

**The Scientific Legacy
of Beppo Occhialini**

Edited by
P. Redondi, G. Sironi, P. Tucci and G. Vegni

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P. Redondi, G. Sironi, P. Tucci and G. Vegni

Pietro REDONDI
Dipartimento di Psicologia
Università degli Studi di Milano-Bicocca
Piazza dell'Ateneo Nuovo, 1
20186 Milano, Italy

Giorgio SIRONI
Dipartimento di Fisica "G. Occhialini"
Università degli Studi di Milano-Bicocca
Piazza della Scienza, 3
20186 Milano, Italy

Pasquale TUCCI
Istituto di Fisica Generale Applicata
Università degli Studi di Milano
Via Brera, 28
20121 Milano, Italy

Guido VEGNI
Dipartimento di Fisica
Università degli Studi di Milano
Via Celoria, 16
20133 Milano, Italy

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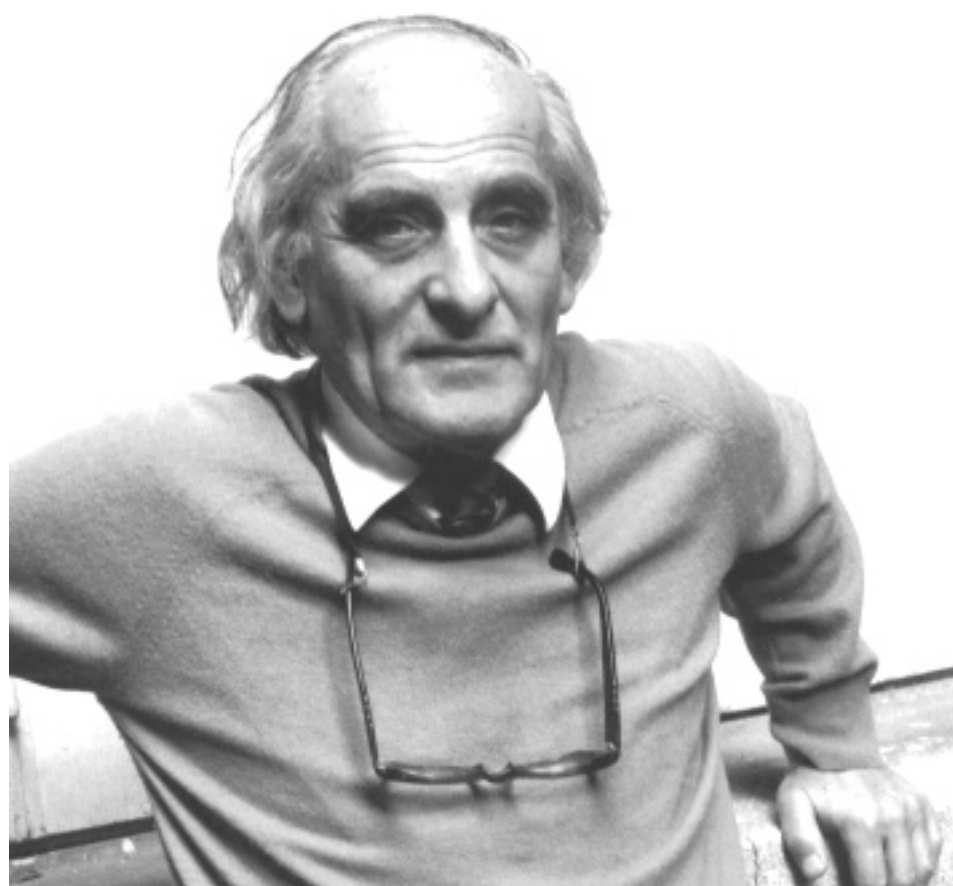
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G. orchialini

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Foreword

The year 2007 will be the centenary of the birth of Giuseppe (Beppo) Occhialini. After about twenty-five years spent between England, Brazil and Belgium, in 1950 Occhialini returned to Italy. The following year, 1951, after a short period in Genoa, he won the chair of Modern Physics (Fisica Superiore) at the Università degli Studi in Milan.

At that time Occhialini was well known. His standing as a scientist on the international scene is evident in the photograph of the invited participants at the 1948 Solvay Conference. Occhialini remained in Milan up to his retirement in 1983. In 1951, his arrival gave new impetus to significant research activities. The advances in this research rapidly spread throughout Italy and Europe and produced important scientific results in elementary particle physics, space physics and astrophysics. In particular through his continuous efforts Occhialini contributed to the rejuvenation of Italian physics and astrophysics. He worked to build new research structures and to create the necessary support for the birth of space activities in Italy and in Europe.

During his entire professional life Occhialini pointed out new scientific pathways and such activities need to be placed on record for posterity. Occhialini in fact could be described as one of those traditional “natural philosophers” or “curious of nature”, whose scientific production in terms of published papers was often negligible compared to the enormous proliferation of papers of the present day. In the case of Occhialini, by the '60s practically no scientific papers were signed by him. Busy in organizing the research activities of his group and in finding structures and financial supports essential to transform projects into real experiments, he assumed (erroneously) that his contribution to science was insufficient to justify signing as an author of a scientific paper. However without his continuous efforts many of today's Italian astrophysical Laboratories, Italian and European space agencies as well as their scientific results would not exist. During his life Occhialini witnessed a double transition in the practice of research. At first research was an activity engaged primarily for pleasure, usually by University Professors whose official duty was teaching. Later researchers were paid mainly for doing research with minor teaching assignments. Finally for many, research became a full-time job. Despite this evolution, he continued to seek support for research activities but observed this trend with a bit of perplexity and sometime expressed ironically his point of view (for instance, when the writer in the '60s became a regularly paid CNR researcher, Occhialini, jokingly, called him a “mercenary of research”).

The Physics Institute where Occhialini arrived in 1951, evolved into the Physics department of the Università degli Studi di Milano. In 1998 an important fraction of the original astrophysics group of Occhialini moved to the Physics department of the Università degli Studi di Milano-Bicocca which is named after him. It was here, two years ago, that the planning of this volume began. The aim was to preserve the memory of a lifetime of achievements by Beppo Occhialini. Simultaneously an archive was created where the two Universities collected papers and documents left by Occhialini and his wife Connie Dilworth at the Physics Department or donated after the death of Beppo Occhialini and Connie Dilworth by their daughter, Etra.

The various aspect of Occhialini activities are discussed in detail in the present volume. Many of the authors had the opportunity of working with Beppo at various levels. They are now close to retirements or retired, as one can see looking at their present affiliations, but their contributions are particularly important to recognize apparently small traits of Occhialini personality which made him a reference for at least two generations of researchers. Unfortunately almost all the Occhialini colleagues of the Cambridge and Brazil period are now passed away, so contributions on these periods have been written by history of physics experts who went through archives and papers. Fortunately the flavour of the experience of colleagues and direct witnesses of Occhialini activity in the '30s and '40s has been partially preserved and can be found in the proceedings of two symposia, one held in Milan in 1968 to celebrate the 20th anniversary of Occhialini return to Italy, published by Seminario Matematico e Fisico di Milano, and one held in Rome in 1995 by the Accademia Nazionale dei Lincei to celebrate three friends and important Italian scientists, Giuseppe Occhialini, Bruno Pontecorvo and Bruno Rossi (*Atti dei Convegni Lincei*, Vol. 133, 1997). Particularly interesting is the paper presented in Rome by Beppo's friend, Valentino Telegdi.

This volume, whose original idea is due to Pietro Redondi, Full Professor of History of Science at the University of Milano-Bicocca, has been prepared by a Scientific Board made of Puccio Bellotti, Ettore Fiorini, Tonino Pullia, Pietro Redondi and Giorgio Sironi of Milano-Bicocca University, Pasquale Tucci and Guido Vegni of Milano University and Antonio Vitale of Bologna University. Editors are Redondi, Sironi, Tucci and Vegni. Financial support was provided by the two Milano Universities, the Italian National Institute for Nuclear Physics (INFN) the Italian National Institute of Astrophysics (INAF), the Italian Physical Society (SIF) and Fondazione "Giuseppe Occhialini" (Fossombrone): we thank these institutions for their help. I would also like to thanks Daniele Minelli, Giovanni Sala, Sebastiano Spinelli e Mauro Tacconi, four students who accepted to spend many hours in reorganizing the contributed files and put them in a form ready for the printer. One hopes that they learned a lot about the past history of Physics in Milan and other parts of Italy from which derives a large fraction of present-day research activity in Physics and Astrophysics.

GIORGIO SIRONI
Milano, May 11th 2006.

Giuseppe Paolo Stanislao Occhialini (1907-1993)

A Short Biography

LEONARDO GARIBOLDI and PASQUALE TUCCI

*Istituto di Fisica Generale Applicata, Sezione di Storia della Fisica
Università degli Studi di Milano, Italy*

1. – Introduction

“The development of my studies is typical of those of many people of my generation. Those among us who started their research trying to understand the nature of cosmic rays had been induced to the hunting of new particles. Actually, for more than twenty years, the only source of new particles had been the cosmic radiation. In such a way, the positron, the π - and μ -mesons, the K -mesons, and the hyperons had been discovered. In 1954, when the Bevatron, the big accelerating machine, able to produce strange particles and the anti-proton, started to work, this time of grace of the cosmic rays found its end. At that time the physicists, who had had an easy, adventurous, and less expensive way, found themselves in front of a choice. To go on with the particles, but with the accelerating machines, or to go back to cosmic rays and improve the research on their origin. There is who chose the first alternative —many people in Italy took the way to Frascati and to the CERN— a hard and very exciting way. Only a minority felt itself more bound to the old cosmic rays —some of them oscillated, hesitated. To those people (and I am one among them), the year 1957 was a strong draw. The launch of the first Sputnik offered the possibility, only dreamt in the thirties, to go out of our atmosphere, of our magnetosphere, to go and study the real primary radiation. Stratospheric balloons existed even before, that’s true, and they went till a few grams of residual atmosphere, but this idea of being able to get rid of our whole curtain was fascinating. Here we are, the equerry of cosmic rays in space — but another transformation is even happening, the

more we go on, the more we notice that we are becoming close relatives of astrophysicists. From particles to astrophysics, it seems that this subject of cosmic rays be unable to keep a proper personality!”⁽¹⁾.

In these few lines, we can appreciate Giuseppe Paolo Stanislao Occhialini’s feeling for his exciting scientific adventure in the researches on cosmic-ray physics and for their human background. Occhialini was a privileged actor whose adventurous scientific life went through three of the classical periods of cosmic-rays physics: the first and second particle periods, and the astrophysical one [1]⁽²⁾.

2. – His father and the Arcetri period

Giuseppe (“Beppo”) P. S. Occhialini⁽³⁾ (1907-1993) was born in Fossombrone (Pesaro Province, Italy) on December 5th, 1907, son of Raffaele Augusto Occhialini⁽⁴⁾ (1878-1951), a physicist, and Etra Grossi⁽⁵⁾. His father played an important role in the years of Beppo’s training, as the latter writes in his own autobiography:

“The milieu created by his father’s personality, culture, and rigour, was fundamental for the choice of the course of his work and for his social and political formation”⁽⁶⁾.

Augusto Occhialini studied at the University of Pisa where he got a degree in physics in 1903. A collaborator of Angelo Battelli until 1918, he directed the physics lab and taught physics and electrotechnics. He was the assistant of Antonio Garbasso in Florence (1918-1921), professor of physics in Sassari (1921-1924), Siena (1924-1928), and Genoa (1929-1951). Augusto Occhialini was known not only in Italy but also abroad, since he used to spend his holidays visiting the most important laboratories of physics abroad.

⁽¹⁾ Hand-written note by Occhialini’s wife. Undated, written after 1957. Original in Italian. Occhialini Papers 8, 3, 7.

⁽²⁾ The classification advanced by Brown and Hoddson (the historical table is on page 7) is based on a division in five time intervals of the whole history of cosmic-rays physics: a) the prehistory (up to 1911); b) the age of discovery and exploration (1911-1930); c) the first particle age (1930-1946); d) the second particle age (1947-1953); e) the astrophysics age (from 1953 on).

⁽³⁾ An enjoying fact concerns Beppo’s proper name: *“The origin of his initials, G. P. S., supplies an amusing example of Occhialini’s unorthodox approach. At the beginning of his career, he styled himself, as most Italians, with a single first name: Giuseppe. Upon joining Blackett, he opted for multiple initials. He explained that he added “P” for Peppino (a nickname for Giuseppe), and “S” for Sommerfeld, a pseudonym under which he had run as a sprinter in his student days (during an exam period). In actual fact, the Biographical Encyclopedia of Scientists lists, in addition to Giuseppe, the names Paolo and Stanislao”* [2]. A copy of his birth certificate, delivered in the ’50s (having been the original documents destroyed during the war) attests his full name to be Giuseppe Paolo Stanislao, though it is right that he signed his first papers only with his first name.

⁽⁴⁾ Biographical notes about Raffaele Augusto Occhialini can be found in [3].

⁽⁵⁾ Biographical information about Giuseppe Occhialini can be found in [2, 4-9].

⁽⁶⁾ Ref. [9], quotation on page 322.

His scientific production comprises works of experimental physics: electromagnetism, gas physics, and atomic physics. Among his most known opera is the treatise “*Elettrotecnica*” (1921-1922) and “*La radioattività*” (1910) written with Chella and Battelli.

Augusto Occhialini’s family followed him every time he moved to a new university. Beppo spent his youth first in Pisa, and then in Florence. He attended the Scientific High School in Florence, and he decided then to study physics at the University of Florence in 1926. The years spent at the Institute of Physics in Florence represented

“one of the three fundamental steps in his existence. The other two are the period at the Cavendish Laboratory in Cambridge and the period at the Wills Laboratory in Bristol”⁽⁷⁾.

The course of physics in Florence started in 1924, and the director of the Institute of Physics⁽⁸⁾ was Antonio Garbasso, a physicist who was also Senator of the Kingdom of Italy and mayor of Florence. The Institute was on a hill in Arcetri, not far from Galileo’s villa.

“The graduation in physics had been instituted recently; the activity of the physics laboratory was supported by the faith of its animator, Garbasso. The view offered through the windows made forgive the meagre equipment, the lack of functionality of its conventual structure, and the difficulty of access”⁽⁹⁾.

A considerable development of the activity of research done at the Institute of Physics in Arcetri started with the arrival of Bruno Rossi⁽¹⁰⁾ in 1927 and Gilberto Bernardini in 1928. Enrico Persico, professor of Mechanics and Theoretical Physics, remained in Florence until 1930, when he transferred to the University of Turin.

In the Institute of Physics, under Garbasso’s direction, they researched⁽¹¹⁾ optical and X spectroscopy, in particular the Lo Surdo-Stark effect. Of valuable importance for the cultural and technical training were the collective reading of the main scientific journals, and the Physical and Astrophysical Seminar that promoted the contact with both Italian and foreign scientists. The Physical and Astrophysical Seminar was founded by Giorgio Abetti and had Gilberto Bernardini as an ardent supporter. The spectroscopy naturally kept physicists in touch with astrophysicists.

Besides Occhialini, among the students of physics, there were Daria Bocciarelli, Attilio Colacevich, Lorenzo Emo Capodilista, Giulio Racah, and Guglielmo Righini. Occhialini graduated in physics with Rossi in Florence in 1929, with a dissertation on some research

⁽⁷⁾ Ref. [9], quotation on page 322.

⁽⁸⁾ Information about the course of physics in Florence, in the period took into account here, is in [10].

⁽⁹⁾ Ref. [9], quotation on page 322.

⁽¹⁰⁾ On Bruno Rossi, and his role in cosmic-ray physics in this period, see [11-14].

⁽¹¹⁾ On the general situation of physics in Italy of that time, see [15, 16].

on cosmic rays inspired by his father who was familiar with Millikan's theory⁽¹²⁾. After his graduation in physics, Occhialini remained at the Institute of Physics in Arcetri as volunteer assistant, then regular assistant (1932-1937).

The change of subject of research from spectroscopy to cosmic rays⁽¹³⁾ gave birth to the Arcetri School⁽¹⁴⁾, that had to face the definition of a new, stimulating, and less expensive program of research. The solution was an article written by Walther Bothe and Werner Kolhörster [22] on the application of Bothe and Geiger's coincidences method to the study of cosmic rays. Bruno Rossi directed the research of the Arcetri group towards this new direction. To continue the studies begun by Bothe and Kolhörster, the Arcetri School produced some GM counters⁽¹⁵⁾ to study their features. At this time, Rossi invented "Rossi's circuit" [23] that permitted to detect the simultaneous discharge of several counters (multiple coincidences), thus realising a decisive advance compared to Bothe's coincidence circuit.

At this time, Occhialini was one of Rossi's main assistants, with Daria Bocciarelli. The articles written in this period are but signed by Rossi alone, and in his memories Rossi does not highlight particular contributions of his assistants.

"Besides working with Rossi with counter-coincidence circuits, Occhialini was already interested in imaging particle detectors. These generate a photograph (or its electronic equivalent) of the particle's physical reality, rendered visible for the first time. At the end of the 1920s the most important instrument in this field was the Wilson chamber [...] Emilio Segré, a physics Nobel Prize winner, vividly remembers meeting Occhialini for the first time, perhaps in 1929, in Rome: '...two things worried him: how to avoid military service and how to make a one-meter diameter Wilson chamber ... an idea which was then considered megalomaniac and rather ridiculous...' "⁽¹⁶⁾.

In 1931, the first kind of research Occhialini made was a particular application of Geiger-Müller counters to the measurement of the energy of the β -rays emitted by weak-radioactive sources. Although Bruno Rossi did not sign the paper [24], his role, together with Bocciarelli's one, was valuable.

⁽¹²⁾ "Beppo Occhialini, still a student, was looking for a theme as argument of the Thesis for his Laurea. One day he told me this story: his father Augusto, Professor of Physics in Genova, used to spend the summer months in Germany for his research programs in Atomic Spectroscopy and coming back in fall 1927 reported about the experiments of Bothe and Kohlhorster on the Cosmic Radiation. Beppo proposed "Cosmic Rays" as theme for the thesis to his tutor and Bruno, after a reconnaissance tour in Germany, decided to make "Cosmic Rays as the main field of the research activity for his group in Florence." Quotation from [17] on slide 7.

⁽¹³⁾ On the early history of cosmic rays, see [18, 19], and the fundamental recollections in [20].

⁽¹⁴⁾ On the Arcetri School, see [21].

⁽¹⁵⁾ According to Occhialini, the Geiger-Müller counter was like the Colt in the Far West: a cheap instrument usable by everyone on one's way through a hard frontier.

⁽¹⁶⁾ Ref. [4], quotation on page 334.

