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Laboratori Nazionali di Frascati

FRASCATI PHYSICS SERIES

BRUNO TOUSCHEK
MEMORIAL LECTURES

Frascati, May 11, 1987

Editors

M. Greco, G. Pancheri

BRUNO TOUSCHEK MEMORIAL LECTURES

FRASCATI PHYSICS SERIES

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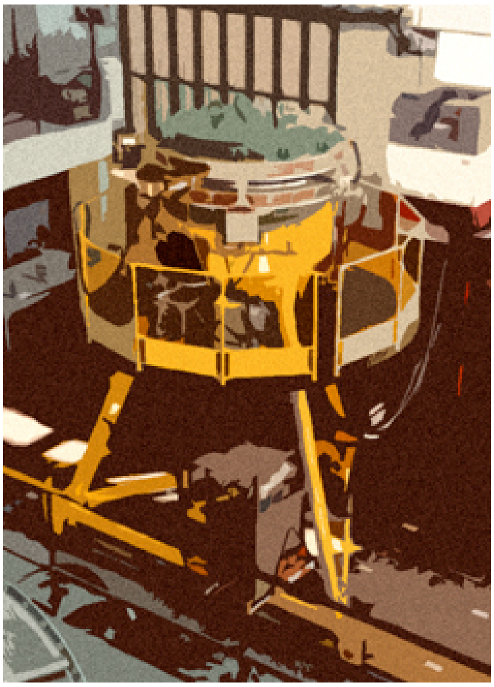

Laboratori Nazionali di Frascati dell'INFN
Frascati, May 11, 1987

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EDITORS' NOTE

In 1987, at Frascati National Laboratories of INFN, together with Etim Etim, who had graduated with Touschek in 1967, we decided to establish a series of Lectures in honour of Bruno Touschek, to be called "Touschek Memorial Lectures". We planned to start them with three days of Lectures by John Bell, who had been a friend of Bruno Touschek and shared many interests with him. To inaugurate the series, we invited a number of Italian and foreign physicists who had known Touschek and who could recall his personality and achievements.

We had planned to record these recollections and publish them in a Memorial volume. To this aim, we gathered contributions from the physicists who had spoken at these Inaugural Lectures in memory of Touschek. Some of these contributions were sent to us in written form, others were transcribed from tapes. Unfortunately, circumstances did not allow us to carry this project through.

Seventeen years later, we have decided to go back to this material and publish it as it was, adding some photographs and drawings by Touschek. In the Appendices we have reproduced some of his unpublished papers which are witness to his achievements and personality. We also reproduce his last paper published in Physics Letters. We believe that this collection of writings, albeit incomplete and somehow dated, is still very relevant, in order to remember Touschek through the written words of those who knew him. In its present form, it is directed to the young researchers who will attend the Frascati Spring School “Bruno Touschek”, named after him a few years ago.

The graduates students and young post-docs, who attend the School, do not know much about him and we think it is important for them to get acquainted with Touschek, and understand how much he has contributed to our present day physics. The history of particle accelerators and its contribution to human welfare is still relatively unknown. We hope this small volume might give its contribution, by telling part of the story of the man who invented electron-positron collisions. It is also the story of a great European, who was born and died in Austria, and, in between, worked in most of Europe, from Germany to Scotland, to Italy and France. This volume also tells an important piece of the story of elementary particle machines and its development in Italy and France.

We are thankful to those who helped us preparing this volume, Luigina Invidia for her untiring support and technical assistance and Claudio Federici for the photographic and digital collaboration. Also we wish to thank Etim Etim, who has greatly contributed to the planning and realization of the Lectures and followed the first draft of these notes.

Frascati, April 26, 2004 Mario Greco and Giulia Pancheri

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BRUNO TOUSCHEK MEMORIAL LECTURES

INFN - Laboratori Nazionali di Frascati

Frascati 11-15 May, 1987

The exact principles of Quantum Mechanics

By

JOHN S. BELL

CERN, Geneva, Switzerland

The Frascati National Laboratories of the Italian Institute for Nuclear Physics (INFN) are instituting a yearly Memorial Lecture to honour BRUNO TOUSCHEK. The inauguration of the series, by Nicola Cabibbo, the President of INFN, will take place in Frascati on 11th May, 1987.

Inauguration Day

E. Amaldi	Bruno Touschek - The legacy of the man and the physicist
	Open Session: Remembering Bruno Touschek
J. Bell	The exact principles of Quantum Mechanics
B. Richter	e^+e^- colliding beam physics - Status and future prospects

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BRUNO TOUSCHEK MEMORIAL LECTURES

11-15 May, 1987

INAUGURATION CEREMONY

Programme

Monday 11 May, 1987 - Tokamak Hall (ENEA)

9.30 - 9.45	Welcome address by the Director of the Frascati National Laboratories S. Tazzari		
9.45 - 10.15	Inauguration of the Bruno Touschek Memorial Lectures and introduction of the first lecturer, Prof. John Bell, by the President of the Italian Institute of Nuclear Physics (INFN) N. Cabibbo		
10.15 - 11.00	E. Amaldi Bruno Touschek – The legacy of the man and the physicist		
11.00 - 11.30	*Coffee Break*		
11.30 - 13.30	*Open Session:* Remembering Bruno Touschek		
	Chairman: M. Conversi		
	C. Rubbia	The role of Bruno Touschek in proton-antiproton collider physics	
	F. Amman	The Adone Storage Ring	
	G. Salvini	Matter-antimatter collisions from Frascati to the outside world	
	R. Gatto		
13.30 - 15.00	*Lunch Break*		
15.00 - 16.30	*Open Session:* Remembering Bruno Touschek		
	G. Morpurgo	My work with Bruno Touschek	
	C. Bernardini	The AdA Storage Ring	
	U. Amaldi		
	G. Sacerdoti		
	G. Pancheri	Bruno Touschek and the Frascati Theory group	
16.30 - 17.00	*Coffee Break*		
	Chairman: C. Rubbia		
17.00 - 17.50	J. Bell	The exact principles of Quantum Mechanics: I. Quantum Mechanics, an inexact science	
17.50 - 18.40	B. Richter	e^+e^- Colliding Beams - Status and Future Prospects	

The lectures by J. Bell will continue in the INFN main lecture hall as follows:

Tuesday 12 May	11.00-12.00	II.	Exactness with Complementarity
Wednesday 13 May	11.00-12.00	III.	Exactness with Nothing but the Wave Function

11 May 1987: Inauguration of the Bruno Touschek Memorial Lectures.

1987 INTRODUCTION OF THE BRUNO TOUSCHEK MEMORIAL LECTURES

Bruno Touschek died nine years ago, on the 25th of May, 1978, in Innsbruck, Austria, after a long illness. He left behind lasting imprints wherever he has been, but more so in the Frascati National Laboratories. He was one of the initiators of the development of e^+e^- colliding beam machines.

With his vigorous participation, this development culminated in the construction of the AdA storage ring in Frascati, later upgraded into Adone. His other most important legacy to the Frascati National Laboratories is the theory division which he formed from a group of his former students and collaborators. The activities of the theory division of Frascati bear so much of Bruno's influence that it is almost as if he were still around. He was not just a group leader but a personal friend to each member of the theory division.

As a tribute to these legacies, the Research Division in Frascati proposed to the director of the laboratory, Prof. Sergio Tazzari and to the President of the INFN, Prof. Nicola Cabibbo, to institute a memorial lecture series, to be held yearly, in honour of Bruno. The proposal was promtply adopted.

Prof. Nicola Cabibbo inaugurated the Bruno Touschek Memorial lectures on May 11, 1987 in the Frascati Laboratories. Eduardo Amaldi, very rightly, was the dean of the occasion, after his memorable biographical portrait "The Bruno Touschek Legacy", published as CERN yellow report No. 18-19 of 23rd December 1981 and its Italian translation in "Quaderni del Giornale di Fisica Vol. No. 7, 1982.

Three Nobel prize winners, who had, in various ways, interacted with Bruno attended. They are Carlo Rubbia and Simon Van der Meer both of CERN and Burton Richter of SLAC. Also present were many of the physicists who shared with Bruno the excitements of the construction of AdA and ADONE storage rings. Amongst these were, Carlo Bernardini, Marcello Conversi and Giorgio Salvini. There were also many of those who had collaborated with Bruno in one or other of his many scientific activities, amongst which were: Edoardo Amaldi, Ugo Amaldi, Francesco Calogero, Marcello Cini, Raoul Gatto, Giacomo Morpurgo, Italo Federico Quercia and many former students and friends.

Many physicists who could not attend sent regrets. Amongst these must be mentioned the moving letter of Bruno's former professor, Paul Urban, of the University of Graz, Austria, who though now blind regretted his inability to arrange for a travelling companion at short notice and promised to be present at the second Bruno Touschek Memorial Lecture next year.

Another former professor of Bruno, Rolf Wideröe with whom Bruno worked in the development of the Betatron, also wrote to regret his inability to attend the inauguration ceremony.

The first Bruno Tousheck Memorial Lecture was delivered by Prof. John Bell of CERN, Geneva, on the subject: " The Exact Principles of Quantum Mechanics". There could hardly have been a better choice of lecturer nor a more timely subject. John Bell knew Bruno very well and was one of the regular visitors of Bruno when he (Bruno) was in the La Tour Hospital in Geneva. The subject of the lecture is especially timely. In fact experiments are now successfully testing those fundamental assumptions of quantum theory, which for well over half a century have been argued, all too uncomfortably, on the basis of philosophy rather than on physical principles. These assumptions bear on the so-called objective existence of physical reality. So formulated there is little wonder that philosophy is tempted to intrude into a department of pure science. The distortions of contrived philosophical interpretations deny the objective existence of reality, preferring to invest the observer with the ability to bring reality into some form in the act and instant of observation. A world so conceived requires for its consistency other artificialities. One of these, the so-called collapse of the wave function, has exercised great influence over the years. In his memorial lecture, Prof. Bell argued with disarming calmness and clearity that wave functions do not collapse and observers have no part to play in the creation of reality. Quantum mechanics, he asserts, can be formulated exactly, without these vaguenesses, and in agreement with what experiments have so far established. His arguments were simple, enjoyable and accessible also to non-experts.

The institution of the Bruno Touschek Memorial Lecture is a merited tribute to the memory of a great scientist. The Frascati National Laboratory is proud to be associated with this honour to Bruno. Accordingly, it is proper to thank the President of INFN and all friends of Bruno who contributed to the success of the inauguration of this lecture series.

ACKOWLEDGEMENTS

We would like to thank Mrs. Luigina Invidia and Mrs. Franca Scacchi for their assistance in the organization of these memorial lectures.

Mrs. Invidia helped in an invaluable way also in the editing and publication of the lectures. We owe her particular thanks for this service and for her accommodating patience.

Frascati, 1987 The Organizers

Bruno Touschek (second from left) and Marcello Conversi (on the far right).

Bruno Touschek with Edoardo and Ginestra Amaldi (to his right)
in the Frascati hills (1953).

From Archivio Amaldi, Università La Sapienza, Rome, Italy.

INAUGURATION OF THE BRUNO TOUSCHEK MEMORIAL LECTURES

Nicola Cabibbo
President of the Italian Institute of Nuclear Physics (INFN)

It is a pleasure for me to inaugurate the Bruno Touschek Memorial Lectures. Bruno Touschek was one of the great physicists of this century. He will be remembered as the inventor of electron-positron colliding rings, and for his many contributions to the unraveling of the mysteries of elementary particles physics, among which, one that stands out is his discovery of chiral symmetry. This symmetry, which he discovered while studying the consequences of parity breaking, has become the very cornerstone of the Standard Model. Bruno had a very deep understanding of symmetries. It was very characteristic of him that in thinking about accelerators he would also have symmetry in mind. I recall that he would say, in his characteristic Viennese accent that AdA, the first electron positron machine, was guaranteed to work "because of the CPT theorem".

Together with Francesco Calogero, I was one of his first students in Rome, but there are many others, and we are all really glad for this initiative of the Frascati laboratory to remember Bruno. I would like to mention the many scientists who were unable to be with us today, but sent letters of appreciation for this initiative, among them Ferdinando Amman, Gilberto Bernardini, Samuel Ting, Volker Soergel, Abdus Salam, Paolo Budinich, Charles Enz, Valentino Telegdi and Richard Wilson. Professor Paul Urban of the University of Graz, who was Bruno's professor and one of his earliest friends, is unable to come for health reasons.

Prof. John Bell is the first Touschek Memorial Lecturer. John Bell was very close to Bruno, they were among the first to work on symmetries. Both of them are known for their contributions on discrete symmetries, time reversal, and the CPT theorem.

I will not try to remember in detail the many contributions to physics which are due to John Bell. Let me recall his famous work on the Adler-Bell-Jackiw anomalies: anomalies sharpen our understanding of chiral symmetries and play a central role in modern theories. Among his recent interests, a beautiful and illuminating paper on the Hawking radiation, the radiation emitted by black holes. The subject of these lectures, the fundaments of Quantum Mechanics, is one of the lasting interests of John Bell, and one to which he gave important contributions. Is quantum mechanics the ultimate description of matter? John will speak on this subject in a series of lectures which starts today.

The first talk of today is by Edoardo Amaldi. Prof. Amaldi has been a close friend of Bruno, and has written his definitive biography. The subject of his lecture is: “Bruno Touschek, the Legacy of the Man and the Physicist”.

Bruno Touschek and Edoardo Amaldi.

From Archivio Amaldi, Università La Sapienza, Rome, Italy.

BRUNO TOUSCHEK
THE LEGACY OF THE MAN AND THE PHYSICIST [1]

Edoardo Amaldi
University of Rome, Italy

1. - Bruno Touschek education in Austria and Germany -
2. - His experience in Glasgow (1947-1952) -
3. - Bruno Touschek arrives in Rome -
4. - The first period of his scientific work in Rome (1952-1960) -
5. - The period of the e^+e^- rings -
6. - Beam stability and radiative corrections -
7. - The contribution of Touschek to Adone -
8. - Other activities during the last period in Rome -
9. - The departure of Bruno Touschek –

1. - Bruno Touschek education in Austria and Germany -

When Bruno Touschek moved from Glasgow to Rome in December 1952, we did not know much about this young Austrian physicist. We had been told by Bruno Ferretti, then professor of Theoretical Physics at the Department of Physics of the University of Rome, that he was a good theoretician; and when we had met him, for a few days, for the first time in September of the same year, we had immediately admired his brilliant mind and appreciated his temperamental and amusing personality.

We learned only years later that he was born in Vienna in 1921 and was the son of a Staff Officer of the Austrian Army (Franz Xaver Touschek), who had fought on the Italian front in the First World War. His father had left the Army and entered the reserve at the age of 31, with the rank of Major, when in 1932 the power in Austria was taken over by Dollfuss' Christian-Socialists, in reality Clerico-Fascists.

When Hitler took over control of Germany on 30 January 1933, the German pressure on Austria became immediately very strong. On 19 June 1933, Dollfuss had succeeded in declaring the Austrian National-Socialist Party illegal, but, on 25 July 1934, a group of 154 members of this party, wearing uniforms, burst into the Federal Chancellor's Office in Vienna, and murdered Dollfuss.

Dollfuss' successor was his party companion Kurt Schuschnigg, who tried to save Austria's independence by following a policy of detente with Hitler's Germany. But the Austro-German agreement, signed on 11 July 1936, contained concessions that spelt disaster for Schuschnigg and his country. The Austrian National-Socialist Party was reconstituted, with a strong renewal of antisemitism, the roots of which in Austria dated back to the years 1880-1890.

Bruno had attended school in Vienna and, at the beginning of the summer of 1937, he had completed the 8th class of the Piaristen Gymnasium, that is a year before the Abitur (state examination) when he was told that he could no longer attend school because he was of mixed blood, as his mother (Camilla Weltmann) was Jewish.

He stayed away from school but he had many friends that he met in cafés, and this kept him in touch with what was happening. A friend who attended another school suggested that he sat the exam at a different school as an external student without making any mention of his real position. Bruno took his advice and sent in his application to the Director of Education for the Schottengymnasium. He was allowed to take the exam and passed it very well.

In February 1938 he went to Rome for the "school-leaving holiday" according to the tradition of the bourgeoisie of that period.

Around the end of the same month, Vienna entered a period when the Schuschnigg government was engaged in a death-struggle, which ended on 13 March 1938 with the proclamation of the "Anschluss" of Austria by Hitler's Germany. This occurred without Great Britain and France taking any measure, and with the consent of Mussolini who, first with the Abyssinian war (1935-36) and later through participation in the Spanish civil war (1936-39), had once and for all espoused Hitler's cause.

Bruno had thought of studying engineering in Rome and so he began to attend the first two-year course in engineering in the spring of 1938. He attended, in particular, Francesco Severi's course on "Mathematical analysis". In the meanwhile, however, he had applied for a visa to enter Great Britain in order to study Chemistry in Manchester. He was told that this could be obtained through an organization established in Vienna and run by the Quakers, who were very active in that period, as in other dramatic circumstances, trying to save people persecuted for political or racial reasons. Towards the summer of that year he returned to Vienna.

Following the Hitler-Stalin Pact, at the beginning of September 1939 the Second World War broke out, when the Russian and German armies entered Poland from opposing fronts.

With the war raging in Europe, every possibility of going to study in Great Britain vanished. As a result Bruno remained in Vienna and started to attend the University courses in physics and mathematics, trying to avoid attracting attention. But in June 1940 he received a notice that he could no longer attend the University for racial reasons.

Luckily, some time earlier he had, with the help of Paul Urban(2), studied the first volume of the famous treatise Atombau und Spektrallinien written by Arnold Sommerfeld(3), Professor at Munich University. Touschek had spotted a few minor errors and, encouraged by Edmund Hlawka(4), wrote to Sommerfeld. Sommerfeld replied asking Touschek to read also the second volume of the same treatise, which, at that time, was one of the best of its kind for both clarity and mathematical rigor. In the preface of the second edition of this second volume, Sommerfeld thanked Bruno Touschek for his critical review of the text.

When Bruno was expelled from the University of Vienna, Urban endeavoured to obtain the support of Sommerfeld. Sommerfeld wrote a letter of introduction to Paul Harteck(5), who was teaching in Hamburg, and Bruno moved to that town, where nobody knew of the "racial imperfection" of the young Austrian. That of course, was not true for Sommerfeld nor for Harteck or a few other professors who were perfectly well aware of it.

Harteck was a chemical physicist well known for his work on the production of heavy water (1934), on the chemistry of deuterium compounds (1937-38), and on artificial radioactivity and neutron physics (since 1938). He welcomed Touschek and advised him on how to behave when approaching the other professors.

In order to keep himself Bruno was forced to work; in fact he had to do several jobs simultaneously. There were periods when he had to do four or five jobs at the same time. In addition, he did not have a fixed residence, but frequently moved so that he could not be easily found. In Hamburg he worked for a long time for the Studiengesellschaft für Elektronengeräte, an industry affiliated to the Dutch firm Philips, where "drift tubes", forerunners of the klystron were being developed. This was a very important problem at that time for high-frequency communications.

At the University, Bruno attended various courses, without being registered, in particular the courses on theoretical physics, at the invitation of W.Lenz(6), who gave a course on relativity, and H.J.D.Jensen(7), who, about twenty years later, in 1963, won the Nobel Prize for Physics with

Maria Goeppert-Mayer for the nuclear shell model.

For long periods Touschek lived in the flat of Professor Lenz in Hamburg and he had considerable difficulty bringing the old and often sick man to the cellar when the bombers came.

Once in a train in Berlin he met a girl, M.Hatschek - she too was half Jewish - who worked in a factory that had changed its name from Lowenradio, typically Jewish, to that of Opta. Miss Hatschek introduced Touschek to the management, who emplyed him in a section directed by Dr. Egerer, working on the development of Brown's small tubes (i.e. cathodic oscillographs) for television. At that time Egerer was also Chief Editor of the scientific magazine Archiv für Elektrotechnik. Bruno worked at Opta for a long time, even after Dr. Egerer had left to work only for the Archiv für Elektrotechnik. Egerer had Bruno's help in this work too, and it was thus that, at the eginning of 1943, Touschek heard of a proposal presented by Rolf Wideröe(8) to construct a 15 MeV betatron. The proposal was kept secret because of its possible applications. Such secrecy, to tell the truth, appears today and certainly would have seemed to me (and to many others) rather curious even at that time. It is true that already in autumn 1922 Wideröe had proposed a scheme which does not essentially differ from that of the present betatrons(9) but the Physical Review of 1940-41 contained the papers by D.W.Kerst(10) in which he describes, with an abundance of detail, the 2.3 MeV betatron that he had independently conceived, designed, constructed and put into operation at the University of Illinois, together with the theory, practically complete, of the orbits of the electrons, which had been developed by Kerst and R.Serber(11). Furthermore, it was already clear that the betatron could be employed only as a source of X-rays used mailny for medical purposes.

Reading Wideröe's proposal Touschek had the impression that the relativistic treatment of the stability of the orbits contained some mistakes. He wrote to Wideröe, who replied and invited him to go and work with him when, towards the end of 1943, he was ordered to build a machine of this type. So Touschek began working with Wideröe, R.Kollath(12), and G.Schumann(13), to develop a betatron. His principal contribution at that period was the use of the Hamiltonian formalism to study the orbits of circular machines. As Wideröe wrote to me: "He was of great help to us in understanding and explaining the complications of electron kinetics, especially the problems associated with the injection of the electron from the outside to the stable orbit where they are being accelerated. Touschek showed that the process could be described by a Painlevé differential equation".

More or less at that time he had got the habit of going to the Chamber of Commerce in Hamburg, where there was a room in which one could read all the foreign newspapers. These repeated visits of him caused people to notice him, with the result that at the beginning of 1945 he was arrested by the Gestapo on racial grounds. At first, Wideröe went to see him in prison and brought him some food, his dear books and, even more important, cigarettes. During these visits, Rolf and Bruno continued to talk of the betatron. It was in prison that Touschek conceived the idea and developed the theory of "radiation damping" for electrons circulating in a betatron, which he wrote in invisible ink in the pages of Heitler's book *The quantum theory of radiation*.

Around the end of February, or the beginning of March 1945, an order arrived to transfer the prisoners from Hamburg prison to a concentration camp in Kiel. Touschek had a very high temperature but was nevertheless ordered to leave the prison. He carried with him a heavy package of books and while he was marching, escorted by the SS, in the outskirts of Hamburg, he felt ill and collapsed into the gutter at the side of the street. An SS officer took out his pistol and, pointing at his head, shot at him, wounding him behind the left ear. It was not a serious wound but he lost a lot of blood. As they thought he was dead, the column with the SS guards went on. A short time after, a group of civilians gathered on the edge of the road, discussing whether the prisoner abandoned in the gutter was dead or not. Really Touschek was still conscious and could hear their conversation, but, as they went on for a long time, at a certain point he got up and to the general surprise asked where the nearest telephone was. They pointed to a building not far away and he went there. It proved to be a hospital, and he was treated there, but the Greek director told the police, who arrested him again and transferred him to the prison of Altona.

As Bruno said, this was a "prison of bats"(14) where everything was extremely old. In particular, the guards and staff were all very old and kind to the prisoners. On Sunday a number of Czech prisoners of war were brought in and they did various odd jobs such as cutting wood for the stoves.

In the meanwhile the betatron group, in particular Kollath and the machine itself, were transferred to Wrist (in Holstein, near the Danish border), where some time later (probabaly in June 1945) the English arrived. Touschek was freed and went to Wrist, where the English asked him if he was willing to go with the troops as an interpreter. Having thought over the proposal, Touschek refused, also because at that moment both the occupying troops and the German - in particular the peasants in the country - were all extremely violent and killed each other practically without reason or purpose.

At the beginning of 1946, Bruno succeeded in going to Göttingen, where he had been attracted by the presence of a large number of physicists and the existence of a 6 MeV betatron. This machine had been constructed by K.Gund(15) in the Siemens Reiniger Laboratories at Erlangen, and had been transferred to the Institute of Physics of the University of Göttingen as a place that, presumably, would be left more or less alone by the Allied Authorities in Germany.

Bruno in Göttingen came into contact with a number of well known physicists: R. Becker(16), O. Haxel(17), H.C. Kopfermann(18), W. Heisenberg(19), F.G. Houtermans(20), W. Paul(21), L. Prandtl(22) and C.F. von Weisäcker(23).

During the summer, Bruno obtained the title of Diplomphysiker with a thesis on the theory of the betatron, made under the supervision of Becker and Kopfermann. A short time later, he was appointed a "wissenschaftliche Hilfskraft" (research worker) at the Max Planck Institute of Göttingen, where he began to work under the direction of Heisenberg. During this period he did two pieces of work, one on the double beta decay [6] and the other on the branching points of the solutions of Schrödinger equation [7].

2. - His experience in Glasgow -

In February 1947 Touschek moved to Glasgow on being awarded a fellowship of the Department of Scientific and Industrial Research (DSIR) and started to be interested in the construction of the 350 MeV Synchrotron, initiated more or less at that time under the direction of P.I.Dee(24), who pretty soon became one of his closest friends.

Touschek's friendship and collaboration with Dee enabled him to study in depth the problems related to the working of the synchrotron, and he published an article on its characteristics some years later [10]. That same year, he was awarded his Ph.D. with a thesis on nuclear excitation and the production of mesons by electrons, of which John C.Gunn(25) was the internal rapporteur, and Rudolf Peierls(26) the external one. Immediately after this he was appointed "Official Lecturer in Natural Philosophy" at the University of Glasgow, a position he held until he left for Rome.

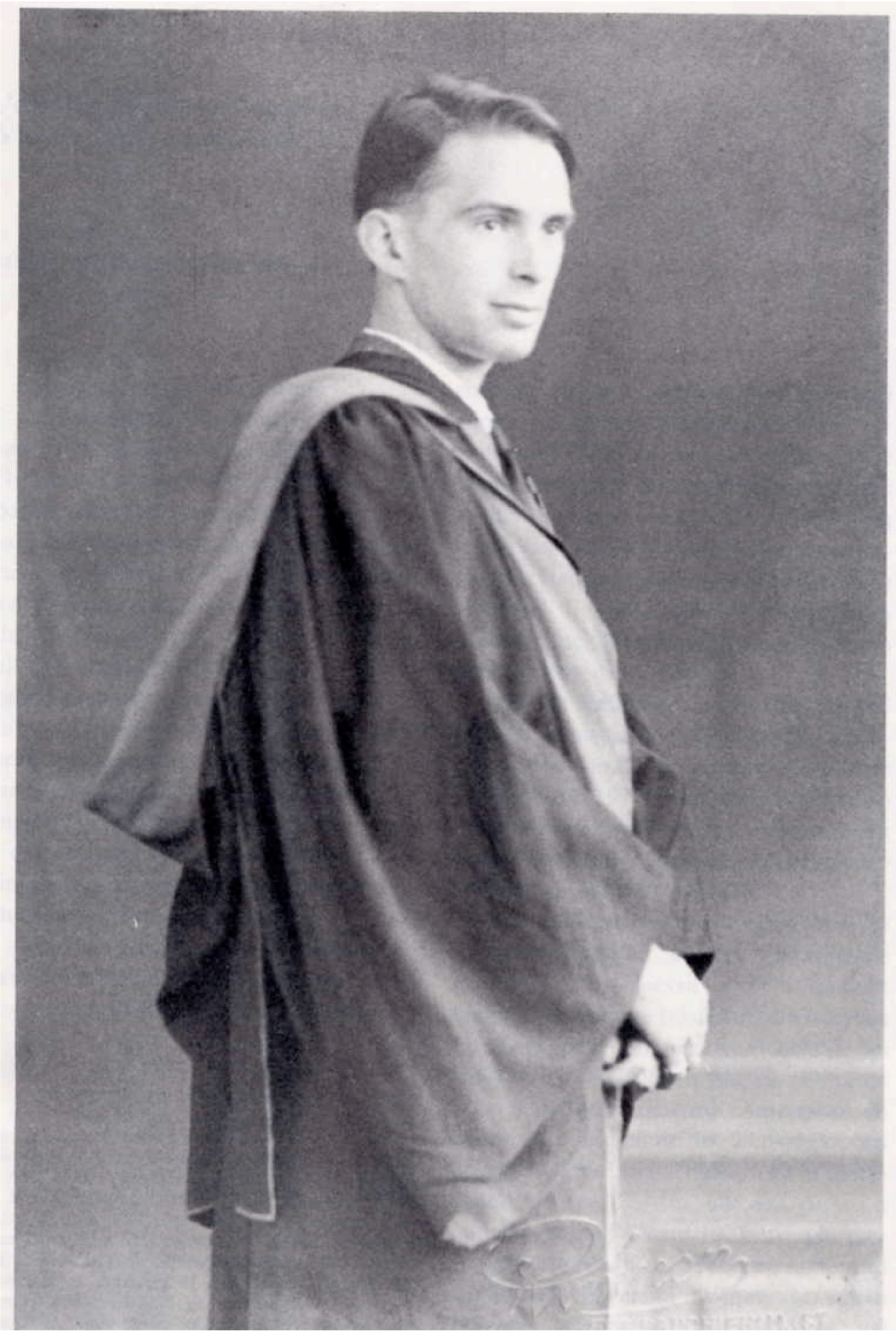

FIG. 1 *- Bruno Touschek in Glasgow (1949).*

During the Glasgow period he published a number of papers: some in collaboration with Sneddon(27) on the nuclear excitation by electrons [1,5], nuclear models [3], the density of the energy levels of nuclei [4], on the production of pions by electrons [8,9], or, in collaboration with Gunn and Power, on the production of pions in proton-proton collision [13,14].

In September-October 1950 Walter Thirring(28) from Vienna came up to Glasgow and met Touschek for the first time. They published a paper made in collaboration on the covariant formulation of the Block-Nordsieck method to solve the general electrodynamics problem in the presence of an external current [15].

In a paper written with Chisholm(29) Touschek discussed the spin - orbit coupling in nuclei [19] as essentially due to the exchange of pions between the nucleon and the rest of the nucleus. On this same problem he returned years later in a paper made during the Rome period with Bietti [55].

