section of the single bremsstrahlung
process was discussed in the Tesi di Laurea of Guido Altarelli and Franco Buccella,
two of R. Gatto's students, published in the same issue of Nuovo Cimento as the AdA
paper of Ref. [18].

\(^{20}\) “It was the first time in the world that it was shown that particles actually
interacted and collided with one another, and thus this showed that, one could say,
these machines could be used to make physics at very high energies”.
with a high-energy physics program, where most of the basic storage ring machine physics was also studied and analyzed. Thus, in the late 1960’s-early 1970’s, a generation of powerful electron-positron colliders - ADONE in Italy, ACO in France, SPEAR in California, DORIS at DESY and CESR at Cornell - started operating. Expectations for new physics to explore and discover were high. In Frascati, in 1972 a surprisingly high rate of particle production was reported, a phenomenon of multi-hadronic production also observed at the Cambridge Electron Accelerator (CEA) bypass, and which signaled the appearance of new particles and possible application of the new theory of strong interactions, perturbative Quantum ChromoDynamics. In Stanford, as the c.m. energy rose to and passed the 3 GeV c.m. energy, the hadronic cross-section was observed to become anomalously large. Rumors in the US had it that a new state of matter was being created around this energy, a phenomenon observed also in hadronic fixed target collision. What became known as the November Revolution in particle physics started with a press conference in Stanford on November 11 1974, jointly held by Burton Richter, the director of the Stanford Linear Accelerator Center (SLAC), and Samuel Ting, the head of the Brookhaven experiment where the early signals of a bump in the cross-section had been observed. Frascati, with ADONE, confirmed the discovery on November 13th. The machine which had been invented by Touschek and had been built to discover new particles had succeeded in doing so, but it had come in second.

After the discovery of the $J/\Psi$, particle physics entered a new phase, something like a Renaissance period. Both proton and new electron-positron colliders were planned and built, with a thousand-fold (and more) increase in the available center of mass energy, and accompanying luminosity. From AdA with 500 MeV c.m. energy and 4-meter circumference to the Large Electron Positron (LEP) collider with up to 209 GeV and 27 Kilometer around, from the Intersecting (proton-proton) Storage Ring at 53 GeV to the Large Hadron Collider in the LEP tunnel.

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21 The news reached Frascati first by way of a telephone call from Su Lan Wu, from the Brookhaven team, alerting Giorgio Bellettini, Director of the Laboratory at the time, and then from Mario Greco, staff member of the Frascati Laboratories, then at Stanford, who communicated the exact value of the mass of the newly found particle. This information was essential in order to tune the machine to the right energy, since the $J/\Psi$ decays very rapidly, and could be missed. Private communications to G.P. by G. Bellettini and M. Greco.

22 Proton colliders include the CERN ISR and LHC (proton-proton), a proton-antiproton ISR run at 62 GeV, the CERN Super antiproton-proton Synchrotron, FermiLab Tevatron (proton-antiproton), and HERA at DESY (positron-proton).
with up to 13 TeV c.m. proton-proton energy, particle colliders ushered in new discoveries and confirmed the validity of the Standard Model of particle physics.

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References


The CERN ISR and its Legacy

P.J. Bryant

Abstract

The CERN Intersecting Storage Rings (ISR) were two intersecting synchrotron rings each with a circumference of 942 m and eight-fold symmetry that were operational from 1971 to 1984. The CERN Proton Synchrotron (PS) injected protons at 26 GeV/c into the two rings that could then accelerate to 31.4 GeV/c. Typically, the ISR worked for physics with beams of 30–40 A over 40–60 hours with luminosities in its superconducting low-β insertion of $10^{31}–10^{32}$ cm$^{-2}$ s$^{-1}$. Interest in the ISR dates back to 1960 when machine design was still influenced by the weak focusing era and experimental physics was based on fixed targets. From these rather rudimentary beginnings up to the closure of the ISR, our concepts of accelerator design and the way experimental physics is conducted changed radically and came to look very much as they do today.

Figure 1. Layout of the ISR and injection chain (Ref. [2] K. Hübner)1.

23 The author joined the CERN ISR in 1968 and stayed with the project until it closed in June 1984.
1. **Introduction**

The CERN Intersecting Storage Rings (ISR) was the world’s first proton-proton collider (Figure 1). It was commissioned in 1971 and worked as a collider until 1983. It then ran for a further year with a 3.5 GeV/c antiproton beam in a single ring with a gas-jet target before closing in June 1984. The ISR was a very successful machine that acted as a strong stimulus to accelerator physics as well as contributing to high-energy physics. The critics of ISR point to the Nobel prizes for the $J/\psi$ (1974) and tau (1976) that were within the energy range of the ISR, but were discovered at the lower-budget machine SPEAR at Stanford. Similarly, it was Fermilab that discovered the upsilon and bottom quark. The critics tend to overlook physics discoveries such as the proton-proton rising cross section, that were apparently less media-worthy, and the wide range of advances in machine physics such as stochastic cooling.