In summer 1930, Rossi went to Bothe's laboratory to learn the best use of the GM counters. In Berlin, Rossi met several important physicists of that time, among them Patrick Blackett who was an expert in the use of the cloud chamber. Rossi was interested in the latter technique because of the results obtained with it by Skobeltsyn in Leningrad.

“Rossi thus asked Blackett to admit at the Cavendish one of his young collaborators, so that the latter could learn the cloud chamber technique to study cosmic rays”⁽¹⁷⁾.

In summer 1931, the Arcetri group decided to send a physicist to Cambridge, to the Cavendish Laboratory, to learn from Blackett the cloud chamber technique to introduce it then back in Florence. Their choice fell on Bernardini who could not go because of the military service. Occhialini was sent instead of him, with a letter of introduction written by Bruno Rossi, and with a CNR scholarship for three months. The first English period started so, and it lasted not three months but three years.

3. – The Cambridge Period (1931-1934)

When Occhialini went to Cambridge⁽¹⁸⁾, Ernest Rutherford directed the Cavendish Laboratory⁽¹⁹⁾. The research at the Cavendish mainly concerned nuclear physics and was still made in a relatively simple way. The Cavendish continued a tradition of research made producing visible phenomena. It was not the case that they used scintillators, photographic plates, and cloud chambers. Very less used at the Cavendish was instead the GM counter. The technique of cloud chamber⁽²⁰⁾ was developed for nuclear physics applications, after Rutherford's suggestion, first shortly by Shimizu and, then, by Blackett⁽²¹⁾.

The modernity of the Cavendish could only impress young Occhialini. He worked side by side with Blackett, and together they developed the controlled cloud chamber technique⁽²²⁾.

“He brought with him the technique of the coincidence counting of cosmic rays developed by Rossi. The marrying of the counter technique with the cloud chamber was an obvious step”⁽²³⁾.

⁽¹⁷⁾ Ref. [7], quotation on page 64.

⁽¹⁸⁾ On the history of physics in Cambridge in the '30s, see [25] and Blackett's own reminiscences in [26].

⁽¹⁹⁾ On Rutherford at the Cavendish, see [27].

⁽²⁰⁾ On the history of the use of the cloud chamber in physical researches, see the chapter “*Cloud Chambers: The Peculiar Genius of British Physics*” in [28], pp. 65-141.

⁽²¹⁾ On Blackett's contributions to cosmic-ray physics with the cloud chamber, see [29].

⁽²²⁾ On the controlled cloud chamber, see [30].

⁽²³⁾ Ref. [31], quotation on page 18. Besides Lovell's work, biographical informations on Blackett can be found in [32].

This marrying permitted a valuable progress in the obtaining of useful images: 76% of the photographs contained tracks of particles, and they could take one photograph every 2 minutes. In practice, the particles themselves started the controlled cloud chamber when they came into it and ionised. This fact aroused Blackett and Occhialini's comprehensible enthusiasm.

"I can still see him, that Saturday morning when we first ran the chamber, bursting out of the dark room with four dripping photographic plates held high, and shouting for all the Cavendish to hear 'one on each, Beppe, one on each!'" ⁽²⁴⁾.

The controlled cloud chamber was applied to the study of cosmic rays, the subject of research that had engaged Occhialini in Arcetri. In a work [34] of August 21st, 1932, published on *Nature*, Blackett and Occhialini proposed for the first time the results on their joined studies of the penetrating corpuscular radiation by means of the photographic technique applied to a Wilson cloud chamber triggered by a Rossi's coincidence circuit. The experimental disposal of the cloud chamber used by Blackett and Occhialini was similar to that proposed by Johnson, Fleisher, and Street [35].

The most important paper published by Blackett and Occhialini on their researches with the controlled cloud chamber was "*Some Photographs of the Tracks of Penetrating Radiation*" [36], communicated to the Royal Society by Rutherford on February 7th, 1933. The core of the paper was the study of cosmic-rays showers, with side, but absolutely not less important, items such as the positive electron (the positron)⁽²⁵⁾ and the non-ionising links.

Blackett and Occhialini interpreted the positron according to Dirac's theory as the anti-electron whose destiny was to annihilate with an electron producing one or more photons. The Meitner-Hupfner effect —that is the γ -rays anomalous absorption— was so interpreted as caused by the process of pair production, as well the re-emission of low energy γ -rays as caused by the process of annihilation of positrons and electrons. Positrons could also account for the apparent backwards trajectories of negative electrons from a neutron source.

In 1948, the Nobel Prize in Physics was awarded to Blackett for his contributions to the development of the Wilson method and his discoveries, made by this method, in nuclear physics and on cosmic radiation. It is commonly stated that Occhialini was quite too young —he was only twenty-five years old— when he invented the triggered cloud chamber in Cambridge with Blackett, but nomination to a Nobel Prize is actually independent of any consideration on the age of the nominees. In his Nobel Lecture [40], Blackett expressed himself outspokenly by mentioning several times Occhialini's fundamental contribution to their researches in Cambridge. In a letter to Occhialini's father,

⁽²⁴⁾ Ref. [33], quotation on page 144.

⁽²⁵⁾ On the discovery of the positron see Hanson's classical studies [37], De Maria and Russo's historical reconstruction [38], and Roqué's analysis [39].

Blackett motivated his disappointment for having awarded the prize alone, without a common assignation to Occhialini himself:

“I am very happy and proud to have awarded this prize, but I would have been still happier if Beppo had been honoured at the same time. For it was certainly his arrival in Cambridge which stimulated my embarking on this field of cosmic rays which I have never left. And our work together in 1932-33 was a real collaboration of the happiest kind”⁽²⁶⁾.

Occhialini always admired Blackett with reverential attitude and never suffered from this exclusion. Blackett was indeed already a master of the Wilson cloud chamber, both from a theoretical and an experimental point of view, before Occhialini’s arrival to Cambridge, and continued to develop that technique also after Occhialini’s return to Florence. Blackett’s nomination was based on his sixteen years long work with the cloud chamber.

4. – An intermezzo: Arcetri and São Paulo

Once back to Arcetri, in 1934, Occhialini found a situation that had worsened with respect to the one he had left in 1931. Garbasso died prematurely, Persico had moved to Turin, Rossi was professor in Padua. Above all, it was difficult to him to agree with the new political climate of fascist Italy, and to adapt himself to the new life and work conditions. He could not be indifferent to the war events in Ethiopia and Spain. In 1932, he had a deep crisis on conscience in taking an oath and enrolling as a member of the fascist party, to be allowed to continue his university career.

The Italian scientific world was well aware of the importance of the results obtained by the young Occhialini while in Cambridge. In 1934 he was awarded the Sella Prize of the Reale Accademia dei Lincei. It was but very hard to find a financial support to continue such kind of researches in Italy. Occhialini asked the Italian National Council of Researches (CNR) to finance the construction of a cloud chamber to study cosmic rays and neutrons. Guglielmo Marconi solicited this support too, but Occhialini got no answer at all.

Occhialini was in Lucca at the Military School in 1934-1935 for his military service, and taught physics at the Royal Institute of Arts (1935-1936) and at the Faculty of Architecture (1936-1937) both in Florence. He was then enrolled as professor at the Scientific High School in Macerata from 1937 on.

In June-July 1937, Occhialini was invited by Gleb Wataghin to join him in organising a school of physics in São Paulo⁽²⁷⁾. Occhialini discussed with Dr. Parini, of the Head Office of Italians Abroad, about his transfer to the staff of Italian middle schools and

⁽²⁶⁾ Letter from P. M. S. Blackett (Manchester) to A. Occhialini (Genoa), November 21st 1948. Occhialini Papers 1, 1, 3.

⁽²⁷⁾ On Gleb Wataghin in São Paulo, see [41].

his integration in the staff of Italian schools abroad. In August 1937, Occhialini left Italy to Brazil and was nominally admitted to the school “Dante Alighieri” in São Paulo. Actually, he became appointed professor of Experimental Physics at the University of São Paulo.

Due to the lack of local human resources, Brazil had to call from Europe many professors to build a new large university, the University of São Paulo, with the new Faculty of Philosophy, Sciences, and Letters⁽²⁸⁾. The presence of Italian scientists at the University of São Paulo was supported by the Italian government, that paid their salary, because it was considered an activity of cultural and political mission in a Latin country with a consistent Italian immigration.

“In a modest lab, populated by a group of Wataghin’s young students, such as M. De Souza Santos⁽²⁹⁾, M. Schönberg⁽³⁰⁾, P. A. Pompeia, A. de Moraes, and, in a second time, U. Camerini and C. Lattes⁽³¹⁾, he [Occhialini] prepared an experiment to observe large cosmic rays showers with Wilson chambers and counters”⁽³²⁾.

They were interrupted during the Second World War, when most of the Brazilian physicists were engaged in the researches concerning the production of sonars able to detect German U-boats.

In March 1942, Brazil joined the nations fighting against Italy and Occhialini was recalled back to Italy because of the breaking of diplomatic relations. Many Italian professors came back to Italy but the English government refused to permit Occhialini the free passage. During this period, Occhialini escaped to the Agulhas Negras (or Itatiaya) mountains, between São Paulo and Rio de Janeiro, where he worked as an alpine guide. When Italy signed the armistice, on September 10th, 1943, he offered to fight with the United Nations, but the invasion of France delayed his arrival to England till the beginning of 1945.

In the meanwhile, Occhialini was again in touch with Brazilian researchers and worked in the Biophysics laboratory, directed by Carlos Chagas Jr.⁽³³⁾. There, he met a French-Canadian researcher, Charles Leblond, a pioneer of cell biology who performed physiology experiments with brain tissues that had absorbed radioactive compounds. The track left by the radioactive material, where it had been absorbed, was the phenomenon that attracted Occhialini’s attention. It suggested to him a new way to study elementary particles. By the use of plates with thick emulsions, it could be possible to fix the tracks of the particles which, while penetrating into the plate, excited the grains of the exposed emulsion, and to study their physical properties.

⁽²⁸⁾ On the early history of physical sciences in Brazil, see [42-46].

⁽²⁹⁾ On Marcelo Damy de Souza Santos, see his interview [47].

⁽³⁰⁾ On Mario Schönberg, see his interviews [48, 49].

⁽³¹⁾ On Cesar Lattes, see [50-52].

⁽³²⁾ Ref. [9], quotation on page 323.

⁽³³⁾ On the Biophysics Laboratory in Rio de Janeiro, see [53].

5. – The Bristol period

Blackett, at that time at the Admiralty, called Occhialini from Brazil to work with the allied project to build the atomic bomb. As a first step, Blackett asked the Foreign Office, in February 1943, to look for information on Occhialini in Brazil, suggesting to get in touch with the British embassy in Brazil, and make clear the anti-fascist position of Occhialini himself.

The answer from the Embassy to the Foreign Office contained useful information on Occhialini's status and his willingness to collaborate with the Allied forces. The suggestion to let Occhialini come to England to work in the atomic research was kept secret, mostly because of the after-effects on his family in Italy.

Occhialini's position in 1944 was not at all easy. Brazilian scientists tried to make him stay in Brazil to go on with his researches in cosmic-rays physics. Similar proposals came from the Ohio University with a Rockefeller fellowship, but Occhialini preferred to wait for the safe-conduct to leave Brazil to England. He was but not admitted to join war research because of his nationality. Occhialini landed unconditionally at Cardiff on January 23rd, 1945, and spent a short time at the Department of Scientific and Industrial Research (DSIR) in London with Sir Edward Appleton, while waiting for an engagement in the Allied war efforts. This limbo state happened but to have no solution at all. After having spent a few days at the Research Laboratories of the General Electric Company in Wembley, because of the refusal of his employment from the Aliens War Service Department, Occhialini accepted the invitation of A. Tyndall from the Wills Laboratory⁽³⁴⁾. He arrived in Bristol in September 1945 where he had been exempted by the Secretary of State from the Special Restrictions applicable to enemy aliens under the Aliens Order.

In the H. H. Wills Laboratories, they made use of nuclear emulsions⁽³⁵⁾, experimentally produced by Ilford and Kodak, and they exposed them at high altitude to detect the disintegration due to cosmic-rays collisions against the atmosphere.

“With his stubbornness and his intuition, Beppo started again from basics, working with the technicians from the Ilford photographic laboratories. He transformed their emulsion plates into a formidable instrument for detecting and studying elementary particles, about which almost everything remained to be understood. Occhialini immediately thought of cosmic rays, his first love in Arcetri when he worked with Rossi, as the ideal source of high-energy particles”⁽³⁶⁾.

⁽³⁴⁾ On the history of the Department of Physics in Bristol, see [54]. On the history of the Cosmic Ray School of Physics in Bristol, see [55].

⁽³⁵⁾ On the history of the use of nuclear emulsions in physical researches, see the chapter “*Nuclear Emulsions: The Anxiety of the Experimenter*” in [28], pp. 143-238.

⁽³⁶⁾ Ref. [4], quotation on page 336.

They expected to find among the disintegration products the π -meson, a particle whose existence had been predicted by Hideki Yukawa, in 1935, as the particle responsible for the strong nuclear interaction⁽³⁷⁾. Cecil Frank Powell⁽³⁸⁾ had been interested in the technique of nuclear emulsions from 1938 on, following a suggestion of Walter Heitler, and, after the end of WW II, was also a member of the emulsion panel chaired by Joseph Rotblat.

By referring to precedent works of other physicists, Powell, Occhialini, Livesey and Chilton wrote in 1946 a perfect description of the technique of photographic emulsions in nuclear and particle physics [62]. The tracks of the particles recorded in photographic emulsions were studied in order to obtain some physical quantities. By using some empirical relations, physicists were able to gain useful information on the energy and the velocity of the particle, leading thus to a determination of its mass.

In 1946, the main disadvantage of the photographic emulsions lay in the wide gaps in the succession of the grains forming a track left by a particle, preventing a correct determination of the true length of the tracks. It was thus very important to be able to produce new kinds of photographic emulsions, containing higher quantities of silver bromide.

The Bristol group with Cecil Waller of the Ilford invented a new kind of borax-loaded plates. It was an impressive step in the technique of nuclear emulsions. Lattes, at the University of São Paulo, had built in the meanwhile a cloud chamber to study slow mesons. After having received from Occhialini some photographs of protons and α -particles tracks, Lattes asked to join Powell's team to study the new Ilford emulsions. In winter 1946, thanks to Occhialini and Powell's help, but also to the lack of experimental physicists in minor British universities, Lattes obtained a scholarship and could really go to England.

During one of his holidays as a speleologist in the caves on the Pyrénées, Occhialini exposed about two dozens of C2 Ilford emulsions on the Pic-du-Midi. It was Occhialini, on the very night of his coming back to Bristol, who developed the emulsions after having recovered them from the one-month exposure. The difference between normal plates and borax-loaded ones was evident at once. Since the borax slowed down the image fading, the loaded emulsions were able to keep the record of a greater number of tracks, whereas in the normal plates the detection power considerably decreased after about a week.

“When they were recovered and developed in Bristol it was immediately apparent that a whole new world had been revealed. The track of a slow proton was so packed with developed grains that it appeared almost like a solid rod of silver, and the tiny volume of emulsion appeared under the microscope to be crowded with disintegrations produced by fast cosmic ray particles with much greater energies than any which could be generated artificially at the time. It was as if, suddenly, we had broken into a walled orchard, where protected

⁽³⁷⁾ On the discovery of the π -meson and the contemporary discovery of the V-particles, see the recollection of reminiscences and papers in [56]. See also [57, 58]. On the researches concerning particle physics in the '30s and '40s, see also the recollections in [59].

⁽³⁸⁾ Biographical information on Powell is in [60] and [61].

trees had flourished and all kinds of exotic fruits had ripened in great profusion"⁽³⁹⁾.

Powell decided to concentrate the whole lab staff in the study of low energy normal events. Marietta Kurz (a member of "Cecil's beauty chorus") found the track of a meson (a π -meson) till its stopping point, and a second track, beginning from the end of the first one, of a second meson (actually: a muon), stopping in the same emulsion too. Within a few days, Irene Roberts found a similar case. These results were published on *Nature*, in May [63].

"The excitement of the discovery of the π -meson was intense and the occasion was such that Occhialini of undoubted rationalistic outlook could only express his feelings by going into the R.C. Cathedral to light a candle!"⁽⁴⁰⁾.

To confirm the discovery of the π -meson made on the Pic-du-Midi, the Bristol team decided that they had to get quickly other recordings of similar events to grant to their work the necessary scientific validity. Lattes found the indication of a meteorological station at high altitude on the Bolivian Andes, Chacaltaya station, with an extremely advantageous geographical position granting a cosmic-rays flux 100,000 times greater than that on the Pic-du-Midi. The British government decided to finance Lattes' mission, in the conviction that the development of nuclear physics would have had political-military advantages. The first plate was developed after one month in La Paz, and Lattes found a complete track of the "double meson". All the other emulsions were developed and studied in Bristol and gave as a result about thirty tracks of "double mesons". The discovery doubtless represented an important step in the history of the comprehension of the structure of matter, and the results obtained were convincing in considering that the observed process was a fundamental one. The first meson (the π -meson) was thus identified with Yukawa's meson, and the second meson with Anderson's meson (the muon). It was furthermore postulated the typical decay reaction of the π -meson that, to grant momentum conservation, included a neutral small mass particle (later identified with the muonic neutrino).

The positive result of these researches was of course the outcome of the different contribute of various scientists and institutions. The official communication, apart from the published articles, was at the Conference on Cosmic Rays and Nuclear Physics held at the Institute of Advanced Studies in Dublin (July 5th-12th, 1947). Powell, spokesman of the Bristol team, exposed their conclusions strengthening their scientific character, but the scepticism of a valuable part of physicists circles, more engaged in the analysis of Conversi-Pancini-Piccioni's experiment, required further open discussions of the argument and close examinations of the calculations to confirm their validity. The University of Bristol officially recognised Occhialini's important contribution to the researches he

⁽³⁹⁾ Ref. [60], quotation on page 36.

⁽⁴⁰⁾ Ref. [54], quotation on page 38.

made in cosmic-rays physics and the development of the related technology with the award of the doctorate *honoris causa*.

The rapid development of the nuclear emulsions technique was strictly connected with a parallel development in the microscopic technique. Nuclear emulsions exposed on the Pic-du-Midi by the Bristol group, which collaborated with Cooke, Troughton and Simms, were studied by means of a reflecting microscope⁽⁴¹⁾. By the examination of the nuclear emulsions, Occhialini and Bates, using a reflecting microscope, reversed the normal microscopic procedure, inverted the plate, and observed through the backing glass plate. They were thus able to observe stars and heavy fragments, not visible by the ordinary technique [65].