3. - Bruno Touschek arrives and settles in Rome -

As I said at the beginning, Bruno Touschek moved from Glasgow to Rome in December 1952. He had always been attracted to Rome owing to cultural and family ties.

It was in Rome that his aunt Ada resided having come there many years before. She was his mother's sister and had married an Italian. Aunt Ada was the owner and joint manager with her husband of an agency in Rome representing an Austrian firm (Garvens s.r.l.), specialized in the manufacture of water pumps and irrigation systems. His aunt and uncle also owned a house in the neighbourhood of Albano and, as they had no children, were always happy when their nephew visited them.

Rather than for family reasons, however, Bruno Touschek was attracted to Rome as a result of his acquaintance with Bruno Ferretti, owing to their papers, which appeared in the scientific press.

Ferretti(30), in fact, had been in Rome until he was appointed Professor of Theoretical Physics of the University of Milan in 1947, but in 1948 he was invited to be Professor of Theoretical Physics at the University of Rome, a post previously held by G.C.Wick, who had accepted an offer from the University of Notre Dame at South Bend (Indiana).

In September 1952, Bruno Touschek had visited Ferretti at the Guglielmo Marconi Physics Institute. A few hours after their first meeting, spent discussing scientific questions of mutual interest, they established such a marked professional respect and personal attachment for each other that Touschek decided to remain permanently in Rome. This became possible because he was appointed to the post of researcher (grade R2, equivalent to that of an Extraordinary Professor) in the section of the Istituto Nazionale di Fisica Nucleare, of which I was director at that time(31).

On his return to Rome in 1948, Ferretti had succeeded in strengthening the group of young theoretical physicists and instilling considerable life into it. I should mention that among the members of the group were the following, in order of age and training: E.Corinaldesi, M.Verde, B.Zumino, G.Morpurgo, R.Gatto, E.Fabri e C.Bernardini.

Bruno Touschek and Ferretti never published a joint paper, perhaps because both were too individualistic in their manner of thinking, and because they had complementary qualities. This was so much the case that they were always ready to engage in a detailed discussion but had difficulty in following a systematic approach in solving a specific problem. Their daily discussions of the very diverse and most difficult problems of theoretical physics provided, however, for many years, an extremely strong incentive to a deep understanding of these problems, not only by themselves, the two main protagonists, but also by other young theoreticians, who had studied or were studying in those years at the Institute of Physics.

This form of discussion came to an end in 1956, when Bruno Ferretti moved to Bologna. But Touschek's influence on the group of theorists continued to have an effect for many years and began to diminish only from 1960 onward, when his interest shifted towards the possibility of constructing accelerator machines for studying the processes produced by electron-positron collisions.

When Touschek moved to Rome, the main interest which he shared with Ferretti was the construction of a quantum field theory, which would also include bound states, i.e. a theory which would go beyond perturbation methods.

Ferretti pointed out to me that Touschek was among the first to maintain that it was possible to construct a unified theory of electromagnetic and weak interactions, the first example of which was actually constructed by S.Weinberg and A.Salam shortly after(32). A passing reference to this idea had already been given in a work by Touschek on the neutrino theory [43] but was dealt with in a more detailed manner in a subsequent paper prepared with the collaboration of

I.M.Barbour and A.Bietti [51]. Ferretti keeps some correspondence on this subject with Touschek.

In those early years of his life in Rome, Bruno Touschek owned a motor cycle which he called Josephine; he claimed that when he had been out late drinking in a pub or at a friend's, Josephine knew how to bring him home safely.

In spite of his confidence in Josephine's wisdom, one night on his motor cycle he ran into the rear of a large car at a cross-road, flew right over the top of it and injured his skull during the fall. He was immediately taken into the Neurological Clinic, which at that time was directed by Professor Ugo Cerletti.

The injured person was in a state of great agitation and even if what he said was not understandable, he appeared to be of German tongue. The next morning, the doctor of the Psychiatric Section asked Dr. Valentino Braitenberg(33) of the Bolzano province, to go from the laboratory where he was working to fill in the hospital sheet for the newly admitted person. As Braitenberg relates "...a first superficial examination (that would not have lasted more than a few minutes) allowed my colleagues to determine that he was not Italian and to suspect psychosis. The injured man had declared he was a theoretical physicist, Vice-Director of the Scuola di Perfezionamento di Fisica Nucleare and a specialist in time reversal (see below). I found Bruno sitting on his bed in the ward, still rather angry but already occupied in observing with interest the spectrum of mental alienations displayed by the surrounding patients. He wore a turban applied by the first-aid doctors when they treated the wound that he had received during the fall. The slight concussion which he suffered in combination with the high alcohol content in his blood had caused a state of agitation, as frequently happens, as a result of which the police decided to apply the rules of psychiatry rather than those of the Penal Code. We immediately became friends. His story was convincing and not at all psychotic, his German was delicious, rich and precise, his humor was uncontrollable even in such embarrassing circumstances. I wrote his clinical story with his help. The only slightly abnormal detail was the daily quantity of wine, but Bruno gave good reasons: tennis, the scarcity of water in aqueducts, etc. I thought it appropriate to give him the "routine" sermon. Bruno answered that his liver was his and that if I wanted to associate myself with his habits, he would be very glad to bring me to Nemi the following Sunday. I accepted and, after that, for many months we spent almost all weekends together, Bruno, my future wife, whom I had just met at that time, and myself. We started to talk of cybernetics, there was a seminar on the computer machines in which I participated as a guest, and finally the avalanche of cybernetic activities that carried me with it and transformed my life".

This was the beginning of a lasting friendship between Bruno Touschek and Valentino Braitenberg and also of the brilliant carrier of Braitenberg, who since many years is codirector of the Max Planck Institut für Biologische Kybernetic in Tübingen.

In 1955, Bruno returned to Glasgow to marry Elspeth Yonge, the daughter of a well-known professor of Zoology at the University of Glasgow, Sir (Charles) Maurice Yonge(34). Elspeth gave Bruno two sons, Francis in 1958 and Stefan in 1962.

At home he had two cats, a black one, which he called Planck, and a striped cat which he called Pauli. After making Pauli undergo a series of intelligence tests, Bruno decided that he was extremely intelligent. He made him undergo another test but Pauli failed to pass this one: after very careful preparation, Bruno gave him a tin of sardines and a key to open it, but Pauli did not open the tin and so did not eat the sardines.

Shortly after his arrival, we acquired the habit of playing tennis together two or three times a week. Sometimes we were joined by Francesco Calogero.

4. - The first period of his scientific work in Rome (1952-1960) -

The first work carried out in Rome, at the beginning of 1953, shortly after his arrival, was in collaboration with Mathew Sands(35) of the California Institute of Technology. Sands was in Rome on a "fellowship" awarded by the Fullbright Foundation. The work [17] followed a few months after the discovery of strong focussing(36) and concerned the errors of magnets' alignment in synchrotrons based on this principle. The authors showed that certain ideal orbits (i.e. calculated without alignment errors), which were periodic in a single revolution, are transformed by alignment errors into open orbits which are secularly unstable. This was found to be the case in the vicinity of certain values of the betatron frequency. But far from such "resonances" there are, however, stable orbits. They also produced estimates for the misalignment tolerance necessary to give rise to a reasonable amplitude of perturbed motion. At the same time, similar calculations were made also by other authors(37), who are quoted, together with that made by Sands and Touschek, in all treatments of the stability of strong focusing accelerating machines(38).

The short paper written with E.Fabri on "The mean lifetime of the τ meson" [22] contains a discussion of the possible existence of correlations between the energies of the three charged pions emitted in the decay of this particle and paves the way to the paper, published by Fabri alone shortly later(39), where the problem is re-examined in detail and clarified with the introduction of the same graph submitted a little earlier, without Fabri's knowledge, by R. Dalitz(40) and known today in the literature as the "Dalitz-Fabri plot".

In another paper, Touschek discusses the final states produced by the capture of K-mesons, of which, at the time, only seven events had been observed [25].

The paper with G.Stoppini [31] presents certain phenomenological expressions for the differential and integral collision cross section, for the photoproduction of pions on a nucleon, valid in the region between the threshold and the first resonance (I=3/2, J=3/2).

These expressions were obtained by introducing suitable corrections to the perturbation expression, in a similar manner to that used in the Born approximation with distorted waves, which is currently used in nuclear physics. The fundamental idea is to include phenomenologically the two main causes which make the perturbation method unsuitable: i.e. the correction to the final state due to the pion-nucleon interaction (in state 3/2, 3/2) and the interaction due to the anomalous magnetic moment of the nucleon.

The expressions, which the authors derived in this manner, are in very good agreement with the experimental results. The general trend of the paper follows the ideas by G.Chew(41), although it differs from these in certain important details.

In addition to these papers, some of them of an engineering and others of a phenomenological nature, Touschek published during these years a whole series of papers, some of which tackle problems relating to calculation methods, whereas others deal with fundamental questions or questions of principles.

To the first category belong three papers ([21]) [23] [35]), in collaboration with Morpurgo who had graduated in Rome in 1948, was gone to work in Chicago in 1952 and, at his return in August 1953, had found Touschek in Rome. Since Morpurgo will speak later of his collaboration with Bruno Touschek, I will not recall any detail about this work, which in great part deals with time reversal.

In some of these papers, Bruno had also concerned himself with the problem of parity which was specifically dealt with in two papers, entitled

"Parity conservation and the mass of the neutrino" [32] and "The mass off the neutrino and the non-conservation of parity" [33], both of which are very important, particularly the former. In the first of these two papers, Touschek is the first to introduce what was much later referred to as *chiral symmetry*. Abdus Salam(42) had proposed the operation of *discrete symmetry*

$$\psi \rightarrow \gamma_5 \psi, \qquad (1)$$

where Touschek introduced the operation of *continuous symmetry*

$$\psi \rightarrow e^{i\alpha\gamma_5} \psi. \qquad (2)$$

He had proposed this operation in order to define the conservation of the leptonic number in the presence of parity violation, which is also the definition adopted today, for which the leptonic number of the neutrino is equal to its helicity. Later it was recognized that this continuous symmetry introduced by Touschek intervenes in a very general manner, for example in the algebra of currents, as was pointed out by Gell-Mann(43).

In this paper [32], received by Nuovo Cimento on 26 January 1957,Touschek quotes the famous paper by T.D.Lee and C.N.Yang, in which it is suggested that parity is not conserved by weak interactions(44); he also quotes the paper by Mrs C.S.Wu and collaborators(45) announcing the experimental confirmation of the predictions made by Lee and Yang with regard to ^{60}Co decay. In fact, Touschek quotes an issue of *Time Magazine* of 28 January 1957, which appeared a few days later, in which the news was published for the first time. This suggests that the quotation was added when corrections were made to the proofs. Touschek's paper was, however, fully written, or almost so, before the experimental confirmation of parity non-conservation in weak interactions.

In this paper Touschek explores the consequences of the idea that the masslessness of the neutrino is due to the invariance under the chiral transformation (2) of the neutrino field. If the same transformation acts on the charged lepton fields as the phase transformation associated with the lepton number, than the conclusion is reached that the neutrino field must appear in the weak interaction with the factor $(1 \pm \gamma_5)$. Analyzing the experimental data(46) on the chain decay $\pi \rightarrow \mu \rightarrow e$, Touschek notices that the only allowed are V and A.

This paper was received by "Il Nuovo Cimento" on March 5, 1957 and appeared in the issue of May 1, 1957, and therefore represents a partial anticipation of the result obtained by Marshak and Sudarshan and Feynman and Gell-Mann(47) that the weak interaction should be essentially (V-A).

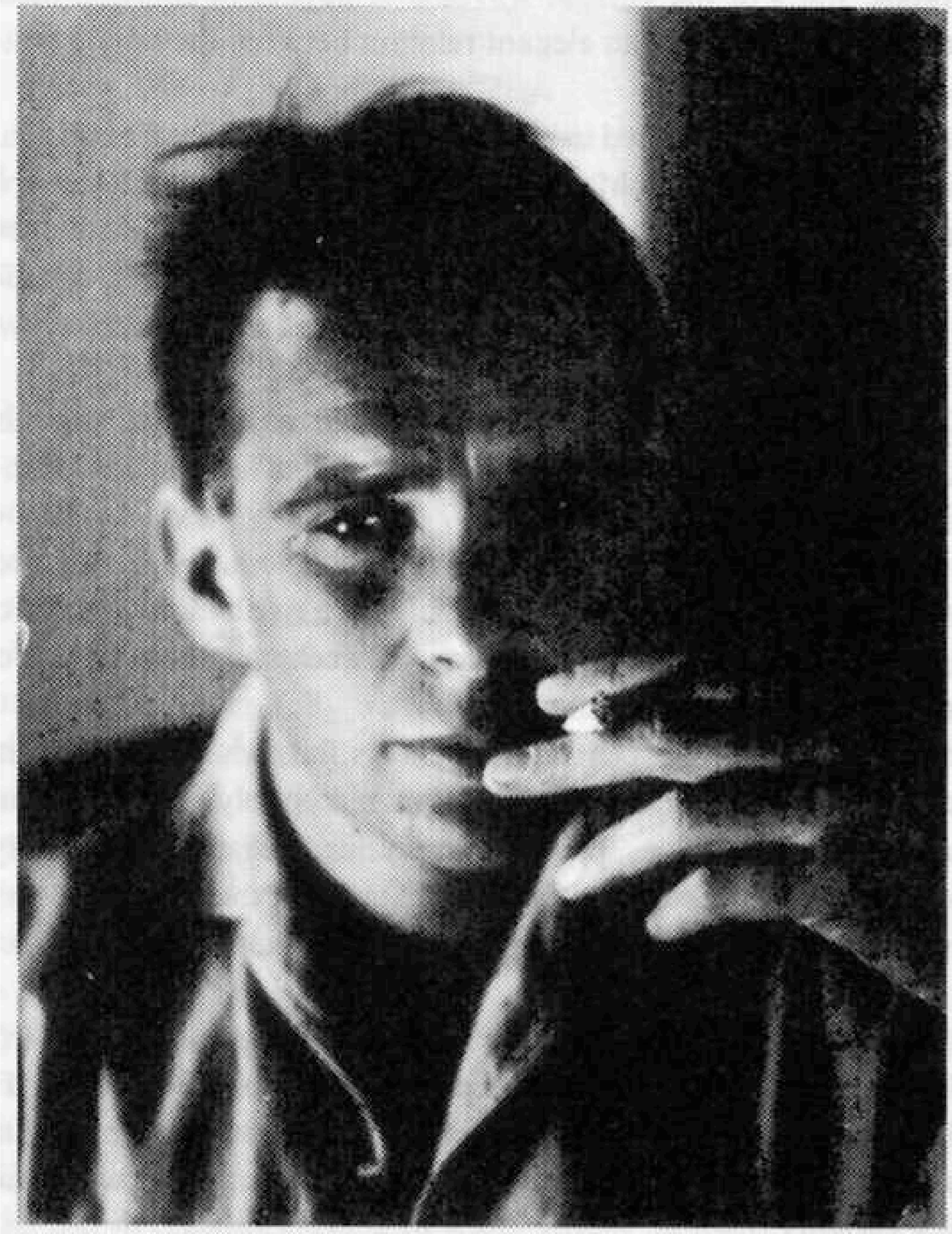

FIG. 2 *- Bruno Touschek in Rome (1955).*

Another paper - the last in collaboration with Radicati - is entitled: "On the equivalence theorem for the massless neutrino" [34]. It concerns the magnetic moment of the neutrino. The authors show that, at the limit of the mass of the neutrino equal to zero, the magnetic moment of this particle tends to zero as a consequence of the invariance of the wave function under the operation of chiral symmetry (2).

This paper was written after Radicati had moved from the University of Naples to that of Pisa, where Bruno regularly went in order to lecture on "field theory" at the Scuola di Perfezionamento in Fisica of this University.

The paper written with Cini on "The relativistic limit of the theory of spin 1/2 particles" [35] links up with the two papers on the neutrino properties [32,33], of which we have already spoken. When Touschek spoke of them to Cini, stressing the interest of the invariance of the wave function of the neutrino under the operation (2), the problem arose as to whether it was not possible to deal with the case of a particle having a non-zero mass, starting, as zero approximation, from the case of a zero-mass particle. The problem appeared, to a certain extent, as the reverse of that dealt with by L.L.Foddy and S.A.Wouthuysen(48), who had developed a systematic theory, i.e. valid at all orders, for the case of Dirac particles having a kinetic energy much lower than the rest energy.

The view taken by Cini and Touschek is the opposite: they start from the solution of Dirac's equation, valid for a momentum p vastly grater than *mc* (i.e. rigorously valid for zero mass), in which the spinor has only two components of opposite helicity, and try to find a canonical transformation which takes into account the finite value of the mass. The result is that in this case too there is a canonical transformation which separates the major components of the spinor from the small components.

The three papers [36] [38] [40] by Touschek alone, concern essentially the same problem, and represent an extension of the concepts introduced in papers [32] and [33]. The most important of these three papers is [36] entitled "The symmetry properties of Fermi-Dirac fields". In this paper a presentation and discussion are given of the first example of non-Abelian chiral symmetry, although it is expressed in a different form from that which is now more customary. This is probably the reason why this important result obtained by Touschek in practice has never been duly attributed to him.

The reason for this kind of presentation of the subject is that Touschek dealt with this problem taking his inspiration from Heisenberg's non-linear theory(49), in which an attempt is made to construct all the possible fields starting from the Majorana-type Fermionic fields, whereas, following the discovery of parity non-conservation(44), use is made of the Dirac-type fields and a separation is made from the outset between the fields of different chirality, i.e. left-handed and right-handed.

5. - The period of the e^+e^- rings -

On 7 March 1960, Bruno Touschek held a seminar at the Laboratori Nazionali di Frascati, where he demonstrated for the first time the importance of a systematic and thorough study of electron-positron collisions (e^+e^-) and how this could be achieved, at least in principle, by constructing a single magnetic ring in which bunches of electrons and positrons circulate at the same energy E, but in opposite directions(50). Bruno's views, however, were much more elaborate. They contained already in this first presentation four of the many arguments which made his proposal interesting:

i) First of all the possibility of obtaining a considerable *kinematic advantage* since the energy in the centre of mass frame of two particles of equal mass and equal and opposite momenta is equal to twice the energy of the single colliding particle while in a fixed target machine it increases in proportion to the square root of the energy of the incident beam.
This aspect of the problem had been already pointed out by Kerst et al.(51) and O'Neill(52) in 1956. These authors, however, had considered only the case of e^-e^- collision obtained with the bunches of electrons circulating in opposite directions in two magnetic rings tangent to each other at a point where the collisions take place(53). None of the articles of the two American groups mention the work by Wideröe and of which I shall talk below. Following O'Neill's proposal, a group at Stanford University had even started to design and construct a machine of this type(54), which was the first one producing very interesting scientific results on the e^-e^- collision already in 1966(55).
ii) If the circulating particles have equal and opposite electrical charges, a considerable constructional advantage is obtained because a single magnetic ring can be used in which the particle bunches circulate in opposite directions. Also this point had been made by Wideröe who, years before, had discussed it with Touschek.

In his talk Bruno, however, emphasized, two other important aspects of his proposal.

iii) The electron-positron system (i.e. e^+e^-) has the same quantum numbers as a neutral boson, so that at high energy it should become an electromagnetic particle source which is especially useful for studying strong interactions and electrodynamics. It also offered a number of various possible '"two-body reactions", i.e. reactions in which, starting from the initial state e^+e^-, a final state is reached in which there is only one electron and one positron disappear simultaneously owing to annihilation.

iv) Touschek also pointed out that, in any process which begins with the annihilation of a particle and its antiparticle (of initial equal and opposite momenta: $p_+=p_-$), the four-momentum transfer q^2 is always time-like i.e.

$$q^2 = -(E_+ + E_-)^2 = -4E^2 < 0, \qquad (3)$$

i.e. it enters a region of values which can be reached only through a few other processes in which e^+e^- pairs are produced by a (real or virtual) photon(56) or in hadron-hadron collisions(57). The interest of processes of the first of these types was indicated in 1961 also by S. Drell and F. Zachriasen(58).

Two other arguments in favour of this line of research that were not indicated by Touschek but recognized years later, are the following:

v) The detailed study of storage rings has shown that these machines have an extremely high energy resolving power

$$\frac{\Delta W}{W} = 10^{-3}.$$

It is just this extraordinary property that has made possible a detailed study of extremely narrow resonances such as the J/Ψ, Ψ', etc.

vi) As it was clearly understood only years later(59), the e^+e^- collision opens up the possibility of studying two-photon processes

$$e^+ + e^- \rightarrow e^+ + e^- + X$$

in which the particle X produced has the value c = +1 of the charge conjugation quantum number. This became a line of investigation of great interest in the 1980's(60).

All of the arguments discussed by Touschek, and their brilliant exposition, made a considerable impression on everyone present, including the then Director of the Laboratori Nazionali di Frascati, Giorgio Salvini, and Carlo Bernardini, Gianfranco Corazza and Giorgio Ghigo.

During the same day, the three last mentioned persons began to work with Touschek on a project for the first e^-e^+ storage ring, essentially designed as a prototype for checking the feasibility of accelerators based on the ideas set forth by Touschek during the seminar.

This first machine for the study of collisions between a particle and an antiparticle is known as AdA (Anello di Accumulazione e^+e^-). Carlo Bernardini, who has been one of the collaborators of Touschek in this work, will talk about AdA in the afternoon.

Touschek had also found the financial resources. Giorgio Salvini, who had immediately realized the importance of the proposal, succeeded, shortly after this, in obtaining from the CNEN an extraordinary grant of 20 million lire for the construction of the AdA prototype. This sum was almost entirely spent on the construction of the magnet, which was designed in a few days in a very original manner by Giorgio Ghigo(61) and produced with the assistance of G.Sacerdoti. A description of the project was set out by Ghigo in an internal note of the Laboratori Nazionali di Frascati, dated 8 December 1960(62).

Later C.Rubbia will talk about the role of Bruno Touschek in proton-antiproton collider physics and Salvini will present an overview of the development undergone, at the world scale, by the ideas of Touschek about matter-antimatter collisions.

It should however be recalled that the idea of constructing machines based on the collision of two particle beams, instead of one particle beam which strikes a fixed target, was devised for the first time by Rolf Wideröe in the late summer of 1943, during his holiday at Tuddal, near Telemark in Norway(1).

As Wideröe wrote me years ago: "When I discussed this idea with Touschek later, he did not appear very impressed at the moment. All he said was 'It is something obvious and trivial and cannot be patented'. But Wideröe nevertheless managed to obtain a patent in May 1953(63), when he had already been working for many years in the Brown Boveri research laboratory in Baden, Switzerland. This was some three years before Kerst et al.(51) suggested, independently, that use should be made of the collision of two equal particle beams, such as a proton-proton or electron-electron beam, circulating in two different magnet rings.

In his is patent, Wideröe discussed collisions between equal particles (proton-proton), and different particles (proton-deuteron), or particles of opposite charge (electron-nucleus, in particular electron-proton), suggesting various possible systems for the magnetic rings illustrated by four different figures. The collision always occurs in a ring (reaktionsröhre): in the case of particles bearing an opposite charge, these are kept on their orbit by the magnetic field itself. In the case of particles of equal charge, Wideröe suggests the utilization of electric fields, but does not give any detail about their design.

6. - Beam stability and radiative corrections -

After the operation of AdA at Orsay, Touschek's "experimental period" had come to an end, but not his interest and participation in the development of the e^+e^- rings. For a few years more his attention was concentrated on two fundamental problems also in connection with the design and construction of ADONE. The first was the problem of stability of electron and positron ultrarelativistic beams, and the second was that of radiative corrections.

The first work on beam stability was made in collaboration with C.Bernardini [44] and dates back to 1960, in other words the period prior to the construction of AdA. It concerns the losses incurred by the electron bunches which circulate within a synchrotron at a velocity close to that of light. The fact that the electrons in a bunch are not rapidly lost during the motion is represented by a potential well which moves at the velocity of the centre of mass of the bunch and in which the electrons remain trapped. Both the problem of the longitudinal (synchrotron) oscillations and that of the transverse (betatron) oscillations are dealt with in this manner, except that the potential well related to the longitudinal oscillations is much shallower and shows a sharp edge (stability limit) which depends on the radiofrequency voltage amplitude. Naturally, the containment of the beam is never complete, and there are always electron losses which are very important to calculate.

The problem was tackled by R.F.Christy(64), who had calculated the losses due to synchrotron oscillations. Matthew Sands, who was also at Caltech, had carried out certain measurements on the electron synchrotron of that laboratory, and had communicated his experimental results(65), which disagreed with Christy's predictions, to Fernando Amman. The latter started to take a few measurements with the Frascati electron synchrotron and found that, in agreement with Sands, the losses were considerably higher than those computed by Christy.

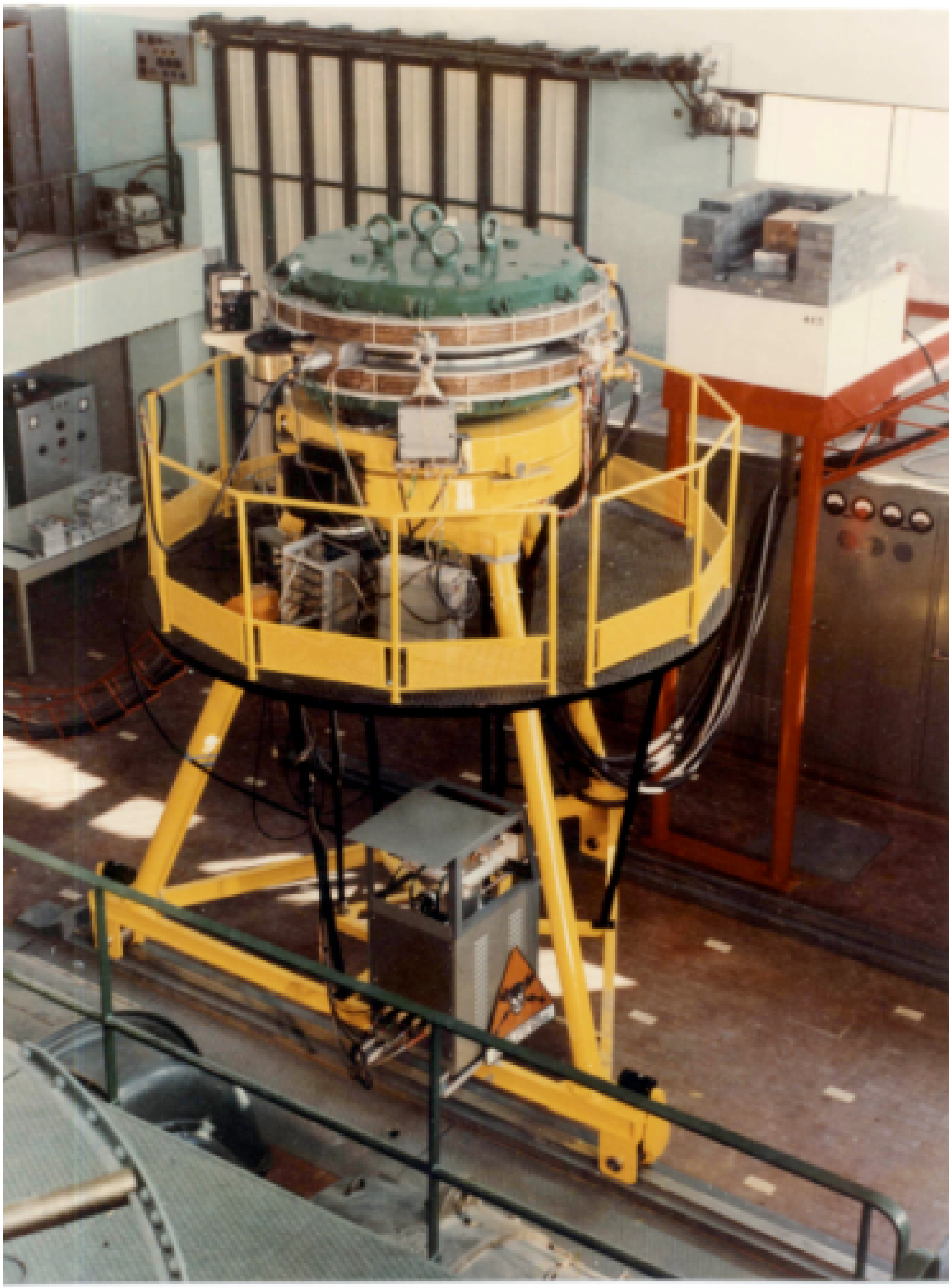

FIG. 3 *- Photograph of AdA mounted in Frascati on its movable support*[1].

Informed by Amman, Bernardini began to work on the theory of this problem; pretty soon, Bruno Touschek joined him and the problem was quickly solved by their joint work.

For low excitation the quantum levels of the electrons inside the potential well are essentially those of a harmonic oscillator. But as the excitation increases, an ever-increasing anharmonicity appears since the top of the potential well becomes closer. The levels, however, are generally so close to each other as to form a continuum.

The transition from one state to the other are due to the recoil caused by the emission of the photons of synchrotron light. Bernardini and Touschek tackled the problem as a diffusion process, described by a Fokker-Planck equation, and reached the conclusion that the loss of electrons was mainly determined by processes involving a large number of quantum jumps, whereas those calculated by Christy, which involve one or a few transitions, provide a much lesser contribution. In their work, Bernardini and Touschek derive the expression for the lifetime of a bunch of electrons in terms of the attenuation of the synchrotron oscillations and a parameter, between zero and one, related to the amplitude of the potential provided by the synchrotron's resonant cavities.

The paper written by Touschek with E.Ferlenghi and C.Pellegrini [56] concerns the instability of circulating beam of electrons caused by signals transmitted by the actual bunches to the walls of the chamber. These signals generate currents, which in turn produce fields that have a delayed phase effect on the beam bunches.

These instabilities, due to the wall's resistivity, had been observed at MURA(66) and later in a few synchrotrons. The theory had been elaborated by L.J. Laslett, V.K. Neil and A.M. Sessler(67) for the ISR (Intersecting Storage Rings) of CERN. Touschek, Ferlenghi and Pellegrini extended the theory to the more complex case of bunched beams.