The best source for all types of reports and images about ISR is the CERN document server [http://cds.cern.ch](http://cds.cern.ch). For the present paper, I have drawn heavily on Refs [i] and [ii] for general topics, but for the technical topics I have listed specific references.

2. **Historical background for the machine**

In the early 1900s, Europe was the cradle of particle accelerators but, in 1929 when Ernest Lawrence picked up a paper by Rolf Widerøe on a proof-of-principle linac, the cyclotron or “coiled” linac was born\(^{24}\) and the ‘pendulum’ for accelerators swung away from Europe towards Berkeley on the US west coast. By 1947, the Brookhaven National Laboratory (BNL) had been founded on the US east coast and Europe slipped further behind. In 1952, BNL commissioned its 3 GeV Cosmotron, while Birmingham, UK launched their 1 GeV machine a year later. There was however one bright hope for Europe. The convention for a European accelerator laboratory called CERN was ratified on 29 September 1954 by 12 countries. This was the start of a long and friendly competition between the two sides of the

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\(^{24}\) This was almost classifiable as a mistake. Widerøe and Eugen Flegler had already discussed the use of a magnetic field as early as 1924, but were dissuaded by doubts about orbit stability. It was said that Lawrence would not have continued had he known the Europeans’ conclusion that the cyclotron would not work.
Atlantic. First, CERN commissioned its 28 GeV proton synchrotron (PS) in 1959, only to be upstaged one year later by the BNL 33 GeV AGS machine. Tacitly, the world leadership at that time was determined by the highest energy proton machine and the question on the table in 1960 was what should CERN do next?

The European community was split. Was it to be the unsure promise of a collider to be called the ISR, or the conventional option of a more powerful accelerator dubbed the 300 GeV machine? For the adventurous the answer was the ISR. The centre-of mass energy available for physics would be twice that of the primary beam. Furthermore, with the technology of the day, there was no hope of building an accelerator powerful enough to match that performance on a fixed target. The collider concept was not new. It had been patented by Rolf Widerøe in 1943 and its advantages were well known. Furthermore, the simpler, single-ring, electron-positron version was demonstrated by AdA in 1961 and others followed. The opposition was quick to point out that ISR could not provide the secondary particle beams they wanted and the machine itself posed fundamental problems that were seemingly insoluble. The experts attributed the success of electron-positron colliders to radiation damping. Firstly, synchrotron radiation reduced the beam sizes and energy spread, so that the number of collisions was increased to a useful level for physics and secondly, it damped resonances and dissipated mishaps such as injection errors. Sadly, radiation damping was missing in the proton scenario. In short, a proton-proton collider could never work. The ISR would be an expensive mistake that funding agencies would be unlikely to forgive.

The viability of the ISR was a difficult question because in essence the doomsayers were right, but in practice some new techniques, combined with a parameter space that was more forgiving than expected, would lead to workable beams over many tens of hours of operation. In fact, one important step was already in place. In 1956, MURA the Mid-Western Universities Association in the US had proposed RF stacking to increase the beam intensity, opening the way to workable luminosities in colliders such as the ISR. This key technique was tested by CERN in a specially-built 24 m circumference 2 MeV electron ring (CERN Electron and

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25 Owing to WW II, the patent was not published until 1953.

26 AdA was built in Frascati in 1961 under the guidance of Bruno Touschek. It was taken to Orsay in 1962 where there were the facilities to inject beams and the first collisions were recorded in 1963.
Accumulation Ring – CESAR) that scaled to the ISR at 25 GeV. In 1965, although many questions were still being debated, the CERN Council courageously decided in favour of the ISR\textsuperscript{27}. This opened the door to 13 years of operation (1971-1984) filled with rapid advances in accelerator technology and a revolution in the way HEP experiments would be conducted.

3. **Historical background for physics**

In the early 1960s, there was a controversy over the general trend of hadron behaviour at high energies and this influenced the ISR experimental equipment and proposals that were accordingly biased towards small angle scattering. Indeed, it was rather disparagingly said that the ISR was an expensive small-angle scattering experiment for the PS. The ISR was duly rewarded with the first results on the rising total cross-section in 1971 followed by further results from different experiments up to 1978. Indeed, this was one domain that the Americans could not reach. One daring experimental set-up that was pioneered at the ISR used “Roman pots”. These were thin-walled vacuum chambers housing scintillators that approached within millimetres of the beam to measure the small-angle scattered particles. This led Carlo Rubbia to refer to the ISR programme as “key-hole physics”. The consequence was that during the 1970s the ISR detectors were not designed to observe events with large transverse momenta and the ISR was disadvantaged for discoveries such as the $J/\psi$ and tau. The ISR experimental programme was still important, but the $J/\psi$ and tau proved more news-worthy even against the elusive “pomeron”.