The thickness of the emulsions always set a limit to the number of useful observed events. The aim of the researches in the technique of nuclear emulsions was to produce emulsions with a thickness comparable to the length of the tracks left by the particles to be recorded. A growing thickness of the emulsions led but in a first time to the impossibility to process the whole photographic material in the same way, and then to the impossibility to process in any way even an increasing part of the photographic material itself. Conventional nuclear emulsions were about 200 μm thick and could be processed in a non-uniform way: the surface of the emulsion (the “top”) was more processed than the layer against the supporting material (the “bottom”). Grain density was thus observed to be different in tracks left by particles with the same ionising power, as a function of the depth in the emulsion.

Occhialini, with his future wife Constance Dilworth and Ron Payne, were able to invent the “Temperature Development” processing method ([66-68]) which led them to the production of a nearly uniform development of 300 μm thick emulsions, with a variation in grain density of less than 10% between the “top” and the “bottom”. New nuclear emulsions and their processing were thus studied by Occhialini with the help of Waller of Ilford, and of Berg, Berriman, Herz and Stevens of Kodak. The problem to be solved was the permeation of the whole emulsion with the developer before the developer could act, that is the separation of the processes of permeation and development by stopping the permeation before the development. Occhialini and his group preferred to separate the two processes by acting on the soaking temperature and making use of the lower temperature coefficient of the rate of permeation with respect to the one of the rate of development.

The role played by Occhialini in the discovery of the π -meson was as important as that one he played in Cambridge⁽⁴²⁾. In this case too, Occhialini did not win the 1950

⁽⁴¹⁾ On the reflecting microscope, see [64].

⁽⁴²⁾ “Here we had two of the greatest physicists of their time, experimenters who were destined to become Nobel Prize winners and men who had the ability to inspire the countless researchers who were attracted to work with them. It would be hard to find British cosmic-rayites who had not worked in either of their laboratories, and indeed many of the European (not forgetting Eastern European) cosmic-ray physicists had received part at least of their training in Manchester or Bristol.” Ref. [69], quotation on page 27.

Nobel Prize in Physics awarded to Powell alone for his development of the photographic method of studying nuclear processes and his discoveries regarding mesons made with this method. The denied acknowledgement was less self-evident than in Blackett's case. Powell never quoted Occhialini in his Nobel Lecture [70]. Hypothetical political motivations have been advanced to explain Occhialini's exclusion to the prize:

"Blackett and Powell separately won the Nobel Prize for their work on elementary particles. Both awards were made in difficult, Cold War years, and Occhialini had never made a secret of his political ideas"⁽⁴³⁾.

Occhialini's father, in a letter to his son, on November 11th, 1950, wrote his very hard comment:

"I saw a telegram to be sent to Powell signed by the Istituto di Fisica and I stopped it. In that signature, mine own was implicit; they did not ask me for it, and I would not have signed it because that would have meant a hypocrisy. Your only signature is worth in a general case; you can allow it or not depending on what you feel. Sending a telegram that implies your signature and not ours is, according to me, a double hypocrisy, that would lower you in this moment. My suggestion is to send your own telegram, as generous as your not repayed work. Only so you will pass for a sacrificed in front of an imposer, and you will be over the miseries"⁽⁴⁴⁾.

These very hard words are a father's word, but it is a matter of fact that Occhialini suffered quite a lot from the exclusion of him from the Nobel Prize awarded to Powell. He maybe suffered more from what seemed to be the lack of an official recognition of his own role by Powell himself.

In the same year when Powell was awarded the Nobel Prize, an important award of Occhialini's contribution to cosmic-rays physics and the relative technology came with the Charles Vernon Boys Prize of the Physical Society of London. Powell showed a particular happiness for this award to Occhialini. This fact might contribute to see from a different point of view Powell's accusing silences on the role played by Occhialini in his researches in Bristol.

"We were very glad to hear that the Physical Society of London has awarded you the Vernon Boys Prize for 1950. We were particularly glad that this award was made by an English Society and therefore represents some small act of appreciation in recognition of the contributions you have made to physics"

⁽⁴³⁾ Ref. [4], quotation on page 340.

⁽⁴⁴⁾ Letter from Augusto Occhialini (Genoa) to Giuseppe Occhialini (Brussels) (1950). Occhialini Papers 5, 1, 2.

and in particular to English physics. I hope it will remind you also of the happy and exciting time we spent in this laboratory from 1945-48”⁽⁴⁵⁾.

Powell, not only brought pressure to bear on the Physical Society, but suggested to award Occhialini the Nobel Prize. Actually, he wrote to Wolfgang Pauli in order to convince him to put Occhialini’s name forward to the Nobel Committee in Physics. Pauli, who was a great friend of Occhialini and had admired his scientific work since the years the latter spent in Cambridge with Blackett, was happily willing to support his nomination.

6. – A new laboratory in Brussels and his return to Italy

Occhialini left Powell’s group in Bristol and went to Brussels to work at the Centre de Physique Nucléaire of the Université Libre de Bruxelles. He worked continuously in Brussels from 1948 to 1950. Thereafter, he was appointed professor at the University of Genoa from 1950 to 1952, and at the University of Milan from 1952 on. While teaching and making research in Italy⁽⁴⁶⁾, Occhialini continued to collaborate with the Centre de Physique Nucléaire where he spent a lot of time every year until his sabbatical year at the MIT in 1959. The Brussels and Genoa/Milan groups in the ’50s can be considered a single one group of research under the scientific leadership of Occhialini.

Occhialini was called to Brussels, together with Constance Dilworth⁽⁴⁷⁾, by Max Cosyns, a friend of his speleological adventures, to give birth to a new laboratory where they could study nuclear emulsions. They also printed a new journal, the *“Bulletin du Centre de Physique Nucléaire de l’Université Libre de Bruxelles”*. Yves Goldschmidt-Clermont, who had worked in Bristol with Occhialini, joined them too.

In the Brussels laboratory they went on with the researches on the new NT2 and NT4 Kodak plates by Berriman, and G5 Ilford plates by Waller. The emulsions group, under the scientific leadership of Occhialini, became soon one of the most important groups of research on nuclear emulsions, after the one in Bristol.

While in Brussels, and then in Genoa and Milan, Occhialini played a fundamental role in the development of several groups of research in nuclear emulsions in Italy and in the birth of a European network of groups devoted to the studies of cosmic radiation. Brussels was the school where to learn the emulsion technique for Bonetti and Scarsi from Genoa, Merlin from Padua, Cortini from Rome, Levi-Setti from Pavia. The new way of life in Brussels, and then in Genoa and Milan, was mainly like the old way of life in Bristol, full of hard work on cosmic-ray physics and the development of the technique of nuclear emulsions.

A very sad fact shattered the laboratory in Brussels in 1952: the so-called winch affair. On August 14th, 1952, during a speleological exploration of the cave of Pierre-

⁽⁴⁵⁾ Letter from Cecil Powell (Bristol) to Giuseppe Occhialini (Brussels), May 12th, 1950. Powell Papers. Bristol University Special Collections DM 1947/E.303.

⁽⁴⁶⁾ On the state of physical researches in Italy soon after WW II, see [71].

⁽⁴⁷⁾ On biographical information on Constance Charlotte Dilworth, see [72, 73].

Saint-Martin, Marcel Loubens, a member of a speleological group formed by Occhialini, Cosyns, Labeyrie, Casteret and Tazieff, died while climbing to the earth's surface, since the hook connecting his cable to the winch broke and he fell for about forty meters and crashed on the rocks below. He was in his last agony two days long, assisted by Occhialini, Labeyrie and Tazieff. Occhialini was able to tie a particular kind of knot used in sailing to join the cable with the winch. The speleologists outside the cave could so pick them up very slowly. Unfortunately Loubens' body was salvaged only two years later.

The very deep crisis in the Brussels group, due to the irremediable dissidence between Occhialini and Cosyns, lasted through 1953. Baudoux was appointed new director of the group in May 1952. The winch affair caused but a tremendous internal division. Occhialini's scientific collaboration with Cosyns came so to its end. Occhialini went on to work with the Brussels emulsion group, while Cosyns had to leave Belgium and went to Paris. The Brussels group was able to get over the worst, but

“the internal division and later loss of staff seriously compromised the scientific activity and led to the loss of that preeminence which the group had till then maintained”⁽⁴⁸⁾.

Occhialini succeeded in making it again a part of the European network of laboratories that were flying stacks of plates on balloons at high altitude. Occhialini asked Powell to let the Brussels group join the collaboration organising flights of nuclear emulsions.

Occhialini was Professor of Physics at the Institute of Physics of the University of Genoa from 1949 to 1951. He had namely won a competition for the chair of physics in Cagliari, but he eventually had the one left by his father because of the latter's age. With the help of Alberto Bonetti, he was able to give birth to a small group of research in Genoa too.

During 1952, Occhialini was also at the Brazilian Centre of Physical Researches in Rio de Janeiro. His travel was supported by the UNESCO and had the aim to help Lattes and Camerini in the organisation of the group of research in Rio de Janeiro and its researches made in the laboratory on Chacaltaya. The new laboratory in South America should have worked following the same lines used by the European network: the use of nuclear emulsions as detecting device of cosmic rays, exposition of plates at high altitudes, flights of stacks of plates on balloons.

Occhialini arrived in Rio de Janeiro on February 8th, 1952, and was pleased to find the Brazilian Centre of Physical Researches better than any optimistic prevision. The moral among the young researcher was very high, the financial position was quite good, and the technical positions were also good due to the establishment of a workshop with eight technicians, a glass laboratory and a vacuum pump able to mass produce very good GM counters. During his stay for a period of about six months, Occhialini was able, with the precious help of Lattes and Camerini, to install the electronic department which had the task to design and produce all the coincidence apparatuses to be sent to Chacaltaya.

⁽⁴⁸⁾ Ref. [74], quotation on page 737.

In the Chacaltaya laboratory, a controlled Wilson chamber was built and tested, and a small group of people started to work on nuclear emulsion.

In the short time spent working both in Genoa and Brussels, Occhialini and his team developed further methods to obtain better images with nuclear emulsions, the wire method of loading emulsions [75] and the use of cylindrically-shaped emulsions [76]. Emulsions produced with the Wire Method gave interesting results in the study of cosmic-rays phenomena, with a previous analysis of the possible distortions of the tracks introduced by the wires themselves.

During the development of nuclear emulsions, one of the difficulties met with was the fact that the emulsions became more and more opaque. One of the main sources of this opacity of the emulsions was the deposit of colloidal silver in the gelatine. This problem was particularly annoying with thick emulsions, from 100 μm on, affecting in such a way the new thick emulsions used with the “temperature development” technique. Occhialini, Dilworth and Eric Samuel found a simple method to avoid the deposit of colloidal silver [77].

Another use of variations of temperature was made by Occhialini’s group to clear nuclear emulsions of the tracks left by cosmic radiation (or other radiation sources) in them before their use. Improvements in the production of more sensitive nuclear emulsions and in the technique concerning their development and examination permitted the study of relativistic phenomena of particle physics and the observations of the β -decay of the μ -meson, not to mention the possibility of new analysis on already known phenomena. In particular, further analysis on disintegration stars put in evidence the existence of kinds of particles not yet studied, such as highly charged particles.

7. – An actor in the long search for strange particles

Occhialini was Professor of Physics at the Institute of Physics of the University of Milan⁽⁴⁹⁾ from 1951 on, where he moved after his father’s death, sharing his time with the emulsions laboratory in Brussels too. The coming of Occhialini to Milan was supported by Giovanni Polvani and Piero Caldirola, and must be seen within the frame of the birth of the INFN (National Institute of Nuclear Physics).

The scientific production in the early ’50s concerned both the study on the development and use of nuclear emulsions, and the studies on new kinds of particles.

“The effort of the organizing and scientific direction of the group of research of Brussels and Genoa and then of Milan found an answer in the scientific production which, in part, inserted itself on an international level: mesons and pions capture in the elements of the nuclear emulsion; measurements of very high energy particles with the method of multiple scattering and with the curvature in emulsion; individuation of the decay schemes of the positive hyperon, and contributions to the study of the hyperfragments”⁽⁵⁰⁾.

⁽⁴⁹⁾ On the history of scientific and physical studies in Milan, see [78-80].

⁽⁵⁰⁾ Ref. [9], quotation on page 324.

Maybe the most important characteristic of Occhialini's studies on cosmic rays was the participation of his groups to international cooperations organising flights of balloons carrying stacks of nuclear emulsion plates at high altitude. The prelude to the European collaborations was, in 1947-1948, the launch of balloons to expose nuclear plates at cosmic radiations at 30 km altitude. This launch was organised by Powell's group in Bristol with the collaboration of the University of Padua. The further development of the Bristol activities with the Italian groups saw the engagement of the INFN Sections of Milan, Padua, and Rome. The first great expedition, involving thirteen groups, was, in 1952, the launch from the Italian bases of Naples and Cagliari, and the following recovery of stacks of plates (with a glass support) after the landing on sea of the balloons. Besides the importance of the tracks recorded in the plates themselves, the study of the flight of the balloons permitted to get useful information on the velocity and direction of wind at high altitude.

In June-July 1953, eighteen groups of research launched twenty-five balloons from the Elmas airport in Sardinia, and exposed more than one thousand stripped emulsions, corresponding to a volume of 9.3 dm^3 , seven hours long at an altitude between 25 and 30 km. All emulsions were processed in Bristol, Padua, and Rome; in October 1953, a meeting was held in Bern in order to distribute the processed plates among the different groups. The first results were discussed in an international congress held in Padua in April 1954. Further results were the subject of the second course of the International School of Varenna, in summer 1954.

The third significant experience was the launch of the so-called G-Stack from Novi Ligure, in October 1954. The G-Stack was a single stack of emulsions with a volume of about 15 dm^3 . The choice to launch a single "giant" stack came from the aim to study in the most advantageous way part of the recorded tracks in their whole length in order to obtain precise values of their energy and decay modes. The most important result of the G-Stack was the determination of the equality of the values of the masses of the then supposed different K-mesons, and the statement that the different decay modes were alternative decay processes of a same particle. The solution of the related θ - τ puzzle was, in such a way, a first step to the discovery of the non-conservation of parity in weak interactions.

After the G-Stack, the results on elementary particles obtained by means of accelerating machines soon outnumbered the ones found in cosmic rays⁽⁵¹⁾. In the second half of the '50s, Occhialini thus went on with his studies on elementary particles by exposing the nuclear emulsion plates to beams of particles produced by accelerating machines at the CERN or elsewhere.

⁽⁵¹⁾ The G-Stack flight itself had been prepared in a very short time in order to precede the results that would have been with the accelerating machines soon after. *"Leprince Ringuet expressed, with an elegant metaphor, the attitude to this threat. Rather than to 'retire to the country and wait six months for Brookhaven to give the answers', the community would continue to work in the field in the hope that the higher energy components of the cosmic rays would still reserve some surprises. It was in this sort of climate that the G-Stack collaboration was born. It was a last minute attempt to beat the machines."* Ref. [74], quotation on page 739.

The conferences held by cosmic-rays physicists in the early '50s were of the utmost importance in the history of physics. The 1953 International Cosmic Rays Conference held in Bagnères-de-Bigorre was an epoch-making event where “order emerges from chaos”⁽⁵²⁾. The discussion on the plethora of K-meson decay modes, thanks to “Rossi argument”⁽⁵³⁾, led to the conclusion that many different events actually corresponded to different decay modes of one kind of particle.

The results of the plates impressed in the 1953 launches from Sardinia discussed in April 1954 at the above-mentioned “International Congress on Heavy Unstable Particles and High Energy Events in Cosmic Rays held in Padua”. The main subject of the Padua congress was a problem that arose from the analysis of the tracks left by the τ -particle, or $K\pi_3$, that is a K-meson decaying into three coplanar charged π -mesons. The problem was known as the θ - τ puzzle. Another problem discussed at the Padua Congress was the new $K\mu_2$ -decay mode (the “Camus”), suggested by the French group of the École Polytechnique of Paris. To solve both the θ - τ puzzle and the $K\mu_2$ problem, the groups of Bristol, Milan, and Padua decided to undertake the G-Stack flight.

The results of the G-Stack experiment⁽⁵⁴⁾ were the main subject of the “International Conference on Elementary Particles” held in Pisa in 1955. Seven different decay modes (τ , τ' , $K\pi_2$, $K\pi_3$, $K\mu_2$, $K\mu_3$, Ke_3) were definitely assigned to only one particle, the K^+ -meson. The θ^0 -decay mode corresponded to the K^0 -meson. The θ - τ puzzle continued to be unsolved since the spin-parity of the τ -decay was confirmed to be 0^- . Strange particles led to the definition by Murray Gell-Mann of the strangeness S , a new quantum number. The θ - τ puzzle was solved in 1957, when cosmic rays were no more the source of K-mesons, but accelerating machines, the Berkeley Bevatron in a first time, were used instead.

Researches by means of nuclear emulsions had an improvement in Milan thanks to the application of an already known principle to microscope technique. The afforded problem concerned the possibility to make small relative shifts between the observed object and the ocular micrometer. The solution consisted in the measurement of the rotation around axis perpendicular to the optical axis of one or more leaves with plane and parallel faces, introduced along the optical canal of the microscope. This method was already applied in optical devices, such as the Klausen’s blade micrometer, and permitted to shift the image of the object in the plane of the micrometer, and to measure its shift. A device of this kind, a MS2 Koristka microscope, was made for Occhialini and his group by Dr. Cantù of Koristka.

A second important European collaboration followed a few time after the G-Stack Collaboration: the K^- -Collaboration. The new aim was a thorough study of the interactions and decay of K^- -mesons. For the publication of the three classical papers on the K^- -

⁽⁵²⁾ See the paragraph on Bagnères-de-Bigorre in [81].

⁽⁵³⁾ “*I would like to take the point of view that two particles are equal until they are proven different.*” Ref. [81], quotation on page 8.

⁽⁵⁴⁾ For the results of the G-Stack experiment see [82,83].

Collaboration ([84-86]), Occhialini's group in Milan and Brussels was more and more involved in the studies on K^- -mesons, both from cosmic radiation and artificially produced. During this period, Occhialini stopped to sign his contribution to published articles.

8. – The astrophysical period

We can consider the second half of the '50s as a “transition period” in Occhialini's scientific career. The K^- -Collaboration was not only a renounce to cosmic rays as the primary source of elementary particles, but was also the last great experiment made with stacks of nuclear emulsions. Even if Occhialini played an important role both in the coordination of the K^- -Collaboration and in the further development of new kinds of microscopes to scan nuclear emulsions, the importance of this last technique was decreasing.