The other problem, closely related to experiments on the reactions produced in e^+e^- rings, was that of the radiative corrections, where Touschek was assisted by various young collaborators. The first approach to this complex problem is set out in the thesis of Gian Carlo Rossi, who graduated with Touschek in 1966. The case concerned two beams moving in directions which formed between them an angle slightly less (~15°) than 180°.

Touschek's general idea was to find a reasonable compromise between a strictly defined geometry of the final state, in which the radiative corrections could be calculated more easily but are, in terms of percentage, large, and an open geometry in which the corrections are smaller but difficult to calculate since they involve many perturbation orders.

The problems was tackled more thoughlty in collaboration with Etim Etim [57], and subsequently extended, and to some extent concluded, in the subsequent work with Etim Etim and G. Pancheri [58].

7. - The contribution of Touschek to ADONE -

From an examination of the publications and other printed documents, the person interested in the historical development of e^+e^- rings might be led to think that Bruno Touschek had only a marginal role in the design and construction of ADONE. And this would seem strange, having in mind how profoundly and directly he had been involved in the construction and experimentation on and with AdA. The design and construction of this machine(68) was lead by Fernando Amman, who wrote to me(1): "..................

Bruno lived intensely the various phases of ADONE: the design as well as the construction period (1965-67) and especially the difficult year 1968 during which various beam instabilities were studied and cured. We did not meet frequently, but with regularity. Bruno was afraid of causing me to waste precious time, as he would say to C.Bernardini, and therefore followed almost daily the activity of the group, keeping in contact with Gianfranco Corazza. He was afraid of the dimension of ADONE: he considered it an enterprise of industrial type and with this he justified his reverential awe; but at the same time he felt that this was really the materialization of his original idea. Whenever a new problem came up on which he was certain to be able to contribute, Bruno was present, without the need to look for him; an example was the case of the transverse instabilities (1965).

... much more time and effort was devoted by Bruno to the Committee for Experimentation with ADONE which started to operate in 1966; in this body he was a strong believer in the importance of a preparation of the experimental equipment adequate to the machine, but at the same time he was against all-embracing and monopolizing approaches".

Amman continues: "How does Bruno fit into all that: he was the initiator, but also the element of continuity during the ten golden years of the Laboratories, the person that had a great idea and allowed it to be materialized by others; his scientific and human qualities, I believe, were decisive in maintaining the connections which have been essential in achieving success; if success there has been, or in the limits in which there was success. For these reasons I believe that one should accord to Bruno a primary role in the adventure of the electron and positron storage rings, an adventure that saw the emergence on the international level of an Italian laboratory more than that of individual Italian physicists..."

FIG. 4 *- Bruno Touschek in Catania (1964).*

8. - Other activities during the last period in Rome -

Among the many more activities of Bruno during the last part of his life I should also recall two papers of thermodynamics, one [59] concerning the law of transformation of the absolute temperature under Lorentz transformations, the other [60] presenting a discussion of the link between the reversible microscopic world and the irreversible macroscopic world. Bruno's attention was devoted to these problems in connection with his university course on statistical mechanics on which he wrote a fine book with G.C.Rossi [67].

Bruno's influence on his students and on those that had recently graduated was very marked.

Among the many who submitted their theses with Bruno I should mention: for the "laurea" in physics, N.Cabibbo, F.Calogero, P.Guidoni (who graduated in 1958), A.Putzolu (in 1961), Giovanni Gallavotti (in 1963), Paolo Di Vecchia (in 1965), Aurelio Grillo (in 1968), and for the "laurea" in mathematics: Etim Etim (in 1966) a young Nigerian who came to Italy with a scholarship from "Agip Mineraria".

His influence was strong even when their interests were not directly those which concerned Bruno at that moment. This was particularly true in the case of Luciano Pietronero, who came into contact with Bruno immediately after graduating in physics in 1971. Bruno suggested that he should re-examine a classic problem which had been set by Hans Thirring in 1918(69) but had not yet been fully resolved. The solution of this and other related problems kept occupied Pietronero for a couple of years(70).

9. - The departure of Bruno Touschek -

On 25 May 1978, Bruno Touschek died in the Medical Ward of the University Hospital, Innsbruck, as a result of a series of hepatic comas. He had been suffering from this illness for several years. He had had it in a serious form since February 1977, when he was taken to the Medical Ward II of the Policlinico of the University of Rome. This "dramatic collapse" as he wrote to me on 29 March 1977, a few days after he had returned home, "is somewhat providential as it has convinced me more than any preaching to put an end to this childish alcoholism, which has led me to my climateric, and has made me realize that my Bursche(71) days are over". The doctors had already explained, however, that not only his liver but also his kidneys were in a bad condition, so that it was not possible to carry out any major clinical or surgical treatment.

After a fresh collapse which had caused an ever greater irritability towards his family - an irritability characteristic of his illness - he was taken at the beginning of July 1977 to the Medical Ward I of the Policlinico, where he remained for practically the whole summer. In the meanwhile, he had been appointed "Senior Visiting Scientist" at CERN for a year from autumn of 1977. As soon as he was fit to face the journey, he moved to Geneva at the beginning of October.

Another attack in the middle of November forced him to enter the

Cantonal Hospital of Geneva. A little later he was transferred to the Hospital of La Tour, near the CERN Laboratories in Meyrin.

The nearness of the CERN laboratories made it easier for him to receive visits from many physicists, his friends, of different nationalities - above all Italians, Germans and Austrians - so that every day he saw different people with whom he discussed his state of health, his family problems and - above all - physics.

He always put forward, in an original and unexpected way, a point of view of substantial value on matters not yet sufficiently clarified. As he had always done, he read a great deal of literature and, in particular, history, and he had a great interest in figurative arts.

Nevertheless, Bruno was not happy in the La Tour Hospital, chiefly because the staff spoke French, and he felt that he did not know it well enough. It is true that he did not speak French so well as English and Italian, which he spoke fluently, using precise expressions - even if they were sometimes unusual or betrayed in their origin a Viennese mentality. But he understood everything and could express any thought in this language too. He probably succeeded in doing this by a much greater effort of concentration than he needed in order to express the same needs or thoughts in German.

One day, during his stay at La Tour Hospital, he picked up the phone next his bed, with which he communicated regularly with his wife and younger son in Rome or the elder one in London, and managed to book a room in the Sport Hotel in Igls, 5-6 km from Innsbruck.

When I went to see him in the La Tour Hospital on 27 and 28 February, he spoke to me with a certain amount of enthusiasm about this plan of his and of his success in arranging to be transferred on 8 March 1978 from Geneva to Innsbruck by a car, put at his disposal by CERN.

In a letter to me dated 2 May 1978 he spoke of the "very comfortable journey" and said that the hotel was without doubt the best that one could find, "with the staff always smiling, excellent food, and a 20-metre indoor swimming pool at 28°, with also a doctor in attendance. "In the same letter he also wrote of "the Alps in Springtime" and of having "enjoyed the balcony, from which one can see the Patscherkofel".

In his letter of 1 May, Bruno wrote to me: "I can write - still badly - and can read; I am still a little weak even for short walks in the village....I do everything at a snail's pace. So far, the only unwise action has been to hold a seminar in Innsbruck ... Cap and Rothleitner, (dean)[72] took care of me with Ernst[73], Valentino[33], etc., while I was in hospital. Everything went well, except that after 30 minutes I had to sit down".

As Rothleitner wrote to me, when together with Cap he visited Bruno for the first time in the clinic in Innsbruck:

"...he was very weak but still had strong will to live. He told us: 'I have been in coma and I have forgotten everything. I should start again from the beginning: I should again learn to speak, I should learn everything again'. He kept on his table a heft, in which he noted the important thoughts as soon as he found them again. On top of the first page I read: "Cogito ergo sum".

"In spite of his weakness, he expressed the desire to give a seminar and offered a number of themes..."

Talking with Valentino Braitenberg, who also had visited him in Innsbruck a few days before his death, Bruno expressed the desire to read a biography of Ludwig Wittgenstein(74). He had noticed, he said, in himself a desire of identification with the philosopher, his fellow-citizen, perhaps due also to the already remote readings of logic he had made at the gymnasium in Vienna. He had the impression of having neglected the more general aspect of knowledge. Valentino, however, did not succeed in providing him in time with the Wittgenstein biography.

At the hospital, first in Geneva and later in Innsbruck, Bruno read with great interest books on history and literature, mainly those regarding the Viennese life of the beginning of our century, and specially concerning the books of Karl Kraus (1874-1936)(75), and on Karl Kraus(76) and Gustav Klimt(77).

Karl Kraus had been always his favourite author: Kraus had founded, in 1899, the review *Die Fackel* which he wrote virtually unaided for some 37 years, hinting at the pending collapse of the Habsburg Empire, satirizing the monstrous day-to-day events, and putting to shame the police chiefs who had committed murder, as well as the criminal financiers.

Kraus' famous aphorisms(78) were probably the source of Bruno Touschek's paradoxical expressions or remarks. In the 1950's and 1960's he would often refer to this country of origin in scathingly critical terms. However, on reaching the end of his life, he seemed to find rest and contentment only by re-immersing himself in the culture of that Viennese and partly Jewish atmosphere of the beginning of the century, which had been his background and had so profoundly affected his youth.

As a researcher, Bruno Touschek struck everyone by the originality of his thinking, the Cartesian clarity of his approach, and his enthusiasm in what he himself or others were doing.

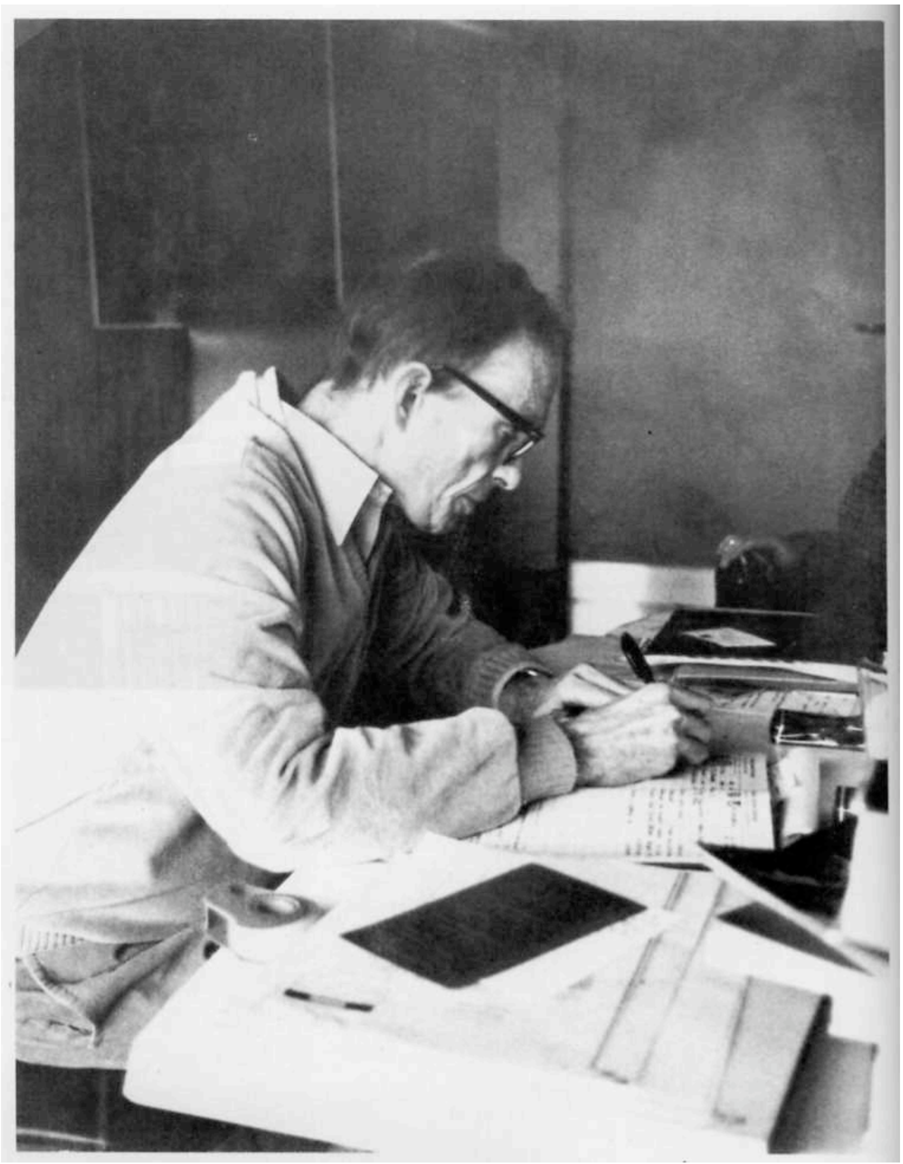

FIG. 5 *- Bruno Touschek in Geneva (Ospedale La Tour, 1978).*

Since the earliest days after his arrival in Rome, Bruno had acquired the habit of knocking on the door of my study at least three of four times a week, when he arrived at the Institute, rather early in the morning on his way to his own study. He would come in and tell about his latest thoughts, usually those of the night before, concerning the problem which be was concentrating on, or about an interesting result achieved by one of his young pupils and collaborators. His enthusiasm and incisive remarks were extremely stimulating and pleasant to hear. Even when he was talking of scientific problems he would very often introduce a subtle degree of humor, which would emerge from his texts and especially his drawings.

Bruno possessed an unusual skill in caricaturing his surroundings and local customs, which he would draw with a pen on the first piece of paper which came to hand, during the degree examination of Faculty sessions, or during the various meetings of the commissions or working groups dealing with the activities of the Institute or of the Laboratori Nazionali di Frascati.

This skill was a very marked characteristic on his mother's side of the family. She was rather good at drawing and this was even more true of her brother, Oscar Weltmann, a well-known doctor and dilettante painter.

The 10 sketches reproduced at the end of my speech have been taken from a large collection, which friends and relatives gathered from the wastepaper basket or on the table where Bruno had left them at the end of a committee meeting or examination session.

His caricatural approach, which is often very amusing and sometimes grotesque, is always present, and in certain cases (Nos. 1-3) is the only real purpose of the drawing.

Further drawings (Nos. 4-6) concern the life at the Faculty of Mathematical, Physical and Natural Sciences: the contempt of the Council of the Faculty for an article of a recent law or a ministerial circular (No. 4), the decision taken by the chemistry professors who, failing to reach agreement on which of them should be proposed as Director for all of the chemical activities performed at their department, had decided to keep the single-professorship institutes (No. 5), and the disagreement which arose at a certain moment among mathematical colleagues (No. 6). Drawing No. 7 represents a discussion concerning the direction of the magnetic field on the basis of the "three finger rule"; it was drawn by Bruno on a page of the record of measurements made with AdA at Orsay, during the Symposium on Storage Rings held in that laboratory from 26 to 30 September 1966.

This drawing was printed in the proceedings of the symposium as the initial page of the session on "Magnetic Detectors -Radiative Corrections". Drawing No. 8, concerning the introductory nature of the courses attains the heights of efficiency in its schematic symbolism.

Still other drawings concern the period of the protest movement (1968-1976). No. 9 represents an "assembly" in the large hall of the Istituto di Fisica Guglielmo Marconi. No.10 represents a group of unidentified persons who wanted to enter the Institute which was occupied by the "students, at the time when Giorgio Careri was the Director of the Institute (1968-70), and was therefore accused, by all the factions, of extremely serious and absurd failing and problems of management. No. 11 shows the discussion for the choice of the Director of the Istituto

Guglielmo Marconi, which Bruno Touschek had renamed "Istituto Maria Montessori", to stress the attitude taken by a part of the teaching staff, whose only thought was to allow the students to do whatever they wished. The symbol CB stands for "Carlo Bernardini".

Drawing No. 12 is an example of drawing which contained a fundamental contradiction, and No. 13 is an example of those based on the merging of two different concepts or objects. In this case the combination was between a lynx, which is the emblem of the Accademia Nazionale dei Lincei, and the six-legged dog, symbolizing the Ente Nazionale Idrocarburi.

Some of these drawings recall those of Egon Schiele (1890-1918) who, together with Gustav Klimt (1862-1918), an admirer of and sometimes inspired by oriental art, was among his preferred painters. These artists of the Viennese Secession had always attracted him by their culture and sensitivity, as well as by their almost sickly refinement, but this attraction grew immensely in the final months of his life.

We will continue to remember and admire Bruno Touschek for his intelligence and scientific work, we will never forget this man who could certainly be hot tempered, angry and emotional but in the end was basically kind and reasonable. A person who - as it has been written by his friend Philip Ivor Dee - "led an intense and vigorous life and one who, by his example and friendliness, helped others to achieve greater happiness and awareness in their own lives".

LIST OF PUBLICATIONS OF BRUNO TOUSCHEK

[1] B.Touschek, "Excitation of nuclei by electrons", *Nature* **160**, 500 (1947).

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SELECTED DRAWINGS BY BRUNO TOUSCHEK

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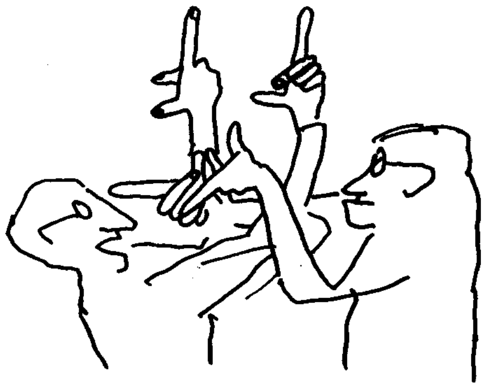

MAGNETIC DISCUSSION

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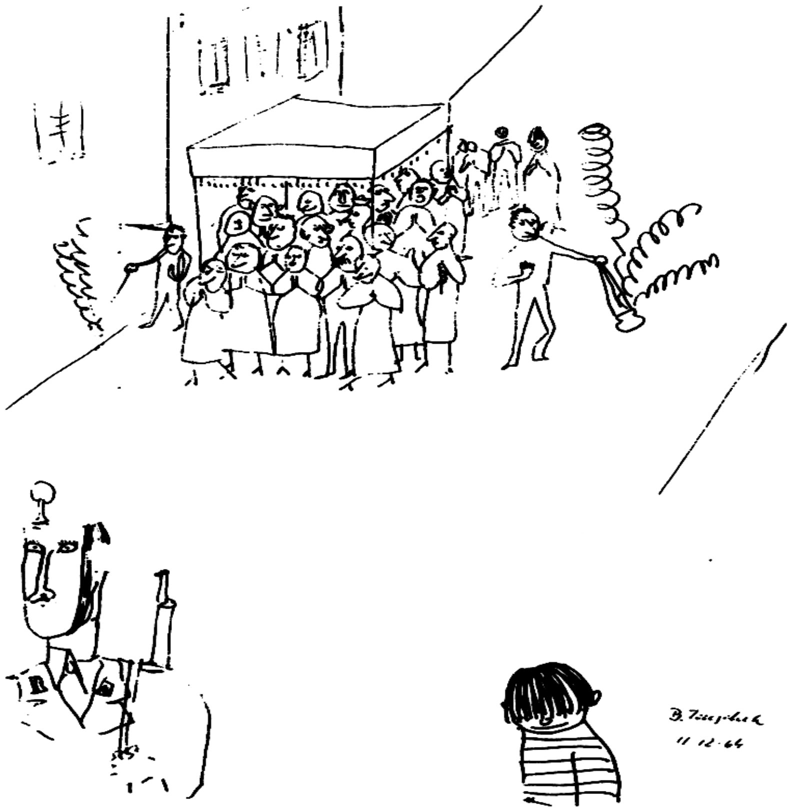

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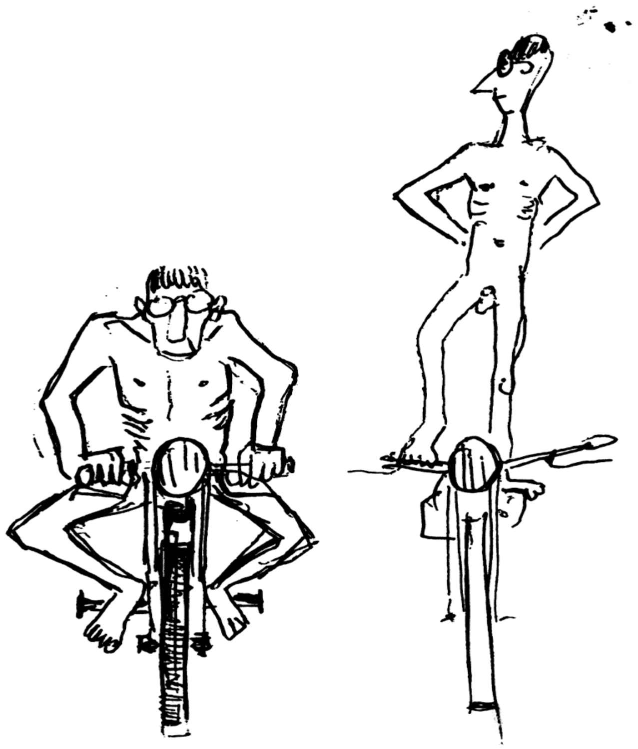

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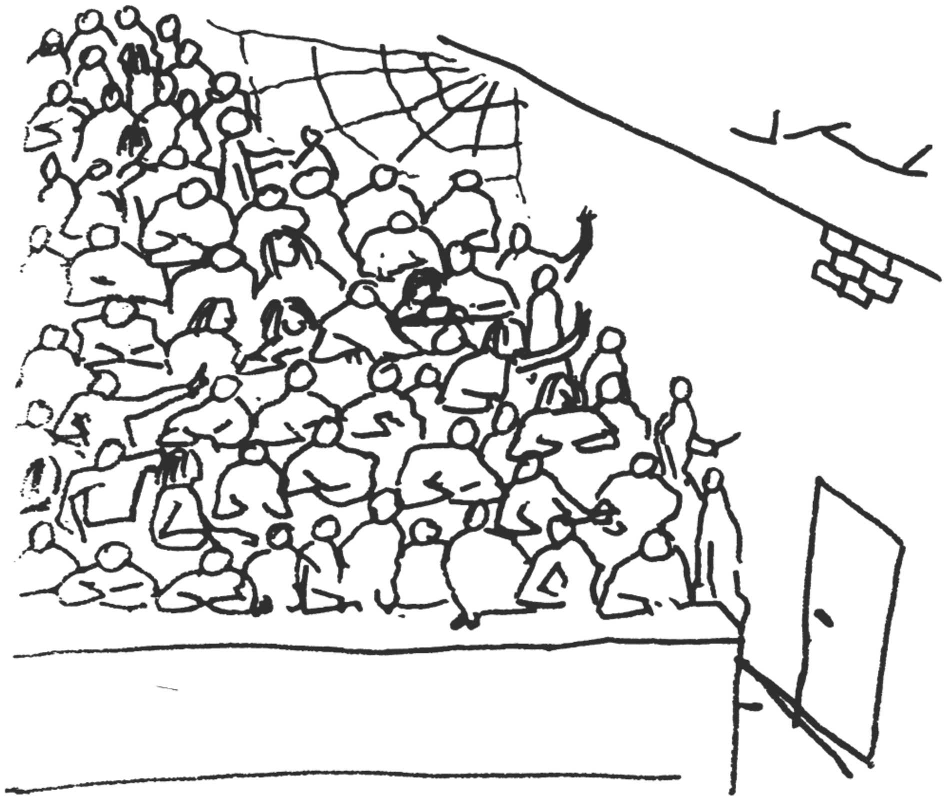

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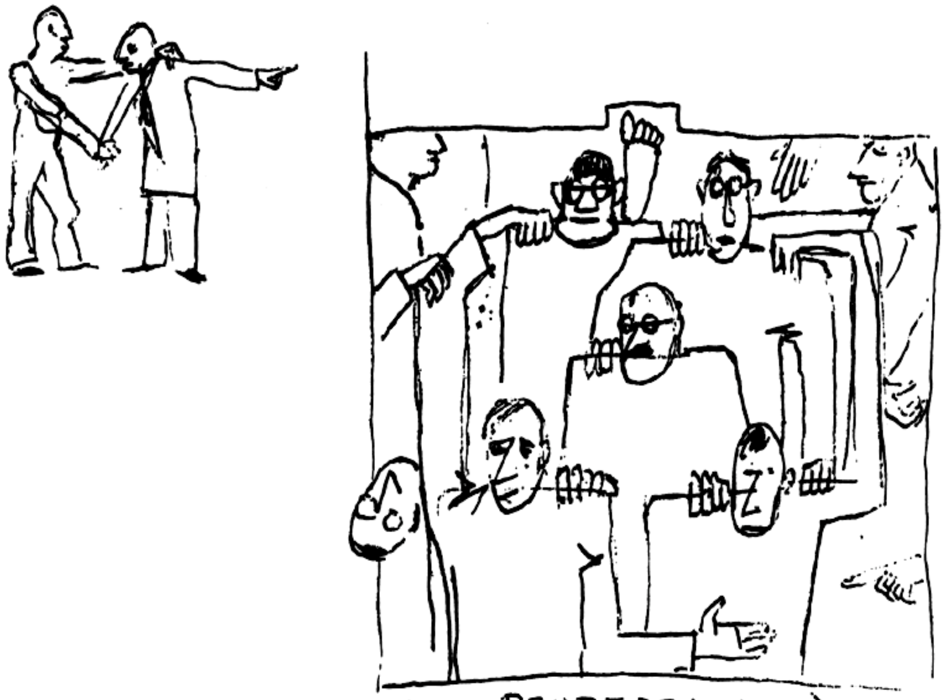

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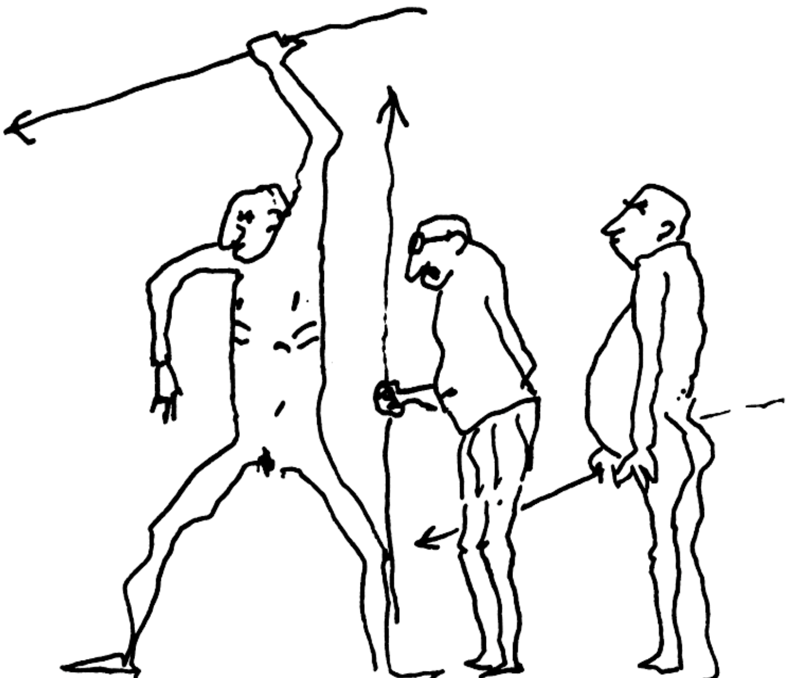

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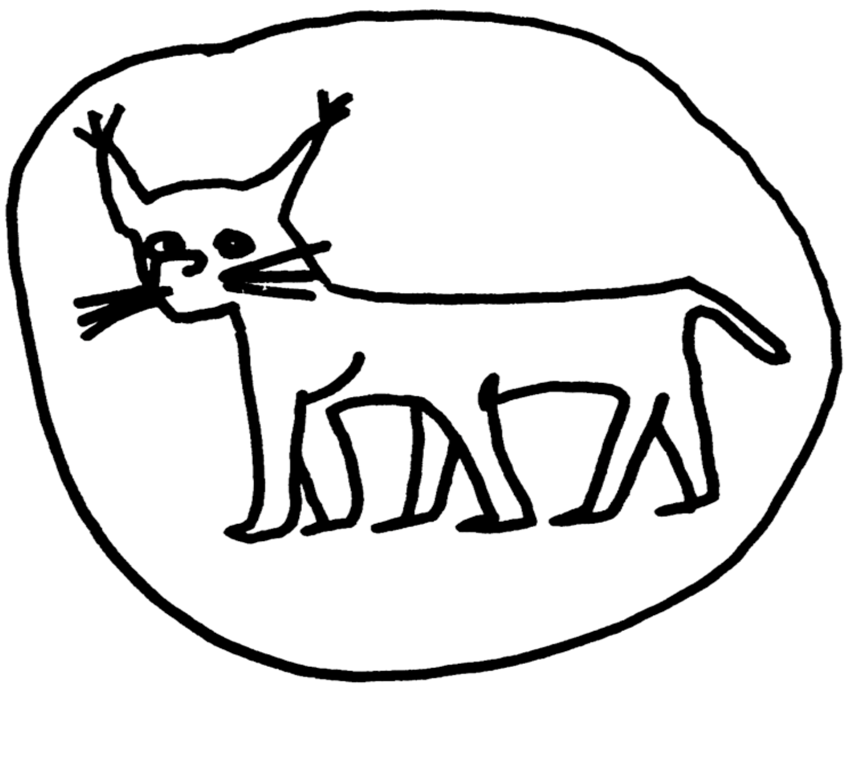

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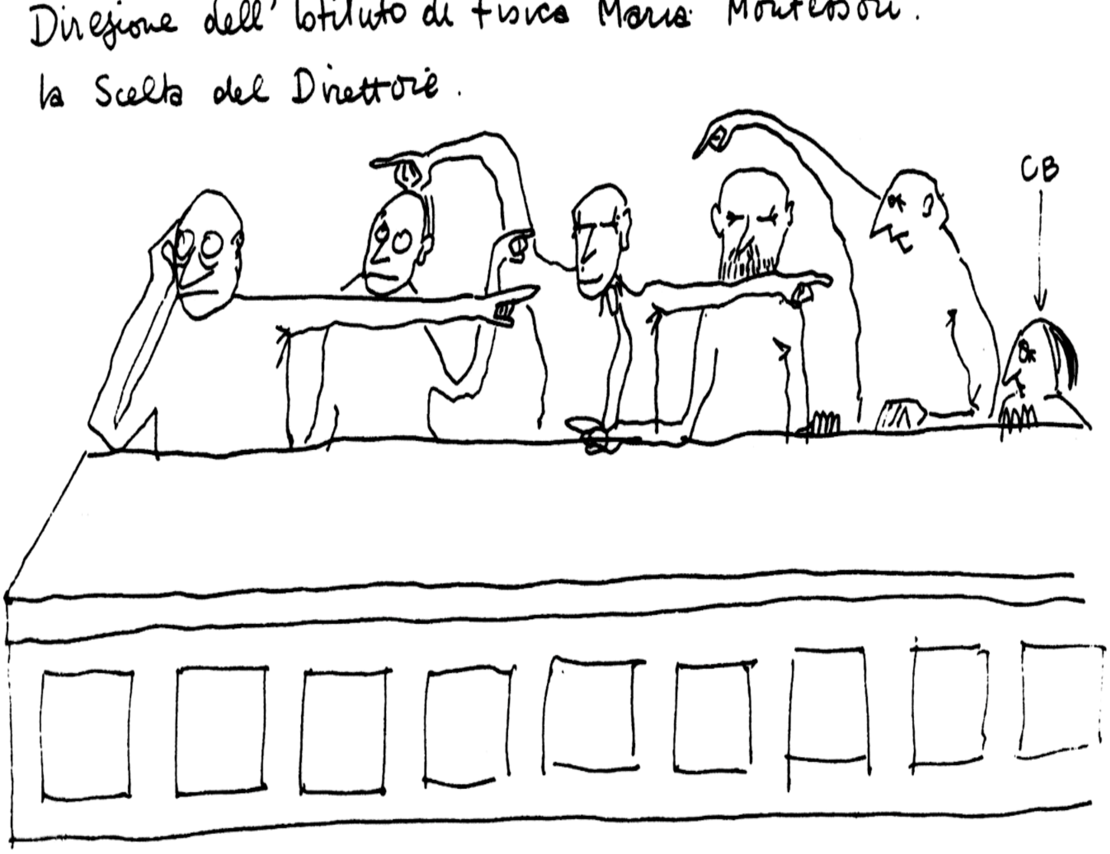
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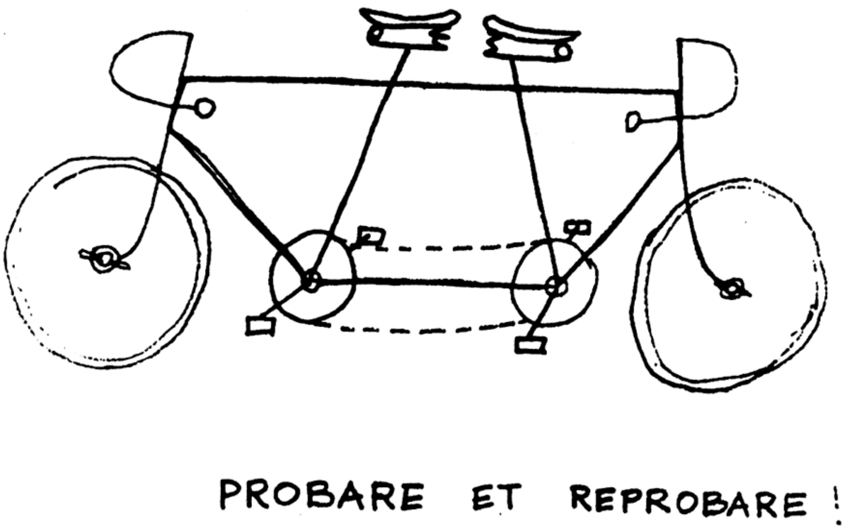
PROBARE ET REPROBARE !