4. **Advances in accelerator design and technology**

4.1. **Advances in lattice design**

*The ‘1965’ lattice*

Figure 2 shows the original ISR lattice functions from the centre of an outer arc to the centre of an inner arc. The underlying lattice is a FFDD structure, but in the crossing regions and inner arcs the lattice is opened between the F units to form FDDF cells. The longer drift spaces are a welcome innovation compared to the tightly-packed FD-DF cells of the PS. The ISR magnets

\textsuperscript{27} We note that the 300 GeV project was only delayed by ISR and was approved in December 1970 as the Super Proton Synchrotron (SPS). This was an important step because the SPS was to become part of the injection chain for the CERN LHC (son of ISR).
Figure 2. Design lattice functions of the ISR (based on ISR Parameter List Rev. 5 CERN/ISR-GS/76-4)

Figure 3. ISR main magnet model (1965 CERN Document Server)
are, however, still combined-function of the open ‘C’ type (Figure 3), a legacy from the days of weak–focusing. Note that the betatron amplitude functions are very rounded. This is due to the spread-out gradients of the combined-function magnets. The lattice has been manipulated globally to fit the interlaced geometry and to provide space for physics equipment in the interaction regions. The ‘split-F’ structure provides local betatron minima at the crossing points, although these are not as low as would have been liked. There are no dispersion-free regions or low-β insertions as the local customization of a lattice (i.e. insertions) had still to be developed.

**The ‘1977’ SCISR upgrade lattice**

![Figure 4. Lattice functions of the proposed superconducting ISR upgrade (Ref. [3])](image)

Some years later in 1977, a project was published to convert the ISR to a superconducting machine, SCISR [iii]. Figure 4 shows the lattice functions in the outer arc for this conversion and Figure 5 shows the cross-section of the new quadrupole. Immediately, one sees the advances that have taken place. The arc is a tightly-packed FODO structure, terminated by a dispersion suppressor and matched into a low-β insertion, which is exactly how the job would be done today. The use of separated-function magnets provides more efficient focusing with clearer features in the shapes of the lattice functions, and the superconducting quadrupole, using the Roman arch principle to support the coils, is right up to date. This is just one illustration of the rapid changes mentioned in the introduction that were to take place during the brief 13-year life of the ISR. Eberhard Keil [iv] had
proposed the elegant method used for the dispersion suppressor and, although low-\(\beta\) insertions were not an ISR invention, the matching was based on an analytical solution for a variable-geometry triplet published by Bruno Zotter \([v]\) in 1973. This is perhaps the most useful of all the analytical matching modules ever published.

### 4.2 Conventional steel low-\(\beta\) insertion

By 1974, the concept of a local insertion had been demonstrated in the ISR by a conventional low-\(\beta\) insertion built in Intersection 7 using largely borrowed quadrupoles from the CERN PS, DESY and the Rutherford Appleton Laboratory. Since the ISR had no dispersion-free regions and the lattice functions were not regular, matching the low-\(\beta\) was a significant challenge, but the result was highly successful and increased the luminosity by a factor of 2.3 \([vi]\). The conventional low-\(\beta\) insertion was initially an ‘experiment’ to test the fear that the marked super-periodicity of unity would cause the ISR beams to be unstable or noisy. In reality, this did not prove to be an issue and two years later, in 1976, the insertion was demounted and moved to Intersection 1, where it was used in conjunction with a superconducting solenoid until the ISR closed.

### 4.3 Lattice programs

During the construction of the ISR, lattice computations were made with the programs SYNCH \([vii]\) (LBL) and AGS \([viii]\) (CERN). By the time ISR
closed in 1984, AGS had been replaced by MAD [ix] (Methodical Accelerator Design), which is now a de facto world standard for the design of large synchrotrons.

4.4 Coupling

ISR also contributed strongly to the theory and design of coupling compensation schemes. A complete Hamiltonian theory for sum and difference resonances was published in 1976 by Gilbert Guignard [x]. It later turned out that Phil Morton from SLAC had reached many of the same results in an unpublished and unfinished note [xi]: a story that is similar to those of Rolf Wideröe and his betatron, and Lee Teng, who did not publish his rotator for medical gantries. Since there were no dispersion-free regions in the ISR, the compensation scheme for the global coupling was of a special and unique design [xii]. Similarly, the physics solenoid in Intersection 1 had horizontal slots in its end plates to accommodate the beams that crossed at an angle. This new feature was described analytically and compensated. The ISR was also first to be equipped with an electronic coupling meter that directly gave the modulus of the coupling coefficient defined in the theory [xiii].