“In this transition period Beppo Occhialini continues to be a drawing leader; he coordinates the collaboration among the groups on a European scale, he continues the experimentation and the technical development on new kinds of high precision microscopes, he is engaged at close quarters in the analysis of the events due to the capture and interaction of the K^- . In this connection I remember, in Milan, the classification of the “hyperfragments” widespread among the microscopists, following the increasing difficulty of interpretation: “Normal”, “G.O.K.” (God only Knows), “D.O.K.” (Devil only Knows) and “B.O.K” (Beppo only Knows)”⁽⁵⁵⁾.

The solution was offered by the launch of the Sputnik in September 1957. Bruno Rossi, at the Massachusetts Institute of Technology (MIT), began at once a series of researches in space physics on the interplanetary plasma and cosmic γ -rays. Occhialini, accompanied by Constance Dilworth, decided to spend a sabbatical year at the MIT as Visiting Professor to rise, one back to Milan, a group of cosmic and space physics. The Milan group of nuclear emulsions was converted to the new space adventure by means of spark chambers on balloon and satellite⁽⁵⁶⁾, in a strict collaboration with the French group in Saclay, while the cloud and bubble chamber group was sent to work at the CERN.

“The University of Milan group, led by Occhialini, was increasing in both the number of its staff and the importance of its research projects. [...] It was an exciting time; everything was still to be discovered. Were there, for instance, neutrons reaching the Earth from the Sun and influencing our atmosphere? Although most of the primary cosmic radiation had been found to be made up of protons, were there also electrons among them? What about positrons?”

⁽⁵⁵⁾ Ref. [5], quotation on pages 614-615.

⁽⁵⁶⁾ “Beppo is certainly not a home cooking man: he discerns the potentiality of the “spark chamber” as a detector fit for balloons and satellites and begins a collaboration with Saclay which masters the spark chamber technology and that is also bring to pass a partial change, from particles to space.” Quotation from [5] on page 616.

And what of the never forgotten γ -rays (of which Millikan wrote in the 1920s) —were there any at all, coming from the Sun, or from our Galaxy, or from deep space, like the X-rays that Rossi and Giacconi had just discovered? The Milan group attacked all these problems, with Beppo’s enthusiasm and energy acting as a motor”⁽⁵⁷⁾.

The Milan group was one of the main characters of Italian space physics, together with the ones in Bologna and Rome. Occhialini was, with Castagnoli and Puppi, the actor of the passage of the Italian groups of cosmic physics from the INFN to the CNR in 1966-67 with the constitution of the Italian Group of Cosmic Physics (GIFCO)⁽⁵⁸⁾. The Milan group evolved into the Laboratory of Cosmic Physics and Related Technologies (IFCTR), led by Occhialini till 1974, with two dozen researchers and technicians.

Occhialini was one of the founding fathers of the European space physics⁽⁵⁹⁾, as well as Pierre Auger, Robert Boyd, Marcel Golay, Bengt Hultqvist, Reimar Lüst, Harrie Massey, Bernard Peters, Pol Swings, Hendrik van de Hulst. After the first steps of the European Preparatory Committee for Space Research (COPERS — Comité Préparatoire Européen pour la Recherche Spatiale) in Paris, Occhialini was one among the most important members of the Council and of the Scientific and Technical Committee of the new-founded European Space Research Organisation (ESRO). He was Chairman of the COS-Group (Advice Committee for Cosmic Rays Physics) and member of the restricted Launching Program Advisory Committee (LPAC) devoted to choose and define European space missions that were organised following the “Street-car” Principle: each mission was a cluster of experiments proposed by the various scientific communities.

Among the different space missions organised by Occhialini too, we can remember the HEOS A1, the TD1, the HEOS A2, and the COS-B.

The HEOS A1 mini-satellite was launched from the Eastern Test Range, Florida, on a Thos Delta DSV3-E launcher, on December 5th, 1968. HEOS A1 penetrated interplanetary space to about 33 earth radii, and was re-entered the atmosphere on October 28th, 1975. There were devices to make several experiments on-board: electric fields, magnetic fields, high-energy cosmic rays anisotropy, low-energy solar protons, solar wind, flux and spectrum of cosmic rays, and primary cosmic-ray electrons. Milan and Saclay had developed the instrument that measured the primary cosmic-ray electrons.

⁽⁵⁷⁾ Ref. [4], quotation on page 338.

⁽⁵⁸⁾ The GIFCO was born with the constitution of four laboratories: the ITESRE in Bologna, the IFSI in Frascati, the IFCTR in Milan, and the CosmoGeofisica in Turin, with two divisions in Florence and Palermo. The Palermo division will be the fifth laboratory in 1981, the IFCAI. After the event, this well-intended operation might have suffered of some deficiencies in the coordination of the groups: “*The CNR responded to the initiative, with the agility of an asthmatic pachyderm, by mistaking (I’m quoting Occhialini’s words) the timing typical of archaeological excavations in Pompei, for that necessary to the reaction to a fast countdown for a rocket on the launching pad.*” Quotation from [5] on page 617.

⁽⁵⁹⁾ On the history of space physics in Europe, see [87, 88].

The TD 1 mini-satellite was launched from the Western Test Range, California, on a Delta-N launcher, on March 12th, 1972. TD 1 had not been active since May 4th, 1974, and decayed on January 9th, 1980. Seven experiments were carried on-board: multicolour celestial scanning in the UV and IR, UV stellar spectrometry, spectrometry of primary charged particles, spectrometry of celestial X-rays, solar γ -rays, celestial γ -rays⁽⁶⁰⁾. Milan developed the instrument for solar γ -rays, while that for celestial γ -rays had been developed by Milan, Munich (“Monaco” in Italian) and Saclay (“MIMOSA”, the name of a flower). As for the studies on γ -rays,

“The TD-1 experiment was not successful due to high background and the failure of the vidicon system. Its impact was two-fold: it was the beginning of the later Caravane-COS-B collaboration with Leiden and ESTEC as additional partners, and it cleared the way for the acceptance of the wire spark chamber in Europe”⁽⁶¹⁾.

The HEOS A2 mini-satellite was launched from the Western Test Range, California, on a Thor Delta launcher, on January 31st, 1972. It was the first vehicle to penetrate into the area of the neutral point at the border of the Earth’s magnetic field and that of interplanetary space. Seven experiments were carried on-board: magnetic field measurements, plasma measurement, solar v.l.f. observation, particle-counter telescope, high-energy electrons, solar wind measurement, and micrometeorite detector.

The COS-B satellite was launched on a Delta 2913 launcher, on August 8th, 1975. COS-B failed on April 26th, 1981. This satellite was the product of the Caravane Collaboration, formed by the Laboratory for Space Research (Leiden), the CNR Institute of Cosmic Physics and Informatics (Palermo), the CNR Laboratory of Cosmic Physics and Related Technologies (Milan), the Max-Planck Institute for Extraterrestrial Physics (Garching), the CEN Service of Physical Electronics (Saclay), the ESRO Scientific Laboratory (ESLAB) (Nordwijk). COS-B permitted to draw the first detailed γ -map of the Galaxy and to have a first catalogue of discrete sources in the range of a few 100 MeV.

“With COS-B, as well as other ESRO instruments, Beppo showed how he could work constructively on a supernational scale while maintaining his unique Italian character. COS-B was launched in 1975 and soon became the first great European success in high-energy astrophysics”⁽⁶²⁾.

In 1975, ESRO and ELDO, an industrial organisation projecting European launchers, merged giving birth to the European Space Agency (ESA). After the launch of COS-B, the new, more bureaucratic, way to manage the ESA was the beginning of the end of Occhialini’s scientific activity. COS-B was Occhialini’s last scientific success, even if he did not play a central role throughout the seven years the mission lasted. Besides

⁽⁶⁰⁾ On the history of gamma-ray astronomy, see [89].

⁽⁶¹⁾ Ref. [89], quotation on page 45.

⁽⁶²⁾ Ref. [4], quotation on page 339.

the valuable scientific results obtained with COS-B, we have to note that the project permitted the Milan group to learn how to build second-generation space instruments.

During the '80s, Occhialini spent more and more time in Marcialla, a hamlet of Certaldo, Tuscany, not far from Arcetri. The choice between two proposals of an X-ray astronomy satellite made Occhialini emerge from his retreat. He supported, in a committee with Bruno Rossi⁽⁶³⁾ too, the SAX satellite, that was then renamed Beppo-SAX after him.

In 1979 Occhialini and Uhlenbeck were awarded together the prestigious Wolf Prize, the last of a consistent series of awards (1934 Sella Prize, 1935 Vallauri Prize, 1949 Einaudi Prize, 1951 Charles Vernon Boys Prize, 1955 Feltrinelli Prize). He was doctor *honoris causa* of the universities of Brussels (1949) and Bristol (1959), member of several scientific academies in Italy and abroad, and foreign fellow of the Royal Society.

Occhialini, Bruno Rossi, and Bruno Pontecorvo died in the same period. Occhialini, in particular, died in Paris on December 30th, 1993. Italy lost three among the main characters of its scientific history in the twentieth century. It was but a loss for the whole scientific world too.

9. – Some personal notes

Occhialini was an eminent scientist. He made research with great attention and enthusiasm, a feeling that he transmitted to his students and collaborators. He was considered to be a legendary figure because of his discoveries and the vicissitudes of his life. Occhialini was surely an experimental physicist and not a theoretician according to the usual meaning of these categories. He had a deep sense of nature's laws and was more interested in how nature actually works than in the mathematical laws describing natural phenomena. Nevertheless, he was in wonderful relation with some theoretical physicists, such as Wolfgang Pauli.

Many physicists in Europe and America consider themselves disciples of Occhialini's school of physics. Occhialini was certainly a great teacher but not in a professional sense. When he felt sure enough to give a lesson, it was really an event. His few lectures were exemplary, more suitable for a graduation *ad honorem* than for a normal lesson to a physics class. As a teacher in the laboratory, Occhialini was instead peerless. He tried to teach, first of all, the experimental sense and the interest in the ways nature works. He wanted also to teach his enthusiasm to make a difficult research successfully and in a neat way. Another equally direct way to teach Occhialini's participation to the drafting of a scientific paper. He discussed the text with pedantry and insight so that his students could learn how to write a paper.

Occhialini was not only a great scientist but also a sincere humanist.

“Beppo had many friends because he felt well among the people. He was sincerely interested in every human being he met, of whatever condition, and

(⁶³) After Bruno Rossi was named the NASA X-Ray Explorer.

treated everybody on an equal footing. He was unselfish, kind and tender; but he reacted, even harshly, to abuses and did not forgive falsity. He did not like to speak to the public, but he talked with pleasure. He was a storyteller, and he hold his listeners spellbound when he talked about his discoveries in physics, his speleological explorations, the scientists and artists he met, the beauty of Tuscany and Umbria landscape, his adventures during his travels”⁽⁶⁴⁾.

Occhialini had a very deep cultural life. He was so endowed of a refined sensibility to be able to quote the finest passages in Shakespeare’s works by heart. His own thought was quite always original and stimulating and was the very proof of his tremendous literary and artistic culture. At the bottom of his culture there was always the human being in his multiform aspects. He was very fond of sport: he was a skilled alpinist and speleologist⁽⁶⁵⁾, a fan of soccer, and an enthusiast motorcyclist.

Both in Brussels and in Milan, Occhialini was at the heart of an informal humanistic cenacle. In the Brussels laboratory, coffee-breaks were often interrupted by conversations on subjects not concerning physics: psychoanalysis, literature, music, and so on. Occhialini was considered a model from this point of view. In Milan, he and his wife organised once a week, particularly in winter, an informal meeting with an important physicist, and hosted friends, acquaintances, and students. These evenings spent in his home are among the best memories of the people who knew him personally.

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⁽⁶⁴⁾ Ref. [8], quotation on pages 238-239.

⁽⁶⁵⁾ Occhialini was one of the discoverers of a large cave in the French Pyrénées, the Pierre St. Martin Cave, 1342 m deep, which will be for a long time the deepest cave the world over.

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The Occhialini-Dilworth Archives

ETRA OCCHIALINI

Milano, Italy

PASQUALE TUCCI

Istituto di Fisica Generale Applicata, Università degli Studi, Milano, Italy

The Occhialini-Dilworth Archives are formed essentially of two blocks of papers: the first one coming from the Physics Department of the University of Milan and the second one coming from the country house of Occhialini and Dilworth in Marcialla, a small village near Certaldo and Florence.

The papers coming from the Physics Department were in the room occupied by Giuseppe (Beppo) Occhialini until his retirement in 1983. Afterwards the room was given to Giorgio Sironi who took care of Occhialini's papers and arranged them in two large metal cupboard in order to avoid their dispersion. When Costance (Connie) Dilworth retired in 1985 also her papers were moved to Sironi's room.

In 1997 Occhialini's and Dilworth's papers, were transferred, by the good offices of Sironi and Tucci, and the assent of Dilworth, to the Section of the History of Physics of the "Istituto di Fisica Generale Applicata" of the University of Milan, where are deposited other archives of Milanese physicists: Polvani, Succi, Tagliaferri.

Less linear the course of Marcialla's papers. When Beppo Occhialini and Connie Dilworth moved from Milan to their country house in Tuscany, they brought with them everything they had collected during their life —furniture, objects, records, photographs, some paintings, and above all, books, papers and notes. As well as pieces of other lives, Beppo's parents' —Augusto and Etra Occhialini.

Some years ago (after Beppo's death) Connie had ordered —in a chronological sequence— Augusto's papers, separating the personal from the scientific ones.

Occhialini's and Dilworth's daughter Etra decided that the papers of her grandfather Augusto Occhialini could be handed over and kept in the institution where he was active. Her choice was therefore to give them to the Arcetri Astrophysical Observatory, a place that had been in her opinion meaningful for Augusto Occhialini's scientific story.

Different was the situation of Beppo's and Connie's papers. Connie was never able to put order in Beppo's and her own papers. (She once told Etra she didn't feel "ready".) When Connie died, in 2004, Etra found herself with a great quantity of documents—meaning by "documents" not only writings but also photographs, notes, objects—in an apparent (in all senses) disorder, much in the Occhialini style, that she had no capacity to select and order. Not being a physicist—a non regretted decision taken very early—Etra couldn't tell what was relevant and what not from a scientific point of view among those documents.

So she got in touch with Giorgio Sironi and Pasquale Tucci, and asked them to help her. Etra had in fact decided that the best place where to place her parents' records of a lifetime of common work were the university research groups, now splitted between the University of Milan and the University of Milano Bicocca, which continue Beppo and Connie activities. So in summer 2005, Giorgio Sironi from the University of Milano Bicocca and Pasquale Tucci from the University of Milan went to Tuscany and together with Etra gathered and transferred to Milan all there was that could become part in the future of the "Occhialini-Dilworth" Archives. By common agreement, all the collected materials were deposited at the Section of History of Physics of the "Istituto di Fisica Generale Applicata" of the University of Milan, where were deposited the papers coming from the Department of Physics. It would have been, in fact, extremely difficult to create distinct archives separating Beppo's and Connie's private life and scientific activity.

So far under the name "papers" we have included all the records, hand-written and not, left by Beppo and Connie. And that is not casual: it would be, in fact, completely undue to name them "archives" if with this name we mean a set of papers linked by some logic. All that we have until now collected does not seem to have always had an order.

Etra was aware of the amount of work it would take to really build up the archives. It is certainly difficult to give a so-called rational order to the life and work of two persons who were (and were believed to be) extremely rational, but also "peculiar" to the point of appearing "extravagant" when compared to most people. Which is what made them somewhat special. For those who have known Beppo at work it is difficult to imagine him filing his correspondence or his notes.

Agnese Mandrino gave a first order to the papers coming from the Physics Department. She tried to keep connected the papers who seemed to be a programmed archival unit, although sometimes the connection among them could seem rather weak: a little folder in which they were inserted, an envelope with some handwriting and so on. She was aware that what seemed an embryo of classification could be instead the casual result of manipulation suffered by the papers.

Anyway nucleuses of papers which could undoubtedly be considered related by some deliberately given connection, have not been disassembled, as imposed by the archival principles that Agnese Mandrino has always followed closely and meticulously. Papers

which seemed completely unlinked have been collected in archival series which follow the main steps of Occhialini's scientific life. At the end of this work the archivist has drawn up an inventory of the papers coming from the Department of physics.

Several problems have arisen from the papers coming from Marcialla: some of them are analogous to those encountered for the material coming from the Physics Department. Other problems are instead completely new. The prevailing view is to insert the papers coming from Marcialla in the archival series created for the papers coming from the Physics Department, if their content allows this kind of insertion. As for the Marcialla papers that are connected with the extant archival series, new series will be created trying to save, however, that little but precious work made by Connie to give an order to some of the papers.

The distinction between private papers of Beppo and Connie and documents dealing with their research activity is very weak and, sometimes, it simply does not exist. This is why the documents in the ordered part of the Archives are open to scholars on request and with the limitations imposed by the delicacy of the topics discussed in some documents, as correctly reported and stressed by the archivist to the responsible of the Archives. The same procedure will be followed with the papers coming from Marcialla the border between private and public being, in this case, even narrower.

The ordering of the papers coming from Marcialla is on the right track and will presumably take several months. When also this inventory will be completed it will be possible for researchers in History of Physics and in connected areas to consult the documents.

The Arcetri School of Physics

ALBERTO BONETTI

Università di Firenze, Italy

MASSIMO MAZZONI

Dipartimento di Astronomia e Scienza dello Spazio, Università di Firenze, Italy

1. – Introduction

The years between the first and the second World War (broadly from 1920 to 1940) are remarkable in Italy for the achievements attained in physical research. This was because of rather peculiar circumstances which made the Physical Institutes of the Universities of Rome and Florence the centre of advanced research and of formation of research leaders. Both groups originated through the dedication and the vision of enlightened men, Orso Mario Corbino in Rome and Antonio Garbasso in Florence, both good physicists open to the extraordinary discoveries of the years before and after the first World War, both sincere patriots willing to give their country a sound and up-to-date scientific culture.

The making and performance of the Group of Rome received wide attention and recognition in years due to the personality of Enrico Fermi in spite of the death of Corbino in 1937. The Group of Florence did not receive the same recognition, presumably because of the early death of Garbasso in 1933 and the quick dispersal of its members thereafter. Both groups were heavily hit by the racist campaign sparked off by fascism and culminated in the shameful racial laws of 1938.