- NOTES -

(1) For more detail see: E.Amaldi: "Bruno Touschek Legacy" (Vienna 1921 - Innsbruck 1978) CERN - Yellow Report, CERN 81-19, 23 December 1981;
"L'eredità di Bruno Touschek", Quaderni del Giornale di Fisica" 5, N°7 (1982) pp. 3-72.

(2) Paul Urban (b. Purkersdorf, near Vienna, 1905) obtained an engineering diploma (electrotechnics and machine construction) in 1928 at the Technische Hochschule in Vienna, and a Ph.D. (in physics and mathematics) at the University of Vienna in 1935. He has worked in industries (1928-30), in the Technical Section of the Austrian State Railroads (1931-39), and as Assistant (to Professor Hans Thirring) at the Institut für Theoretische Physik of the Universities of Vienna (1940-45) and Innsbruck (1945-46). Finally he became Professor of Theoretical Physics at the University of Graz (1947-1975), of which, since 1975, he has been "Professor Emeritus". He is the author of more than one hundred papers dealing with quantum mechanics, atomic and nuclear physics and elementary particle theory. He is also the author of a book of considerable interest: *Topics in applied quantum electrodynamics* (Springer Verlag, Vienna-New York, 1970), and edited, in collaboration with his pupil, Walter Thirring: "The Schrödinger equation", Lectures presented at the International Symposium 50 years of the Schrödinger Equation, *Acta Phys. Austr. Suppl.* **7** (1977).

(3) Arnold Sommerfeld (1868-1951) studied mathematics at the University of Könisberg and in 1893 went to Göttingen where he made his "habilitation" under the supervision of Felix Klein, whom Sommerfeld considered always his "master". In 1900 Sommerfeld was appointed Ordinary Professor of Mathematics at the Techinsche Hochschule of Aachen, where for six years he collaborated with a few high-level engineers in the solution of a number of technical problems (resonance phenomena in bridges, construction of locomotives, construction of ships, etc.). Sommerfield was one of the first supporters of the theory of special relativity of Einstein, which constituted one of the many subjects he used to deal with in his many courses of lectures. Starting from 1920 he made many important contributions to quantum theory, in the use of which he published, in 1919, a famous treatise *Atombau und Spektrallinien* (Vieweg, Braunschweig, 1919), of which a few editions appeared in the successive years. During the twenties Sommerfeld made various important contributions to the quantum theory developed by Heisenberg, Born, Schrödinger, Bohr, Dirac, and others, and to

which he devoted a further volume: *Atombau un Spektrallinien*: *Wellenmechanischer Ergänzungsband* (Vieweg, Braunschweig, 1929). Sommerfeld is well known not only for his many important papers and this fundamental book, but also for his six volumes of lectures in theoretical physics (Leipzig, 1942-62). Among his many pupils it is enough to recall: E.Fuess, H.Höul, W.Kassel, W.Lenz, W.Pauli, W.Heisenberg, H.A.Bethe. An extensive biography, in which also his vicissitudes during the nazi regime are recalled, has been published by Ulrich Benz: *Arnold Sommerfeld*, Vol. 38 of the collection "Grosse Naturforscher" published under the direction of Dr. Heinz Degen (Wissenschaftliche Verlasgesellschaft m.b.H., Stuttgart, 1975).

(4) Edmund Hlawka (b. 1916), Professor of Mathematics at the University of Vienna has made fundamental contributions, especially to the thoery of numbers.

(5) Paul Harteck (b. 1902), Professor of Chemical Physics, and subsequently Rector (1948-50) of the University of Hamburg and (since 1951) Distinguished Research Professor of Physical Chemistry at the Rensseleer Polytechnic Institute, Troy, N.Y. He is the author of more than 150 papers on experiments on para- and ortho-hydrogen, deuteron plus deuteron nuclear reactions, separation of hydrogen isotopes, artificial radioactivity, diffusion of slow neutrons, isotope separation by diffusion, and the chemistry of the Earth's atmosphere.

(6) W.Lentz [Frankfurt (Main, 1888-1952)] studied in Göttingen (1906-1908) and Munich (1908-1912) and later worked on various developments of quantum mechanics with G. Wentzel, W.Pauli, P.Jordan, A.Unsöld and J.H.D.Jensen. See P.Jordan, "The life of W.Lenz", *Phys.Bl.*. **13**, 269 (1957).

(7) H.J.D.Jensen (1907-1973), Professor of Theoretical Physics at the Universities of Hamburg and Heidelberg, author of numerous works on nuclear physics; shared with Maria Goeppert-Mayer the 1963 Nobel Prize for Physics, for their discoveries concerning the nuclear shell structure. No biography of Jensen has been published, according to his wishes, *Phys.Bl.* **29**, 233 (1973).

(8) Rolf Wideröe (b. Oslo, Norway, 1902), gained a Degree in Engineering at Karlsruhe, conceived the betatron in 1922, and submitted a thesis (Aachen, 1927) in which he set out the bases for the multiple acceleration of charged particles. He constructed the first European betatron (Hamburg 1943-44), and later a number of other machines of this same type for therapeutic use, working in the

laboratories of Brown Boveri (Baden, 1946-49). Wideröe has made a few other inventions in the field of accelerators; in particular, in 1943, while working in Hamburg, he proposed for the first time the use of storage rings for high-energy particles, in order to study nuclear reactions produced in the collision of particles moving with the same energy but opposite velocity [German Patent No. 876279 (1943)].

(9) A detailed history of the development of these ideas until 1940 is given by Wideröe: "Das Betatron", *Zeit, für angew. Phys*. **5** (1953) 187-200; "Die ersten zehn Jahre der Merfachbeschleunigung, Einige Historischen Notizien", Wissensch, Zeit, der F. Schiller Universität, June 13 (1964) 431-436.

(10) D.W.Kerst *Phys.Rev*. **58**, 841 (1940); **59**, 110 (1941); **60**, 47 (1941).

(11) D.W.Kerst and R.Serber, *Phys.Rev*. **60**, 53 (1941).

(12) R.Kollath (1900-1978), Professor of Physics at the University of Mainz, author of papers on collisions between slow electrons and ions against the molecules of gases. At that time Kollath belonged to the AEG Research Laboratory (founded and directed by C.Ramsauer). He also had problems because his wife was Jewish". He is the author of the book *Teilchenbeschleuniger* (Friedr. Vieweg and Sons, Braunschweig, 1955).

(13) Gerhard Schumann (b. Dresden 1911) studied in Halle and Leipzig, where he worked with Smekal on the mechanical resistance of glasses under traction. In 1950 he moved to Heidelberg where he worked under O.Haxel. Later he studied the "fall out" by means of the filter method and became an expert in the exchange phenomena in the atmosphere.

(14) Bruno, talking with me, used this expression in Italian, but Sir Rudolf Peierls has suggested that probably he referred to the operetta "Die Feldermaus" of Johann Strauss, where a very permissive prison is described.

(15) K.Gund (1907-1953) born in Vienna, studied in his native town and, starting from 1931, worked in Vienna, in the laboratories of Siemens und Halske. In 1936 he transferred to Siemens Reiniger Werke, Erlangen (Germany) where, in 1941, he started to develop a 6 MeV betatron for medical applications.

(16) Richard Becker (1887-1955), Professor of Theoretical Physics at the Technische Hochschule in Berlin, and at the University of Göttingen, author of numerous papers and excellent books on atomic physics, ferromagnetism, and plasticity.

(17) O.Haxel (b. 1909), Professor of Physics at the University of Heidelberg, author of numerous papers on nuclear physics. Particularly important is the paper in collaboration with H.J.D.Jensen and H.Suess on the shell model of nuclei.

(18) H.C.Kopfermann (1895-1963), Professor of Physics at the Universities of Göttingen and Heidelberg, author of numerous important papers on spectroscopy and nuclear physics, and of the book *Kernmomente* (Akademische Verlagsgesellschaft, Leipzig, 1940). See the biography by V.Weisskopf in *Nucl. Phys*. **52**, 177 (1964) where the list of his papers is also given.

(19) Werner Heisenberg (1901-1974), Professor of Theoretical Physics at Leipzig, Berlin, Göttingen and Director of the Max Planck Institut für Physik un Astrophysik in Munich. One of the founders of quantum mechanics, and author of many fundamental papers and books on atomic and molecular physics, ferromagnetism, cosmic radiation, and elementary particles. For an extensive biography see: Armin Hermann, *Heisenberg*, Rowohlt Monographien (Rowohlt Taschenbuch Verlag GmbH, Reinbek bei Hamburg, 1976 and 1977).

(20) F.G.Houtermans (1902-1966), Professor of Experimental Physics at Kharkov, Göttingen and Berne, author of numerous papers on spectroscopy, nuclear physics, and on the determinations of the age of rocks based on the measurement of their content of uranium and potassium and their decay products. Of particular importance are certain papers dated 1929, written in collaboration with G.Gamow on the alpha decay of nuclei, and others, written in 1930-31, in collaboration with R.d'E.Atkinson on the production of energy by means of nuclear reactions inside stars.

(21) W.Paul (b. 1913), Professor of Experimental Physics at the University of Bonn, author of numerous papers on atomic physics. He introduced high-energy experimental physics with accelerators in Germany.

(22) Ludwig Prandtl (1875-1953) was the founder of the boundary layer theory and the originator of the German school of aerodynamics. Among his many students in Göttingen the most notable was

Theodore von Karman. By their competitive efforts the problem of describing turbulent flow was clarified in the mid-1920's.

(23) C.F. von Weizsäcker (b. 1912), a pupil of W.Heisenberg, has made various important contributions to physics and astrophysics. He developed the liquid-drop model of the atomic nucleus which led him to the derivation of the so-called Weizsäcker mass formula. He was also one of the first people to recognize that the energy irradiated by stars is provided by certain nuclear reactions taking place at their centre. Weizsäcker has also devoted a considerable part of his activities to philosophy and politics.

(24) Philip Ivor Dee (b. 1904), Professor of Natural Philosophy at the University of Glasgow, is well known in nuclear physics as an author of numerous papers, among which I should mention his collaboration with James Chadwick in the discovery of the neutron. He is the author of the third of the three papers which appeared together in *Proc. R. Soc. London, Ser.* **A, 136** (1932).
J.Chadwick, "The existence of the neutron";
N.Feather, "The collisions of neutrons with nitrogen nuclei";
P.I.Dee, "Attempt to detect the interaction of neutrons with electrons".
From 1939 to 1945, Dee was a superintendent of the Telecommunications Research Establishment, where he was in charge of the group responsible for the development of airborne centimetric radar devices.

(25) John Currie Gunn (b. 1916), Professor of Natural Philosophy and Head of that department since 1973, at the University of Glasgow, is author of a long list of papers on theoretical physics.

(26) Sir Rudolf Peierls (b. Berlin, 1907) studied theoretical physics in Berlin, Leipzig, and at the ETH of Zurich, under Sommerfeld, Heisenberg, and Pauli. He has been Professor of Applied Mathematics at the University of Birmingham (1937-63) and of Theoretical Physics at the University of Oxford (1963-74). In addition to many important papers on the theory of solids, nuclei, and fields, Peierls has published a few books: *Quantum Theory of solids* (1955); *The laws of nature* (1955); *Surprises in theoretical physics* (1979). He is the editor of A perspective of physics, Vol. 1 (1977), Vol. 2 (1978).

(27) Ian Naismit Sneddon (b. 1919), Simson Professor of Mathematics at the University of Glasgow, is author of almost one hundred papers on Fourier transforms and mixed boundary value problems in potential theory.

(28) Walter Thirring (b. 1927), Professor of Theoretical Physics at the University of Vienna, author of many major papers on field theory and relativity.

(29) Roy Chisolm (b. 1926) is now Professor of Applied Mathematics at the University of Kent. He has also worked at Glasgow, Cardiff, CERN, Texas, and Dublin. He has done research in quantum field theory, especially on computational methods and on gauge theory, and in non-linear approximation theory, in particular on multivariate and other generalization of Padé approximation.

(30) B.Ferretti (b. Bologna, 1913), Assistant and later deputy of E.Fermi at the University of Rome. Subsequently Professor of Theoretical Physics at the University of Milan (1947), Rome (1948-1956) and Bologna (from 1956 onwards). Author of many important publications concerning quantum electrodynamics, field theory, and elementary particles.

(31) E.Amaldi: "The years of reconstruction", *Giornale di Fisica* **20**, 1986 (1979); *Scientia* **114**, 29 (1979).

(32) S.Weinberg, *Phys. Rev. Lett.* **19**, 1264 (1967); **27**, 1688 (1971).
A. Salam and J.Strathdee, ICTP/71/145, Int. Center Theor, Phys. Trieste (ICTP).

(33) Valentino Braitenberg (b. Bolzano, 1926) studied medicine and specialized in neurology and psychiatry at the University of Rome. After a few years devoted to research on the brain in Germany and the USA, he entered, under the influence of Bruno Touschek and Edoardo Caianello, into the research group of the Institute of Theoretical Physics of the University of Naples, which later became the Naples Section of the National Group for Cybernetics of the Consiglio Nazionale delle Ricerche (CNR). From 1961 to 1968 Braitenberg was Associate Professor of Cybernetics at the University of Naples. Called to a chair of Biology and Applied Science at CalTech, he howeveer accepted the almost contemporary offer of the codirection of the Max Planck Institut für Biologische Kybernetic, in Tübingen. On my request Braitenberg sent me a long letter, dated 27 July 1980, parts of which have been used here.

(34) Sir (Charles) Maurice Yonge, C.B.E., F.R.S. (b. 1899), Professor of Zoology at the University of Glasgow, has devoted a great part of his activities to research on marine biology and has occupied administrative, national and international positions. Among his publications I recall: The seas (1928); *A year on the grate barrier*

reef (1930); *British marine life* (1944); *The sea shore* (Collins, London-New Yord, 1949); *Oysters* (Collins, London-New York, 1960); *Physiology of mollusca* (ed. with K.M.Wilbur; Academic Press, New York, 1964-1967).

(35) M.Sands (b. 1919), at that time Professor of Physics at the California Institute of technology. The friendship between Touschek and Sands went on for the successive years and gave rise to a renewed tight collaboration in 1968-69 on problems concerning the electron storage rings. In 1952-55 Sands participated in the design and construction of the first electron accelerator of energy greater than 1 GeV (1.5 GeV Synchrtron of CalTech), in 1959 he was the first to propose a design of a 300 GeV proton-synchrotron, together with W.K.H.Panofsky, in 1963-69 he was in charge of the SLAC project during the building and first operation of the 3 kilometre linear electron accelerator. He is the author of various books, among which I recall (in collaboration with W.C.Elmore): *Electronics: experimental techniques* (McGraw-Hill, New York, 1949); and (in collaboration with R.P.Feynman and R.B.Leighton): *The Feynman lectures in physics* (3 volumes) (Addison Wesley, Reading, Mass., 1963-64). In recent years Sands became Faculty dean at the University of California, Santa Cruz.

(36) E.D.Courant, M.S.Livingston and H.S.Snyder, *Phys. Rev.* **88**, 1190 (1932).

(37) E.D.Courant, and H.Snyder, Brookhaven Internal report 1953 (unpublished) and *Ann. Phys.* (USA) **3**, 1 (1958).
J.B.Adams, M.G.Hine and J.D.Lawson, *Nature* **171**, 926 (1953).
E.R.Caianello, *Nuovo Cimento* **10**, 581 (1953).
E.R.Caianello and A.Turrin: *Nuovo Cimento* **10**, 594 (1953).

(38) E.Persico, E.Ferrari and S.E.Segre, *Principles of particle accelerators* (Benjamin, New York, 1968).
A.A.Kolomensky and A.N.Lebedev, *Theory of cyclic accelerators* (North Holland, Amsterdam, 1962).

(39) E.Fabri, *Nuovo Cimento* **11**, 479 (1954).

(40) R.Dalitz, *Philos. Mag.* **44**, 1068 (1953). Dalitz presented his results at the Congrés international sur le Rayonnement cosmique, organized by the University of Toulouse, under the patronage of IUPAP, with the support of UNESCO, at Bagnéres-de-Bigorre (6-11 July 1953).

(41) C.F.Chew, *Phys. Rev.* **89**, 591 (1953).
C.F.Chew and F.Low, *Phys. Rev.* **101**, 1570 (1956).
C.F.Chew, "Theory of pion scattering and photoproduction" (University of Illinois, Urbana, Illinois, 1956), p. 1-140.

(42) Abdus Salam, *Nuovo Cimento*, **5**, 299 (1957).

(43) M.Gell-Mann and Y.Ne'eman, "The weak current of the hadrons", Chapter 8 of the *The eightfold way* (W.A.Benjamin Inc., New York, 1964), pp. 171-206.

(44) T.D.Lee and C.N.Yang, *Phys. Rev.* **104**, 254 (1956).

(45) C.S.Wu, E.Ambler, R.W.Hayward, D.D.Hoppes and R.P.Hudson, *Phys. Rev.* **105**, 1413 (1957).

(46) R.Garwin, L.Lederman and M.Weinrich, *Phys. Rev.* **105**, 1415 (1957).

(47) R.E.Marshak and E.C.G.Sudarshan, in *Proc. Int. Conf. on Mesons and recently Discovered Particles* Padua-Venice, 1957 (Padua, 1957) and *Phys. Rev.* **109**, 1860 (1958).
R.P.Feynman and M.Gell-Mann, *Phys. Rev.* **109**, 193 (1958).

(48) L.L.Foldy and S.A.Wouthuysen, *Phys. Rev.* **78**, 29 (1950).

(49) W.Heisenberg, *Z.Naturforsch. a* **12**, 177 (1957).

(50) Fernando Amman has pointed out to me that a first mention of e^+e^- rings was made by Bruno at a meeting held in Frascati on 17 February 1960, in the frame of a series of discussions organized by Giorgio Salvini and devoted to the future of the Laboratori Nazionali di Frascati. There still exists a copy of the minutes of the meeting, prepared by Dr. Icilio Agostini, then Administrative Secretary of the Laboratories.

(51) D.W.Kerst, F.T.Cole, H.R.Crane, L.W.Jones, L.J.Laslett, T.Ohkawa, A.M.Sessler, K.R.Symon, K.M.Terwilliger and Niels Vogt Nilsen, *Phys. Rev.* **102**, 590 (1956) received 26 January 1956. At the beginning of their paper, these authors write: "... The possibility of producing interactions in stationary coordinates by directing beams against each other, has often been considered, but the intensity of beams so far available have made the idea unpractical. Fixed field alternating gradient accelerators offer the possibility of obtaining sufficiently intense beams so that it may now be reasonable to consider directing two beams of approximately the same energy at each other...".

(52) G.K.O'Neil, *Phys. Rev.* **102**, 1418 (1956), received 13 April 1956. In a footnote O'Neill writes that between the mailing and the publication of his Letter to the *Physical Review* he had become aware that similar suggestions had been made also by W.M.Brobeck of the Berkeley Accelerator Group and by D.Lichtenberg, R.Newton and M.Ross of the MURA Group.

(53) G.K.O'Neil,"The storage ring synchrotron", "*Proc. CERN Symposium on High Energy Accelerators and Pion Physics*, Geneva, 1956 (ed.:E.Regenstreif) (CERN, Geneva, 1956), Vol. 1, pp. 64-65.

(54) W.C.Barber, B.Gittelman, G.K.O'Neil, W.K.H.Panofsky and B.Richter, Stanford University Report HEPL 170 (June 1959). This report contains a description of the e^+e^- tangent storage rings and the proposal for its construction in view of an electron scattering experiment as a test of the limits of QED.

(55) W.C.Barber, B.Gittelman, G.K.O'Neil and B.Richter: "Test of quantum electrodynamics by electron-electron scattering", *Phys. Rev. Lett.* **16**, 1127 (1966).

(56) Typical examples are:

$$\gamma + N \rightarrow e^+ + e^-$$
$$\pi^- + p \rightarrow N + e^+ e^-$$

(57) Typical examples are:

$$p + N \rightarrow e^+ + e^- + X$$
$$p + p \rightarrow e^+ + e^- + X$$

(58) S.Drell and F.Zachariasen, *Electromagnetic structure of nucleons* (Oxford University Press, London, 1961), pp. 18, 19.

(59) L.D.Landau and E.M.Lifschitz, *Soviet. Phys.* **6**, 244 (1934), had considered the processes

$$e^+ + e^- \rightarrow e^+ + e^- + \left\{ e^+ + e^-, \mu^+ + \mu^- \right.$$

in the frame of pure electrodynamics. Processes of this type were later re-examined by F.E. Low [*Phys. Rev.* **120**, 582 (1960)], F. Calogero and C. Zemach [*Phys. Rev.* **120**, 1860 (1960)] and later by others: A. Jaccarini, N. Arteaga-Romero, G. Parisi and P. Kessler [*Compt. Rend.* **269B**, 153, 1129 (1969); *Nuovo Cimento* **4**, 933 1970)]; V.E.Balakin, V.M.Budnev and I.F.Ginzburg [*Zh. Eksp. Teor. Fiz. Pis'ma* **11**, 559 (1970); *JETP Lett.* **11**, 388 (1970)]; S.Brodsky, T.Kinoshita and H.Terazawa [*Phys. Rev. Lett.* **25**, 972 (1970); *Phys. Rev.* **D4**, 1532 (1971)]; H.Terazawa [*Rev. Mod. Phys.* **45**, 615 (1973)].

(60) See, for example: Proc. Int. Workshop on $\gamma-\gamma$ *Collisions*, Amiens, 8-12 April 1980 (ed.:G.Cochard) (Springer, Berlin, 1980).

(61) Giorgio Ghigo (b. Turin, 1929 - d. Rome, 1968); in 1948 he started to study engineering at the Turin Polytechnic, but some two years later was influenced by Gleb Wataghin to change over to physics, taking his degree in 1953. After a year spent studying cosmic rays in the Turin Section of the INFN. Ghigo joined, in September 1954, the Magnet Group of the Accelerator Section of the same Institute. As a highly skilled instrument engineer, he contributed substantially to the design and construction of the magnet for the Italian electron synchrotron, developing various high-quality instruments (flow-meters, magnetometers, quantum-meters, etc.). In 1959 he was appointed Machine Director of the Laboratori Nazionali di Frascati. He then participated in the AdA project and later in ADONE. After moving to Naples for two years he constructed, in 1962, in connection with the researches by Eduardo Caianello and Valentino Braitenberg, a completely transistorized machine capable of simulating 100 neurons, the purpose of which was to clarify certain aspects of the functioning of the brain. During the same period he dealt with various other problems of electronic simulation of the structure and/or functioning of the brain. On returning to Frascati he resumed his activities as a skilled instrument engineer, but was suddenly taken ill with an incurable disease and died on 15 March 1968, thereby leaving a serious gap among the researchers of the Laboratori di Frascati, and was sadly missed by all his friends.

(62) G.Ghigo, "Preliminary discussions on AdA", Internal Memorandum No. 62, 8 December 1960, Laboratori Nazionali di Frascati del CNEN.

(63) Deutsches Patentamt, Patentschrift Nr 876279 Klass 21 g Gruppe 36, Ausgegeben am 11, Mai 1953: Dr. Ing. Rolf Wideröe, Oslo, ist also Erfinder genannt worden: Aktiengesellschaft Brown, Boveri & Cie, Baden (Schweiz). Anordnung zur Herbeiführung von Kernreaktionen.

(64) R.F.Christy, "Synchrotron beam loss due to quantum fluctuations in the radiation", California Institute of Technology (1957), unpublished.

(65) M.Sands, "Observation of quantum effects in an electron synchrotron", California Institute of Technology (1956), presented at the West Coast Meeting of the APS in December 1956.

(66) C.P.Curtis, A.Galonsky, R.H.Hilden, R.A.Mills, R.A.Otte, G.Parzen, C.H.Pruett, E.M.Rowe, M.F.Shea, D.A.Swenson, W.A.Wallenmeyer and D.E.Young (MURA: Midwestern Universities Research Association), *Proc. Int. Conf. on High-Energy Accelerators*, Dubna, 1963 (Atomizdat, Moscow, 1964), p. 20.

(67) L.J.Laslett, V.K.Neil and A.M.Sessler, Lawrence Radiation Laboratory Report (UCRL 11090 (1963)) and *Rev. Sci. Instrum.* **36**, 436 (1965). See also: C.Pellegrini and A.M.Sessler, *Stanford Linear Accelerator Center Storage Ring Summer Study*, 1965 (SLAC, Stanford, Calif, 1965), Report SLAC-49, p. 61;
E.D.Courant and A.M.Sessler, *ibidem*., p. 36.

(68) F.Amman, F.Andreani, R.Gatto, G.Ghigo and B.Touschek, "Storage ring for electrons and positrons (ADONE)", Internal Report No. 68 of the Laboratori Nazionali di Frascati, 27 January 1961.
F.Amman, R.Andreani, M.Bassetti, M.Bernardini, A.Cattoni, V.Chimenti, G.F.Corazza, D.Fabiani, E.Ferlenghi, A.Massarotti, C.Pellegrini, M.Placidi, M.Puglisi, F.Sosò, S.Tazzari, F.Tazzioli and G.Vignola, "Two-beam operation of the 1.5 GeV electron-positron storage ring ADONE", *Lett. Nuovo Cimento* **1**, 729 (1969).

(69) H.Thirring, *Z.Phys.* **19**, 33 (1918); **22**, 29 (1921).

(70) L.Pietronero, "On rotating reference systems in Einstein's theory of gravitation", Internal Memorandum No. 337, 29 September 1971, Istituto di Fisica Guglielmo Marconi, University of Rome.
L.Pietronero, "The mechanics of particles inside a rotating cylindrical mass shell", *Ann. Phys.* (USA) **79**, 250 (1973).
L.Pietronero, "Gravitational intepretation of the centrifugal and Coriolis force", *Istituto di Alta Matematica, Symposia Mathematica* **12**, 57 (1973); "Mach's principle for rotation", *Nuovo Cimento* **20B**, 144 (1973).

(71) Student in the sense of 'goliard" (wandering student).

(72) Ferdinand Cap (b. 1924) Professor of Theoretical Physics at the University of Innsbruck, has worked in elementary particle physics and on general relativity. He had met Touschek for the first time during the fifties on the occasion of a few international conferences. Joseph Rothleitner, Professor of Theoretical Physics at the University of Innsbruck, met Touschek for the first time at the University of Heidelberg about 15 years before. At my request he has sent me a letter (dated 8 October 1980) about the last weeks of the life of Bruno in Innsbruck.