4.5 Advances in magnet technology

Superconducting quadrupoles for a low-β insertion

It has already been mentioned that a superconducting upgrade for ISR was published in 1977. This upgrade was not approved, but another project to build a superconducting low-β insertion [xiv] had already been accepted. At that time, CERN made an important decision not to outsource the magnets and cryogenics to a member state laboratory such as Rutherford, UK, but rather to start accumulating in-house expertise, which was later to be important for the LHC. A number of superconducting magnets already existed in transfer lines in various laboratories, but nobody had operated superconducting quadrupoles in the lattice of a synchrotron. CERN then made a second important decision to build the models and prototypes in-house in order to reduce the new technology to a detailed engineering specification. On the basis of this specification, tenders were then invited from industry for series production. This was a middle-of-the-road approach between building everything in-house, as is often done in US laboratories, and issuing a performance specification to industry, as some Member States wanted. In the ISR approach, the magnetic design is the
responsibility of CERN and the manufacturer is only required to respect the tolerances, choice of materials and adherence to the various qualified procedures. The superconducting low-\(\beta\) was a great success, increasing the luminosity by a factor of 6.5 \([xv]\) and laying the foundations for the LHC magnets and cryogenics. This was the first time that industrially-built superconducting quadrupoles had been operated in a synchrotron lattice for regular operation. In a typical run, the magnets would operate at top energy for 60 hours.

At the start of a physics run in December 1982, the record luminosity of \(1.4 \times 10^{32} \text{ cm}^{-2} \text{ s}^{-1}\) was measured in the superconducting low-\(\beta\) insertion. This record was not beaten until 1991 by CESR at Cornell with \(1.7 \times 10^{32} \text{ cm}^{-2} \text{ s}^{-1}\).

**Physics detector magnets**

In 1965, colliding beam physics was more or less a blank page. By the late 1970s, detector magnets with \(4\pi\) acceptance were standard equipment in the ISR. A superconducting solenoid was installed in Intersection 1, the Open Axial Field Magnet (OAFM) was in Intersection 8 and an air-cored toroid was in Intersection 6. This was effectively a single jump to today’s technologies in just a few years. The OAFM is shown in Figure 6 because it is perhaps the least well known of the examples.

![Figure 6. The Open Axial Field Magnet in Intersection 8 (1979 CERN Document Server)](image-url)
4.6 Vacuum

**Base vacuum**

The original design criterion stated that the lifetime imposed by gas scattering should be at least one order of magnitude longer than the time needed to fill the rings. This was interpreted as $10^{-9}$ torr in the arcs (using ion pumps) and $10^{-10}$ torr in the crossing regions (using cryo-pumps). The chamber itself was to be stainless steel bakeable to 300°C to 350°C, although initially it was only baked to 200°C. This made the ISR the world’s largest ultra-high vacuum system, which was an enormous challenge for the technology at that time. In fact, the pressure in the arcs was $10^{-10}$ torr (beams < 2 A) for the first run in January 1971 and $10^{-11}$ torr (beams < 2 A) was quickly reached in the crossing regions in 1972. In subsequent years, the pressure fell steadily to an average around the machine close to $10^{-12}$ torr. This meant that the principal source of background in the experiments came from beam losses in non-linear resonances. This led to hundreds of hours of machine development investigating working lines in the tune diagram, halo scraping exercises and tests with cooling.

**Beam-induced vacuum instability**

As the beam current increased, the residual gas was ionized and positive ions were repelled by the beam to crash into the chamber walls. This released adhered gas leading to a runaway effect that could cause catastrophic beam loss. A staged programme to improve the vacuum was started in 1971 and progressed over several years. Baking at 350°C, instead of the initial 200°C, helped and some 500 additional titanium sublimation pumps were added. The vacuum system was demounted arc by arc during shutdowns and cleaned using a new technique called glow-discharge cleaning. Later this was done *in situ* during bakeout by using the clearing electrodes to excite an argon discharge. Incredibly, a glow-discharged chamber could be opened to the air and left for many hours and still recover its ultra clean condition when pumped. This was the first demonstration of the efficiency of glow discharge cleaning on a large-scale [xvi].

**Neutralization from ionization of residual gas**

The unwanted neutralization caused by electrons trapped in the potential well of the beam following ionization of the residual gas caused tune shifts
and eventually beam instability. Bunched beams could flush out such regions, but for the coasting ISR beams it was necessary to install clearing electrodes. Although the ISR design had foreseen hundreds of these, the inadequacy of the clearing system was already evident in 1971 and many more electrodes were added. When the ISR became operational, it provided the perfect test bed for measurements and many papers were published on neutralization tune shifts, e-p instabilities, electron removal by RF clearing and ion clearing in anti-proton beams. A good pedagogic account is to be found in Ref [xvii].

**Thin-walled vacuum chambers**

Another pioneering activity in the ISR was the design and production of large, thin-walled vacuum chambers for the intersection regions. Typically the chamber walls were 0.28 mm to 0.4 mm thick and the materials used were stainless steel and titanium. Beryllium was considered, but never used.