In fact, the two groups were not formed in a desert. The tradition of scientific research in Italy gave remarkable results in time after Galileo, with such names as Torricelli, Spallanzani, Volta, Lagrange, Avogadro. But the Restoration after the French Revolution and the Napoleonic adventure, not only reinstated, in the first half of 19th century, the

previous situation of political fragmentation in several small traditionalist principalities, among which the State of the Holy Seat, but also revived a reactionary attitude towards the heritage of Enlightenment. Fragmentation and conformity did not affect the development of mathematical research, mainly because it did not need financial investments, but were detrimental to the development of “natural” sciences of dimension and openness comparable with contemporary European, *i.e.* English, French and German, research and capable of becoming an active part of it. Restricting the interest to Physics, the Galilean tradition was maintained almost only in the sense of a careful experimentalism with good measurements often based on original instruments, more devoted to the discovery and description of peculiar “effects” than to the assessment of theoretical developments. Achievements recognized at the European level were those attained by Leopoldo Nobili (the thermocouple), Macedonio Melloni (the infrared radiation), Ottaviano F. Mossotti (the structure of dielectrics), Giovanbattista Amici (the immersion objective), Father Angelo Secchi (stellar spectroscopy and the dawn of Astrophysics). No less important the contributions to “applied” Physics, such as the telephone of Antonio Meucci, the dynamo of Antonio Pacinotti, the rotary magnetic field of Galileo Ferraris. It is a sign of the weakness of scientific and technical consciousness, as well as of economic structure, that neither the inventors nor the dawning Italian industry took a direct advantage of such results. On the other hand, by now we are well into the second half of the 19th century, after the independence wars and the political unification (1849-1870); now it is the stage of the greatest efforts for the integration and modernisation of the country and for the recognition of Italy at an international level. Many scientists, in particular mathematicians and physicists, were very active politically, taking part also in military actions and getting involved in governmental duties. It is worthwhile mentioning Carlo Matteucci (1811-1868), active in Pisa, interested in Florence, a good physicist founder with chemist Raffaele Piria of the journal *Il Nuovo Cimento*. He was Minister of Public Education in 1862-63, just after the proclamation of the almost unified Italian Kingdom, and made a first attempt for a structural reform and modernisation of the system of Italian Universities, too many and generally too weak as a consequence of the already pointed out localism. The Physical Institutes in particular were generally understaffed and poorly equipped also because of the prevailing petty humanistic culture of the ruling class, to the detriment of a more open attitude in consonance with the rest of European culture.

2. – Physical research in Italy from the end of the 19th century to the outbreak of First World War

It was a time of exceptional flourishing of Physics all over Europe, and also in the United States, from electromagnetism, spectroscopy and statistical mechanics to radioactivity, relativity and atomic structure. At the same time mathematical research was going on along the path initiated in the 18th and 19th century, contributing heavily to the building of what was to become the modern Theoretical Physics. The Italian mathematicians were well on the front line of this path, with Enrico Betti and Luigi Bianchi

in Pisa, Giuseppe Peano in Turin, Gregorio Ricci Curbastro in Padua, Tullio Levi Civita e Vito Volterra in Rome. Not equally impressive the contributions of the contemporary Italian physicists, still bound to the experimentalist attitude inherited from their predecessors of the 19th century. While important results were obtained, among others, in electromagnetism and later in spectroscopy, some of them refused stubbornly Einsteinian relativity in spite of the position of their mathematical colleagues, and only a few caught the importance of the Rutherford-Bohr atomic model. The development of research and teaching in Physics from the end of the 19th century to the outbreak of the first world war, can be outlined through the story of four Universities, Bologna, Rome, Pisa and Florence.

2'1. Bologna. – Bologna was dominated by the personality of Augusto Righi (1850-1920), perhaps the most prominent Italian physicist before the 1st World War. He is better known for his elegant experiments in the wake of H. R. Hertz, proving the identity of electromagnetic oscillations of any frequency and light, but his ingenuity and thoroughness were present in all the subjects he treated, including the methodological approach to physical research [1]. This gave him recognition at the European level and power in improving the facilities of his Physical Institute. However he remained in doubt about relativity, characteristically lamenting the lack of a sound “laboratory” experimental basis⁽¹⁾. Perhaps only his death in 1920 prevented Righi from elaborating the successes of the new theoretical (and experimental!) Physics. His equally doubtful successor Quirino Majorana (1871-1957), a good experimentalist in the old tradition, to the end of his life made use of the good equipment of the laboratory to carry out carefully designed experiments aimed at falsifying the results of Michelson and Morley. Of course, those experiments kept confirming the constancy of the velocity of light irrespective of the frame of reference.

As a matter of fact, the Physical Institute of Bologna did not contribute to the formation of the schools of Rome and Florence (with the exception of Bruno Rossi, but this occurred through the initiative of Rita Brunetti, of Pisan and Florentine origin).

2'2. Rome. – Modern Physics in Rome begins with Pietro Blaserna (1836-1918). Born in Friuli under Austro-Hungarian administration, he completed his education in Physics at the University of Wien and then in Paris with H.-V. Regnault. Called by Carlo Matteucci in 1862 (just one year after the first step of the unification of Italy) as teacher at the Museo di Fisica e Scienza Naturale of Florence, he became in 1863 Chair Professor of Physics at the University of Palermo. In 1872 he was called by the newly established University of Rome to join E. Keller in the establishment of a “Scuola Pratica di Fisica” in recognition of his contributions to electromagnetic induction and to the dynamic theory of gases. Interested in Terrestrial Physics, he was president from 1879 to 1907 of the

⁽¹⁾ Even after the Eddington's expedition, in 1920, he wrote to the French physicist Violle: “Après la brillante confirmation que l'éclipse de Mai a donné à la théorie d'Einstein, il est juste que des preuves sûres en faveur soient fournies même par les expériences de laboratoire” [1].

Consiglio di Meteorologia e Geodinamica. In 1881 he founded the Physical Institute of the University in via Panisperna and there he called (1908) Orso Mario Corbino (1876-1937), who was then Chair Professor in Messina after spending several years in Palermo with remarkable achievements in various fields (magneto-optics and the Macaluso-Corbino effect). In Rome Corbino continued his successful scientific career (photoelasticity and the effect of Volterra distortions; specific heat in high-temperature metals; improvements in X-ray generators), also with the collaboration of young Giulio Cesare Trabacchi (1884-1959) who was to become director of the Physics Laboratory at the Istituto Superiore di Sanità in 1922 (Trabacchi had an important part in the development of Nuclear Physics in Rome).

Like several other prominent colleagues, Corbino was deeply involved in the First World War. The war caused a violent stirring of emotions, being perceived by most Italians as the way to the completion of national unification; also, it stimulated initiatives to overcome the weakness of the economic and industrial structure of the country, disclosed by the war needs. The very first outcome was the 1915 Committee, made mainly of industrials and scientists. A further initiative (see later) was the creation of an Office for Research and Inventions, attached to the Under-Secretariat for Weapons and Ammunitions (Ufficio Invenzioni e Ricerca, UIR): this government support made the difference, because, while the first Committee gave scarce results, the Office, directed by mathematician and physicist Vito Volterra, was the first step toward the foundation (1923) of the National Research Council (CNR). Corbino with other colleagues had an important part in it, as well as in other bodies created with the aim of developing the interaction of the scientific and industrial world also beyond the needs of the war [2]. Corbino initiated an intense public life in which he displayed both at the governmental and at industrial level his technical preparation, his leadership and his broadmindedness. He became senator shortly after the war and briefly Minister of Education, the first scientist after Matteucci. He was Minister of Economy for a few months at the beginning of the fascist government in a particularly troubled political period. He was able to keep his authoritative stand in the industrial and scientific world without becoming a member of the fascist party. The public life did not prevent him from continuing his scientific activity and above all from building what was to become the Group and School of Physics of via Panisperna.

2'3. Pisa. – Pisa is peculiar because of the existence of the Scuola Normale Superiore next to a University of old tradition (13th century). Founded by Napoleon in 1810 as the core of his “Italian program” of reform of knowledge, the Scuola Normale followed the model of its twin Ecole Normale Supérieure in Paris, with the same scope of formation of high-level secondary-school teachers; in fact, since its origin it became a school intended for the preparation of a selected cultural elite. Teachers at the SNS were specially appointed lecturers, often from the University of Pisa. This was the case in the first thirty years of the 20th century, when directors were the mathematicians Ulisse Dini and

Luigi Bianchi, while Physics teachers were Angelo Battelli and Vito Volterra⁽²⁾. Several personalities who appear in the following were “normalisti”.

As for the University, Angelo Battelli (1862-1916) succeeded in 1893 to Riccardo Felici (1819-1902, renovator after Carlo Matteucci of the Studio di Pisa and well known for his contribution to the interpretation of electromagnetic induction). Battelli, originating from le Marche, “laureato”⁽³⁾ in Turin, 1884, was briefly Chair Professor in Cagliari and Padua, showing from the beginning his taste and ability in rigorous experimentalism (thermal properties of vapours, Peltier effect, thermoelectricity). In Pisa, he founded the Italian Physical Society, revived successfully the journal *Il Nuovo Cimento* and rebuilt and re-equipped the Physical Institute, receiving and stimulating a number of researchers and pupils, whom he would involve directly in the design and running of experiments. He was against specialisation, perhaps to the detriment of coherence in his projects, but he was ready to open his mind and his activity to the more recent results (gas discharge, cathode rays, X-rays, radioactivity), which were leading to the experimental and theoretical approach to the structure of matter beyond the limits of the chemical atom. This interest of Battelli, and also the method, is well presented in the treaty on Radioactivity (1909), written with his pupils and co-workers R. A. Occhialini (see below) and S. Chella and translated (1910) in German and French⁽⁴⁾.

Battelli had little time left for elaborating the next results of Rutherford (1911) and Bohr (1913) on the planetary atom: a fatal illness in the last years of his life brought him to a premature death in January 1916, only 54. Furthermore, in those years (as in previous years) he was deeply engaged in public life, becoming repeatedly a Member of Parliament with particular interest in the school system. Like so many Italian fellow-scientists in the 19th century, Battelli was an active patriot: a few days after the engagement of Italy in the first World War, he wrote to a leading newspaper an open letter, July 11, 1915, urging the Government to take immediate steps for the formation of a body, a “Scientists Section”, where scientists would put their expertise in the selection and production of ideas and inventions useful for the war effort (similar bodies were already active in Germany, France and Great Britain) [5]. A “National Committee of Inventions” was promptly formed with the active participation of pain stricken Battelli

⁽²⁾ With the reform of Giovanni Gentile, 1928, the Scuola added explicitly the further mission of promoting the scientific and literary national culture, with special postgraduate courses open to graduated from all over Italy and since 2002 from all over the world [3].

⁽³⁾ At that time the Italian “laurea” was based traditionally on a (minimum) 4-year curriculum (this is the case for Physics and Mathematics) and on a written thesis preferably on an original subject. In the period discussed here the theses in Physics were typically experimental. The Italian words “laurea” and “laureato” will be used in the following.

⁽⁴⁾ See E. Gamba [4]. Together with a meticulous review of the experimental results updated to 1909, none of them original, but many of which carefully replicated in the laboratory, the book contains a critical presentation of the current dubious models of atomic structure in the light of an electric theory of matter —a remarkable behaviour for a researcher strictly devoted to the empirical basis of Physics. Notably, no reference is made to the work of Einstein, presumably because of a supposed absence of an experimental basis.

and others. Alongside with other similar initiatives, this led to the creation in 1917 of the Ufficio Invenzioni e Ricerca, mentioned in the above.

Luigi Puccianti (1875-1952), born in Pisa, succeeded Battelli in the direction of the Institute in 1917. An enthusiastic pupil of his, “laureato” in 1898 with a thesis on the absorption of near-infrared light in a large sample of organic liquids: this was actually a first observation of vibrational spectra of molecules. From 1900 to 1915 Puccianti was in Florence, first in the position of assistant and “aiuto” (aide) to Antonio Ròiti at the Istituto di Studi Superiori (see later), then keeping his activities in that Institute while acting, with a better salary, as professor of Physics at the Istituto Superiore di Magistero Femminile. Chair Professor in 1915, first briefly in Genoa and Turin, after two years he was back in Pisa to the end of his life. His scientific contributions are in electromagnetism and, more importantly, in spectroscopy, where he shares with Antonio Garbasso the merit of the rebirth of spectroscopy in Italian Physics (see later). He was a good and dedicated teacher and had the chance of being the director of a well organised institute with a good mathematical school nearby⁽⁵⁾. Differently from Battelli, Corbino and Garbasso, Puccianti did not engage in political and administrative life.

2.4. Florence. – For centuries, Florence did not have a University, although there was intermittently a Studio opened since the 14th century [8]. This was conceived as a place for “natural” (according to the meaning of the time) investigations and was housed from the end of the 16th century in the Uffizi as “Gabinetto delle Matematiche”. During the House of Lorraine grand duchy, this became in 1775 the Museo di Fisica e Storia Naturale, well equipped with instruments and collections and housed nearby Palazzo Pitti. In spite of the label Museum, it was intended to be also a laboratory for Physics experiments. A few years later, a small astronomical observatory (later known as la Specola) and a room for meteorological measurements were added. It is worth to stress that shortly after the death of its first director, Felice Fontana (1730-1805), a very early professorial chair in Astronomy was instituted, during the period of the Napoleonic domination of Florence. In 1859 the provisional government, installed in Tuscany after the expulsion of the Lorenese family, gave rise to the Istituto di Studi Superiori Pratici e di Perfezionamento according to the plans of Carlo Matteucci for a kind of super-university concentrating high-level competence and adequate financial means, with emphasis on observational and experimental activities. The Museo di Storia Naturale became a part and the basis of the “Sezione di Scienze Fisiche e Naturali” of the Istituto, strongly oriented towards experimental research and education. The local rivalries and the financial difficulties of the newly born Italian State frustrated the project, not so much in the chemical and naturalistic section as in the physical section, in spite of

⁽⁵⁾ Perhaps Puccianti was not active enough to fill the gap between the traditional experimentalist culture and the culture of the “new” Physics which was stimulating the interest of eager young people. He was however generous and broadminded, to the point of asking the still student Enrico Fermi “to teach him something” of the new Physics “which he might still learn”: see [6] and [7].

the support of Matteucci. The situation worsened after his death (1868). However, the meteorological and geophysical observatory kept on working [9] under the Direction of, among others, Antonio Ròiti, Antonino Lo Surdo and then Antonio Garbasso (see later). In 1876 the Istituto was made “equivalent” to Italian universities, with the possibility of offering “laurea” theses of experimental kind but without the structure of a regular faculty (and classed B, *i.e.* mainly supported by local financial contributions, a condition whose consequences were felt in years, discouraging teachers at Chair Professor level to remain for long). In spite of that, in 1880 Antonio Ròiti accepted the offer of a chair professorship⁽⁶⁾. Since he was a respected scientist, he obtained quickly an “aiuto” and an assistant, and increased and updated the equipment of the Physical Institute, still named Sezione di Fisica. In spite of the absence of a regular “corso di laurea” and of the obsolescence of the seat, Ròiti was able to attract in Florence several eager young elements in the position of “aiuti” and assistants: among them Luigi Pasqualini, Luigi Puccianti, Antonino Lo Surdo, naming only the ones who are directly involved in the story of Florence Physics. When Ròiti retired in 1913, keeping for himself only the position of co-director of *Il Nuovo Cimento*, his place was taken by Antonio Garbasso.

Garbasso (1871-1933) was a remarkable mix of a naturalist scientist and a humanist: born in Piedmont, he changed into an enthusiastic Florentine, extending his patriotism towards united Italy into a kind of worship for the adopted Tuscan homeland. To a large extent, this was due to that deeply appreciated “natural and positive” approach to reality as distinctive of the “flower of the Latin culture, namely the Tuscan thought” [10]. “Laureato” in Turin, 1892, with a good physicist and teacher, Andrea Naccari, he completed his scientific preparation in Bonn with Hertz and in Berlin with Helmholtz and initiated his interesting scientific activities working on the optical properties of electromagnetic waves. After teaching appointments in Turin, in Pisa with Battelli (working with him on X-rays) and again in Turin, in 1902 won two competitions, one for Mathematical Physics in Pisa, the other for Experimental Physics in Genoa. This latter was his choice and there he remained for 10 years, continuing his research in electrodynamics and spectroscopy (in full development at the time in Europe, and about which he published a treatise in German). His research and teaching method was based characteristically on the association of the mathematical treatment of problems and the accurate experimental verification of results⁽⁷⁾ but he did not refrain from proposing “analogic” models, as in the case

⁽⁶⁾ With an honourable record as companion of Garibaldi in the 1866 war against the Austrian Empire, Antonio Ròiti (1843-1921), native of Ferrara, got the laurea in Pisa (1868) with Felici and was also a “normalista”. He taught at secondary school level (at the time a non-diminutive position for many young scientists!) in Leghorn and Florence and, 1878, was Chair Professor in Palermo with a good recognition as a careful experimenter, gaining him an authoritative membership in the International Commission for Electric Standards. In Florence he took a particular commitment in teaching, producing a successful text, *Elementi di Fisica*, comparable with recognised texts at the European level.

⁽⁷⁾ See Manlio Mandò, [8] page 599 and following. An amusing statement to the benefit of his students was: “Mathematics is very important for a physicist, almost as much as mercury”.

of his “electromagnetic model” of atomic structure intended to explain line spectra⁽⁸⁾. Apart the limits of his model, Garbasso was ready to appreciate the new field of quantum spectroscopy, at the time when he was appointed professor of Experimental Physics at the Istituto Superiore of Florence in 1913. The staff he inherited from Ròiti comprised Puccianti, already active in Florence part-time since 1905, and the “aiuto” Antonino Lo Surdo⁽⁹⁾. Garbasso quickly set Lo Surdo to investigate spectroscopically the Doppler effect in the light emitted by the positive “retrograde rays” discovered, 1886, by Goldstein near the cathode of a discharge tube⁽¹⁰⁾. With an original design of the discharge tube Lo Surdo, in summer 1913, rediscovered in more efficient conditions the effect found in the same months by Stark. While a not interesting dispute followed about the priority, in which also Corbino was involved, Garbasso was able within 1913 to propose a first theory of the effect based on the Bohr model which had appeared a few months before. While his calculations contained an error pointed out to him by Bohr himself, Garbasso can be correctly considered the initiator in Italy of the use of the Bohr model along the first steps of quantum mechanics⁽¹¹⁾.

3. – The hill of science

3'1. *The sciences of light: Astronomy.* – To provide a complete picture of the scientific environment in Arcetri, two institutions must be mentioned: the Arcetri Astronomical Observatory and the Laboratory that, in successive steps, became the National Institute of Optics. Besides the possibility of new overlapping fields of research, the benefits for the growing Florentine Physics were the international collaboration scenario (Astronomy) and a special care for applied Physics (Optics). The two institutions are close to the Physical Institute, although built before and after it, in a period just longer than half a century. The first was the Observatory.