(73) Ernst Gartner (b. Vienna), designer and painter, teaches arts and drawing at the Gymnasium in Reutte, Tyrol. He has been a schoolmate of Bruno from the primary schools in Vienna and remained his closest friend during all his life.

(74) Ludwig Wittgenstein (b. Vienna, 1899 - d. 1951). After having frequented the Technische Hochschule in Berlin in order to study engineering, he dedicated himself to mathematical logic, fundamental problems of arithmetic, and philosophy of science. Initially he was strongly influenced by the works of Frege and Russell, from whom he later completely separated on philosophical views. His most famous works are: *Tractatus Logico Philosophicus* [edition in German and English, with an introduction by Bertrand Russell (Kegan Paul, Trench, Trubner and Co., London, 1932)], which he wrote in his early years, and his posthumous work: *Philosophische Untersuchungen* [English transl. by C.E.M. Anscomde: *Philosophical investigation* (Basil Blackwell, Oxford, 1953)]. He was primary school teacher in Austria and University Professor in England, musician, and architect. Although he was not a member of it, he was one of the main inspirers of the "Vienna Circle".

(75) For example, Karl Kraus, *Briefe an Sidonie Nadherny von Borutin* (1913-1936) (Deutscheer Taschenbuch Verlag, Munich, 1977), Vols. 1 and 2.

(76) Hans Weigel, *Karl Kraus oder Die Macht der Ohnmacht* (Deutscher Taschenbuch Verlag, Munich, 1972).
Werner J.Schweiger, *Das Grosse Peter Altenberg Buch* (Paul Zsolnay Verlag, Vienna-Hamburg, 1977).

(77) C.M.Nebe Hay, *Gustav Klimt: sein leben nach zeitgenössischen Berichten und Quellen* (Deutscher Taschenbuch Verlag, Munich, 1976).

(78) Karl Kraus, *"Sprüche und Widersprüche"* (1909) in *Beim Wort genommen* (Munich, 1955).

C. Rubbia with E. Predazzi, M. Ghigo, G. Salvini (hidden) and A. Ghigo during the Inauguration of the Bruno Touschek Memorial Lectures (1987).

THE ROLE OF BRUNO TOUSCHEK IN THE REALIZATION OF THE PROTON ANTIPROTON COLLIDER

Carlo Rubbia
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Here today , I would like to speak briefly on some of my personal recollections and of Bruno Touschek. I have met for the first time Bruno when I was a student at the Scuola Normale di Pisa. Luigi Radicati had succeeded in convincing Bruno to come periodically to Pisa by train from Roma and to give some lectures on subjects of his choice. Parity violation had just been discovered and the question of the true nature of the neutrino fascinated and obsessed Bruno. However he was even more fascinated by the role in Nature of fundamental symmetries like C, P, and T. The originality and uniqueness of his personality and of his ideas, even his strange accent, and, most of all, the enthusiasm and the drive with which he was literally *aggressing* the subjects in his lectures and in the subsequent long discussions, made a deep impression on all of us young students at that time.

Then I spent a few years in the United States. On my return to Italy, I moved to the University of Roma, where in the meantime Marcello Conversi had become professor. I met then often again Bruno in the wide and relatively dark corridors of the Physics Department. He had not changed, not even a bit. At that time he was in his full creative effort on electron positron colliding beams. I was extremely surprised that he could be talking about such "practical" devices, like those needed to accumulate particles, since I had known him only as a "champion of the Majorana neutrino". Then I understood that in his mind electron-positron collisions were nothing else than the way of realizing in practice the idea of symmetry between matter and antimatter, in the deep sense of the Dirac equation. I still remember him saying with a very loud voice, resonating in the corridors, "the positron and the electron *must* collide because of the CPT theorem". His boundless enthusiasm for particle-antiparticle collisions was dominated by a sense of perfect and intellectual esthetics, and rivaled only by his contempt for the other and more mundane alternatives of collisions of electron with electrons or of protons with protons, being explored at that time for instance by Jerry O'Neill, Andy Sessler and others.

One must recognize that talking about *practical* collisions between particles and antiparticles was at that time perfectly and totally crazy in the views of most of the so-called "reasonable" scientists, since neither the accelerator technology, nor the vacuum, —without mentioning the problem of accumulating *realistic* amounts of positron current—were known at the time. Norman Ramsey told me later that returning in those days from a trip to Europe and the Soviet Union he got as an answer: "there will never be enough luminosity to do *any* physics".

It was however evident that all these concerns had absolutely no influence on Bruno and that he was only attracted by the perfection and the beauty of a machine capable of producing "an excited vacuum". I remember him explaining that in this way "*all* possible (charged) particle states *must* be produced". In other words, it was the ultimate and definitive spectroscopy of all particle states. When later I met Budker, I realized how similar his and Bruno mental attitudes were toward *realizing the impossible* and of *thinking the unthinkable.*

The first time I met Budker it was in the United States, where Budker had come for a short visit, and precisely in California at a dinner with Wheeler at O'Neil's home. At that time proton-antiproton collisions had become the next "unthinkable idea". Shortly afterwards, Budker visited CERN with Skrijnsky, since he was very curious to see the progress on the ISR, which was being started at that time. He was not terribly respected by the accelerator community of CERN, much too conservative and attached to formalisms to fully appreciate the genius of the man. So I had to take personally a significant role in the visit, showing him around CERN etc. In order to smooth further the harshness of the reception at CERN and also in order to have a further chance "to pick at his brain", I decided to accompany both of them in their visit to Roma and to Frascati, where instead he was received very warmly and with an immense enthusiasm. Carlo Bernardini was the ufficial host. On the next day—which was some kind of a holiday—we were all invited for lunch in Bruno's home. Of course, the "lunch" lasted a major fraction of the afternoon. This has been for me the only occasion of witnessing the interesting interactions between Bruno and Budker, at the same time so similar and so different. While Budker tended to jump constantly from one subject to the other in a continuous firework of ideas, Touschek was saying much less and concentrating stubbornly on the same idea.

It is usually believed that the idea of transforming a conventional accelerator into a proton-antiproton collider was developed by me and collaborators in the late seventies and in order to observe the production of intermediate vector bosons. Actually the idea is to be traced far back in time and to Italy. About ten years before, Giorgio Salvini—at that time President of INFN—had asked a number of physicists, including myself, to meet in Pisa under "coach" Stoppini in order to come up with a recommendation for the next step in accelerators in Italy. At that time the SPS was not yet accepted and many people thought that one should have launched the "next step" on a national basis, and why not, also in Italy. I must say that hopes were not riding very high, if one considers that the name with which the project was unofficially labeled was Macchina Acceleratrice Italiana Protosincrotrone Inter Universitario—MAI-PIU' (Never Again). At that time, we had two alternatives: one was a conventional 80 GeV proton synchrotron, the other a proton-antiproton collider, based on Budker's electron cooling, in the same tunnel and 160 GeV in the centre of mass.

I remember I had a long discussion with Bruno on what one should do next. He had no doubt that the colliding beam solution was the correct line to follow. Clearly in his and in our mind at the time the proton-antiproton option was the logical continuation of the ADA-Adone line. Bruno's enthusiasm was—as usual—very contagious and Ghigo and myself started to work out in detail a possible and "least unrealistic" scheme. We concluded that the first step was the one of testing the idea of electron cooling experimentally. To that effect, we had planned to borrow from CERN the "electron analogue" of the ISR, at that time left unused in the Adam's Hall at CERN. We spent in fact several days at CERN and found that all components for cooling experiment were easily at hand at that time. What was lacking—and that we were prepared to provide—was the real interest in proceeding with the studies and the courage to take these things seriously.

Unfortunately, the end of that summer coincided with the end of our dreams, shortly followed by the tragic and sudden death of Ghigo. The whole matter was set to rest, since it was decided by the scientific community at large to concentrate all European efforts toward the political consensus needed for construction of the SPS. The Italian initiative for a collider-accelerator, as well as the projects in France and Germany for conventional medium energy accelerators were in the way of the larger CERN machine and had to be sacrificed. In a way this has not been all bad, since the MAI-PIU' option would never had the energy to reach W and Z thresholds!

Ten years later the fire of the proton-antiproton collision was still burning in the back of my mind, and I must say that so it was in the mind of Bruno. (The third person would—no doubt—have been Ghigo, if still alive!). As soon as he knew that the proton-antiproton collision adventure at last was actually going to start—although already terribly affected by his illness—Bruno decided to move immediately to CERN. I remember having long discussions with him first at CERN and then, toward the end, at the nearby Hospital de La Tour, where he was periodically admitted for intensive care. Although the body was clearly weakening, his mind was his sharp and lucid as ever. He was trying to assess for his own mind the relative merits between the electron cooling of his old friend Budker and the more modern stochastic cooling being worked by Simon Van Der Meer, Lars Thorndhal and Frank Sacherer (also tragically deceased soon after).

His approach was very indicative of the way in which his mind worked, totally polarized and almost uninterested of the way in which the problem was being tackled by others. His last paper—posthumously published by one of his then young disciples at Frascati—has been on stochastic cooling. Although it is clearly an unfinished job and it does not contribute significantly to the practical realization of the new device, it has all the flavours of his unique way of observing the world through the eyes of a true theoretical physicist.

It has been often pointed out that the contributions of Bruno in the field of antiproton cooling have been negligible. It is very likely so, especially if one looks at the impact of such a last, notebook paper. However there are ways of contributing to a field of science which cannot be quantized in terms of published papers and identifiable contributions. So it has been for instance the case of Niels Bohr who, in comparison with other top scientists of his time, has produced almost nothing—there is no Bohr equation, no Bohr effect, no Bohr constant, no Bohr discovery. As yet, without Bohr, today there will be no quantum mechanics. Likewise without Touschek's and Budker's contributions today there will be no colliding beams of matter-antimatter.

I have learned from Bruno how to love matter-antimatter reactions. Without this fact, my own scientific career would certainly have been very different. So I believe it is the case for many of us.

Thank you.

FROM ADA TO TRISTAN AND LEP

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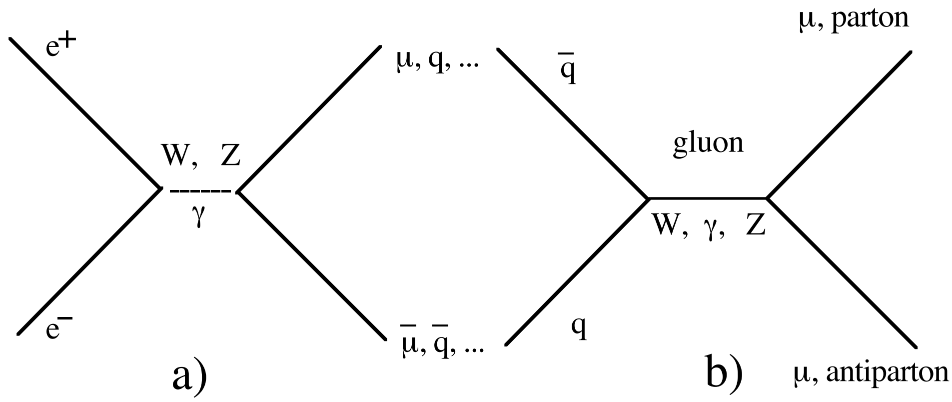
1. - Introduction -

Bruno Touschek, it has already been said, started to think of electron-positron annihilation in 1960, and dedicated the following years to this process with his Frascati group; preparing and controlling AdA in its experimental details, and studying and anticipating the scientific program for e^+e^- colliders in Quantum Electrodynamics, hadron physics, and weak interactions.

As you know, things went straight and fast; but there are some good reasons for this. The physicists working with him had been together seven years in the successful enterprise of building the Frascati electrosynchrotron in record time. Moreover their competence, still with large overlappings, was very specific and deep - from magnets to vacuum to detectors to theory, Bruno was extremely good and incredibly lucky.

But his interest enlarged very soon to all the matter-antimatter physics, something that we express in the simplest, tree level form, with the two diagrams, a), b):



He was soon regretting that e^+e^- colliders could not arrive at the high center of mass energies of proton-antiproton collider.

Let me interrupt for a moment the development of e^+e^- colliders from AdA and Adone to TRISTAN and LEP, which I shall report in § 3, to relive the immediate interest of Bruno for proton-antiproton physics in 1966. So, I break the logical continuity of my talk. But when presenting the development of physics, Bruno always was in favour of the historical order as the first fundamental point of reference. And this I do.

2. - In Paris, 1966 -

We were at Saclay (Paris) at the end of September 1966; I remember the session dedicated to Novosibirsk and the method of cooling antiprotons, as suggested by G.I Budker. But Budker was only at the beginning of his report, and Bruno Touschek had understood everything; he was getting excited, could not keep himself. The Budker's way of using the electrons to cool antiprotons by soft collision and heat exchange was deeply inside his statistical sensitivity." That's it", Bruno told us that morning. "We cannot get highest energies with electrons, but we'll get them by proton-antiproton collisions. It is a most important development, and probably this is not the only way to tame antiproton beams".

In fact, in that morning G.I.Budker said "If we suppose a proton yield of $3x10^{11}$ particles from the injector, a proton-antiproton conversion factor of 10^{-7} and a transverse beam cross section of 10^{-1} cm^2, it is possible to contemplate a luminosity as great as 10^{36} cm^{-2} day^{-1}. We do not consider this figure as quite realistic but it leaves us the large security factor which is necessary for undertaking such a complicate project".

I am sure Bruno would have been happy for my recalling these words of Budker (Proc. Int. Symposium on Electron-Positron Storage Rings, Saclay, Sept 26-30, 1966. Page II, 1.1, Presse Universitaires de France). Many years had to pass before the Budker vision - and with method of cooling different from the electron cooling - could become reality. But I wish to recall that the maximum hope for the present $\bar{p}p$ rings is a luminosity of 10^{31} cm^{-2} s^{-1}, which, in the Budker units, is very close to 10^{36} cm^{-2} d^{-1}.

I'll come back to hadron colliders; but now I must give a tracking shot of e^+e^- physics, from AdA to TRISTAN.

3. - From AdA to TRISTAN -

The wonderful story of e^+e^- colliders has been a world phenomenon, something like mammals taking over hadronic dinosaurs (this analogy has not been invented by me). It would be a mistake not to spread the credit of the e^+e^- success all over the world, and Bruno, whose stature was unsurpassed by any other physicist in this field, would in this case be the first to protest. Frascati and Novosibirsk, with the e^-e^- two ring system of Stanford just a few years before, were the little mammals who started first.

In Table I we gave an indicative, incomplete list of colliders, from 250 MeV up to 100 GeV, the size of the forthcoming LEP.

The first important results came from France (ACO) and USSR (Novosibirsk) (1966-70). It was immediately clear that the width and cross sections of vector mesons (remember the famous vector dominance) could be analyzed and interpreted (this is the main point) to a new level of scientific understanding. But it was Adone of Frascati that threw a beneficial stone in the pool: the multihadronic production. This rather high production of 2,3, ...pions in e^+e^- annihilation could not be explained by the efforts of Sakurai, Greco and others to extend vector dominance. As B. Richter noted [B. Richter: Proceedings XVII Int. Conference on High Energy Physics, London July 1974, P.IV-37] in 1974, just before the J/Ψ discovery - "Theory is in a confusing state, and it is to the experimenters to clear the air a bit".

After the Adone multihadronic step, the next big discovery came from machines of higher energy. It is the J/Ψ, observed at SLAC and Brookhaven with SPEAR and with the conventional Proton Synchrotron of Brookhaven. Unfortunately the mass of J/Ψ was 3100 MeV, and Adone had its maximum nominal energy at 3000 MeV c.m. As soon as we heard of J/Ψ we broke the severe limit imposed on our machine Adone and jumped to 3100-3200 MeV. Just in time to share the joy of the discovery in the same issue of Phys. Rev. Letters, but with a note in the text which I am very happy we added "Soon after the news that a particle of 3.1 GeV...."(C. Bacci et al., Phys. Lett. 33 (1974) 1406)

After that year, 1974, a fantastic series of experimental results were produced, which formed a consistent tissue whose weft and warp are energy and resolving power. New quarks were identified, Charm and Bottom, and gluons recognized.

Pretty soon the different levels of the charm-anticharm system (Ψ,Ψ'…) and the mesons formed by c quark (antiquark) bound to lighter quarks were identified and measured with a precision which only e^+e^- colliders could allow. So we saw in the seventies the rise of the precise spectroscopy of the "new Schrödinger atoms", as someone called the J/Ψ with its excited states and the descending D mesons and η_c, and later the Υ (upsilon) with its excited states producing the B mesons. In fact the structures of the Ψ and the upsilon families are one of the most elegant sights of nature. It is true that we do not know yet what is the real exact potential of this Schrödinger representation, but the general levels are described by a very good approximation, and the theoretical predictions are beautifully respected.

One thing was uncertain in the 70's: the possible mixing between particle and antiparticles, at least in the case of B mesons. It is just in these last two years that the mixing has been measured, still with large uncertainty in the case of B_s ($\equiv \bar{b}$ + s) and B_d ($\equiv \bar{b}$ + d). First indications came from $\bar{p}p$ physics at UA1, but it is clear that precise results can only come from the e^+e^- collider. The mixing of $B\bar{B}$ systems, the decay modes of the b from b to c and b to u quark, and perhaps, some years from now, the measurement of CP violation, are among the great questions today.

This new trend is very important and it is felt all over the world: there are B factories proposed in Switzerland (SIN), in Frascati (linear accelerator), and in the Soviet Union (Novosibirsk) and of course we expect a rejuvenation of CESR, the Cornell (USA) e^+e^- collider ring. It seems that in this case there is the pre-eminence, once in a while, of luminosity, precision, and intensity over the absolute center of mass energy of the interaction.

This also gives us the occasion to pay homage to the electrons, our oldest and lightest friends: they can be measured, energy and momentum, up to a precision better than 1%; their radiation properties are very well understood and easy to verify. When circulating, the high energy electrons give rise to the synchrotron radiation. It is not necessary here to dwell on the tremendous importance of the synchrotron light spectroscopy in atomic and molecular physics, and in biology.

Now, we expect LEP and SLC (Stanford Linear Collider). They shall verify the electroweak theory at a new level, and we know that the width of $W^\pm$ and Z shall be measured with new precision, impossible to $\bar{p}p$ physics. The first clear result could be the measurement of the number of (light) neutrinos, through the precise measurement of the width of the Z boson.

Yes, it was unjust that Bruno, who would be 65 years old today, could not participate in the great developments from the "November revolution" (the J/Ψ, 1974) until today. And Budker with him. Neither was able to enjoy the great developments, which descended from the fundamental seed, AdA and first VEP.

B. Touschek and G.I. Budker deserve, and have, a place in history. When the trumps of glory bypassed Bruno for recognizing the importance of the J/Ψ discovery, he was not happy, although he accepted this with great dignity. Perhaps I felt this was an injustice even more than him.

4. - e+e- versus $\bar{p}p$-

As I said, the main interest of Bruno was for matter-antimatter physics, even if he always considered the e^+e^- ring the excellent machine. Unfortunately, the total energy available was not enough to exploit the 100 GeV and above realm, which was essential to fulfill his curiosity in the field of weak interactions. So he was intensely interested in $\bar{p}p$ rings, from Budker's suggestion to the fundamental proposals and work of Carlo Rubbia and Simon Van der Meer. This has already been said by E. Amaldi and C. Rubbia.

I recall the discussion with him on the merits of lepton versus hadron colliders. The proton was too complicated, too rough to develop clean "indisputable" experimental fact. He warned me more than once to watch carefully all details before being sure of a $\bar{p}p$ result. Only a pure - impossible - quark antiquark ring could be as clean as an e^+e^- ring. But still in that case (we now know how true this is), the ubiquitous gluon would make results rather complicated. QCD is beautiful and bristly, and Bruno was well aware of it.

All that I am saying is commonplace and obvious, and cannot give the real image of a man who could not care for details, academy, standard models. He had character, his irony, joyous intuitions, deep human feelings, his stars to watch. There have been many times in these past ten years I have regretted of no longer having the possibility to consult him. During his last days in Geneva (1978) I visited him, told him about our work at CERN, and almost every day discussed the developments of UA1 in detail. He was severe, but we sometimes had light moments, when he laughed heartily at something, close or not to $\bar{p}p$, to physics, to physicists.

There is one thing I wish to recall. In his last weeks of life, he collected art books dealing with painters and sculptors of this and past centuries especially those born or working in central Europe, and he felt happy in contemplating them and showing them to his friends. With this last interest he was focusing on himself as a great European, with his deep roots in the middle of our continent.

Yesterday and the day before I had the pleasure of spending more than two hours with Gilberto Bernardini and with Luigi Radicati. They were regretting not being able to be here today, and allowed me to report some of their thoughts.

Gilberto, commenting on the death of Bruno in 1978, three years before the success of W, Z which crowned the Standard Theory, was saying that Bruno had lived and interpreted only the first (1960-78) Renaissance of physics, something like experiencing the full "Quattrocento", missing the "Cinquecento"(our eighties). This brought Gilberto to connect the person of Bruno to those dry and vigorous figures expressed by Donatello in his sculpture and in particular he showed me one famous portrait (attributed to Donatello): that of Niccolò da Uzzano, a masterpiece of sculpture, where dignity and intellectual power emerge. I present it to you at the end of this talk.

Luigi Radicati gave me a note. I shall not translate it(*), for it is, with his style, clear and transparent:

> *"Ho avuto la fortuna di stare vicino a Bruno Touschek durante più di vent'anni. L'ho conosciuto come fisico e da lui ho imparato molto: anzi vorrei dire che Bruno é stato una delle persone che ha avuto più influenza sullo sviluppo del mio pensiero scientifico. La forza di Bruno era la sua straodinaria originalità, che si innestava su una conoscenza profonda della fisica. Era chiaro che egli veniva da una tradizione fisica assai più ricca di quella nella quale ero cresciuto io. Per Bruno la fisica era cosa quasi innata che aveva le sue radici nel pensiero di Boltzmann, di Gibbs, di Sommerfeld, di Heisenberg. Per lui la meccanica statistica era una cosa innata. [L'ho dovuta penosamente imparare e quel poco che ne so l'ho appreso da lui].*
>
> *Ma Bruno non lo ricordo solo come fisico: per me è stato soprattutto un carissimo amico con il quale ho passato delle ore indimenticabili, con il quale ho discusso di tutto, di storia, di arte, di letteratura, di filosofia. Era una personalità ricchissima e poliedrica: venivamo da tradizioni profondamente diverse, ma questa diversità anzichè dividerci ci legava.*
>
> *Con lui ho perso uno degli amici cari e a lui penso dopo tanti anni con immenso rimpianto"*

Bust of Niccolo' da Uzzano by Donatello.
Circa 1430. Polychrome terracotta, Museo Nazionale del Bargello, Florence

(*) *Note translated by the Editors*

I have had the great fortune to be close to Bruno Touschek for more than twenty years. I knew him as a physicist and I have learnt from him a great deal: I would even say that Bruno has been one of the persons who had the greatest influence on my scientific life. His great strength was his extraordinary originality of thought, based on a deep knowledge of physics. He clearly was coming from a physics tradition much richer than the one I had been brought up in. For Bruno physics was something to be born with, something which had its origins in the thinking of Boltzmann, Gibbs, Sommerfeld, Heiseberg. For him statistical mechamics was something intrinsic to his being. [I had to painfully learn it and the little I know came from him].

But I remember Bruno not only as a physicist : for me he has been mostly a very dear friend with whom I spent unforgettable days and hours, with whom I discussed about everything, history, art, literature, philosophy. He had an extremely rich and polyedric personality : we came from very different cultural backgrounds, but this diversity, instead of dividing us, was the greatest bound.

With him I have lost one of my dearest friends and to him, after these many years, I still think with immense longing..

TABLE I

Storage ring (laboratory)	Particles	E_{CM} (GeV)	Luminosity($cm^{-2}s^{-1}$)	Physics	Results
ADA(Frascati)	e^+e^-	0.4	10^{24}	1961*	1963
VEPP-1(Novosibirsk)	e^-e^-	0.32	$5x10^{27}$	1962*	1965
Stanford Storage Rings	e^-e^-	1.1	$2x10^{28}$	1962*	1965
VEPP-2 (Novosibirsk)	e^+e^-	1.4	$3x10^{28}$	1964*	1966
ACO (Orsay)	e^+e^-	1.1	$1x10^{24}$	1965*	1967
ADONE (Frascati)	e^+e^-	3	$6x10^{29}$	1969	1970
ISR (CERN)	pp	63	$>10^{32}$	1971*	1972
CEA (Cambridge)	e^+e^-	6	$3x10^{28}$	1971*	1971
SPEAR (Stanford)	e^+e^-	8.2	$2x10^{31}$	1972	1972
VEPP-2M (Novosibirsk)	e^+e^-	1.4	$3x10^{30}$	1974	1975
DORIS (Hamburg)	e^+e^-	11	$3x10^{31}$	1974	1974
DCI (Orsay)	e^+e^-	4	$\sim10^{31}$	1975	1976
CESR (Cornell)	e^+e^-	16	$6x10^{31}$	1979	1979
VEPP-4 (Novosibirsk)	e^+e^-	11	$2x10^{31}$	1979	1981
PETRA (Hamburg)	e^+e^-	45	$>10^{31}$	1978	1980
PEP (Stanford)[26]	e^+e^-	30	$>10^{31}$	1980	1980
$\bar{p}p$ Collider (CERN)	$\bar{p}p$	900	$\sim10^{30}$	1979	1980
TRISTAN (KEK)	e^+e^-	70	$\sim10^{32}$	1986	1987
TEVATRON(Fermilab)	$\bar{p}p$	2000	$\sim2x10^{30}$	1985	1986
SLC (SLAC)	e^+e^-	100	$\sim2x10^{30}$**	1988	1988
LEP (CERN)	e^+e^-	100-200	$3x10^{31}$**	1989	1990

* No longer running

** Design luminosity

MEMORIES OF BRUNO TOUSCHEK

Raoul Gatto
Department de Physique Théorique, Université de Genève

When Mario Greco called some months ago, he asked for a general talk on the present status of electroweak theory. I accepted with pleasure and I felt honored to be able to present such a talk within this commemoration of Bruno Touschek, one of the most intelligent physicists I have ever known, and a dear friend. Later on, Greco informed me that the program had to be modified and that he rather expected a talk within the present open session. It is much harder for me to talk on things that go beyond present day physics, essentially because of the limitations of my personality. But, I consider a compelling duty to dedicate my thoughts to Bruno Touschek and to some of the physics to which he contributed. We are here to commemorate Bruno, who was a friend of most of us, a most original and profound physicist, who disappeared so prematurely, leaving all of us in great sadness. I think that for what he did, he deserved much more than the difficult times and the circumstances of his life offered him(1). Particularly to me, the memory of Bruno is so dear, as he was, together with Bruno Ferretti and Edoardo Amaldi, one of my first teachers in physics. I learned a lot from him, discussing entire afternoons at my early times in physics during the years 1953-1956.

I never had unfortunately the chance of directly collaborating with Bruno. I must say that the only paper where our two names appear jointly was the internal Frascati report(2) containing the Adone proposal, written together with Giorgio Ghigo, Fernando Amman, and Carlo Bernardini (*).

But I had only been asked to add a few pages on the theory to this proposal, which was essentially the work of Bruno and his collaborators. The reason for this lack of direct collaboration was mostly logistic. I was too inexperienced in the period in Rome before I went to the United States, and, afterwards, I had to travel so frequently between Universities that I could not enjoy that constant precious contact with Bruno that I had had before. So my most intense memories go mainly back to the years

(*)Editors Note: This report is repoduced in Appendix A.

from 1953 to 1956 when I met Bruno almost every day and I talked with him and learned so much from him. Looking back at the work of that time, I see how often I felt I had to acknowledge his generous help and encouragement. Most important was his friendliness and his consideration. At that time, especially at the beginning, I felt rather lost and insecure, in a career which seemed to be very competitive and where some people occasionally exhibited an intense pride of hierarchies. Bruno, on the opposite, was friendly, cordial, encouraging. I remember I was 22, at a Conference in Cagliari. He was sitting at a café with Pauli, who participated to the meeting, and I was passing on the sidewalk, rather trying to get unnoticed. He called me and wanted me to sit down with him and Pauli and take part to the discussion. Similar things happened many times. When a foreign visitor arrived, we often went with the visitor to Albano or Nemi, two small towns here in the neighborhoods, for a walk and a glass of wine. He had bought at that time a strange sport car, I think it was a Triumph, an extremely uncomfortable convertible. He used to drive in full winter with the windshield lowered so that all the air would blow directly into our faces. Before returning to Rome, in the not very dense but totally disordered traffic of the Roman fifties, he would not separate from the colleagues before pronouncing the historical sentence that the fighters in the Coliseum would tell Cesar in the old Rome: "morituri te salutant", in his wonderful precise Latin. He was referring to the uncertain conditions of his car. I think we lived in that period a rather adventurous life, but the friendliness and generosity of Bruno were an incomparable and unforgettable compensation.

I have been instructed to try to give a view on what were the theoretical problems of the late fifties, which related to the yet inexisting electron-positron physics. As always happens when one tries to compare with older times, one cannot avoid to remark how different it was from nowadays, how much more limited were our problems and purposes. Of course, it would not be correct, historically, to judge on such a perspective. At the same time, comparing with all that was later done illustrates, I think, the courage and vision of Bruno, with his unique combination of competences in so many different fields of physics.