**4.7 Operation**

**Computer control**

In 1965, the new dual Ferranti-Argus computers with a 16k core store of 24-bit words and a 1 μs store cycle time to a 640k disk store were not fully trusted, and the control room was equipped with manual control panels. By the mid-term of the ISR attitudes had completely changed and a highly-sophisticated control system was in place. Manual interventions were discouraged and automated procedures dominated operation.

**Closed-orbit correction**

One of the first operations to be carried out in any machine is the correction of the closed orbit. Of the two methods developed for the ISR, the algorithm MICADO [xviii] written by Bruno Autin was the more efficient. After some years of development MICADO became a de facto standard for many laboratories. The most advanced versions now have feedback loops and can be found in synchrotron light machines. A subject closely related to closed-orbit correction is injection optimization and the ISR also had a sophisticated automatic injection procedure [xix].
Luminosity calibration method

The luminosity calibration method, which is still used today in LHC, was invented in 1968 by Simon van der Meer, specially for the ISR [\textsuperscript{xx}]. The two beams are moved relative to each other in the vertical plane so that one beam effectively sweeps through the other while the interaction rate is monitored. The closed-orbit bumps used to move the beams were corrected for the coherent beam tune shift [\textsuperscript{xxi}] and hysteresis in the magnets [\textsuperscript{xxii}].

RF stacking

RF stacking in momentum space was an essential technique for accumulating high beam intensities. In this scheme, proposed by MURA and tested in CESAR, the PS beam was accelerated in the ISR and the first pulse was deposited on the highest acceptable momentum orbit in the relatively wide vacuum chamber. Subsequent pulses were added at lower momenta until the vacuum chamber was filled down to an orbit close to the injection orbit in the inner half of the chamber.

Phase displacement acceleration

Much of the operating life of the ISR was at 31.4 GeV/c, the maximum energy the magnet system could reach. After stacking at 26 GeV/c the coasting beams were accelerated by the novel method of phase-displacement acceleration first suggested by MURA, but first extensively used in the ISR. The technique consisted of moving empty buckets repeatedly through the stacked beam from high to low energy. In accordance with longitudinal phase-space conservation, the whole stack was accelerated while the magnet field was increased to keep the stack centred. One amusing feature of this system was that the current would increase slightly as a beam was accelerated.

4.8 Diagnostics

Schottky noise and stochastic damping

If one key discovery in the field of accelerator physics has to be singled out then it would be Schottky noise - a statistical signal generated by the finite number of randomly distributed particles in a beam - which is well known to designers of electronic tubes. Van der Meer had worked on stochastic
damping in 1968, but his ideas had seemed too far-fetched at that time.

This changed in 1972 when Wolfgang Schnell looked for and found the longitudinal and transverse Schottky signals at the ISR [xxiii] (Figure 7). This prompted van der Meer to publish his work from 1968 about Schottky noise and the possibility of stochastic damping [xxiv].

The longitudinal Schottky signal made it possible to measure the current density in the stack, without perturbing it, as a function of the momentum (transverse position), while the transverse Schottky signals gave information about how the density of the stack varied with the betatron frequency, or “tune”. The combination of the two signals yielded a complete picture of the beam in the tune diagram.

**Stochastic damping (cooling)**

The possibility of damping the betatron oscillations was experimentally demonstrated in the ISR in 1974 [xxv] (Figure 8). Stochastic cooling was the decisive factor in the conversion of the SPS to a p-pbar collider and hence

Stochastic cooling became the cornerstone for the success, not only of the p-pbar collider in the SPS, but also for the more powerful Tevatron at Fermilab. CERN’s Low Energy Antiproton Ring and the Antiproton Decelerator, as well as similar programmes at GSI and at Brookhaven, also owe their existence to stochastic cooling.

**Direct current beam transformer**

The ISR was also the home of the zero-flux, direct-current transformer [xxvi] (DCCT). This device became another de facto world standard. Beam current monitors of this type developed at CERN in 1981 and 1990 became national primary standards in Germany, certified and operated by the PTB (Physikalisch-Technische Bundesanstalt) in Berlin.

4.9 Beam-chamber interaction

‘Brickwall’ instability

Shortly after the start of the ISR a coherent transverse instability was observed that limited the beam intensity. This phenomenon was dubbed the ‘brick wall’ instability [xxvii]. The intensity would increase while stacking to around 3 A where there would be a sudden loss of ~15% of the beam after which stacking would resume only to have a repeat beam loss at around 3 A. This gave the appearance of hitting a ‘brickwall’. The phenomenon was due to the resistive wall instability [xxviii] and it had been correctly
predicted that a tune spread of 2 would stabilize the beams \(^{xxix}\). The additional tune spread was applied and the effect disappeared. Studies of the shape of the working line in the tune diagram revealed how the action of tune shifts induced by space-charge loading could destroy the tune spread and hence the stability of the beam. From these beginnings, the ISR spawned many papers on incoherent and coherent space-charge tune shifts on central and off axis orbits in variously shaped chambers and the correction of these effects.