⁽⁸⁾ Garbasso expressed his conception of models with the following words: “Any theory in its essence is a model, better, is a description of a model ... the only connection between nature and model, in the most favourable case, is that the laws which describe the variations of corresponding quantities are the same in both systems ... so, a theory can be true without containing anything of the real”. An interesting treatment of the impact of Garbasso and Puccianti in the development of spectroscopy in Italy is found in [11] and references therein.

⁽⁹⁾ Antonino Lo Surdo (1880-1949), born in Siracusa, a good experimentalist with interests in terrestrial physics and spectroscopy. He became “aiuto” of Antonio Ròiti in 1908 and was also appointed director of the Meteorological Observatory at la Specola two years later. Lo Surdo moved to Rome in 1918 and became “aiuto” of Corbino, obtaining in 1919 the chair of Fisica Superiore. In 1937 Lo Surdo became director of the Physical Institute after the death of Corbino. He founded and directed to the end of his life the Istituto Nazionale di Geofisica of CNR.

⁽¹⁰⁾ See [12] and references therein. This article gives a vivid picture of the results obtained in a few weeks confirming and completing the first observations, also Puccianti taking part in them.

⁽¹¹⁾ A touching presentation of the scientific and human figure of Garbasso is due to Rita Brunetti [13]. A comment showing the interest and the limitations of the scientific attitude of Garbasso is found in [14]. See also note ⁽⁸⁾.

It is well known that Galileo Galilei spent the last years of his life confined in the Villa del Gioiello, a country house at the Pian de' Giullari on the hill of Arcetri, a few kilometres from the centre of Florence. There he carried out his last heavenly observations and wrote fundamental Physics works. Accordingly, that hill seemed to be the most suitable place, when, in the second half of the 19th century, a new site for the Astronomical Observatory was sought. Still positioned downtown, it was by then incompatible with some aspects of the post-unification developing city, first of all with the street lighting. The decision was for Arcetri, at walking distance from Galileo's historical house. The new Observatory was inaugurated in October 1872.

Unfortunately the Astronomy research suffered from the same restrictions affecting Physics: first of all inadequate teams, in the present case two or three people, usually a director and an assistant. As for the scientific activity, the main fields beyond eclipses were "terrestrial" phenomena, like, *e.g.*, northern lights or meteoric showers, and of course the hunt for comets. The name of Giovan Battista Donati (1828-1873) is associated to many celestial bodies, but he died only a few months after the inauguration of the Observatory⁽¹²⁾. His "aiuto" became the new director, but he too died some six months later: then it was pretty hard to find a replacement. Eventually Giovanni Virginio Schiaparelli (1835-1910), director of the Observatory of Brera (Milan) and world famous discoverer of the so-called Martian canals, had his German assistant, Wilhelm Tempel (1821-1889), appointed by Florence. The fame of Tempel too is based on the observation of comets and quite a few were named after him, but he was not a real astronomer. From our modern point of view, he was slightly more than an amateur, and actually he was only an assistant never in charge of the direction. However he had a very valuable ability: in those days when the photographic emulsions were not fast enough he was a gifted drawer, really skilled and accurate. His hand-painted plates were a good tool for the sky studies, and moreover nice to look at. Those plates yielded him the Royal Award of the Accademia dei Lincei in 1879. But a lithographer does not open research lines and for a while, after his death in 1889, the Observatory was neglected: twenty years elapsed since the opening and almost never there was a director.

Only in 1893 the professor of Astronomy Antonio Abetti (1846-1928) came from Padua. He had to make a great effort in the restoration and maintenance of instruments. Thanks to him the already existing "Officina Galileo", specialized in Fine Mechanics and Optics, underwent a strong development. Abetti was known as a bright scientist: his scientific career already comprised work done at the Astronomy Institute in Berlin and an expedition in India, in 1874, to observe the transit of Venus before the Sun. His figure was recognised at the international level and even the great American astronomer George Ellery Hale visited him and his Observatory. Hale was looking for a European support for a new scientific journal, *The Astrophysical Journal*. His visit initiated a long-lasting collaboration on Solar Physics with a significant role in Arcetri's astronomical research. Abetti was mainly an observational astronomer but he understood soon the need of a

⁽¹²⁾ A short history of the Arcetri Astrophysical Observatory can be found in [15].

deeper integration between Astronomy and Physics, the so-called “New Astronomy” or Astrophysics. Donati himself, after Father Angelo Secchi, carried out investigations on the spectral classification of stars. Aware of this evolution, Abetti favoured the plans of Antonio Garbasso to transfer the Physical Institute from the decaying seat in the centre of Florence to a new building on the same hill of Arcetri, close to the Observatory (see later). The emphasis on the international quality of research and the need of evolution from Astronomy to Astrophysics were the remarkable features of the scientific policy of Antonio Abetti. Following his steps, the son Giorgio Abetti (1882-1982) carried out studies and collaborations abroad, mainly in German universities. Back to Italy, he obtained a position at the Collegio Romano in Rome and in 1913-1914 he took part to an engaging multi-scientific expedition in the Himalayas. In 1917 he went to the USA as a member of a military mission organized by the just founded Italian UIR, the already mentioned Research and Development Board of the Department of War. In 1921 he was again at the Arcetri Observatory to become, shortly after, its director. Since the beginning, his scientific production was noticeable and most of it concerned astronomical spectroscopy. In the same year he succeeded in changing the Observatory’s denomination to Astrophysical Observatory, as recommended by Garbasso [10]. The Faculty of Science introduced the teaching of Astrophysics, beside Astronomy, already after the end of the World War. The time was ripe for this new approach to heavenly phenomena. In fact the first Italian Astrophysical Observatory was established in Catania, as early as the end of the 19th century, along with the first Chair of Astrophysics [16]. This was a model for Arcetri, but while Catania was unable to develop an Astrophysics school, this succeeded in Florence. Indeed a few years later, in 1925, a Solar Tower was built on the hill to study high-resolution solar spectra. It has to be stressed that it was the first Solar Tower in Europe and the third in the world, after the ones already built by G. E. Hale. Actually, both Hale’s scientific and financial help were instrumental for the design and the realization of the Arcetri Tower [17]. The contemporaneous establishment of the “corso di laurea” in Physics (see later) stimulated the formation of a school of Astronomy which produced several of the directors of Italian observatories after having been students or junior astronomers in Arcetri (see table IV later on).

This was the favourable scientific environment found by the bright students and teachers gathered around Garbasso. In those very years Giorgio Abetti devised the Seminar on Astronomy, Physics, and Mathematics according to the model of the seminars in Anglo-Saxon universities. Both Italian and foreign physicist and astronomers were happy to present their ideas and results: lectures were held among others by Hall, Bethe, Persico, Fermi, but also by younger physicists as Rossi and Bernardini. More important, students were encouraged to attend and contribute to the lectures.

3.2. *The sciences of light: Optics.* – With the expansion of the “Istituto di Studi Superiori”, new research buildings were supposed to be built in the proximity of the Observatory. In addition to the new seat of the Institute of Physics, almost completed during the war, there was also a minor building, halfway between the Physical Institute and the Astronomical Observatory. This building was supposed to house the Chair of

Terrestrial Physics which should inherit the activity of la Specola, and was meant also for meteorological measurements through balloon-borne instruments. So, in the words of Garbasso, one would join in the same area “Physics of Earth and Physics of Heaven, the most Tuscan ones among the Tuscan Sciences” [10]. Instead, the scope of the new building changed very soon. The director should have been Antonino Lo Surdo, director since 1910 of the old Meteorological Observatory. The idea was to keep Lo Surdo in Florence. But in 1918 Lo Surdo joined the Physical Institute in Rome and the building remained deserted. Nine years later, it became the seat of the National Optics Institute (INO)⁽¹³⁾. That was the last step of a project stemming from the needs of the “Great War”: in fact, as soon as the conflict began, scientific and industrial Italy had to face with a complete dependence on foreign countries for products based on Optics. Pointing systems, periscopes, binoculars, all these were imported mainly from Germany, but then Germany had become the enemy and among other restrictions a block on import was applied. All of a sudden, Italy realized that optical goods were not only for peace times. Not by chance, “Industrial Mobilization” was the specific aim of the UIR (see sect. 2 and 3’1): it was decided to support the birth in Florence of a Laboratory of Applied Optics and Fine Mechanics⁽¹⁴⁾, following an original idea of Garbasso. Behind this undertaking, there was, of course, a strong military concern together with the will of some Italian enterprises, interested also in civil production. In fact, the real proponent of the whole project was the physicist Luigi Pasqualini (1888-1999), a former assistant of Ròiti, inventor, skilled technician first and then director of a workshop specialised in precision mechanics, the “Officina Galileo”. Moreover, he could rely on the great experience gained in the Italian Navy as “electric” technician, in charge of the Torpedoes Laboratory, close to La Spezia. He was well aware of the Italian deficiency in Optics, which extended to the technique of optical glass, and was strongly motivated in the development of a national industrial production of high-quality instrumentation; furthermore he was convinced that this required the formation of specialised technicians with scientific background. The Laboratory had to be the first step.

To carry out his plan, Pasqualini was able to involve other industries, local politicians and of course Garbasso (details on the role of Garbasso and the evolution of the project at the end of the war are found in the next section). But as soon as the Florentine project was officially approved, in September 1918, and even before the inauguration of the Laboratory, the war was over. As a consequence, the Optical emergency was over and the industrial interest decreased. At variance with the original intentions, Garbasso chose as director a university lecturer, his “aiuto”, Raffaele Augusto Occhialini⁽¹⁵⁾, former

⁽¹³⁾ For an analysis of the complex phases of such evolution and its links with the Florentine political and industrial environment see [18].

⁽¹⁴⁾ As for the governmental side of science in that period see [19], in particular the Appendix with the report of Lo Surdo on a meeting held in Palazzo Vecchio to establish the Laboratory.

⁽¹⁵⁾ Raffaele Augusto Occhialini (1878-1951): “marchigiano” like Battelli, born in Fossombrone, educated in Pesaro (see [20]), student of Battelli and also “normalista” in Pisa 1898, “laureato” in 1903, was his assistant and “aiuto” till the death of Battelli. Briefly in the same positions with

“aiuto” of Battelli in Pisa. Occhialini started working in rather unfavourable conditions also because of the transfer of the whole Physical Institute and of the attached Laboratory to the new seat in Arcetri. He succeeded in publishing the first few issues of the journal *Rivista di Ottica e Meccanica di Precisione*, one of the statutory obligations of the laboratory, containing among other things his study on “moiré” interference fringes and their use in optical and mechanical applications. Unfortunately he was not aware that a rather complete study of the subject had been carried out by Augusto Righi about 30 years before (and had fallen in oblivion!). Frustrated Occhialini abandoned the subject. On the other hand he was on the verge of leaving Florence after winning a competition for a professorship.

The work on “moiré” interference fringes was picked up by the young Vasco Ronchi⁽¹⁶⁾, who had been appointed by Garbasso (1920) assistant to the (empty) chair of Fisica Terrestre under recommendation of Occhialini. Ronchi was for years the only scientist engaged in the activity of the Laboratory, mostly to determine the technical features of lenses on behalf of the Astronomical Observatory (the Amici’s objectives!) and the Officine Galileo. Very soon he obtained (just by chance, as he was proud to say) an important result, that is a new method, based on moiré fringes, to verify smoothness and quality of an optical surface. This easy yet powerful tool is still nowadays called the “Ronchi test”. Thanks to it, Optical techniques gained an official recognition. From then on Ronchi spent all his efforts to revive the original project of the Laboratory. He kept the contacts with Pasqualini on the one hand and with the military ambient on the other, in particular with the “Istituto Geografico Militare”, which had its seat in Florence and was obviously interested in optical devices. The person instrumental in the development of the Laboratory along the lines hoped by the still young Ronchi, was Gen. Nicola Vacchelli, responsible of IGM. On the other hand, with the advent of the fascist government, the policy towards military expenditures and towards the support of the related optical and mechanical industry changed. Pasqualini and Vacchelli joined Ronchi in promoting the renovation of the Laboratory with an extended program which included

Puccianti, 1916-1917. After the war (see text) “aiuto” of Garbasso in Florence, Chair Professor in 1921 in Sassari, in 1924 in Siena and from 1929 in Genoa. With good connections in Germany and the United States, he was an excellent teacher, and notable for his works on radioactivity, gas-discharge, spectroscopy, electrotechnics. He produced also a booklet on relativity of popular character.

⁽¹⁶⁾ Vasco Ronchi, (1897-1988): student in Pisa and “normalista” from 1915, recalled for military service in 1917, back to Pisa in 1919, succeeded in completing the exams and graduating in that very year with the encouragement of Puccianti. Introduced to Garbasso by Occhialini, he was appointed assistant in the Institute of Physics in Florence from 1920 and, when Occhialini left for his chair in Sassari, he took responsibility of the Laboratorio di Ottica e Meccanica di Precisione. In the following years he succeeded in transforming that initiative, which had badly suffered in the aftermath of the war, in the Istituto Nazionale di Ottica with a notable stand in the Florentine and national scientific and technical panorama. He was instrumental also for the foundation of the Associazione Nazionale di Ottica. In his initiatives Ronchi had the support of Garbasso until the latter’s death.

explicitly formation courses intended for civil and military (not only Italian) high-level technicians. A first step was the transfer of the instrumentation of the Laboratory from the inadequate rooms in the Physical Institute to the still empty pavilion which should have housed Fisica Terrestre. The second step was the acquisition of Gino Giotti, an optical expert working at the Merate Astronomical Observatory, who became an excellent co-worker of Ronchi and was also involved in the administrative management. The third step was the foundation of the Associazione Ottica Italiana, in view of promoting the coordination of the interests of the industries involved. The aim was to favour the diffusion of optical culture according to the original idea of forming skilled shop foremen.

At this point it was possible to transform the Laboratory in the Istituto Nazionale di Ottica under the direction of Vasco Ronchi. The inauguration took place in 1928 and the small pavilion was recycled in the seat of a kind of advanced vocational school, with room and some equipment for applied research. In time, the increasing activity led to the expansion of the primitive construction into the present building. Thanks to a strong governmental support and to the determined character of his director the INO underwent a fast growth and reached significant objectives [21] favouring the practice rather than the theory, with a feeling for the evolving civil and cultural needs. Although the part of the program aiming at the formation of skilled technicians was not completely fulfilled, what was left is an efficient school for optometrists. The scientific side followed the personal taste of Ronchi, more and more oriented towards physiological optics in the last part of his life.

Remarkably, never the activity of INO crossed that of the Physical Institute in the period between the two world wars. A more productive relationship was maintained with the Observatory and the Italian astronomers. After the death of Ronchi in 1988, the INO underwent, under the direction of Tito Fortunato Arecchi, a considerable reorganization, with an extension of its scientific and applied landscape (dynamics of complex systems, lighting techniques; restoration and preservation of the cultural heritage).

4. – Garbasso and Florence

4'1. *Arcetri from the beginning to the end of the 1st World War.* – As soon as Garbasso settled in Florence, in summer 1913, he backed a convention between the Administration (and banks) of the town and the Superintendent and Directorate of the Istituto di Studi Superiori, obtaining new positions for the Sezione di Scienze Fisiche e Naturali, and financial support for the renovation of the laboratories, in primis for the building of a new Physical Institute. The actual construction started quickly on the site Garbasso himself had chosen romantically on the hill of Arcetri, not by chance at walking distance from the site of the Astronomical Observatory, which was again in operating conditions after years of abandonment (see sect. 3'1).

The following year 1914 marked the outbreak of the first World War. After one year of negotiation and fierce debate, Italy joined the Triple Entente and engaged in the war against the Austro-Hungarian Empire, in May 1915 (see sect. 2'2). Garbasso, at the age of 54, joined immediately the front-line as a volunteer lieutenant in the Engineer

Corps, setting up a system of phonotelemetry against the Austrian artillery units. But he remained in close contact with his institute and his plans for the development of an advanced scientific and technical Florentine centre. His interest in the technical side was stimulated by his war experience and by his acquaintance with Pasqualini. They had much to share, both physicists, innovators and involved in political life (at the time Pasqualini was also town councillor). Pasqualini visited Garbasso on the front-line sometimes in 1916. Then, on leave for the beginning of academic year 1916-1917, Garbasso sized the opportunity of the “opening address” to recall the convention of 1913 and to thank the administration (and the banks) for the generosity with which the Istituto di Studi Superiori had been endowed with new staff positions and with the almost completed new Physical Institute, with its arcade and cloister in “Tuscan” style, on the hill of Arcetri ([10], page 16-17).

But Garbasso had a wish which coincided with the wish of Abetti, namely the concentration of more Institutes in a common area. So, apart the abundant patriotic rhetoric of the speech, Garbasso presented in full his plan for the Physical Section of the Istituto di Studi Superiori, to be concentrated in Arcetri. In his mind, the hill was to become a kind of City of Science, as can be seen in the decoration of the hall of the Physics building. The ceiling shows, in Art Nouveau style, the Galilean discoveries: the Sunspots, Jupiter’s four satellites, the phases of Venus, the ring of Saturn, the features of Moon surface. On the walls, two large frescos display allegories of Research and of Learning. Moreover, the bas-reliefs of the members of the Accademia del Cimento (1657-1667) are aligned around the central cloister and in the surrounding garden there was a bust (now lost) of Minerva, the goddess of knowledge. Besides the building for the Physical Institute, large enough for housing a number of researchers and technicians, the “pavilion” intended for Terrestrial Physics was already completed (see sect. 3’2).

Furthermore, Garbasso urged also the creation of another Laboratory, better, a Research Institute, where (in his words) “people with scientific formation and aware of the needs of practical work would be prepared to help and advise shop foremen ... in view of the gigantic economic upheaval announced by the gigantic war”. These were the premises of the Laboratorio di Ottica applicata and Meccanica di Precisione discussed in the previous section. Garbasso expressed also the hope to have in Arcetri the “Museo degli Strumenti Antichi” of Lorenese origin (partly dispersed by the Lorena themselves when they left Florence), to become a centre for the study of the History of Science. This part of the project was not realised, but one more thing shows the broadmindedness of Garbasso in envisaging a site devoted to Physical Sciences: he hoped that “the old, glorious Observatory of Donati and Amici would turn at least part of its activity to the studies of Astrophysics, as in the intention of his excellent colleague, professor Abetti”. Antonio Abetti, who supported fully Garbasso’s plan, indeed changed the name from Astronomical to Astrophysical Observatory, the second in Italy, mindful of the work of Father Secchi. The Observatory would have later an important part in the cultural environment of the Group of Florence (see sect. 3’1).