As the older people in this audience will remember, one of the dominant problems of theory in the late fifties, was that of the nucleon's electromagnetic from factors. Measurements had been done at Stanford, a laboratory that was at that time, and still is, at the advancing frontiers of physics. Already since 1955, Hofstadter and McAllister(3) first observed structure effects in the proton, and in the subsequent years an impressive amount of data was collected. In a very short note of remarkable originality, in 1957, Nambu(4) pointed out two main features: (i) the relevance of using a spectral representation, and, (ii), the possible role of mesonic resonances. Specifically he drew attention on the role of a possible isoscalar resonance of the type later called ω (but he called it ρ). The isoscalar property would guarantee same sign for proton and neutron. On the other hand, what Nambu called the pion cloud, the isovector part, would change sign. The electric form factor would thus add in the proton and approximately cancel in the neutron. The dispersion theory approach for the nucleon form factors was soon later developed by Chew, Karplus, Gasiorowicz, and Zachariasen(5) and by Fedurbush, Goldberger, and Treiman(6). Basic to the dispersion analysis is the knowledge of the absorptive contribution, like in optics for the Kramers-Kroning relations. For the nucleon form factors the absorptive part starts with contributions which correspond to a virtual time-like photon going into two pions for the isovector part, and into three pions for the isoscalar part. Having an electron-positron machine would have rapidly settled most of the problems. Nobody however dared to start such a project. When e^+e^- machines became operational, and it was essentially the merit of Bruno and of a few other courageous physicists, part of this particular history had already been unveiled. Frazer and Fulco(7) had already proposed a resonant isovector pion-pion interaction. The experimental evidence came from pion-proton inelastic collisions, preliminarly by Derado(8), and through extrapolation methods by Anderson at al.(9), by Erwin et al(10), by Stonehill at al(11). As for the isoscalar resonance, that Nambu had conjectured, it was Maglic, Alvarez, Rosenfeld, and Stevenson(12) who discovered it in proton-antiproton. But the precision work still came from the electron-positron machines.

This is one particular aspect of the theoretical situation and problematics of that time. Another aspect had to do with the efforts to test the validity of quantum electrodynamics. Again at Stanford, especially Sidney Drell(13) had pushed in this direction. In Europe, we had the successful g-2 experiment(14).

Electron-electron collisions would allow to test the photon propagator. I remember a conference by Professor Panofsky, at the end of 1959, reporting on the pioneering work of Barber, Gittelman, O'Neil, Panofsky, and Richter(15), on electron rings. Answering to a question, Panofsky mentioned that, to test the electron (rather than the photon) propagator, electron-positron collisions would have been suitable, through observation of 2-photon annihilation, but that such a development could present additional technical difficulties and that for the moment had been postponed. This also, I think, shows that a strong courage and optimism was required to embark in the direction of e^+-e^- collisions and, beyond any doubt, without the vision, the optimism, the courage of Touschek, e^+e^- physics would, at least, have suffered a delay.

The Frascati laboratory produced at that time first class physics, in a quiet and almost imperceptible way. The Frascati atmosphere was a typical country atmosphere. From the windows of our offices one could admire a large extension of vineyards and sometimes hear people singing what in America would be called country songs. It was a relaxed and perhaps provincial atmosphere. But it gave all of us the possibility of working hard and of imagining the future not only the immediate future but also what was, for that time, the far-away future. To imagine, for instance, the e^+-e^- production of neutral weak vector bosons, coupled to neutral currents, or the e^+-e^- production of pairs of weak charged vector bosons, and the weak asymmetries which are now being measured. That relaxed Frascati atmosphere may have been provincial, but certainly it gave all of us a feeling of doing something together, and this something was worthwhile. All this we owe to Bruno, to his scientific and human qualities. The contribution of Touschek's direct collaborators, Giorgio Ghigo, Carlo Bernardini, Gianfranco Corazza, who were the initial collaborators for AdA(16), of Querzoli, Sacerdoti, Puglisi, Massarotti, Bizzarri, Di Giugno, of Marin and Lacoste at Orsay at those early times, was of the highest quality(17)(18). Fernando Amman took the responsibility of directing the Adone project(19). As far as theory is concerned, let me mention the contribution of Nicola Cabibbo and the contribution of Francesco Calogero. Much physics was done with Adone. Much more, we all know, could and should have been done, were it not for situations and circumstances, which were essentially external to us physicists.

I shall not go back to those results, to which so many Italian physicists contributed(20). Although mainly concentrated on proton machines, CERN was not insensitive to progress on electron-electron and electron-positron physics. Already in June 1961, a conference on very high energy phenomena was organized at CERN and it was remarkable that all the three invited talks on electromagnetic interactions were on electron-electron and electron-positron colliding beams. One of the three talks was given by Bruno, who gave an exact presentation of Ada and of the Adone project. The report(21) is in the Proceedings, which were edited by John Bell et al.

We know that Touschek had a deep respect for Pauli. His relations with Pauli were steady but they became more intense when Pauli got interested in what were later called the Pauli-Pursey transformations, a general class of rigid, that is global as opposed to local, transformations(22). This was towards the end of Pauli's life(23). But, even before, Touschek always found very attractive Pauli's ideas on non-abelian gauge theories (Professors Enz and Jost have recently helped me in clarifying this part of Pauli's history). Bruno often told me of these, for that time, quite new ideas(24). Touschek and, I must say, also Ferretti, during so many discussions, always showed a special attention to the role of gauge invariance. In a sense I am grateful for this to both Touschek and Ferretti, as they transmitted this interest also to their students.

What I learned from Bruno was also a sort of style. He never liked extremely long calculations and uninspiring formulae. He put ideas and invention before the hard mechanical effort. When he wrote a formula he seemed to carefully draw it, designing, more than just writing it down. He never would waste his time in checking hundredth of papers in the literature, but he would rather try to go directly to the heart of the problem. He first wanted everything to be simplified and reduced to the essential. His beloved books, in physics, were few and of classical authors, Sommerfeld, Pauli, Heitler. Once, he was going on vacation to the mountains, and he told me he wanted to work on beta decay. The only thing he was taking with him was a very small notebook, still empty. No books, no articles, no preprints. The notebook was extremely tiny. Like any good theoretician he always thought that right things have to be simple and not require a cumbersome apparatus.

Touschek had a deep classical culture, which certainly allowed him to assimilate the Italian culture and to adapt himself so easily to our country and our people. A deep side of his personality was however his relation to the Viennese culture. I always found remarkable how Austrian culture, Anglo-Saxon culture, and Latin culture could so well coexist in him. He had deeply thought and elaborated on the aspects of these apparently so disjoined cultures. The Jewish culture was also part of his personality. I think it became manifest in his particular intelligent, sometimes critical, sense of humor, which reminds me of modern Yiddish theater.

In autumn 1977, already seriously ill, Bruno was at CERN. In spite of his evident unhealthy conditions he always was willing to discuss. He often developed typical particular interests, even outside physics. He liked to speculate on that explosion of cultural life that characterized the Vienna of Franz Joseph. For that, he proposed a socio-economical explanation, which included elements of politics and also of urbanism. Unfortunately I have not been able to entirely reconstruct his arguments, which perhaps I never could follow completely because of my incompetence.

I had written, in the first version of these notes, additional recollections of my last encounters with Bruno at CERN. I think they are not really so relevant here, although they will remain vivid in my memory. When I learned of his death in Innsbruck I was so shocked that for a few days I could not do any useful physics. All those, among us, who had the privilege of knowing Bruno, will never forget him, and we have an immense debt of gratitude to his intelligence, generosity, and friendliness.

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C. Bernardini during the Bruno Touschek Memorial Lectures (1987).

BRUNO TOUSCHEK AND THE IDEA OF ADA

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For many people in this room it will not be an easy task to go back in time and imagine the Frascati Laboratories at the end of 1959-beginning of 1960, more than 27 years ago. At the time, after working on the synchrotron for many years with Enrico Persico, I was moving to high energy experimental physics. Actually, I was engaged in 3 simultaneous and different activities: the main one was a μ-pair experiment, a test of QED; an occasional one was a collaboration with the Salvini-Silverman group in a search for ρ° particles and there was a residual love with machine problems, concerning the electron losses due to insufficient RF trapping because of quantum radiation fluctuations.

In this last activity I was tightly connected with Bruno Touschek and Fernando Amman: actually, Fernando was taking measurements on the machine and Bruno and I were trying to produce a decent theory as an improvement of some calculations by Christy and Sands.

Bruno and I were talking quite frequently about the future of the laboratories. There was a lot of disappointment around, people with excellent technical skills had no more machines to build. Physicists from other Italian universities had just come to Frascati to work at their experiments, being rather extraneous to the group of the aborigenes. There were guests, mainly from Cornell, CalTech (USA), and Tomsk (USSR). The criticism against the choice of an electron synchrotron was endless ("pale deluding beams"): a lot of proton-lovers were continuously dreaming of the copious particle production at proton machines.

Bruno was quite sensitive to the general climate. He used to say that the best justifications for an accelerator is some important threshold. The vector mesons had impressed him very much, particularly because of their quantum numbers: a photon can go into a φ° or ω° as it is, without the collaboration of other particles. Also, he did not like very much proton beams and the proton-proton physics just because, as he said, that kind of physics was not gentle and delicate enough. "Protons cry too loud", was one of his usual sentences.

The events of those days have already been told, particularly by Edoardo Amaldi in his very accurate biography of Bruno. There is room left only for very personal memories which, in a sense, might have less interest for the audience. Please allow me to recollect a few of the impressions that I got at the time on Bruno's way of thinking: some of those impressions might be a hint to understand the reasons which led him to e^+e^- rings.

The underlying leitmotiv of Bruno's ideas was the elegance of natural laws, identified in the elegance of their formal transcription. It has often been told that Wideroe was a precursor of e^+e^- rings, but no person might have been more different from Bruno in his motivations. For Bruno, there was a Garden of Eden of theoretical physics constituted by QED, Fermi's theory of weak interactions, the TCP theorem and the mysteries of the universal constants. Some of you will certainly recall that he had a list of remarkable problems summarized in the following way:

$$a_B = \frac{\hbar^2}{mc^2} \qquad \text{quantum electromagnetic, non relativistic}$$

$$\lambda_c = \alpha a_B = \frac{\hbar}{mc} \qquad \text{quantum mechanical, relativistic}$$

$$r_e = \alpha \lambda_c = \frac{e^2}{mc^2} \qquad \text{classical electromagnetic, relativistic}$$

The fine structure constant α appeared to him as a philosopher's stone converting one kind of formal objects into a completely different kind. He was convinced that to understand the properties of this stone one should possibly work on the simplest objects in the world sharing all the listed properties: quantum, electromagnetic, mechanical, relativistic. Electrons and positrons were certainly the best. This was perhaps the main reason why, to my opinion, he did pay only marginal attention to hadron physics and to nuclear physics, these two being not very different in dirtiness (or in lack of formal elegance, which was all the same to him). Of course, neutrino physics was very attractive to him because of the same frame of mind as e^+e^- but he still considered it a separate field.

Now, if you combine these fundamental needs with some other elegant circumstances, although on a different level:

1 – center of mass kinematics is much more elegant than lab system kinematics,

2 – Beam-beam collisions are much cleaner than beam-target ones,
3 – C invariance allows to conceive a simple machine with counter-rotating particle-antiparticle beams,
4 – the size of a ring for e^+e^- collisions is determined by c. of m. energy and not by the much larger energy needed to obtain that c. of m. energy in the lab,
5 – the quantum numbers of e^+e^- , $J^{pc}=1^{--}$, allow to speak with the less inelegant members of the hadronic world, the vector mesons,
– you will arrive at Bruno's conclusion that the main road to gain some deep insight in fundamental particle physics was e^+e^- storage rings. His aesthetic needs did match perfectly with this very promising new beacon on the mysteries listed in his famous table of universal lengths, Bohr radius, Compton wavelength, electron classical radius. The events following his talk given in Frascati in March of 1960 are well known and I will stop here my personal recollection of impressions on Bruno's thinking. If I am allowed to add my opinion, physics has made many steps since then, in these 27 years from the beginning of the AdA adventure. Nevertheless, even considering the results obtained by modern colliders, Bruno's list is still there with its philosopher's stone and no "explanation" of the kind he dreamed of. I cannot refrain to feel that something in physics has been left behind in the new theories and is now-a-days considered out of fashion. Bruno, in a sense, was not a Columbus-like man: he wanted to reach the East going East, which is not, to my opinion, the present trend.

Bruno liked very much aphorisms in the mitteleuropean style. I want to dedicate to him an aphorism by Stanislaw Jerzy Lec which, to my opinion, represents well his approach to fundamental ideas. It is: "Do not call things by name if you don't know their surname".

MY WORK WITH BRUNO TOUSCHEK

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I first met Bruno Touschek at the end of August 1953. As Professor Amaldi stated this morning, I had spent one year at the University of Chicago and returned to Italy at the end of July 1953. My ship arrived at Genoa and from there I proceeded directly to the Dolomites for some holidays. From there, at the end of August, I went back to the department in Rome. People were still on holidays and the department was deserted; in particular neither Ferretti nor Amaldi was there and I could not get any information on what had happened during my absence. On the first morning that I was in my office, someone knocked at the door, came in and introduced himself; he was Bruno Touschek of whom, up to that moment, I ignored the existence. He told me that he had been invited by Ferretti to stay in Rome and that he had arrived at the beginning of the previous academic year. He then asked "What are you doing?". I told him that during my stay in Chicago I had preferred to study rather than to work on a problem that Wentzel had proposed to me (but that I considered boring and not instructive), the perturbative three nucleon forces in the γ_5 theory; however, (I told him) I had in mind something to do now: namely to test the validity of the Tamm-Dancoff approximation - that was then very fashionable - by comparing its result with the exact solution on an exactly solvable model of pion-fixed source interaction, not totally trivial, the so called Wentzel pair theory. I described to him the model and he went to his office, near to mine. One hour later he came again and told me: look, the problem can be treated noting that in the model the pions interact with the source only in S waves and (after having introduced the appropriate S wave variables) using a unitary transformation to diagonalize the Hamiltonian. I knew this because I had thought already of the problem; but I was astonished that he had concentrated on it to arrive so quickly to the essential points.

In this way, without knowing each other one hour before, we started to work together; this was the start of an intense collaboration that - except for an interval of about one year in 1956- lasted till September 1957, when I went to the Institute for Advanced Studies in Princeton and, back to Italy in 1958, I left Rome.

Coming back to the work on the Tamm Dancoff method, that paper was ready in 20 days, in time for its presentation to the Annual Conference of the Italian Physical Society in Cagliari (23-27 September 1953); it was then sent to the Nuovo Cimento [1]. It should be said that, in spite of the short time that we spent on it, the technical problems were not entirely trivial: there was the problem of how to define the mass renormalization (I had some experience on that from my previous work in N.R. electrodynamics) and how to calculate it: to do this Bruno used a very elegant relation between the eigenvalues in a box containing a potential and the phase shifts.

I presume that he had some previous experience on that, but I don't know; anyway that relation was entirely his merit. The result of that paper was that the Tamm-Dancoff method, although improving on the perturbative result, had no quantitative validity for a strong interaction. Perhaps this result (quoted in the book by Bethe and De Hoffman published two years later) contributed to the decline of the interest in the Tamm Dancoff method; it must be added that after the end of our common work I explored the next higher approximation of the Tamm-Dancoff method (in the same problem). This confirmed the previous general result showing, in addition, other difficulties of the Tamm-Dancoff method due to renormalization; I asked Bruno if he liked to publish this together, but he declined; so I sent this small complement to our paper to Nuovo Cimento in December 1953 [2]. Still on the same subject, one month later (January 1954) he participated with Cini and myself in writing a short letter [3] intended to test - with the same solvable model - another method (different from the Tamm Dancoff one) proposed by Cini and Fubini; the method passed the test, but it was rather clear that this was not readily generalizable to more complex situations.

With this paper we stopped thinking of non-perturbative methods and - may be as a reaction, having once more convinced ourselves that a dynamical treatment of the strong interactions is hard - we found ourselves involved in the study of their (space time) symmetry properties. Again this study proceeded at high speed. Presumably we started thinking of the operation of time reversal around January 1954; I don't remember this exactly but I deduce it from several circumstances: 1) together with Radicati, we presented our first paper [4] on time reversal at the Glasgow Conference (13-17 July 1954); 2) at the same Conference I presented another rather heavy paper on a different subject; 3) meanwhile Bruno was actively engaged in his work on the K mesons with Fabri and with several experimentalists. In April 1954 he participated in the Padua Conference on Hyperons and Heavy Mesons; his name appears there in two papers. With all these activities, even going on at the highest speed, we must have started thinking of time reversal in January or, at the latest, in February 1954. As a matter of fact I do not recall how this discussion started. Certainly I did not raise the issue; I was then ignorant on that subject; as far as I recall, the problem of understanding the operation of time reversal was raised by Touschek who had read a paper by Lüders (Zs. für Phys. 133, 325 (1952)); but I don't know if the subject had been previously discussed between Touschek and Radicati. Anyway we found soon that the usual connection between the reciprocity of the S matrix and time reversal, as presented for instance in one of my favourite books of that time (Blatt and Weisskopkf: Nuclear Physics), as well as in any other textbook, was unclear. We wrote a letter to Weisskopf, without receiving an answer. We went on and we realized that the usual definition of time reversal invariance, namely the existence of a matrix K such that for a time independent hermitian Hamiltonian H one has: $KH^*K^+=H$, is no definition, because such a matrix K always exists; it can be more or less complicated to determine, but, as I repeat, it always exists, independently of whether the system is or is not time reversible. As a consequence, on defining the time reversed of any hermitian operator Q as: $\hat{Q}= KQ^*K^+$, and defining the time reversed states by $\hat{\phi}= K\phi^*$ it follows that for <u>any</u> scattering matrix S we have $(\phi^*S\phi)=(\hat{\phi}^*S\hat{\phi})$, a generalization of the reciprocity theorem. Therefore a kind of reciprocity theorem holds for any system independently of whether it is or it is not time reversible.

Usually this difficulty is solved (without saying) by prescribing a specific form of K and requiring the system to be time reversible if, <u>with that K</u>, one has $KH^*K^+=H$. But this is unjustified and too restrictive. Thus we had to start defining time reversal; this we did restricting K by the requirement that the physical observables transformed in the proper way; it then emerged that in the definition of the time reversal operation on a field it is compulsory to leave free some phases: this has the consequence that a system is time reversible if, by an appropriate choice of those phases, its Hamiltonian stays invariant. This is now common wisdom, but it was not at that time (recall that all this took place two years before the discovery of the non-conservation of parity). It was this lack of taking these phases into account in the definition of time reversal that led for many years to a pseudoproblem, that of the Yang-Tiomno "types" of fermions. In a few words one can say that, as far as the meaning of the phases is concerned, our papers anticipated by a few years the modern definition of the space-time reflection operation, which is now currently adopted and has been rediscovered by many people after the non-conservation of parity.

It is impossible here to give more details on these papers: as I repeat we presented the first (Morpurgo, Radicati and Touschek) on time reversal [4] at the Glasgow Conference in July 1954 and a month after, in August, we submitted a more complete version of it with the same title to the Nuovo Cimento [5].

At this stage the basis of the problem was clear and we went on analyzing more specifically the definition and use of time reversal in field theory as well as the extension of the procedure to the other discrete operations: Parity and Charge Conjugation. (To this second phase Radicati did not participate because, as far as I recall, he was busy in moving from Naples, where he had been for one year, to Pisa. I recall an extremely pleasant and productive discussion for a whole afternoon during a visit to him in Naples that Bruno and myself had at the beginning of this collaboration). The extension of the concepts mentioned above are described in two papers [6] [7]; the last one: "Space and time reflection of observable and non observable quantities in field theory" was sent to Nuovo Cimento in April 1955. This means that - although each of us was also thinking at other problems- we spent eight months on it. This (together with a report [8] presented at a Conference in Pisa in June 1955) concluded this chapter of our common work and, of course, I was very pleased (and Bruno too) when, one morning, Bruno came in my office with a sheet of the "Mathematical Reviews" [Math. Reviews 17, 438 (1956)] containing a summary by F.J.Dyson of the above papers that started as follows: "In the extensive literature devoted to the problem of time reversibility in quantum mechanics this is one of the few papers which add substantially to the original discussion by E.P.Wigner..."; and he proceeded giving a one page long summary of the papers [5] and [7]; that summary is perhaps the clearest introduction to these papers.

After these two intense years of common work I believe that (without saying anything) we both felt some kind of saturation and the necessity for a period of independent work; in a sense the transition took place naturally because in the fall of 1955 I was sick and had to stay at home for more than one month. During that period I thought over several problems that interested me, especially on the phenomenology of the new particles (methods for determining the spin of hyperons from their decay, etc.), problems that I could not consider so far, being so absorbed in our common work. I then started to work at these (compare e.g. [9]), and as soon as I recovered I asked Carlo Franzinetti if he would have liked to write with me a detailed report (both experimental and theoretical) on the present status of the new particles (hyperons and heavy mesons). So near the end of 1955 or the beginning of 1956 we started to work with Franzinetti on this big project that kept us busy for the whole of 1956 and the beginning of 1957. Of course, during that year we continued our exchange of views with Bruno; this took place quite naturally and continuously because our offices were nearby. Infact one of the occasions, during the day, to exchange our points of view both on physics and on any other questions was the ceremony of going to the bar to have a coffee. In spite of the fact that the bar was at only 100 meters from the department that ceremony took usually more than half an hour because, as I repeat, we took that occasion to talk on everything. The ceremony was usually started by Bruno Ferretti who, at around 11, knocked at our doors (also his office was near to ours) and we slowly went to the bar, often accompanied by Mrs. Ferretti or by others (sometimes Amaldi came too). Let me say, incidentally, that if we were able to work so intensely and so quietly during such a long period, we are indebted for this to Bruno Ferretti who was, I would say, the best possible leader that a theory group can have. He was always available for a discussion on anything, but, on the other hand he left everybody quite free; and paid much attention to the selection of people in the group. Touschek came to Rome, I recall, following an invitation of Ferretti.

Also in my work with Franzinetti [10] there are traces of this continuing exchange of views with Touschek; for instance we were of course both much interested (as anyone in the world) on the $\tau-\theta$ problem (the decay of the K in two or three pions) and on page 657 of [10] is reproduced a graph [the Morpurgo-Touschek spectra. Fig. 2-13.7] that we calculated in that period to see how a resonant interaction between two of the final pions in the 3π decay could alter the decay distribution of a 2^+ K simulating a 0^- K.

Infact during our discussions of the space time reflection properties and in the period I was writing the book with Franzinetti both Bruno and myself had often thought (as many other people) of the non conservation of parity as a solution to the $\tau-\theta$ puzzle; so that when the article by Lee and Yang proposing many experiments reached us, we both started to think of other ways to clarify the situation, if the experiments suggested by Lee and Yang would have indicated a violation of parity in the weak interaction. Bruno wished to see if there were some characteristics of the neutrino typical of the non conservation of parity and introduced his *exp*($i\alpha\gamma_5$) transformation on the neutrino field (his paper on this was sent to Nuovo Cimento on January 26, 1957[11][12]). I had previous experience with the angular correlations in the decay of the hyperons and, continuing a previous calculation (dated September 29, 1956 [13]) where I had given, under the assumption of non conservation of parity, the angular correlations in the decay for the case of the general spin, I specialized [14] to the case of spin 1/2 (a similar calculation was sent to Phys. Rev. [15] by Lee, Steinberger et al. one month and half later).

I have mentioned all this because it serves as an introduction to our joint last paper with Bruno "Conservation of parity and strong interactions" [16] received by the "Il Nuovo Cimento" on July 15,1957 that never appeared in print due to my fault (although preprints were circulated).The reason why that paper never appeared in print was the following. When I received the galleys (presumably I was already in Princeton-I can deduce that from the date on the label attached by "Nuovo Cimento" to the original manuscript) the contents of the paper looked to me rather obvious. I don't remember if I simply kept the galleys or I wrote to Bruno stating this. Certainly at no time there was a particular reaction on this from Bruno (of course if there had been I would have been ready to publish the paper). But another reason why I did not care too much is that we had written a very long discussion remark presented at the Padua conference in 1957 containing the essential points of the paper; but the Padua proceedings were finally published omitting all the discussion remarks and so we did not have either that published document.

Having reread several times afterwards that paper (I did it also yesterday) I found that its contents were far from trivial; a proof of this assertion is offered, perhaps, by the fact that independently Feinberg and Soloviev did publish similar results a few months after our paper. The essential point of the paper was an attempt to explain why the strong interactions do conserve parity and the weak do not.

We did show that if we consider only the pion nucleon interaction and if we assume time reversal invariance and isospin invariance the on shell pion nucleon vertex automatically does conserve parity. This is not so for the vertices of kaons and hyperons and therefore we suggested experiments to test parity conservation in strong reactions involving hyperons and kaons.

This concluded my scientific collaboration with Bruno; when I came back to Italy, as I already stated, I left Rome and, though we met several times per year and we exchanged our ideas on all the facts or things in physics and in any subject, it never happened anymore that we worked together. But that period was a very intense and pleasant one and I am grateful to Bruno for it.

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[14] G. Morpurgo-A discussion of the possibility of detecting asymmetries in the "Lambda°" decay, Nuovo Cimento 5, 1787 (1957).

[15] T.D.Lee, J.Steinberger, G.Feinberg, P.K.Kabir and C.N.Yang-Possible detection of parity non-conservation in hyperon decays, Phys. Rev. 106, 1367 (1957).

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Bruno Touschek in Frascati during the construction of ADONE (1966).

REMEMBERING BRUNO TOUSCHEK

Ugo Amaldi
University of Milano Bicocca and Tera Foundation

In this short contribution I concentrate on four very diverse recollections of Bruno Touschek in the hope to shed light on some of his many and unique qualities.

First, I want to remind you of Bruno as a class teacher. I have the personal experience to have followed his course on field theory at the post-graduate school of specialization in Rome. I still have a vivid recollection of the first day in which he introduced the Lagrangian density of field theory and proved on the blackboard in only two lines the form of the conserved quantities, i.e. the Noether therem. I was so struck that, when I look back to the first two pages of my logbook, I still remember every detail of that lesson.

In the following months he discussed renormalization theory in a very personal approach which I could not find in any book. I still have and consult my notes of the course and I must say this course has had a great influence on my formation as a physicist - so much that two or three years later, while I was starting on my way as an experimentalist, I could work with two theorists (Franco Selleri and Ezio Ferrari) on a few phenomenological papers developing what was called the (then very fashionably) “peripheral” model, without finding myself at all a stranger to the environment of Feynman graphs and phenomenological calculations. Under the influence of Bruno's course I also wrote two booklets on Feynman graphs and calculations which were published as Internal Reports of the Physics Laboratory of Istituto Superiore di Sanità, where I became a fellow in 1960. These booklets have been used by many post-graduate students of the Rome area.

Coming back from a two years' stay at CERN, I initiated an experiment on nuclear physics at the Frascati synchrotron, the study of the (e,e’ p) reaction in nuclei, and I had much less to do with Bruno. Of course, as all of those working in the INFN laboratory, later I followed with great interest the work of AdA, mainly through Ruggiero Querzoli, who was connected with the Physics Laboratory of Sanità. In particular I lived through the by-now-famous "night asassination" by Peppino di Giugno of the few electrons still circulating in AdA, then its transfer to Orsay and the discovery of the Touschek effect.

The contact was made again by Bruno who, one day in 1964 - if I remember correctly the year - called me in Sanità and asked me to pass by and see him at the Institute of Physics. There he spoke about the status of the Adone project, said that he was worried of the lack of interest by young experimentalists and pushed me to look into the physics and seriously consider to propose an experiment for the forthcoming “grande anello di accumulazione”. Certainly this intervention pushed me, Giorgio Mattiae and other colleagues of the Section Sanità of INFN to think about the detection of the ϕ-meson production and, in particular, about its decay in the e^+e^- channel with its characteristic dispersive behaviour.

I looked into the problem of radiative corrections - which at that time had not been considered for a narrow resonance - and I wrote a report on the subject. I was at the time doubtful about my conclusions, so I went to the Physics department to discuss them with Bruno. I had used a technique developed by Kessler in France, that had suggested a handy way to apply the Weisäcker-Williams method to similar problems. I still remember when I had explained my results, he asked me "Ugo, which method have you used?" and I answered "The Kessler method, do you know it?". Without any hesitation he replied immediately "*Io conosco soltanto le sorelle Kessler*" i.e. “I know only the Kessler sisters”, two tall and beautiful German twins who in the 60s and 70s were often dancing in TV. I have been told by my sons and daughters that from time to time these twins still appear in TV without showing the sign of the age.

My third recollection dates to more than ten years later. In Autumn '77, Bruno was at CERN for a sabbatical leave and we had many more occasions to discuss physics. It is interesting to recall one particular conversation we had on the future of particle physics a few months before he died. He told me very explicitly in this conversation that he now believed that the future of particle physics would have been based on antiproton-proton and proton-proton collisions and not on the electron-positron annihilation. Indeed he had been discussing with Carlo Rubbia the possibility of effectively colliding proton and antiproton beams circulating in a storage ring. Bruno was worried not only because of the strong radiation in electron rings, but also because at very high energies a lepton dresses itself more and more. He knew very well the problem since the time he had introduced – I think after my first attempt in computing radiative corrections – what he called the “Bond” factor, i.e. the coefficient 0.07 which is in front of the Weizäcker-Williams formula when one takes into account the energy of Adone. In CERN, many years later, he said: "Of course, the Bond factor increases with the logarithm of energy and so leptons get dressed very much at high energy” with a reduction of the effective energy of the collision and a confusing effect in the detection of the products of the annihilation.

The problems of high energy electron rings had been discussed in 1975 (two years before) in a note by Claudio Pellegrini and Carlo Rubbia and in a paper, which was published in NIM by Burt Richter. He came to the conclusion that the cost of storage rings increases as the square of the energy and Bruno was much concerned. The increasing cost was for him another reason for believing that the future after LEP would see only hadron-hadron colliders. On that occasion I tried to influence his opinion by pushing with him the idea of high-energy linear electron-positron colliders, on which I had written a paper two years before. I had discussed it and convinced many people at CERN, and also Burt Richter, who was in Europe for a sabbatical leave. I tried to impress Bruno by telling that, as soon as I spoke about the idea of a linear collider to the then young Nobel prize Richter, he had asked by telex some relevant information on beam break-up to his SLAC colleagues. Thus I spoke with enthusiasm about the possibility of constructing linear colliders, avoiding the square law, but I did not move Bruno from the opinion that the future was in the hands of hadron-hadron colliders. Clearly at that time he was very much fond of the project of Carlo Rubbia and was keeping in contact with the group working on it.