**Pre-calculated and on-line space charge corrections**

After diagnosing the “brick wall” instability, a procedure \(^{xxx}\) was developed to reach higher circulating currents by ‘pre-stressing’ the working line in the tune diagram for every 3 A of beam current added, so that the line would assume an ideal shape for stability once loaded. Initially, this was done by dead reckoning, but once longitudinal Schottky scans became operational the current density could be measured at any time, the tune shifts with radial position calculated and the necessary poleface winding currents calculated and applied. This procedure was unique and highly successful. The maximum current recorded in a single ring was 57 A at 26 GeV/c and physics beams were typically 30-40 A at 31.4 GeV/c.

**Impedance and stability criteria**

The ISR was the ideal machine for studies on beam impedance and stability. There were many publications, but some notable topics were the now widely-used longitudinal Keil-Schnell criterion \(^{xxxi}\), the Schnell-Zotter criterion for the transverse plane \(^{xxxii}\) and Landau damping.

**4.10 Beam-beam interaction**

In 1973, Eberhard Keil wrote a short note entitled “Why be afraid of magnet imperfection resonances in high luminosity storage rings?”\(^{xxxiii}\). He was referring to the beam itself being, in most cases, a stronger source of resonance excitation and one that excites all orders since a gaussian distribution can be expressed as an infinite power series. The ISR was a unique test bed for the study of coasting and bunched beams colliding at a small angle. These results were compared at length with those from electron-positron machines and helped to guide the design of future colliders. A large number of papers were published and a useful
introduction is given in Ref \[\text{xxxiv}\]. As part of the beam-beam studies the ISR was also equipped with a non-linear lens \[\text{xxxv}\].

4.11 Other topics

The above is far from an exhaustive list. The ISR was also used for the study of intra-beam scattering and overlap knockout resonances. Other examples of special beam equipment are the transverse feedback systems and the scrapers for beam cleaning. Today there is a strong emphasis on technology transfer and ISR also has examples of this such as the Digital Teslameter designed by Klaus Brandt and commercialized by a company in Geneva \[\text{xxxvi}\].

5. Conclusion

ISR was a major project with an experienced staff that had the critical mass to spontaneously generate new ideas and the capacity to follow them up. It attracted students, fellows, experts on sabbatical leave and many others, all of whom helped to catalyze progress. The timing of the project was such that the world-wide community was poised to advance to what we would now recognize as ‘modern’ accelerators. Much of what has been described depends strongly on the particular attributes of the ISR and the proton beams. Schottky noise exists in a bunched beam, but it is less likely to be discovered. The large momentum spread in the ISR pushed the studies of tune shifts, coupling and chromaticity schemes into more detail. The colliding beam geometry was essential for beam-beam studies. Colliding beam physics would have been held back without this full scale test stand for experimentation. In short, ISR looked into the part of parameter space that would be needed for the next step in HEP. It was the right machine at the right time and an investment in the future. This appears logical now, but it was a brave decision at the time.

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Antimatter

At the end of the 19th Century, a number of published papers were on the theme of “antimatter”, for example two Nature papers by Arthur Schuster, in 1898, in which the “anti-” might refer to gravitational repulsion by ordinary matter. This was years before the discovery, in 1911, of the atomic nucleus by Ernest Rutherford. Nowadays such speculations would not get past the referee.

A scientific basis for antimatter came with the theoretical prediction of antiparticles by Paul Dirac in 1928 and the discovery of positrons in cosmic rays by Carl Anderson in 1932. Only forty years later positrons were already being used as a medical diagnostic in PET scanners.

Antiprotons were discovered at the Bevatron accelerator in Berkeley in 1955 by Owen Chamberlain, Emilio Segre, Clyde Wiegand and Tom Ypsilantis. The production of antiprotons by the 6 GeV internal proton beam colliding with a stationary target were only just above threshold, and the rare antiprotons were selected by momentum, time of flight and Cherenkov radiation from the background particles which were $10^5$ times as numerous. As far as I can tell, about 1 antiproton was found every 15 minutes, and the discovery publication was based on about 50 antiprotons. Chamberlain and Segre received the Nobel Prize for this work. Later they were sued, unsuccessfully, by Oreste Piccioni who claimed that they had used his ideas.

Only 15 years later accelerator and beam techniques had advanced significantly, and we were able to construct a low energy separated beam at CERN giving $\sim 10^9$ antiprotons per day, accompanied by only about the same number of unwanted particles, and a further 15 years later, with the collider, we had a pure beam of $\sim 10^{12}$ antiprotons circulating for more than a day.

The Collider

The suggestion that an existing large proton accelerator (at Fermilab or CERN) could be converted into a collider by turning it into a storage ring with counter-rotating antiprotons, came in a remarkable paper by Carlo
Rubbia, Peter McIntyre and David Cline. The paper showed that this would be used to search for the intermediate vector bosons, required by theories of electroweak unification, However the paper was rejected by Phys Rev Lett, and later was published in the proceedings of the Aachen Neutrino Conference (1996).