The project of the Laboratorio di Ottica Pratica e Meccanica di Precisione (accounted for in previous section), was officially approved September 1918 as a body attached to

the Physical Institute⁽¹⁷⁾. One of the problems was the director, who should have been in principle a technician with a good scientific background, not necessarily a university professor. The choice in the end was Battelli's pupil, Augusto Occhialini, co-author of the treatise on Radioactivity, the second "normalista", after Ròiti, entering the story, on the move from Pisa after the death of Battelli, and already father of Giuseppe, Peppino, not yet GPS or Beppo (see note ⁽¹⁵⁾ and sect. 3'2). Garbasso was in touch with Occhialini while this one served at the UIR: Garbasso encouraged him to move from Pisa to Florence, where the position of "aiuto" was vacant (Lo Surdo had left for Rome, 1918), and was instrumental for his appointment as a member of the Italian War Mission in USA with the task of studying the techniques of optical glass and of setting up agreements of technical cooperation. Occhialini stayed in USA from June 1918 to February 1919 and had the opportunity of meeting several American scientists, among whom R. W. Wood, A. A. Michelson and R. A. Millikan. In the meanwhile he became "aiuto" and, October 1918, was appointed director of the Laboratorio. The official inauguration took place on November 24, 1918, twenty days after the collapse of the Austro-Hungarian Empire. The speech of Garbasso began with the words: "The war ended: we must rebuild the world". The task proved to be much more difficult and even painful than expected⁽¹⁸⁾.

4'2. *From the institution of the university to academic year 1925-26.* – In spite of the circumstances, the scientific and didactic activity of the institute did not stop during the war and this happened by merit of Rita Brunetti, "normalista" and "laureata" in Physics with Battelli with a well recognised work in spectroscopy. After one more year of specialisation in Pisa she took up the position of assistant of Garbasso in Florence and started working on the Stark effect with Lo Surdo until he left for Rome. With Garbasso at war, Brunetti managed to keep going the Physical Institute, still in the old seat downtown, both in teaching and in research, working successfully in X-ray and visible spectroscopy⁽¹⁹⁾. Back from the States in spring 1919, Occhialini took up his appoint-

⁽¹⁷⁾ Many details with some errors and questionable opinions are in [22].

⁽¹⁸⁾ Garbasso left the army as a major of the Engineers Corps and resumed eagerly his place at the Istituto di Fisica with particular care for his duties as a lecturer, but his interest shifted more and more towards public life and political commitment, with the aim of benefiting at the same time his adopted city and his institution in times of economic difficulties and of social unrest. It is not strange that, after years of direct engagement in warfare, patriot Garbasso joined the nationalist party of chauvinist Luigi Federzoni, ending into the fascist party seen as the defender of the values of the Risorgimento and of the sacrifices sustained by so many on the front line during the war. This attitude was common among ex-combatants, even among upright refined intellectuals like Garbasso. So he was elected mayor of Florence in 1920 and kept the position under the fascist government with the title of Podestà until 1928. At the same time, like Corbino, he filled important positions in the organisation and direction of scientific research, in particular in the CNR (see sect. 2'2), supporting actively the financing of well equipped laboratories and promoting the cultural updating and qualification of students and young researchers with the institution of scholarships for stays in foreign advanced institutions.

⁽¹⁹⁾ This was the beginning of a noteworthy career, which led Rita Brunetti (1890-1942) to become "aiuto" of Garbasso from 1921 to 1926, and then Chair Professor for two years in Ferrara,

ments as “aiuto” and as director of the Laboratory during an exhausting time, when the Physical Institute and the attached Laboratory were replaced in the new buildings in Arcetri. He and Brunetti were helped in that job by Vasco Ronchi (see sect. 3'2). In 1921 Occhialini went to his chair in Sassari. Brunetti became “aiuto” and Garbasso promptly filled the vacant position of assistant with a brilliant student of Puccianti, Franco Rasetti, “laureato” by the end of 1922 with a remarkable thesis in spectroscopy. In Arcetri Rasetti found, in his words, *“a very pleasant place ... with a pretty good equipment ... especially for spectroscopy ... and not much teaching ... because Garbasso gave the Physics course”*⁽²⁰⁾.

Actually Garbasso was succeeding in transforming the Istituto di Studi Superiori in a regular University, be it still of class B⁽²¹⁾, and to establish the regular “corso di laurea” in Physics (and Mathematics), with the pattern of teaching subjects provided by the national regulations originally set by Matteucci: it became possible to have students from the beginning of their curriculum. The first regular academic year began November 1924. The teaching staff of the “corso di laurea” in Physics was as per table I: notice the position of Enrico Fermi. Indeed a turning point was the professorship “in charge” (Professore Incaricato) offered him by Garbasso for the teaching of Mathematical Physics and Theoretical Mechanics (Meccanica Razionale).

Apart the famous work of Fermi on Statistics (written in those years in Arcetri), he and Rasetti, old friends from the times of Pisa, initiated a very fruitful collaboration both on experimental (spectroscopy!) and theoretical subjects, the two being endowed with a vivid physical sense, the first adding his profound understanding of the new atomic Physics (and relativity), the latter his ability in devising and handling experiments. Both made friends with spectroscopist Rita Brunetti, exchanging ideas and experience. Later Fermi would quote Brunetti's results of those years.

A second turning point is 1926. Rita Brunetti won a competition for Experimental

for eight years in Cagliari and from 1936 in Pavia. Her work covered spectroscopy from visible to X-rays, magnetic properties of matter, nuclear physics and its bio-medical applications, history of science, good popular works, two treatises at the didactical level. In an academic environment dominated by males Brunetti was the only Italian woman attaining the directorship of a Physical Institute. She died prematurely, probably because of a professional disease, but in the very last years she attempted to use photographic plates for the detection of cosmic rays.

⁽²⁰⁾ Rasetti gives an interesting account of his experience with Garbasso: *“he had been a good physicist, at the time he was only interested in politics”*, but *“he gave his course in elementary Physics and was quite intelligent at it. And later Fermi explained to him what we were doing and he understood...he followed what we were doing and he was a very pleasant person ... as for being fascist he was very moderate, in fact (Rasetti is sure that) had he lived longer, he would have become disgusted with Fascism. But in the first few years ... Fascism didn't seem very bad ... after 1924 ... people lost hope (that Fascism would become a reasonable dictatorship). Still, even in the States there was a lot of admiration for Mussolini.”*, excerpt from [7].

⁽²¹⁾ In the opening address Garbasso underlined that the inauguration of the revived “Studio Generale” was greeted by the representatives of the same Communes already existing in the State of Florence in 1321, when the “Studio” came to existence for the first time.

TABLE I. – *Teaching staff of the “corso di laurea” in Physics, academic year 1924-25.*

Courses	Teachers
Analisi Matematica (I e II)	F. Tricomi
Analisi Superiore	F. Tricomi
Geometria Analitica e Proiettiva	E. Ciani
Geometria Descrittiva	E. Ciani
Fisica Sperimentale (I e II)	A. Garbasso
Fisica Superiore	A. Garbasso
Esercizi di Fisica	A. Garbasso
Chimica Generale e Inorganica (I e II)	L. Rolla
Chimica Fisica	L. Rolla
Meccanica Razionale	E. Fermi
Fisica Matematica (Electromagnetism, Spectroscopy) ^a	E. Fermi
Astrofisica	G. Abetti
Disegno	R. Brizzi
Mineralogia (optional)	P. Aloisi
Chimica Organica (optional)	A. Angeli

(^a) The following year, the course was named Fisica Teorica and Fermi changed the program in topics of Fisica Statistica (Statistical Physics).

Physics and left Florence for Ferrara, destitute of a laboratory: she was hosted for her experimental work by Quirino Maiorana in Bologna. At the same time Fermi and Enrico Persico won the first competition for Theoretical Physics⁽²²⁾, a new entry in the set of physical teachings, strongly supported by both Corbino and Garbasso. Fermi was called by Corbino in Rome, Persico by Garbasso in Florence. It is worthwhile noting here the position of Pisa in the years following the end of the war, a point of excellence with the high-level teaching of Puccianti in Experimental Physics in the wake of Battelli and with the school of Mathematics conducted by Luigi Bianchi after Ulisse Dini. This favourable situation was rewarded by the presence of a number of very good students, of whom three were to play a key role in the development of the Italian school of Physics and in particular of the groups of Florence and Rome: Enrico Fermi, Franco Rasetti and later Gilberto Bernardini. The fourth personality in this context was Enrico Persico, “laureato” and assistant of Corbino in Rome, familiar with such mathematicians as Tullio Levi Civita and Guido Castelnuovo, and a theoretician with a sense for experiments.

Table II highlights the parallel lives of Persico, Fermi and Rasetti as young men. The friendship between Persico and Fermi begins during the Liceo (secondary school) in Rome, that between Fermi and Rasetti during the University in Pisa. The relationship among the three, practically self-taught in the fields of new Physics, is well described by the set of their scientific articles covering the years (1921-1926).

(²²) The third winner was Aldo Pontremoli, called by the University of Milan, where he founded the Physical Institute, and disappeared in the Arctic in the disaster of the *Italia* dirigible in 1928. Pontremoli, born in 1896, was an assistant of Corbino around 1920 and signed a paper on the mass of radiation in an empty space with Fermi [23].

TABLE II. – *The parallel lives of Persico, Fermi and Rasetti as young men.*

Name	Born	Liceo	Laurea	1st Appointments
Persico	July 9, 1900 Rome	July 1917, Rome	Nov. 1921	'21-'24 Rome assistant (Corbino) '24-'26 Rome professor "in charge" (Corbino)
Fermi	August 10, 1901 Rome	July 1918, Pisa	July 1922 ^a	'22-'24 Rome professor "in charge" (Corbino) '24-'26 Florence professor "in charge" (Garbasso)
Rasetti	July 10, 1901 Rome	July 1918, Pisa	Dec. 1922	'22-'26 Florence assistant (Garbasso)

(^a) "Normalista".

When in 1926 Fermi goes back to Rome (and Persico goes from Rome to Florence), Rasetti follows Fermi, as assistant and “aiuto” to Corbino, and in two years will become professor of Spectroscopy (with an important programme on Raman effect). Corbino will attract around the personality of Fermi more promising students: Emilio Segré (1905-1989), Ettore Majorana (1906-1938), Edoardo Amaldi (1908-1983), the group of Rome is formed.

The story of Florence is less simple, but also here Garbasso was able to attract outstanding young people and build a successful group. One must underline once again the action of the two men who were instrumental in those achievements. Both Corbino in Rome and Garbasso in Florence opened their institutes to the best young physicists emerging from Italian universities in those years, several of them from Pisa. This is a recognisable policy: both use their scientific stature and their position in public administration in order to build “schools of Physics” based on the work of young individuals of precocious capacity and qualification, with a keen interest and a fresh understanding of the “new” Physics, which placed them above the average culture of the contemporaneous academic establishment. In the fifteen years or so after the end of the war the two groups were unusually close, with an effective exchange of persons and of knowledge, setting up connections and friendships which would last in time.

5. – The Group of Arcetri and the dawn of Cosmic Ray Physics in Italy (and not only that)

5.1. *A good teaching staff and a good set of students.* – 4 December 1987 was the 80th birthday of Giuseppe Occhialini. On that occasion the Physics Department of the University of Florence organized a round table, with Paolo Blasi as moderator, with the presence of (in order of age) Bruno Rossi, Gilberto Bernardini, Giuseppe Occhialini and Daria Bocciarelli, the four surviving personalities of the “Group of Florence”. Edoardo Amaldi took part in the round table and Manlio Mandò, a student in Florence from 1931

and a witness to the last part of the life of the group, opened the session illustrating the following tables III and IV⁽²³⁾.

All the contributions showed how deeply felt, after so long, was the recollection of that short stretch of years, short but so full of ambitions, hopes, strength, joy of being a part of a significant common effort towards “scientific truth” and overall friendship. Mandò and the external witness Amaldi defined that feeling “the spirit of Arcetri”. What follows is an attempt to present the “administrative” scenario and the meaning of the word “school” as applied to the group.

Table III shows the evolution of the staff of the Physical Institute under the direction of Antonio Garbasso from 1913 to 1933, and of Laureto Tieri⁽²⁴⁾ to 1938, when the racist campaign sparked off by Mussolini led to the 1938 laws, which expelled Jews from one day to another from the Italian scientific community. The notes give details about the fate of Arcetri’s actors when she or he left the group. Table IV lists the “laureati” in Physics (plus some in Mathematics) after the coming into operation of the “corso di laurea” in 1924. The table shows also that the “corso di laurea” provided with fresh young personalities both the Physical Institute and the Astrophysical Observatory, this being one of the successful results of the policy of Garbasso and Abetti.

It is interesting to examine in tables III and IV the four academic years from 1926 to 1930, the years of Persico. For one year Ronchi is the only assistant of Garbasso, becoming later “aiuto”, but he is engaged in his effort to revive the Laboratorio di Ottica e Meccanica (see sect. 3’2) and is not in the least interested in the “new” Physics introduced by Persico. On the other hand the first students are already in their second year of the regular corso di laurea, among them Giuseppe Occhialini and Francesco Scandone, who will be joined year after year by Giulio Racah, Daria Bocciarelli, Beatrice Crinò, Lorenzo Emo Capodilista. Then the turning point of the arrival of Bruno Rossi, Fall 1927, and of Gilberto Bernardini, Fall 1928: the Group of Florence comes to existence⁽²⁵⁾.

⁽²³⁾ From the contribution of M. Mandò to the Round Table 1987, unpublished. The original Tables are integrated and slightly modified with added notes for the purpose of the present work.

⁽²⁴⁾ L. Tieri (1879-1952) “laureato” in Rome 1903 and assistant of Blaserna and then of Corbino. Known for his experiments on the Hall effect in Bismuth, is co-author of the first paper (experimental!) of Persico [24]. From 1924 Chair Professor of Experimental Physics in Messina. From 1933 in Florence in the place of Garbasso. Retired in 1949.

⁽²⁵⁾ Bruno Rossi (Venice 1905; Cambridge Mass. 1993). Among the rich set of biographic material one may choose the autobiography [25] and [26]. From Venice to ill-equipped Padua and Bologna: the happy encounter with Rita Brunetti, “the only person who taught him some Physics” and supervisor of his “laurea”. Brunetti recommends Rossi to Garbasso, who promptly accepts him as assistant.

Gilberto Bernardini (Fiesole 1906, La Romola (Florence) 1995). A good biography does not exist as yet, in particular for the first period of his scientific activity. See Mandò, [8] p. 613, the biographic sketch of the Accademia dei Lincei and Giorgio Salvini [27]. “laureato” *cum laude* in Pisa with Puccianti, 1928, “normalista”, working at first in a small optical industry in Florence, unhappy researcher in the first year of the Istituto Nazionale di Ottica. Attracted by

One after the other the best students get the “laurea” and find a position in the institute through the interest of Garbasso and of Persico. The theses of “laurea” are all on experimental subjects related to the researches initiated by Rossi and Bernardini (except Racah, see table V). But experimentalist Rossi and student Racah collect the first notes from the lectures of Persico, first published in Florence 1929⁽²⁶⁾. As a result Rossi will be professor “in charge” of Fisica Teorica when Persico leaves for Turin, and Racah will inherit that position when Rossi wins the professorship and goes to Padua.

Table V summarises the initial steps of the “young Arcetrini”: the names are those which appear in all the papers published from 1928 to 1937, when, after the death of Garbasso, the winning of professorships and the political situation led eventually to the dispersal of the group.

A facet of the behaviour of these young people is their quick integration in the group since students. This is not only because of the enthusiasm of the leaders, Rossi and Bernardini, who would share their work with the students. They took profit also of two important assets which are frequently referred to in their recollections: the weekly reading of the leading international journals promoted by Persico and sustained by Bernardini with his characteristic zeal, and the Astrophysical, Physical and Mathematical Seminar promoted by Giorgio Abetti. In this way all of them were made aware of the more recent developments in the ongoing physical research; also they became acquainted with leading scientists who were happy to visit Arcetri through the international connections established by Abetti⁽²⁷⁾.

A third important asset was the position of Garbasso in the Italian scientific environment, which gave him the possibility of providing scholarships for stays in leading foreign (mainly European) laboratories. It is likely that these circumstances largely compensated for the scarcity of financial means: those ambitious young men were striving after “fundamental” problems in the investigation of the physical reality (“... of the secrets of nature”, as Bruno Rossi puts it humorously in his autobiography), but they were also enough well-informed, perceptive enough, and wise enough, to identify subjects which would not involve large expenses in costly instrumentation⁽²⁸⁾.

Another facet of the behaviour of the “Arcetrini”, to a certain point different from the behaviour of the group of Rome, is pointed out by Guido Tagliaferri who reports [28] a precise remark of Occhialini: *“The presence in Arcetri of Enrico Persico and the arrival of the newly “laureati” Bernardini from Pisa and Rossi from Bologna as assistants made possible the formation of a group of enthusiastic young physicists. The [scientific] interest*

the lectures of Persico, gets a position as “extra” assistant to his course of Meccanica Razionale. In 1930 Ronchi quits the Physical Institute and Bernardini becomes assistant of Garbasso (Rossi becoming “aiuto”).

⁽²⁶⁾ These notes will become the first draft of the well known treatise by Persico, *Fondamenti di Meccanica Atomica* (Zanichelli, Bologna) 1936.

⁽²⁷⁾ G. Occhialini, private communication to A. B. and contribution to the Round Table 1987. Also Bruno Rossi, *ibid.* and [25, 26].

⁽²⁸⁾ G. Occhialini, private communication to A. B. and contribution to the Round Table 1987.

TABLE III. – *Teaching staff of the Physical Institute of Florence 1913-1937, with A. Garbasso director from 1913 to March 1933, L. Tieri from Fall 1933.*

Academic Years	“Aiuto”	Assistant	2nd Assistant	Other Teachers
1913-17	A. Lo Surdo ^a	Rita Brunetti	–	A. Abetti
1917-18	–	Rita Brunetti	–	A. Abetti
1918-20	A. Occhialini	Rita Brunetti	–	A. Abetti
1920-21	A. Occhialini ^a	Rita Brunetti	V. Ronchi	A. Abetti
1921-22	–	Rita Brunetti V. Ronchi	–	G. Abetti
1922-24	Rita Brunetti	V. Ronchi	F. Rasetti	G. Abetti
1924-26	Rita Brunetti ^a	V. Ronchi	F. Rasetti	G. Abetti E. Fermi ^b
1926-27	–	V. Ronchi	–	G. Abetti, E. Persico ^c
1927-28	V. Ronchi	F. Olivieri	B. Rossi ^d	G. Abetti E. Persico
1928-30	V. Ronchi ^a	F. Olivieri	B. Rossi	G. Abetti E. Persico G. Bernardini ^d
1930-31	B. Rossi ^e	G. Bernardini	G. Occhialini	G. Abetti
1931-32	B. Rossi ^e	G. Bernardini ^f	G. Occhialini	G. Abetti
1932-33	G. Bernardini	G. Occhialini	L. Emo Capodilista Daria Bocciarelli ^h	G. Racah ^g
1933-35	G. Bernardini	G. Occhialini	L. Emo Capodilista ⁱ Daria Bocciarelli	G. Racah
1935-37	G. Bernardini ^j	G. Occhialini ^k	Daria Bocciarelli ^l	G. Racah ^m

(^a) The events concerning Lo Surdo, Brunetti, A. Occhialini, Ronchi, Rasetti, Fermi till Academic year 1925-26 have been accounted for in sect. 4'1 and 4'2 (see sect. 3'2 for Ronchi).