My last recollection has to do with an episode that happened only few weeks after this physics discussion, when Bruno was brought to the La Tour hospital in Meyrin and many of us at CERN visited him many times. Once, I still remember as if it was yesterday, I had some difficulty in finding the room because he had been moved. So, as soon as I got into the room where he was in bed very tired and yellow in face, I told him that I was late because the people I had asked had not been able to tell me where he was. He burst out: "In fact, the real problem is the number of this room". I could not understand what he meant but I did not want to show that I had doubts about his state of mind. Thus I had asked a couple of innocent questions when, at a certain moment, very abruptly he said: "The number is 137. This is the problem around which I have hovered throughout my life without success", and then he elaborated on the many approaches he tried in order to understand the value of the electron charge at different moments of his scientific life.

This episode I recounted in a short article that I wrote for the "Corriere della Sera" when two months after his death we had in Frascati a meeting to pay a tribute to his memory. But in this article I did not say the last part of the story which goes like this. After concluding "This is the problem of all my life" and "I never succeeded in solving it", he said "But you know, Ugo, also Pauli was brought in a hospital room number 137 before he died?"

This was just a few days before he left Geneva at the beginning of March to go to Innsbruck, where he died.

REMEMBERING BRUNO TOUSCHEK

Giancarlo Sacerdoti
Facoltà di Ingegneria, Università "La Sapienza", Roma

When I was asked to speak in memory of Bruno Touschek, I accepted on the precise condition that I would limit myself to recalling Bruno "the man" with his story of hard experience. His scientific contribution is well known and other people have illustrated it better than I would be able to.

When the construction of AdA was begun, I already knew Bruno, but I knew only that he was a theoretical physicist of Austrian origin who worked at the University. I knew nothing about his life. I came to know his life story when we were supervising the construction of the AdA magnet and we used to go to the Terni Company in Terni where the core of the magnet was under construction.

We used to go by car and it was particularly on the return journey that he would tell me of the hard times he had gone through during the war. He told me of things that had happened more than 20 years before, but that had remained impressed on his mind, and now I will relate what I learned 25 years ago. When Austria was annexed to Nazi Germany, the atmosphere at Vienna became stifling for the Jews. There were killings and violence, there were people thrown and drowned in the Danube which certainly was not "blue" any longer.

To these forewarnings of what was to come, there was added same other minor, widespread harassment which was a witness to the hostile attitude of the Viennese against the large Jewish community. An example is the telephone call to the Chancellor of the University by citizens warning him not to allow the "dirty Jews" even into the library. Bruno's mother was Jewish and he was to all effects Jewish and had completely absorbed the Viennese-Jewish culture. His family therefore thought it prudent to send Bruno to his maternal grandmother (his mother was dead) living in Rome where he attended the first year of physics at the University.

The following year, his father with the help of influential friends, managed to arrange for Bruno to continue his studies in Germany.

It was still possible for a Jew to have some sort of cover in Germany;

in Austria where anti-Semitism was violent, that would not have been possible-Touschek told me. The Director of the Institute was Sommerfeld who, due to his prestige and certain friends, could count on the Prussian clan, protected him and permitted him to study at the University without allowing him however to examine students who were "true" Germans.

His situation was indeed singular: he was of an age to go to war but was relegated to staying at a University Institute which received even projects for special arms. When, however, Sommerfeld retired, Von Laue, who replaced him (and who drove like a madman - said Touschek), even though he tried to protect and cover him, could not avoid Bruno being sent to a concentration camp.

I think that he found himself in this dramatic situation (his grandmother had meanwhile been sent to concentration camp where she eventually died in the gas chambers) both because of his (lack of) prudence in talking badly about the Nazis and because of the approval by the authorities of the "final solution to the Jewish question" project. In his misfortune, he was, in a certain sense, lucky.

The Nazi authorities were aware of the dramatic decline in the German war machine and were neurotic in their desperate search for new arms, for some miraculous happening, which could change the tides of the war. It was a mad frantic race led by incompetents. And Bruno was testimony to this dramatic pantomime.

When he was still free to go about, they had interested him in a project for special electric tubes, he had had information on the V.Z. programs in the Bermudas, and he was kept informed on a project for armour which could resist machine-gun bullets, proposed by a braggart of French origin who managed to make a fool of the authorities even as far as obtaining a car with a radio-receiver which let him know where the allied bombers were so that he could escape the bombings, etc. Among the new arms' projects there was one proposed by a stupid "fool" of a professor from Lipsia who was completely tied to the Nazi Party. The project consisted in the realization of a betatron to install on an airplane to create a beam of electrons to be used as a bullet against enemy planes. He did not know - said Bruno - that the beam intensity lowers as 1/zr and that a machine of this type weighs too much. Germany was frighteningly short of technicians, both because the number of graduates in technical disciplines had lowered from 20,000/year in 1933 to less than 8,000/year in 1939 and because many technicians had been called to arms. The Nazis therefore commissioned a Norwegian, Wideroe, who had graduated from

Berlin proposing the first accelerator machines. Touschek was included in the group, but had to collaborate remaining in the concentration camp.

Wideroe used to visit him periodically in the concentration camp even though he had to converse with him in German in the presence of two guards. As he had to consult the book "Radiation Theory" of Heitler, Bruno used this excuse to ask in English for cigarettes, food and wine. Wideroe contented him each time he visited him, permitting him to survive. Bruno thus owed a lot to accelerator machines. It was during these discussions that Touschek realized that the natural radiation of the electrons allows accumulation in a circular orbit.

So it was that Bruno paid his debt to the particle accelerators. He owed a lot, but, likewise, accelerating machines were to be greatly in debt to him.

The betatron was working towards the end of 1944, with the electron beam focusing by a sample gradient, and it was the first operational European betatron (Kerst in the USA had discovered the principles of focusing for electrons with a closed orbit).

However the allied troops were arriving and the prisoners were sent marching to other camps. Whoever fell was shot. Thus it happened that Touschek was hit by a shot in his ear lobe and fell into the roadside. This was his salvation. The betatron was taken to Scotland.

This chilling story explains a lot about Touschek's character of a man torn from his middle-European cultural environment which he had lost for ever, as everything was found destroyed after the war. And thus, in his innermost mind, he lived those faraway memories of a happy youth, and, in remembering, suffered atrociously. He was very reserved during his first years in Italy.

Then towards the end, as it happens to each man approaching death, who sinks, even more obsessed, into the memories of childhood, even Bruno began to remember and talk more and more often about that distant part of his life, as though it would let him relive beloved Vienna so steeped in different cultures, among which the most important was that contributed by the Jews.

REMEMBERING BRUNO TOUSCHEK

Francesco Calogero
Dipartimento di Fisica, Università di Roma "La Sapienza"
Istituto Nazionale di Fisica Nucleare, Sezione di Roma

I will just say a few words and I will be very personal. I hope I will be excused since this session is entitled "Remembering Bruno". As it happens, just by coincidence, today is a special day for me because today in the afternoon there is the official commemoration of my father at the University of Rome, so these two persons who have been fundamental for my own life are being commemorated in the same day. I believe I was, together with Nicola Cabibbo and Paolo Guidoni, one of the first physics students of Touschek in Rome. In fact, if I remember correctly, the first student was Paolo Guidoni, while Nicola Cabibbo and myself were supposed to do our dissertation for the "laurea" in physics with Giacomo Morpurgo. But then Morpurgo went to Genoa to be a full professor there and so the three of us all worked with Touschek, and we wrote three separate theses but on the same theme, weak interactions -- immediately after the nonconservation of parity was revealed.

I have many reminiscences but I will only mention one which is connected with the fact that Bruno was also able to enjoy life: one of the things he enjoyed very much was underwater fishing. I have a very poor memory, but this conversation I remember quite well. He was explaining to me how a particular type of fish is extremely difficult to catch, practically impossible because it is a predator fish, very fast. Then he told me "I have tried many times and I have applied all types of corrections before shooting but I never succeeded". I was very impressed by this scientific approach to underwater fishing.

One last thing. And I would not say this on this occasion if I had not said it many times before. Of all the persons -- and I may be offending some of the people here in the audience -- of all the persons I have known in my life, the single one that motivated me to describe him as a "genius" was Bruno Touschek.

REMEMBERING BRUNO TOUSCHEK

Pedro Waloschek
DESY, - Hamburg

I would like to make a few remarks on the deep connection of the city of Hamburg with Bruno Touschek and since a lot of things have already been said, I will mention a few points which join his life with Hamburg.

So the first pleasant memories are from Hamburg - you know, Hamburg, I think, is the prettiest city of Germany. The first good thing we remember of Bruno is that the Professors at the University of Hamburg gave him the opportunity to attend lessons, an opportunity he did not have in Vienna. I am Viennese, I am very sorry for this, but he was not only allowed to come to see the lessons, he was unfortunately not able to inscribe at the University, but he gave lots of trouble to his professors there: one of the things which I was told in Hamburg is that when he came in the room, the first thing he did was turn head down the picture of Hitler. So that made life not easy with Bruno. He was not only original, he had also an enormous courage because at that time it was in the middle of the War. It was really risking your head.

You have already heard from the beautiful lectures by Edoardo Amaldi, who knows so much more about the life of Bruno, about the 15 MeV betatron which was called the first German betatron of 15 MeV, but I guess it was really the first European betatron. There was no other one in Europe. He this did with Wideröe. During this time he was working at different jobs and one of these jobs is mentioned also in the Yellow Report of Cern by Edoardo Amaldi. One of these companies is at present building the klystrons for LEP, for HERA and for PETRA. These big 3-m long klystrons have been started in one of the companies in which he was doing the small klystrons.

Another point we always remember of Bruno is his strong push for electron-positron rings and we must remember the very hard time in '67 when decisions were taken to build the storage rings Spear and Doris - a time at which quarks were known but were not very popular and we did not have any knowledge about the large cross sections coming up. So the decisions for these storage rings were taken three years before Adone discovered the large values of R, which, if we had known these big values of R, we would have had no difficulty in having our projects approved. So the big encouragement I must say came from the theoreticians , even if they told us we should not build these machines because nothing will happen. At one conference, where these ideas on cross sections for producing point-like particles which we did not like, there was Gatto's contribution. I thinking '61 or '62 - this was very important for these decisions. I must now make a jump over all the physics results.

I will not tell you of all the physics - I think you all know what happened, and you had a beautiful list before-but I would like to remind you that the Doris machine has, a few weeks ago, provided us with this beautiful $B\bar{B}$ mixing and therefore this machine is going on to do this kind of physics. On top of this I will also remind you that these machines are giving a completely new type of physics in the synchrotron radiation field. So a machine like Doris goes on being a very up-to-date machine and is still being used.

Now, I think, you will probably talk about the future so I would like to say that from AdA we have taken the team which built Doris and Petra and these enthusiasts for storage rings are now building the biggest storage ring for protons we have ever built in Europe and this is an enormous effort, which is a European effort. I would like to give you a short status report, as homage to Bruno.

This machine of about 2 km diameter is going to collide 820 GeV protons with 30 GeV electrons and it is 10% an Italian machine. Italy is contributing for the building of the machine with more than 10%. From the 950 million it is going to cost, and from the experiments, 20% of the physicists collaborating to the two experiments of HERA are Italian. So I think we could tell our Bruno: "Look how nice we are doing with this technique of storage rings".

REMEMBERING BRUNO TOUSCHEK

Giancarlo Rossi
Dipartimento di Fisica - Università di Tor Vergata, Rome (Italy)

My first meeting with Bruno Touschek was while I was a student at the University of Rome in 1965. I was so fascinated and impressed by his course on Statistical Mechanics that I decided (together with my colleagues Francesco Drago and Paolo Di Vecchia) that I had to try to obtain a thesis from him. In this way a long, unforgettable collaboration with Bruno Touschek started, that was decisive for my formation as a theoretical physicist. The thesis (1966) was about the application of the Bloch-Nordsieck theorem to the calculation of radiative corrections to e^+e^- annihilation in Adone.

Starting from 1968, Bruno Touschek interests somewhat shifted towards Statistical Mechanics. As a result of the success his course on Statistical Mechanics had among the students (the class room used to be always overcrowded), he decided the time was ripe to write a book on the subject, which could be used as a textbook for third or fourth year students in Physics. At that time I was his "assistente volontario" at the "Cattedra of Meccanica Statistica" and from his lectures I had collected notes that under his guide were published as "dispense".

That was not yet a book and a really fantastic collaboration started in order to bring this first rough notes to the status of a "book". The work lasted almost two years: each chapter was first written in English (he felt, he was not sufficiently good to write directly in Italian) and then translated into Italian. Most of the work was done in his apartment. His footprint can immediately be recognized and perceived from the very personal way in which each concept, even the most elementary one, is presented in the book. Many original and non-standard examples are presented to illustrate all the important points.

I wish to end this brief recollection by expressing my deep gratitude I will always have towards my "Maestro" and by emphasizing how lucky I was to meet Bruno Touschek in my life.

E. ETIM, *et al.*
11 Ottobre 1967
Il Nuovo Cimento
Serie X, Vol. **51** B, pag. 276-302

The Infra-Red Radiative Corrections for Colliding Beam (Electrons and Positrons) Experiments.

E. ETIM, G. PANCHERI and B. TOUSCHEK
Laboratori Nazionali di Frascati del CNEN - Frascati

(ricevuto il 30 Gennaio 1967)

Summary. — The infra-red corrections to be applied to the results expected from an electron positron colliding beam experiment are determined with the help of the Bloch-Nordsieck theorem. Experiments are characterized by a resolution function $g(k)$ of a four-dimensional time-like energy-momentum vector, which represents the probability that a four-momentum loss k escapes detection. The results are applicable to a class of experiments in which the statistical error is matched to the error of the energy-momentum resolution. Various approximations which allow a rapid and accurate estimate of radiative corrections are discussed.

1. – Introduction.

Any search for a possible breakdown of quantum electrodynamics leads inevitably to the necessity of applying important radiative corrections to the measured results. This is due to the fact that renormalization theory makes the form of electrodynamics at low momentum (and energy) transfers a matter of definition, so that any discrepancy with the existing theory has to be searched for by either studying processes in which the transferred momentum is very large or by making high-precision measurements on processes with a moderately high momentum transfer. In either case radiative corrections are important: in the first because high-energy charged particles are created and destroyed—a process which leads to large currents and therefore to the liberation of a considerable portion of the energy in the form of relatively soft electromagnetic radiation—in the second case because the high precision of the experiment requires for its interpretation a high precision in the determination of any correction.

BRUNO TOUSCHEK AND THE FRASCATI THEORY GROUP

Giulia Pancheri
INFN - Laboratori Nazionali di Frascati, P.O.Box 13, 00044 Frascati (Italy)

To conclude this open session, and on behalf of my other colleagues I would like to recall the unique role played by Bruno Touschek in establishing and developing the theory group in Frascati. In fact, I believe that his philosophy concerning the scope and meaning of theorists in a National Laboratory was very important in shaping our work at the time and still is now. He believed that a theory group has to provide its own special kind of technical support to the experiments, but also that, after we had discharged this sort of moral obligation, we were free to develop our own research interests. To use his own words to me: "We must earn our bread and butter". In a sense we have tried to be faithful to his philosophy: whatever usefulness the theory group has had for Frascati it is mostly due, in my mind, to Bruno Touschek's legacy. And most of the phenomenological work which has been done for Adone, radiative corrections, heavy leptons, single and double bremsstrahlung, was started by Touschek in the mid sixties.

In early 1966, when I joined the group, it consisted of a few young people who were graduating or had just finished. These were Mario Greco, Antonio Tenore, Giovanni De Franceschi, Giancarlo Rossi, Francesco Drago, who left to work at IBM, Etim Etim, Paolo di Vecchia, presently at the University of Copenhagen. All of these people are present today, except for F. Drago whom we could not reach and Paolo di Vecchia who is extremely sorry not to have been able to arrive. Later in that year, Pucci Maiani Di Stefano joined us for a short time.

In 1969, Aurelio Grillo graduated with Touschek with a thesis on heavy lepton couplings and subsequently joined the group. It was a real group, in the sense that we were all under Touschek's spell and followed his thinking and admired him enormously.

The main lines of research in which he engaged us concerned searches for possible breakdowns of Quantum Electrodynamics and work in statistical mechanics.(*) It is in connection with the search for possible breakdowns of QED that he started the Radiative Correction work which I will discuss later in a little more detail, and which led many of us much further than Touschek ever knew. But before that, I want to give one more testimonial of Bruno Touschek's humanity and warmth, remembering one of the best times we all had with him, the holiday in Positano, in September 1966. On August 19 of that year, Touschek, Etim and myself, we had just finished the Radiative Correction paper and Touschek suggested that the whole group join him in early September in Positano, where he was going to stay for a month with his family. We all went, De Franceschi, Mario Greco, Giancarlo Rossi, Paolo Di Vecchia and myself. We drove down from Terracina, in Paolo di Vecchia's "cinquecento" and reached Positano where we went immediately to visit Touschek who was staying at Palazzo Murat, with other friends whom I cannot remember now. The experience was memorable. In the morning, we would board a boat with a "marinaio" who took us to isolated coves, not otherwise reachable. We carried sandwiches and swam all day with Toushek and his family until the "marinaio" would come and take us back to the village in the evening. We would have dinner in some relatively simple place, where Touschek had made friends with proprietors and customers alike. I do not recollect, myself, the content of our conversations in Positano, just the obvious pleasure of living that came from Bruno and his desire to share it with us. We stayed only a few days but those few days have left us with one of Bruno's most serene and happy images.

As I said, we went to Positano shortly after completing the Radiative Correction paper. This is an example of what he called "earning our bread and butter". According to his philosophy, to earn this bread and butter we had to take care of the "administration of the Radiative Corrections". His main message at the time was that straightforward perturbation theory does not lend itself easily to dealing with the flood of soft photons which emerge from a high energy collision between charged particles. This, of course, was true at Adone's energies and still is, indeed much more so, at Lep and beyond. To be precise, and I am quoting his written words, he liked to say that the picture of an experimenter as of one counting soft photons is not entirely realistic, since he does not see single photons but rather an imbalance of energy and momentum between the incident and emergent particles. On the other hand existing perturbation theory works in a representation in which the number of photons is diagonal and the

(*) See unpublished work by M. Greco and B. Touschek in Appendix B.

emission of any additional photons requires a further step in the perturbation procedure. To overcome this difficulty, he had developed a very elegant formalism, which he derived from the covariant formulation of the Bloch-Nordsieck theorem. Using this formalism one can sum all the soft massless quanta emitted by a semi-classical source. Present day soft gluon summation techniques use either this same formalism or a slightly different version of it, the coherent state formalism developed by Greco and Rossi during the same time, again under Touschek's inspiration. The Bloch-Nordsieck summation technique subsequently developed in two different directions. One corresponds to straightforward QED applications, like infrared radiative corrections to the cross-section for producing J/ψ resonance or for producing Z^o at LEP. The other corresponds to study the infrared structure of non-abelian gauge theories and utilize the technique to sum soft gluons in QCD where the problem of higher order corrections is much more severe. This has resulted in quite a number of phenomenological works in hadron-hadron collisions, which all utilize the Bloch-Nordsieck or coherent state formalism, like the study of the transverse momentum of the Drell Yan pairs, the KNO distribution, and finally the transverse momentum of the W and Z particles at the CERN collider. Unfortunately Touschek could not see any of these later developments, although most of the theoretical work which led to the transverse momentum distributions in QCD was done while he was still alive.

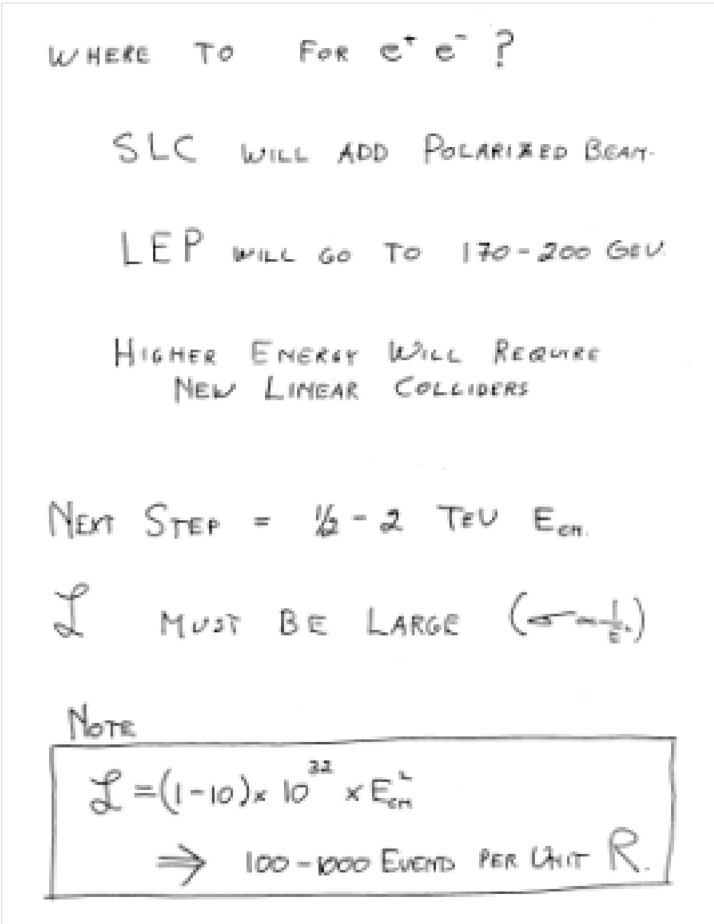
Aerial view of the CERN site just outside Geneva, Jura mountains in the background. The large circle shows the line of the LEP tunnel, 27 km in circumference. The small circle shows the SPS tunnel, 7 km in circumference. The border line indicates the Franco-Swisse border (1987).

CERN Photo: CERN-SI-8701973

e^+e^- COLLIDING BEAMS - STATUS AND FUTURE PROSPECTS

Burton Richter
Stanford Linear Accelerator Center, Stanford, California, USA

We reproduce here the last page of B. Richter' trasparencies



Inauguration of the Bruno Touschek Memorial Lectures.
On first row, from left: E. Amaldi, S. Van der Meer, B. Richter, S. and J.S. Bell. On second row, from right: U. Amaldi and G. Salvini.

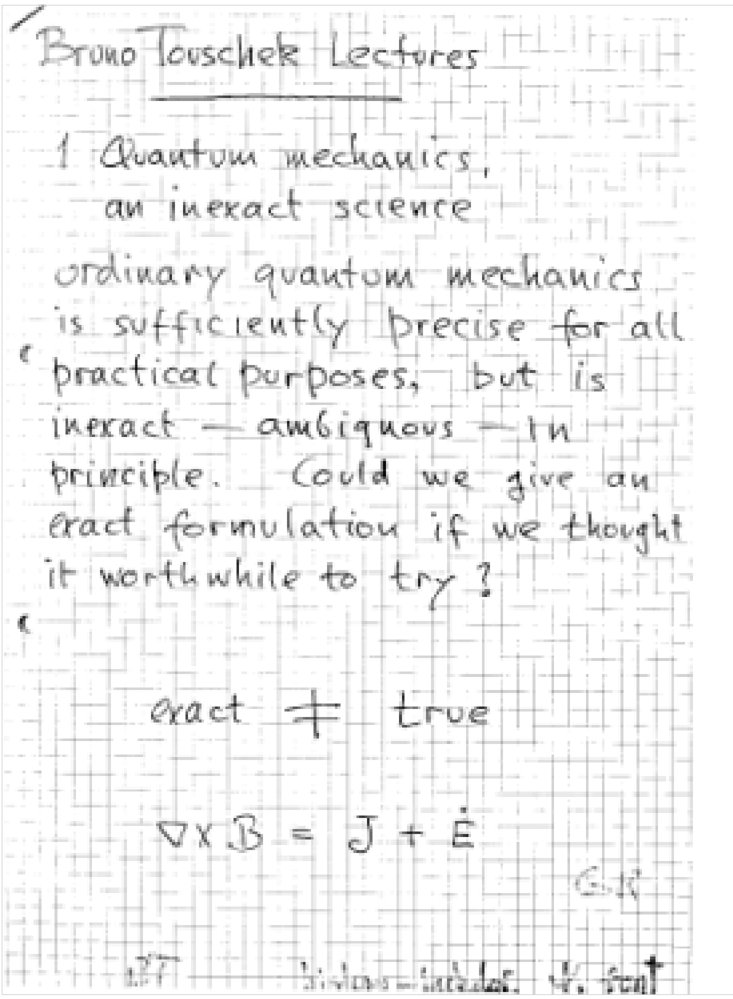
J.S. Bell giving his first Lecture.

THE EXACT PRINCIPLES OF QUANTUM MECHANICS: QUANTUM MECHANICS, AN INEXACT SCIENCE

John S. Bell
CERN, Geneva, Switzerland

The lectures are based on the book J.S. Bell: "Speakable and Unspeakable in Quantum Mechanics", Cambridge University Press 1987.

We reproduce here the first page of J.S. Bell transparencies.



Bruno Touschek Lectures

1 Quantum mechanics,
an inexact science

ordinary quantum mechanics is sufficiently precise for all practical purposes, but is inexact — ambiguous — in principle. Could we give an exact formulation if we thought it worthwhile to try?

exact ≠ true

$\nabla \times \mathbf{B} = \mathbf{J} + \dot{\mathbf{E}}$

The Adone Building in INFN Frascati National Laboratory.

APPENDIX A

Laboratori Nazionali di Frascati del CNEN
Servizio Documentazione

Nota interna n° 68
27 Gennaio 1961

F. Amman, C. Bernardini, R. Gatto, G. Ghigo, B. Touschek:
ANELLO DI ACCUMULAZIONE PER ELETTRONI E POSITRONI (ADONE)

ANELLO DI ACCUMULAZIONE PER ELETTRONI E POSITRONI (ADONE)

F.Amman, C.Bernardini, R.Gatto,
G.Ghigo, B.Touschek.

1.- La conservazione del momento del baricentro nell'urto tra due particelle limita fortemente l'uso di macchine tradizionali. Per esempio, nell'urto di un positrone di energia E ($\gg m_e$) contro un elettrone fermo nel laboratorio, soltanto l'energia

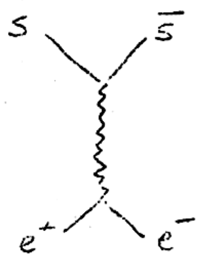
$$E' = (2 m_e E)^{\frac{1}{2}}$$

è disponibile per la produzione di nuove masse nell'annichilamento $e^+ e^-$. In questo senso, un esperimento con fasci incrociati è equivalente ad una macchina tradizionale di energia enormemente più grande, quale non esiste al giorno d'oggi.

Diamo ora un breve sommario dei problemi fisici e tecnici relativi ad una macchina "colliding beams", del tutto nuovi sotto molti rispetti, avendo in mente un anello di accumulazione (Adone) per elettroni e positroni da 1.5 GeV ciascuno. L'energia equivalente di una macchina tradizionale sarebbe di 2.200 GeV.

2.- Disponendo di 3 GeV nel baricentro si può pensare di ottenere dall'annichilamento $e^+ e^-$ la produzione, in coppie, di tutte le masse conosciute. Si tratta di ben 16 reazioni a due corpi presumibilmente osservabili in cattiva geometria senza compromettere seriamente l'informazione (i prodotti sono monocromatici, tra l'altro). L'annichilamento in 2 γ va considerato, tra queste reazioni, come quello che permette la misura della luminosità della sorgente grazie: alla sua grande intensità, all'attendibilità della sezione d'urto calcolata (i momenti trasferiti sono dell'ordine di $2 m_e$) ed alla caratteristica di-stribuzione angolare concentrata attorno alla linea di volo dei primari (entro un angolo m_e/E).

Delle 16 reazioni a due corpi 3 interessano l'elettrodinamica ed un suo eventuale breakdown: $e^+ + e^- \longrightarrow 2\gamma$ (a grande angolo), $e^+ + e^-$ (Bhabha scattering), $\mu^+ + \mu^-$. Le rimanenti, riassunte dal grafico:



(a parte $e^+ + e^- \longrightarrow \pi^0 + \gamma$, che è, in pratica, una reazione a 3 corpi per il rapido decadimento del π^0), mettono in evidenza i fattori di forma di S. Una novità rispetto agli esperimenti usuali sta nel fatto che il fotoni virtuale del diagramma ha momento completamento time-like, di modulo $\geq$ 2 masse prodotte.

Precisamente, indicando con p^+ e p^- i quadrimomenti del positrone e dell'elettrone finale, il grafico precedente corrisponde alla produzione ed annichilamento di un protone virtuale di massa:

$$q^2 = (p^+ + p^-)^2 = -4E^2 < -4M_S^2$$

dove M_S è la massa di S. Pertanto i fattori di forma elettromagnetici di $\bar{S}$ vengono esplorati per valori di q^2 minori di $-4M_S^2$.

La situazione è illustrata dal grafico seguente, che si riferisce, per esempio, al fattore di forma vettoriale di spin isotopico del nucleone.

stribuzione angolare concentrata attorno alla linea di volo dei primari (entro un angolo m_e/E).

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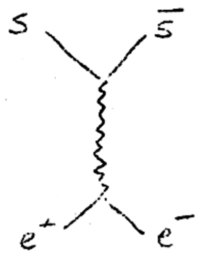

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anche in questo caso, sulla base dello stesso modello si può calcolare un enhancement di un fattore dell'ordine di 15 rispetto al valore perturbativo.

Queste risonanze rientrano in una classe più vasta di stati risonanti la cui esistenza può venire rilevata da questi esperimenti. Si può dimostrare che solo risonanze di spin 1 e numero di charge conjugation -1 possono essere osservate da e^+ e^- : altre risonanze risultano osservabili solo per termini di ordine superiore in e^2. L'esistenza di tali risonanze potrebbe in alcuni casi portare le sezioni d'urto a valori dell'ordine di 10^{-29} cm^2. E' chiaro che la progettazione della macchina e soprattutto la decisione sulla energia massima deve essere subordinata ad una anlisi approfondita dalle possibilità sperimentali, basata più o meno su ragionevoli ipotesi circa il comportamento delle sezioni d'urto ad alti q^2. Questa analisi è tuttora in corso ed i risultati verranno presentati quanto prima.