Since antiprotons are not readily available, they have to be produced in collisions of protons with a target. Typically using the 25 GeV CERN Proton Synchrotron (PS), around 1 useable antiproton is produced per million protons on target, and the antiprotons come with large spreads of energy and angle. In order to inject these into the PS they would need to have these spreads reduced: they would need to be “cooled”. In practice the 6-dimensional phase space of the antiproton beam would need to be reduced by a factor of around $10^9$ (~ $10^2$ horizontal; $10^2$ vertical; and $10^5$ momentum spread). Rubbia was able to persuade the CERN Directorate to check cooling, and if successful to embark on the conversion of the Super Proton Synchrotron (SPS) into a collider.

**Antiproton cooling**

Two methods seemed possible. In 1966 Gersh Budker and Alexander Srinsky at Novosibirsk suggested electron cooling. A well-ordered “cold” beam of electrons, with the same mean velocity as the antiprotons, travels in a straight section, overlapping the “hot” beam of antiprotons. Electromagnetic interactions will cool the antiproton beam at the expense of heating the electron beam.

The other method is stochastic cooling. This was first mentioned in 1968 by Simon van der Meer, and published in 1972. The effect was first demonstrated at the CERN Intersecting Storage Rings (ISR) in 1974. Both methods were then tried in an Initial Cooling Experiment (ICE), which was put together in less than a year using magnets from a completed “g – 2” experiment. The stochastic method was chosen for the collider.

The principle can best be illustrated in the horizontal plane Fig 1. Particles travel around a ring, performing betatron oscillations about the ideal orbit. Pick-up electrodes are placed at a position of maximum deviation, and detect the displacement of the beam. A super-fast signal is sent across a chord of the ring to a kicker positioned an odd number of quarter oscillation cycles later, so as to arrive at the same time as the particles and correct the angle. In an idealised system, the orbit of every individual particle could be
corrected. In practice the detector and kicker electrodes act on the centroid of the beam. Some orbits are improved, others worsened: hence “stochastic”. Simon van der Meer showed under what conditions an overall improvement can be obtained.

The antiprotons are produced by extracting 26 GeV protons from the PS, hitting a target, selecting 3.5 GeV/c negative particles, focussing them with a magnetic horn (another invention of van der Meer, widely used in neutrino beams) and feeding them into a large aperture ring, about 160 m in circumference (one quarter of the PS).
In this ring, called the Antiproton Accumulator (AA), antiprotons are accumulated and cooled. The AA was designed and built in record time. Its action is illustrated in (Fig 2). Pictures of the AA during construction, and some years later with the Antiproton Collector (AC) are shown in Fig 3.

**Beam Gymnastics**

It took 24 hours or longer to accumulate a sufficient number of antiprotons. The dense part of the stack was then extracted, sent into the PS, accelerated up to the full PS energy (26 GeV), and then transferred into the SPS. Prior to this a single bunch of protons from the PS had been injected. Note that this arrangement made it unnecessary to change the polarity of the PS magnets between accelerating protons and antiprotons. See (Fig 4). The counter-rotating bunches were then accelerated to 270 GeV, and were stored for around a day and allowed to collide in two positions where experiments could record them. Although the SPS could reach 400 GeV in its normal pulsed mode, cooling of the SPS magnets limited the energy per beam to 270 GeV (540 GeV collision energy) for DC operation, later raised to 630 GeV collision energy when more cooling was provided. After a few years an additional larger aperture ring, the Antiproton Collector, was added in the AA hall to increase further the number of stored antiprotons...
The luminosity of the Collider (a measure of the rate of collisions) increased from $\sim 10^{27} \text{ cm}^{-2} \text{ s}^{-1}$ in 1981 when it was switched on, to $\sim 2 \times 10^{30} \text{ cm}^{-2} \text{ s}^{-1}$ when it ceased operation in 1990. The integrated luminosity per year went up by an even greater factor of about $2 \times 10^4$.

The Experiments

The whole project was carried out very rapidly. A proposal for a “universal” detector was submitted in January 1978 from a collaboration (later named UA1 “Underground Area 1”) headed by Rubbia. This was signed by 52 physicists including 18 from 3 UK institutions (Birmingham, Queen Mary, Rutherford Lab). By June 1978 the collider project and UA1 were approved, followed soon after by four more experiments: UA 2, 3, 4, 5. A timeline for the project is shown in Fig 5.
UA2, shown in Fig 6, like UA1, was also a general purpose detector, and both were designed to search for the W and Z. UA3 was a search for magnetic monopoles by looking for dense ionisation damage in thin plastic (Kapton) sheets. These were placed in and around some of the UA1 apparatus. UA4 looked at small angle elastic scattering using “Roman pots” – small detectors that could be pushed close to the circulating beams, 40 m from the collision point.
It was felt, within the CERN Directorate, that UA1 would not be ready for physics when the collider started, and hence UA5 was approved. This experiment included physicists from Cambridge. The apparatus (Fig 7 above) consisted of a large pair of streamer chambers, one above and one below the beams, designed to be ready for first collisions and first physics results and then to be moved out to make way for UA2. Fortunately it turned out that both UA1 and UA5 were ready, and published the first physics results, in 1981, a few weeks after the initial collider start-up.