3.- La contropartita della grande energia disponibile in Adone sta nell'intensità. Se vi sono N_+ (N_-) positroni (elettroni) circolanti in k bunches dalla radiofrequenza, vi saranno 2k regioni di incontro lungo la circonferenza (di lunghezza n). La luminosità (numeri di eventi al secondo) d'una reazione di sezione d'urto σ è:

$$L = \frac{1}{k} N_+ N_- \left(\frac{\sigma}{q} \right) \frac{c}{u}$$

dove q è l'area della sezione trasversa di ciascuno dei beams e si suppone che essi si sovrappongono completamente nelle regioni d'incontro. I valori di σ sono presumibilmente tra 10^{-32} e 10^{-34} cm^2; si stima che q possa ridursi a circa 10^{-3} cm^2. Pertanto, con k = 2 ed n = 50 metri (v. avanti), per avere un evento ogni 5 minuti, deve essere

$$N_+ N_- \simeq 10^{20} \div 10^{22}$$

questo numero non sembra irrealizzabilo.

Se si stima il tempo necessario per misurare le sezioni d'urto delle 16 reazioni a due corpi con un errore statistico del 10% e con una distanza in energia di 25 MeV tra i punti sperimentali, si vede che sono necessarie circa 3.000 ore macchina per realizzare questo programma. Quindi, pur trascurando per il momento la possibilità di osservare annichilamenti in tre corpi, si può contare su un tempo d'impiego ininterrotto, di circa tre anni, di Adone.

4.- La realizzazione dell'anello si varrà, in parte, dell'esperienza fatta su AdA . E' bene precisare tuttavia che AdA non è propriamente un modello di Adone, se non per la verifica del meccanismo d'accumulazione.

Adone ricorda grosso modo un elettrosincrotrone da 1.5 GeV, per il magnete e l'impianto a RF. L'iniezione, invece, è un elemento di differenziazione da ogni altra macchina. Di essa, l'elemento essenziale è un linac da 30 MeV, in commercio con tempo di consegna di circa 9 mesi: esso può erogare 10^{11} elettroni per impulso e per la durata di 0.1μsec.

Si possono ottenere positroni producendo uno sciame in una targhetta pesante; pensando di prelevare dallo sciame positroni di 15 MeV con uno spread $\simeq 1\%$, si può contare su una sorgente di circa 10^7 positroni per impulso.

L'ottica d'iniezione termina con un deflettore pulsato che ha la funzione di "mettere in orbita" le particelle provenienti dalla sorgente. Tuttavia, per impedire che nei successi impulsi d'iniezione si distruggano i beams già accumulati, è necessario allontanare questi dalla foce del deflettore. Per questo, si può pensare (ma non è detto che questa sia la soluzione più conveniente) di iniettare beams con grandi ampiezze orizzontali di betatrone, portarli di volta in volta a 500 MeV, dove la radiazione di sincrotrone smorza queste oscillazioni irreversibilmente, e ritornare all'energia d'iniezione per catturare un nuovo impulso. L'operazione richiederebbe circa 1 sec; perciò, con questo meccanismo, per arrivare ad $N_+ N_- \gtrsim 10^{22}$ (vedi 3) occorrerebbero circa 3 ore.

Abbiamo considerato, in alternativa, una macchina strong focusing ed una weak focusing. La prima presenta la difficoltà dell'effetto di antidamping delle oscillazioni orizzontali di betatrone, che si può sormontare con l'inserimento di speciali magneti tecnicamente difficili e rischiosi da realizzare.

Di queste due macchine, la tabella 1 da in succinto le caratteristiche più interessanti.

In quanto all'ambiente che le circonda, è stata curata la possibilità di avere molto spazio alle sezioni diritte, per alloggiare dispositivi sperimentali anche fuori del piano mediano (che è il luogo di massimo fondo). Inoltre, è stato previsto un laboratorio di basse energie a latere, per l'impiego del Linac durante le ore in cui Adone "fatto il pieno" si comporta come una sorgente radioattiva.

Il costo presunto dell'impianto è riassunto nella tabella 2. Il tempo previsto per la realizzazione è dell'ordine di 2 a 3 anni; inoltre, anche nel caso di un pieno impiego dei Laboratori di Frascati in tale periodo, bisogna pensare ad un incremento del personale di più di 30 unità.

TABELLA 1

DATI PRELIMINARI PER UN ANELLO DI ACCUMULAZIONE PER ELETTRONI E POSITRONI A FOCHEGGIAMENTO

	FORTE		DEBOLE	
Energia	1500 MeV		1500 MeV	
Raggio	700 cm		500 cm	
Coefficiente di allungamento	1,6		1,4	
Lunghezza sezioni diritte	(290+140)x6 cm		160x8 cm	
Peso magnete	~150 ton		~ 150 ton	
Peso bobina	~ 15 ton		~15 ton	
Numero settori	12(1/2F,D,1/2F+AD)		8	
Indice del campo	n - 40		n - 0,6	
Potenza alimentazione	~200 kW		~ 200 kW	
Vuoto	10^{-9} mmHg	}	10^{-9} mmHg	}
Armonica RF	k - 6	} t-30 h	k - 2	} t-30 h
Tensione di picco RF	120 KV	}	290 KV	}
Positroni per impulso	$2x10^{7}$ a 15 MeV		10^{7} a 15 MeV	
Elettroni per impulso	$2x10^{11}$ a 30 MeV		10^{11} a 30 MeV	
Limite carica spaziale e^{+} x e^{-}	10^{22} - 10^{23}		10^{22} - 10^{23}	
Dimensione orizzontale del fascio	0,2 cm		2,4 cm	
Dimensione verticale del fascio	?		10^{-3} cm	

TABELLA 2

COSTO PRESUNTO PER UN ANELLO DI ACCUMULAZIONE PER ELETTRONI E POSITRONI

Acceleratore lineare da 30 MeV	L. 550.000.000.-
Edificio	" 250.000.000.-
Magnete alimentazione	" 250.000.000.-
Radiofrequenza	" 200.000.000.-
Vuoto	" 50.000.000.-
Ottica iniezione	" 100.000.000.-
Controlli	" 100.000.000.-
T O T A L E	L. 1.500.000.000.-

APPENDIX B

Unpublished work by M. Greco and B. Touschek, 1966

On the extension of minimal coupling in QED
(M. Greco and B. Touschek)

Recently Pipkin, et al[1] have observed an anomaly in the production of electron positron pairs by γ rays, which could be interpreted as indicating a possible breakdown of quantum electrodynamics.

This theory is based on the following axioms:

1) - relativistic invariance
2) - the correspondence principle
3) - Gauge invariance
4) - Locality
5) - Minimality

It is difficult to picture a theory, which could do without the first four of these axioms: a violation of (1) or (4) leaves us without the mathematical tools necessary to construct a theory, whereas (2) and (3) are needed to define electrodynamics and to distinguish it from other theories.

The most expendable of the axioms seems to be the last- it is not based on any single experimental fact, nor is there any motive to retain it apart from the convenience of not having to review the theory of renormalization.

It also appears that any attempt to introduce variants of existing quantum electrodynamics (as for example a modification of the particle propagators) necessarily leads to the introduction of correcting terms, which are in contrast with minimality.

In the present note we want to discuss the consequences which arise from the introduction of two specific non minimal terms (equations (1a), (1b), in which the parameter M is chosen in such a way as to fit the Pipkin, ... anomaly. It will be shown, that (1b) is not in contrast with existing experimental evidence (Lambshift, anomalous magnetic moment of the electron) and that definite predictions can be made about other processes, in which a similar anomaly might be expected to occur - as in wide angle bremsstrahlung and in the Compton effect.

(1) Blumenthal, et al. Physical Review Letters 660, 14 (1965)
Blumenthal, et al. Physical Review, 1199, 144 (1966)

A first attempt to extend the notion of minimal electromagnetic interaction is to introduce a term of the kind $\frac{e^2}{M^3}\bar{\Psi} F_{\mu\nu} F_{\mu\nu} \Psi$ where $F_{\mu\nu}$ is the electromagnetic field tensor and M a mass ~~which~~ whose value can be determined by fitting the experimental data.

This form of coupling involves two photons vertices. Alternatively one can take into account a pseudo-scalar interaction of the kind $\frac{e^2}{M^3}\bar{\Psi}\gamma_5 F_{\mu\nu}\tilde{F}_{\mu\nu}\Psi$ where $\tilde{F}_{\mu\nu}$ is the dual tensor of $F_{\mu\nu}$ defined as $\tilde{F}_{\mu\nu} = \varepsilon_{\mu\nu\rho\sigma} F_{\rho\sigma}$

In ~~such a~~ this way the total interaction between electromagnetic and electron fields can be described by Languangian:

(1a) $$\mathcal{L} = e\bar{\Psi}\gamma_\mu \Psi A_\mu + \frac{e^2}{M^3}\bar{\Psi} F_{\mu\nu} F_{\mu\nu} \Psi$$

or

(1b) $$\mathcal{L} = e\bar{\Psi}\gamma_\mu \Psi A_\mu + \frac{e^2}{M^3}\bar{\Psi}\gamma_5 F_{\mu\nu} \tilde{F}_{\mu\nu} \Psi$$

It can be easily seen that in any physical process the interference terms between the two parts ~~by~~ of which the Languangian[1] is formed, are always zero in the limit $m \to 0$

(2) 

Fig. 1

To prove this, let ~~no~~ us consider diagrams as in fig. 1, where electron and photon lines can be either internal or external ~~ones~~. Interference terms involve always an odd number of γ matrices or an even number of γ's multiplied by m, for both scalar and pseudo-scalar interactions. *

* (This has been pointed out to us by N. Cabibbo)

APPENDIX C

This note was kindly contributed by Giancarlo Rossi.

A Note on Random Numbers.

Bruno Touschek.

Any attempt at creating experimentally a random series of numbers is beset by at least the two following difficulties: (a) bias, (b) correlation.

(a) In a hypothetical experiment a random process (as for example cosmic radiation) is used to sample the states of a "fast clock" (for example a scalar capable of the 10 states 0,1,...9.) Bias may arise from the possibility that the scaler does not spend equal times in its various states (it might for example prefer to linger in state 7, so that the sampled numbers will show a bias in favour of 7.)

(b) Correlation is a consequence of the fact that the frequency of the clock as well as the frequency of the cosmic ray events is finite. It follows that the correlation between a state and the successive state is of order $e^{-\tau/T}$ in which τ is the average time lapse between two successive cosmic ray pulses and T is the time which the clock spends in a given state. The correlation will be very small if the clock is very fast and the sampling process very slow, but it is always fatally there and provides a certain "stiffness" in any experimentally produced chain of random numbers.

The difficulties (a) and (b) can be eliminated to any given degree by making use of a "binary clock" and by "folding" the experimental series on to itself.

The binary clock has two states, which for simplicity we shall call + (+1) and - (-1). It can be described by a two-valued state function C = C(t). One has

(1) $C^2(t) = 1$ (i.e. $C(t) = \pm 1$) and $C(t+T) = C(t)$

Ideally the tme average of C should be zero, but in any practical system this will not be the case and we assume therefore that the timeaverage $\bar{C}$ of C is small but not necessarily zero. It is therefore possible that our clock has a bias.

The random sampling signal will be denoted by R(t). In the simplest case we shall have

(2) $$R(t) = \sum_n \delta(t-t_n)$$

in which δ is the Dirac δ-function and t_n (n=1,2,...N >>1) is the time of arrival of the nth cosmic ray pulse.

If a multiplier is used to form CR one obtains a first approximation to a two valued randomseries S(n). We write

(3) $$S_1(n) = C(t_n) \quad (S_1^2=1)$$

The idxal random series S = S(n) (n=1,...N) has the following properties:

(4) $$\langle S\rangle = N^{-1}\sum_n S(n) = 0; \quad \langle S(n)S(n+k)\rangle = N^{-1}\sum_n S(n)S(n+k) = \delta_{k0}$$

δ_{ab} is the Kronnecker symbol, the sum in the second equation is evaluated by putting S(n+N) = S(n) (think of the randomseries as written on a drum), k= 0,1,...N-1. The first expression (4) imposes that the meanvalue of S is zero, the second that there be no correlation between the numbers which form the random chain and their successors.

The conditions (4) are necessary but not sufficient to define S=S(n) as a random series. Indeed we should have considered higher correlations of the form <S(n)S(n+k)S(n+k+r)>, which in an ideal sequence should also be zero. It seems to us very probable from the consideration of some examples that if the conditions (4) are satisfied to a good approximation also the higher conditions which we have not written down will be satisfied and satisfied to an even better approximation. At the moment we have no proof for this belief.

The experimental random series S = $S_1(n)$ will in general not be ideal. Indeed we will have

(4.1) $\langle S_1 \rangle = \overline{C}$ and $\langle S_1(n)S_1(n+k) \rangle = \delta_{k0}(1-\overline{C}^2) + \overline{C}^2 + f(k)$

The first equation (4.1) expresses the bias of the clock. In the 2nd the terms with $\overline{C}^2$ describe a correlation due to the bias (+1 is $(1+\overline{C})$ more likely to be followed by +1 than by -1!) f(k), with f(0)=0 expresses a genuine correlation. From what has been said before one expects f(k) to decrease with increasing k, for example $f(k) = \exp(-k\tau/T)$ for $k\neq 0$, and we shall therefore assume that there exists a $N > K \gg 1$ for which f(K) is negligibly small.

This assumption allows one to improve a given chain S_1 by the process of folding. To this end one forms

(5) $S_2(K) = S_1(n)S_1(n+K)$

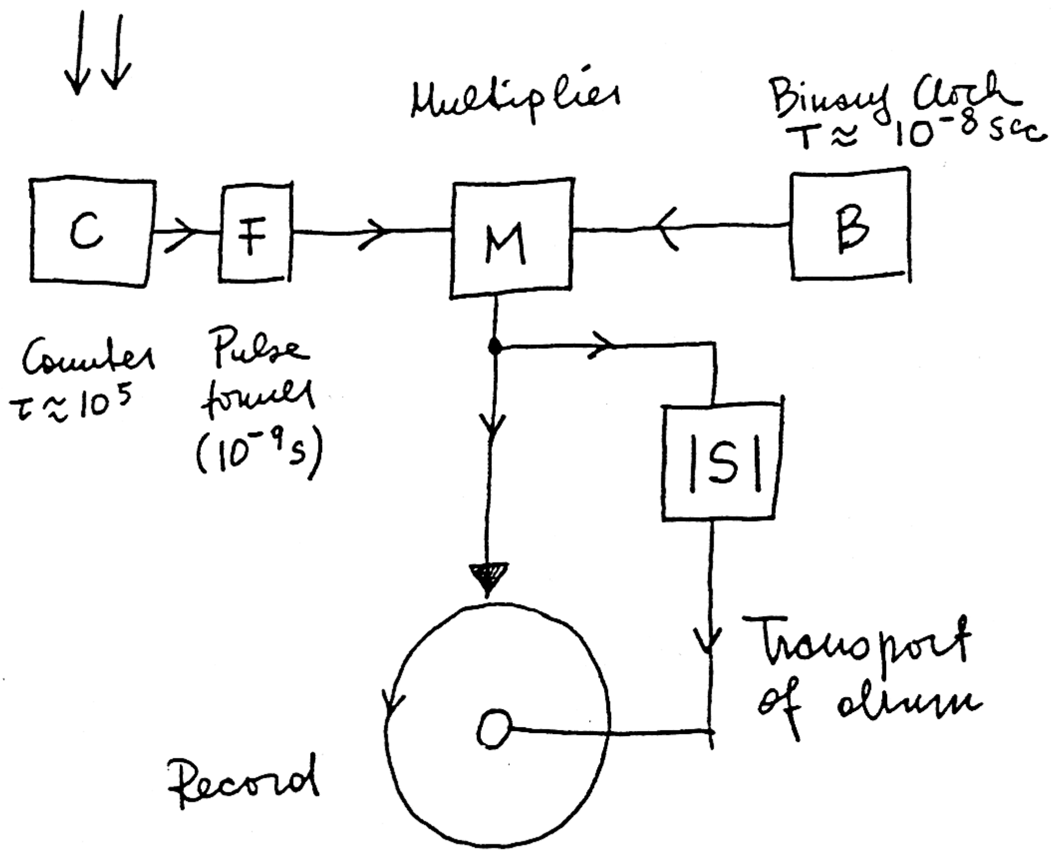
a process which experimentally can be carried out with a multiplying circuit. The new chain S_2 is still a binary chain since ++ and -- give + and +- and -+ give - . It is much "better" than the original chain S_1. For it can be easily checked that

(4.2) $\langle S_2 \rangle = \overline{C}^2$ and $\langle S_2(n)S_2(n+K) \rangle = \delta_{k0} + f(k)^2$

the latter holding only for $\overline{C} = 0$. It is therefore seen that an imperfect chain (the imperfection of which is expressed in terms of $\overline{C}$ and f) can be considerably improved by folding.

Finally we note that the binary series can be used to produce any desired distribution of random numbers. In the simplest case one might wish to dispose of a random chain of numbers between, say, 0 and 1023. To this end the chain is broken up into blocks of 10. Inside the block one reads 1 for +1 and 0 for -1 reading the whole block as a binary representation of a ten digit number. To have a series of for example 1 million random numbers between 0 and 1023 one therefore requires a binary chain of 10 million digits.

Figure!


Multiplier
Binary Clock
T ≈ 10^-8 sec
C
F
M
B
Counter
τ ≈ 10^5
Pulse
former
(10^-9 s)
|S|
Transport
of drum
Record

APPENDIX D

Bruno Touschek
Rome, Viale Pola 23,
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May, 30th 1972.

Dear T.D.,

It was so nice meeting you and your wife in Pisa and I wish that there would be more time to abuse your patience.

I enjoyed reading your article in "Physics Today" - I started on the train - but it confirmed my prejudice to disagree with you, limitedly, since I am sure you are right about the Long Island Railroad.

In this letter I want to describe to you an alternative (perhaps) to what we discussed in Pisa. Could the future of high energy physics be this?:

The "York" a physical historical joke.

I picture a non diffractive process as one in which an energy - measured by s - is transferred from the past light cone of the incoming particles to the future lightcone of the final states. This exchange of energy, I assume to be quantized according to

(1) $$\delta s^2 = \alpha^2 G^{-2}$$

where α is Sommerfeld's constant and G Fermi's. The quantum on the r.h.s. of (1) is called the "York" (the grand old duke of York, he had tenthousand men, he marched them all to the top of the hill and marched them down again).

Cross sections σ_n can be defined according to the number n of Yorks involved. I put

(2) $$\sigma_n = x(n,J)\alpha^2 \frac{1}{s} p_n$$

where x is a numerical factor of order unity (which I shall forget in the following) and p_n is the probability of producing n Yorks. The p_n are subject to the conditions

(3) $$\sum_n p_n = 1 \qquad \sum_n np_n = \bar{n} = \alpha^{-2} G^2 s^2$$

The Yorks are indistinguishable and the p_n can therefore be determine from Planck's formula:

(4) $$p_n = \bar{n}^n/(1+\bar{n})^{(1+n)}$$

Electromagnetic processes are characterized by n=0. According to (2) and (4) one has

(2.1) $$\sigma_0 = x\alpha^2/s(1+\alpha^{-2}G^2s^2)$$

For low energies therefore electromagnetic processes show the typical "Adone behaviour" α^2/s , at high energies the cross section goes down the hill like $\alpha^4G^{-2}s^{-3}$.

Weak interactions have n=1. The typical behaviour is:

(2.2) $$\sigma_1 = G^2s/(1+\alpha^{-2}G^2s^2)^2$$

This gives the classical energy dependence for $\alpha^{-2}G^2s^2 \ll 1$ and thanks to the grand old duke goes down as s^{-3} for $\alpha^{-2}G^2s^2 \gg 1$.

Note, that from (2) and the first equation (3) it follows that

(5) $$\sum_n \sigma_n = \alpha^2 x \frac{1}{s}$$

provided that x does not depend on n. This means of course that the joke can be renormalized.

Comments.

What you say about theoretical speculations at the end of your paper holds even more so for this joke, which, nonetheless has this attempted moral in it: I think that if one goes to very small dimensions of space and time the difference between what is spacelike and timelike should disappear and with it the possibility of putting things right by introducing new particles: these things start out elementary then become not quite so elementary, then irrelevant (as in the scaling region) and then this time really elementary particles have to be found.

Well, I dont know. I hope you have enjoyed the Balaton and I am sending this letter to Cern as well as to Columbia.

With the best greetings and thanks again for the very stimulating Pisan conversation

Yours

Bruno

APPENDIX E

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WHAT IS HIGH ENERGY?

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Received 30 April 1974

The energy region above 1 GeV is subjected to a search for characteristic points. These points exist, their significance is discussed. A strange and unlikely correlation between them and the masses of lower energy physics is pointed out.

A preliminary but unsatisfactory answer is $E \gg m_p c^2 \approx 1$ GeV. It is unsatisfactory because it does not tell us how far we have to go on the energy scale in order to have the right to expect something fundamentally new. It is the purpose of this paper to show that there are "milestones" in the high energy region and to muse about their possible significance. In the following discussion it has to be kept in mind that there is no sure theoretical guidance to predict what happens at high energies this in itself being the reason for wanting to carry on this rather expensive branch of physics.

The milestones indicate the spots where two or three of the different branches developed in low energy physics – strong, electromagnetic, and weak interactions – might meet. In the search for these milestones existing theories must be driven to the breaking point and the best theories (like quantum electrodynamics) defend themselves very well against such an effort: the renormalization of mass as well as the recently discovered scaling laws are formidable defences, which have to be broken, if one wants to arrive at an answer to the question which is posed in the title of this letter.

The first point of reference in our search presents itself in the form of Fermi's constant G, defined by a conventional interaction Lagrangian of the form

$$L_{\text{int}} = \frac{G}{\sqrt{2}}\ (J^+, J) \qquad (1)$$

in which J and J^+ are the weak charged currents and one has $[Q, J] = -J$, where Q is the operator of the charge. The constant G has the dimensions ergcm [3], which, putting $h = 1$ and $c = 1$ leads to

$$G = 10^{-5}\, m_p^{-2} \qquad (2)$$

which by putting $G = 1/M_G^2$ leads to a first critical mass of $M_G = 294$ GeV. This then denotes a point very high up in the energy scale, which even the most ambitious high energy projects are unlikely to reach in the near future. (The energy would have to be produced in the c.m. of the colliding particles.)

M_G marks the point, where – in spite of their weakness – the perturbation theory of weak interactions must break down.

The analogy between electrical and weak interactions allows one to arrive at a critical mass m_W which is considerably smaller than m_G. The argument is the following. The basic equation for the vector potential is

$$\Box A = eJ^{(o)}$$

in which $J^{(o)}$ is the neutral ($[Q, J^{(o)}] = 0$) current and e is the elementary electric charge ($e^2 = 1/137$). This is compared with the equation for a hypothetical field of W-mesons of mass m_W invented to make weak interactions more conventional, namely

$$(\Box + m_W^2)\, W = \gamma J$$

in which γ is a coupling constant which has the same dimensions as e. The similarity between the two equations as well as what may be called a principle of "economy" suggests to put $\gamma = e$. The equation of motion for the charge W-meson may be derived from a Lagrangian of the form $L_{\text{int}} = \gamma(W^+ J + J^+ W)$. At low energies the d'Alembertian in the equation of motion may be neglected and one therefore has $W = \gamma J m_W^{-2}$. This then gives $L_{\text{int}} = 2\gamma^2 m_W^{-2} J^+ J$, which if compared with eq. (1) gives

$$m_W = m_G \left(\frac{2\sqrt{2}}{137}\right)^{1/2} = 42 \text{ GeV} \tag{3}$$

which is already a much more accessible energy.

To understand the physics underlying this argument one does not have to believe in the actual existence of the W-meson. Indeed, the energy at which (according to existing calculations) weak interactions catch up with electromagnetic interactions can be determined by the following argument. A typical electromagnetic cross section (e.g. that for the reaction $e^+ + e^- \to \mu^+ + \mu^-$ is of the form $\sigma_{e.m.} = \alpha^2/s$. A typical weak interaction has a cross section σ_W which goes like $\sigma_W = G^2 s$. The two cross sections become comparable at $s^2 = \alpha^2/G^2$, which defines a mass of the order of magnitude given by eq. (3). m_W can therefore be defined as the energy, where weak and electromagnetic interactions "meet".†

Also strong and electromagnetic interactions seem to want to meet at an energy which is not too different from (3). The total hadronic cross section σ_h of the reaction $e^+ + e^- \to$ hadrons first observed in Adone at Frascati and confirmed at higher energies by the measurement performed on the "Cambridge Bypass" an "Spear" show that the hadron cross section is fairly constant in a region which extends from about 1.5 GeV to 5 GeV [1]. These observations can be described by putting

$$\sigma_h/\sigma_{\mu\pm} = \kappa s \tag{4}$$

$\sigma_{\mu\pm}$ is the cross section for the production of muon pairs in the collision between electrons and positrons. The constant κ can be determined from these – still very rough – experiments and comes out as of the order 0.25 GeV^{-2}. The mass corresponding to this energy is given by $m \approx 2m_p$. This seems to indicate that the surprisingly high hadron cross sections are indeed related to a typical "hadron-quantity" $-m_p$ – since the pure electromagnetic part of the interactions is cancelled out in the ratio (4). One is tempted to sum up (4) by saying that high energy physics starts at an energy of about $2m_p$.

† It must be stated that m_W is *not* a new invention. An energy of this order of magnitude has been in the air since the interest in weak interaction had been renewed by the discovery of the non conservation of parity.

If eq. (4) were exactly true this would lead to a collision with quantum electrodynamics. This has been shown by Cabbibo and Karl in a paper to be published. They find that if (4) were generally true the photon self energy $\Pi(s)$, would be of the form $(\alpha/3\pi)\kappa s \times (\log \kappa s + A)$ in which A is a numerical constant which has to be introduced if one uses "subtracted" dispersion on relations. It is confined to a curve in the complex plane. It is clear that this form of the selfenergy cannot be rigorously true since it gives an asymptotic behaviour of the photon propagator, which is quite at variance with "axiomatic" electrodynamics. However the argument indicates that at a new critical mass $m_{\gamma h}^2 \approx (3\pi/\alpha)\kappa^{-1} = 70^2 \text{ GeV}^2$ (its exact value depends on the choice of A and generally comes out as less than $m_{\gamma h} = 70$ GeV) one has to expect some conflict between quantum electrodynamics and the observed behaviour of strong interactions. This conflict is therefore expected very near the energy where also weak and electromagnetic interactions join battle.

There is no denying that this observation may be based mainly on a coincidence: eq. (4) has been determined from still very inaccurate data in an energy region which extends to only 5 GeV and has to be believed to be valid to energies of the order of 50 GeV, if one wants to give some credit to this breakdown of quantum electrodynamics due to strong interactions.

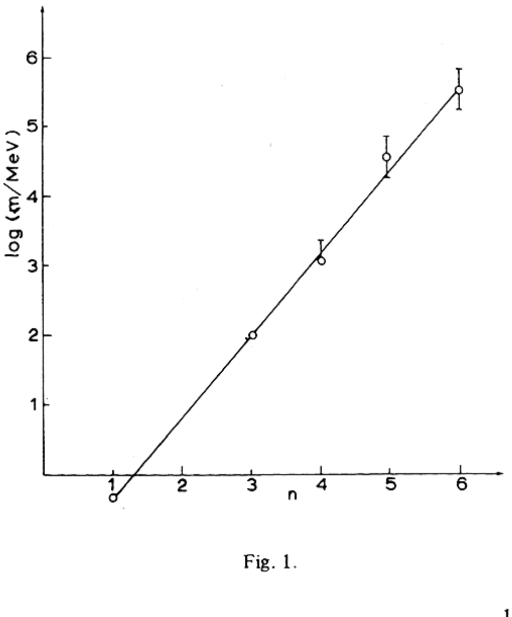

Fig. 1.

It one takes the risk of believing in the reality of the indications furnished by Cabbibo and Karl one must come, however, to the conclusion that at about 50 GeV the trinity of nuclear physics (strong, electromagnetic, weak) loses its significance and that beyond this energy a new and simpler physics should arise.

The milestones $m_{\gamma e} \sim m_{\gamma h} \sim m_W$ and m_G seem to form part of some superior design which is indicated in fig. 1. Calling m_n the energy of the milestone n and letting in particular correspond $n = 1$ to the electron; $n = 2$ to nothing, $n = 3$ to the muon, $n = 4$ to the proton (or some average baryonic mass), $n = 5$ to the W and $n = 6$ to m_G i.e. to the mass where the theory of perturbations for weak interactions ceases to make sense, one gets a nearly perfect straight line by plotting $\log(m_n/\mathrm{GeV})$ against n. The plot shown in the figure covers a range of nearly six orders of magnitude. The "errors" indicated correspond to $\pm\log 2$ for $\log m_W$ and $\log m_G$, whose definition has been very qualitative, and to $+\log 2$ for the proton, which is taken as a representative for all the Fermi type hadrons, all of which have a mass which is bigger than 1 GeV.

It is seen that the points of the plot can be described by

$$m_n = (206)^{n/2} m_e = (m_\mu/m_e)^{n/2} m_e \qquad (5)$$

the scaling factor between the various masses being the square root of "206" – the mass ratio between the muon and the electron. It is also seen that the point $n = 2$ is conspicuously missing in the list of known particles. Whether this is due to the fact that the plot is the product of sheer coincidence, or to some unlikely selection rule, or perhaps to the haste of high energy physics to arrive at higher and higher energies, remains to be seen. The mass of the missing object should be about 7.3 MeV.

A consequence of the puzzle which the plot (if it is at all meaningful) seems to pose might be the involvement of Sommerfeld's constant, which is related to the muon electron mass ratio by the pun: $m_\mu/m_e = m_3/m_1 = \frac{3}{2} \times 137 + 1$.

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