Fig 8

A sketch of the UA1 apparatus is shown in Fig 8. (Some components placed many metres downstream are not in this diagram). The collisions occur in the vacuum pipe of the collider, and cannot be seen, but outgoing particles can be observed in various layers of equipment. A large multiwire image chamber surrounded the collision point and showed the tracks of charged particles. This was surrounded by a lead and scintillator electromagnetic calorimeter in which the energies and directions of electrons, positrons and photons were registered. This in turn was surrounded by a hadron calorimeter, which consisted of about 7000 large plastic scintillator sheets embedded in the iron of a large dipole magnet. This magnet provided the field which allowed momentum and sign of tracks in the image chamber to be measured. Muons penetrate all these layers, giving signals, and are then identified in large chambers surrounding the apparatus. Neutrinos leave no signals in any part of the apparatus and are identified by an imbalance of “transverse energy”. Since the colliding quarks and antiquarks have little
transverse momentum, this is conserved after the collision. A picture of UA1 before the addition of the muon chambers is shown in Fig 9.

![Fig 9](image)

The other multipurpose experiment, UA2, was quite different, and is not detailed in this short article. Both UA1 and particularly UA2 were upgraded during the years that they were active. Another UK group, Imperial College, joined UA1 in around 1984.

The collider project was extremely productive, and the results were impressive at the time. Well over a hundred papers were published in refereed journals, and many more appeared in conference proceedings, PhD theses, and elsewhere. However this talk, and the short written version, were not about the particle physics results, which have mostly been superseded by later colliders (Tevatron at Fermilab, LEP and LHC at CERN).

I will end with some anecdotes, which do not get into the refereed literature. The UA1 apparatus was assembled in a pit (“the garage”) adjacent to the collider ring, and was then pushed into the collider on rails through an opening. UA1 was made by twelve groups, and it was perhaps remarkable that it all fitted together. It was also almost (but not quite) able to pass through the opening. It was a few millimetres too wide!
A technician had to grind off this excess as shown in (Fig 10). This picture has pedagogic value. It shows, decades after the event, tracks of particles (white-hot flecks of iron) which are bent in a field (gravity). From the trajectories of the particles (and also from the camera shutter speed) various properties can be calculated.

During 1981-1982 I was resident at CERN. The UK groups received a cryptic message from the Directorate. It stated that “A senior UK person will be visiting CERN and UA1 on 12 August 1982. We cannot tell you who, but she is very important”. Just before the visit, a flood occurred in the UA1 pit, many centimetres deep. This had to be pumped out in a great hurry, and the equipment dried. Before the visit CERN was swarming with Swiss and French police, and I had to help them check that there were no explosive devices hidden in our apparatus.

On the stated date UK Prime Minister Margaret Thatcher arrived with her husband. She had stated that this was a private visit, and she came as a scientist. I note that she did have a chemistry degree and had worked in the research labs of J.Lyons, the catering and confectionery company, on properties of ice cream.
Alan Astbury, the co-spokesperson of UA1, and I showed her around our experiment. Alan gave a short presentation, which ended on a cautiously optimistic note: “Well Prime Minister, if we are lucky and there is a Santa Claus, we will discover the W particle by Christmas”. “Right” said the Iron Lady “I will contact you to see if you have found it”.

She did not say what might happen to our funding if we did not. Fortunately the W was discovered in January 1983. Mrs Thatcher was kind enough to write letters of congratulations to Alan and myself. In the letter to Alan she wrote “I am not sure which is more exciting, the glimpse you have had of the W particle, or the knowledge that Santa Claus really does exist”. To me she congratulated in somewhat nationalistic tones “I am sure that British physicists will be among the first to unify all the basic forces”. I did not have the heart to tell her that the 8 scientists from Queen Mary included two American physicists, one Canadian physicist, one Italian postgraduate and one with a Greek mother, and I was born in Czechoslovakia!

The W discovery was published by UA1 in January 1983 (1), and almost immediately also by UA2 (2). The Z was found by both groups later that year (3), (4). Hence we had the experimental verification of electroweak unification. We were delighted that Carlo Rubbia and Simon van der Meer, who made the greatest contributions to our adventure, were awarded the 1984 Nobel Prize for Physics (Fig 12).
References:

(1) Phys Lett 122B p.103 24 Feb 1983
(2) Phys Lett 122B p.476 17 Mar 1983
(3) Phys Lett 126B p.398 7 Jul 1983
(4) Phys Lett 129B p.130 15 Sep 1983