(^b) Professor “in charge” of Meccanica Razionale and Fisica Matematica (see table I).

(^c) Chair Professor of Fisica Teorica and “in charge” of Meccanica Razionale. From 1930-31 in Turin. From 1950 in Rome.

(^d) See table V.

(^e) Also professor “in charge” of Fisica Teorica in the place of Persico. From 1932-33 Chair Professor of Experimental Physics in Padua and director of the Institute of Physics. From 1938 in Copenhagen, guest of Niels Bohr, then in Manchester with P. M. S. Blackett and then in the United States.

(^f) Professor “in charge” of Meccanica Razionale.

(^g) Professor “in charge” of Fisica Teorica in place of Rossi.

(^h) “Extra” assistant of Garbasso.

(ⁱ) From 1935 to 1946 in the United States with a scholarship at Berkeley. Back to Italy he leaves research.

(^j) From 1937-38 Chair Professor of Experimental Physics in Camerino. From 1938 in Bologna, also director of the Institute. From 1947 in Rome.

(^k) From 1937 in Brazil. From 1944 in Bristol and, 1948, in Brussels. From 1949 Chair Professor in Genoa and from 1951 in Milano.

(^l) From 1937 at the Physical Laboratory of the Istituto Superiore di Sanità in Rome with G. C. Trabacchi.

(^m) From 1937-38 Chair Professor of Fisica Teorica in Pisa. From the end of 1938 at the Weizman Institute in Israel.

TABLE IV. – “Laureati” in Physics from academic year 1928-29 to 1937-38, “Corso di Laurea” established in academic year 1924-25.

Academic Year	Names
1928-29	Londei Luisa, Marconi Rita, Panerai Tullia, Zini Rodolfo
1929-30	Colacevich Attilio ^b , Occhialini Giuseppe ^a , Romani Abigaille, Francesco Scandone ^c in Mathematics: Calamai Giulio ^b in Chemistry: Franchetti Simone ^a
1930-31	Genovìè Gino, Racah Giulio ^a , Righini Guglielmo ^b
1931-32	Baroni Ermanno, Bocciarelli Daria ^a , Caponi Pier Giovanni, Mari Giovanni Antonio in Mathematics: Foà Alberto
1932-33	Castellani Giuseppe, Cipriani Edvige, Crinò Beatrice ^d , Emo Capodilista Lorenzo ^a in Mathematics: Sestini Giorgio
1933-34	De Benedetti Sergio ^a , Francese Clara
1934-35	Mandò Manlio ^a
1935-36	Castelli Iris, Fracastoro Mario ^b , Persano Aldo, Ricci Elena, Serafini Francesco
1936-37	De Seras Luigi
1937-38	Barsotti Nedda, Landini Oliviero ^d , Orzatesi Giuseppe, Pagani Lina

(^a) Physical Institute (see table III, table V and text).

(^b) Astrophysical Observatory (see sect. 3.1).

(^c) “Istituto Nazionale di Ottica” and then industry (see sect. 3.2 and table V).

(^d) Industry.

of the Laboratory shifted from spectroscopy to nuclear physics and cosmic rays. So, 1927-1928, the School of Arcetri was born.” [29]. Tagliaferri writes: “With the word “school” used by G. O. one should not understand a group of followers of a “maestro”, but rather an informal community of scholars in the same discipline, who share the scope of its advancement, and to that scope they address the investigations of each one of them, using freely the results”. That this was the case is shown by considering the whole of the papers published by the members of the group from 1930 to 1937: most bear only one signature, but all represent the results of a shared knowledge. G. Occhialini provides an interesting addition to Tagliaferri’s commentary. In his words: “the absence of scientific guide by Garbasso was important to train the muscles of Rossi and Bernardini”⁽²⁹⁾. Politician Garbasso was not only a passionate man of science, but also wise and generous enough as to let the intelligence and fantasy of his young researchers free, giving them his constant support in practical problems and encouraging them to publish quickly their results, which he was happy to present in the *Rendiconti dell’Accademia dei Lincei* and in the journal of the CNR, *La Ricerca Scientifica* (see note ⁽²⁹⁾).

⁽²⁹⁾ G. Occhialini private communication to A. B., 1987.

TABLE V. – *The initial steps of the “young Arcetrini”.*

Name	Born	Laurea	First appointment
Rossi	1905, Venezia	1927 Bologna	Fall 1927, 2nd assistant of Garbasso
Bernardini	1906, Fiesole	1928 Pisa	Fall 1928, “extra” assistant of Persico (see note ⁽²⁵⁾)
Occhialini	1907, Fossombrone	1929 Firenze	From 1930 2nd assistant of Garbasso
Scandone ^a	1909, Firenze	1929 Firenze	INO, and then Officine Galileo
Racah ^b	1909, Firenze	1931 Firenze	Fall 1932, professor “in charge” of Fisica Teorica
Bocciarelli ^c	1909, Firenze	1931 Firenze	From 1932 “extra” assistant of Garbasso
Crinò ^d	1913, Firenze	1933 Firenze	Laurea in Chemistry and Officine Galileo
Emo Capodilista ^e	1909, Firenze	1933 Firenze	From 1933 2nd assistant of Garbasso
De Benedetti ^f	1912, Firenze	1933 Firenze	in Padua with Rossi
Mandò ^g	1912, Terni	1935 Firenze	in Palermo with Segré

The following list points out the papers which were the result or the premise of the thesis of “laurea” made under the supervision of members of the staff:

(^a) Persico E. and Scandone F., “L’effetto Hall con elettrodi estesi”, *Rend. Accad. Lincei*, **10** (1929). This paper was splitted into three parts: nota prima 238-249; nota seconda 361-368; nota terza (Scandone only author) 437-440. A very precocious student, after the thesis Scandone finds a position at the Istituto Nazionale di Ottica and then in Industry, becoming soon the director of the Officine Galileo.

(^b) Rossi B., Racah G., “A proposito di un’osservazione di Stark sulla realtà del moto assoluto”, *Il Nuovo Cimento*, **6** (1929) 317.

(^c) Bocciarelli D., “A hard component of the beta-radiation of Potassium”, *Nature*, **128** (1931) 347.

(^d) Rossi B., Crinò B., “Le anomalie di assorbimento della radiazione penetrante”, *Rend. Accad. Lincei*, **15** (1932) 741. A very precocious girl student, Beatrice Crinò shifted her interests to Applied Physics.

(^e) Bernardini G., Emo Capolista L., “Sulla radiazione gamma del Po+Be”, *La Ricerca Scientifica*, **2** (1935) 17.

(^f) Bernardini G., De Benedetti S., “Misure di assorbimento della radiazione penetrante secondo diverse inclinazioni zenitali”, *La Ricerca Scientifica*, **2** (1933) 73.

(^g) Bernardini G., Mandò M., “Sulla disintegrazione del Berillio per azione dei raggi gamma”, *La Ricerca Scientifica*, **2** (1935) 38.

5.2. The science and the scientists. – A proper account of the scientific results is beyond the scope of this paper. What follows is intended rather to shed some light on the attitude towards research and on the efficiency of the “modus operandi” of the “informal community” of Arcetri.

Giuseppe Occhialini states that with the advent of Rossi and Bernardini the interests of the Laboratory shifted from spectroscopy to nuclear physics and cosmic rays (see [29] and note ⁽²⁹⁾). The very fact of this reorientation is an indication of the quality (of the curiosity) and of the ambition of Bernardini and Rossi, in their attempt to attack research along “new” lines, new at least in the Italian environment, these lines being typically of

an experimental kind, associated with the development of “new” instruments⁽³⁰⁾.

The first attempts are daring but not successful: an experimental verification of the corpuscle-wave nature of electrons [30] and a spectroscopic determination of the chemical composition of the cometary tail, a subject which reveals an incipient astrophysical interest⁽³¹⁾. And then in 1929, the cosmic rays. The story has been told mainly in the recollections of Bruno Rossi and Giuseppe Occhialini⁽³²⁾, but some details perhaps are still missing. It has not been possible up-to-now to find the original thesis for the laurea of Occhialini, not even the title (a research is in progress at the Archives of the University of Florence). But a testimony of Livio Scarsi, from the very words of Occhialini sometime in the late eighties [32], is that Augusto Occhialini suggested the subject of cosmic rays to his son Beppino. Augusto was familiar with several American as well as German physicists, in particular Millikan, busy in propounding his theory of gamma-rays emitted in primitive nucleosynthesis, and Kohlhörster, who was working with Bothe in experiments for cosmic ray detection with Geiger-Müller (GM) counters (and with a coincidence method). The point is that the thesis work of Beppino contained a “tesina” (extra contribution) presenting the results of Bothe and Kohlhörster, just appeared in *Zeitschrift für Physik* [33]. Rossi was not moved by Millikan’s theory, but the paper of Bothe and Kohlhörster awoke Rossi’s understanding of new, different, features of cosmic rays and of the possibility of performing new, critical, measurements on them. Rossi obtained quickly a scholarship from the CNR and spent the summer months of 1930 in Berlin at Bothe’s Laboratory. Back to Arcetri with the good recipes, he “put himself immediately at work” with his mates in Arcetri, first of all on the production of GM counters⁽³³⁾. Rossi understood that these were the right detectors apt to open a new field of research, a new chapter of Physics. Furthermore, they were not very costly, which fulfilled one of the requirements of an enthusiastic but poorly financed Laboratory. But Rossi did more than that: with astounding efficiency he invented and realised within the year his coincidence method based on the use of thermionic valves, the “circuitto alla Rossi” [34]⁽³⁴⁾. From then on the results follow one another, giving Rossi the possibility

⁽³⁰⁾ This is not surprising in the case of Rossi, a grateful pupil of Rita Brunetti, who maintained that the history of instruments coincides with the history of Physics (see note ⁽¹⁹⁾). But also Bernardini was born in the experimentalist environment of the Pisa of Battelli and Puccianti.

⁽³¹⁾ Minor contributions of Rossi in 1929 refer to the Raman effect, a spectroscopic subject well in the reach of the Laboratory in Arcetri: in that very year Rasetti would publish his important results on Raman effect taking up the field.

⁽³²⁾ A general information with extended references is found in Leonardo Gariboldi [31], who suggests that Giuseppe Occhialini was influenced in his intellectual formation by Battelli through the influence of his father. The suggestion appears to be correct, if only because of the strong feeling for the motherland, which is characteristic of the “marchigiani”. G. O., native of Fossombrone like his father, was educated in Florence and was one of the first students in the “corso di laurea” just started up by Garbasso.

⁽³³⁾ A lively description is in [25] and [26].

⁽³⁴⁾ “The first counting of the penetrating rays was in 1916 by Hess and Lawson, but Bothe and Kohlhörster used for the first time the wire corpuscle-counter, already in use in researches

of defending publicly against Millikan the notion of the corpuscular nature of cosmic rays in the “International Conference on Nuclear Physics” held in Rome, October 1931.

In a few years Rossi, Bernardini and the younger co-workers, in primis Daria Bocciarelli, produce about thirty short notes and papers on the absorption of cosmic rays (the Rossi curve and the multiple production [35]), their behaviour in the Earth magnetic field (zenithal effect⁽³⁵⁾, the first attempts to E-W effect) and related technical problems. The last paper of Rossi before leaving Arcetri for Padua is with Fermi: “Azione del campo magnetico terrestre sulla radiazione penetrante” [37]. This was also the first work of Fermi on cosmic rays: it also witnesses the connections of the Group of Florence with the Group of Rome.

The measurements on cosmic rays implied the use of ionisation chambers not only for the detection of the “primary” radiation but also for the measurement of environmental low-level ionising radiation. The GM counters offered a new efficient way for this kind of measurements: indeed they were already an important tool in the study of Radioactivity (see note ⁽³⁴⁾). The quick learning of the technique of GM counters is at the basis of the shifting of the interest of the laboratory from spectroscopy to cosmic rays and nuclear physics. So while Rossi proceeded with tireless energy in cosmic-rays investigation, he encouraged just “laureato” Beppino Occhialini to study weakly radioactive substances making use of counters⁽³⁶⁾. The result was the first paper of Occhialini, on the activity of rubidium with a magnetic spectrometer designed and built by him. The detector was a small counter with very thin (less than ten microns) Al wall [40]. The same apparatus was used by Daria Bocciarelli for her thesis and for her first paper on the radioactivity of potassium (see table V). In successive three papers Bocciarelli extends the measurements making use of a method of coincidences. The success of these measurements gives an idea of the skill in producing “refined” counters, in designing instruments and in conducting measurements.

In 1931 Bernardini was prevented by military duties to go to Cambridge, so Occhialini took his place and with a three-months scholarship of the CNR joined P. M. S. Blackett at the Cavendish Laboratory. His mission was to learn the technique of the Wilson cloud chamber mastered by Blackett. Occhialini added the technique of counters and of

on radioactivity and extraordinarily useful. This device sends in a circuit a short electric signal whenever it is traversed by a fast charged particle. The signals can be amplified and the amplified current can reach a counting device. The method of the corpuscle-counter has been adopted in the researches carried out in the Physical Institute of our University and allows, by suitably connecting two or more devices, investigations on absorption, direction, nature of the cosmic rays, which may be very difficult or impossible to perform with electroscopes.” This is how Persico announced the state of the art in Arcetri in the first days of November 1930, in his last opening address to the academic year before leaving for Turin. He quoted also by name “doctor Rossi of our Physical Institute” who was able to show “the formation of secondary electronic rays through a lead shield traversed by the primary radiation”.

⁽³⁵⁾ The first work on this effect was by G. Bernardini [36].

⁽³⁶⁾ See [38]. Bernardini has a contribution on the technique of magnetic spectrometers for slow electrons [39].

fast coincidences learnt from Rossi in Arcetri. The three weeks became three years and the results are contained in the four papers signed between 1932 and 1934 by Blackett, Occhialini⁽³⁷⁾ and later also by Chadwick [43]. Those in Arcetri joined in the enthusiasm for the success when reading Occhialini's letters describing his work (see note ⁽²³⁾).

In the meanwhile the neutron had been discovered opening a new window in the study of the atomic nucleus. The testimony of Rasetti is interesting [7]. He spent one and half year at Lise Meitner's Laboratory in Dahlem between 1931-32. Back to Rome he found that during his absence there were "*only vague talks*" about leading the Laboratory towards nuclear physics (the experimental work in progress was essentially in spectroscopy). He found however that Fermi was ready to shift from spectroscopy to a more exciting field: the actual work started when Rasetti built the first apparatus on the basis of his experience in Dahlem.

The Group of Florence had already abandoned spectroscopy since 1930. Perhaps also because of the departure of Rossi in the Fall of 1932, the investigation in nuclear physics was accelerated, the subject chosen being the production of neutrons from berillium. After some preparatory work (cfr. M. Mandò [8]), Bernardini spent a few months in 1934 at Lise Meitner's Laboratory with a scholarship of the Academy of Lincei (also Emo Capodilista was there in the same year). The result was a study of the reaction ($\text{Be} + \text{He} \rightarrow \text{C} + \text{n}$) with several papers, mostly in collaboration with Daria Bocciarelli (also with Emo and Mandò, see table V). A result of Bernardini and Bocciarelli was also the study of proportional counters⁽³⁸⁾.

But the balance of the Physical Institute was changing. A few months after the departure of Rossi for his professorship in Padua, badly ill Garbasso died, in March 1933. Rossi (already busy with the rebuilding of the institute and the preparation of the E-W experiment in Asmara) expressed later his deep gratitude to a man who had done so much for him and, to the last moments of his life, gave support to Bruno's project recommending its financing [25, 26]. After the death of Garbasso, Abetti became the provisional director of the institute with full satisfaction of the junior members of the staff, but the Faculty, suspicious of their independent attitude, after some hesitation called Laureto Tieri instead of waiting for Emilio Segré, the probable winner of the next competition (as suggested by Fermi). The direction of Tieri was not necessarily antagonistic towards the group [8].

But also the political situation was rapidly worsening. Differently from Rome, the members of the Arcetri Group were quite aware and felt politically involved, there were even harsh debates among them, that only the strong ascendancy of Garbasso had been capable of quenching. In the words of Occhialini: "*Garbasso was that not-existent animal, the intelligent, honest, good fascist ... with a smiling tolerance for the divergent*

⁽³⁷⁾ The first paper [41] was followed by [42]. From then on the signature of Occhialini as author becomes GPS.

⁽³⁸⁾ Contribution to the Round Table 1987 and ref. [44].

opinions of the junior persons"⁽³⁹⁾. In a climate made uneasy by the death of Garbasso, Occhialini, back from Cambridge and the Cavendish, made a big but unsuccessful effort to obtain an adequate financing for the construction of a Wilson Chamber⁽⁴⁰⁾. He would react more sharply than the majority of his friends to the cultural ambience which was leading Italy into wars and racism. So he decided to leave Italy for some time and joined Gleb Wataghin in Brasil, to lay the foundation of a modern school of Physics in São Paulo. He was back in Italy eight years later after his detours in Bristol and Brussels.

In fact 1937 was the year of the dispersal of the group. Bernardini became Chair Professor and, after one year in Camerino, settled in Bologna, where he was director of that Physical Institute till 1947. With his characteristic energy he succeeded in continuing his work in particular on cosmic rays. One of the last papers while still in Florence is with Simone Franchetti, a chemist (see table IV) who was appointed assistant in 1937 by Tieri, and was forced to abandon by the racist persecution. He was back at the end of the war.

Racah had built a very successful career in Theoretical Physics also through an intense relation with the theoreticians of the Group of Rome. He became Chair Professor in Pisa just in time to be forced to leave his position in 1938 and to emigrate to Israel at the Weizman Institute. Once filo-fascist, Racah perhaps remembered the hot discussions with Occhialini.

Daria Bocciarelli found a position at the Istituto Superiore di Sanità with Trabacchi and contributed to the success of that Physics Laboratory during long years, first in nuclear physics (the million Volt accelerator for neutron production), and then in Electronic Spectroscopy.

Lorenzo Emo Capodilista went, in 1935, to the United States for a stay at the Stanford Laboratory in Berkeley. He came back after a couple of years as agent of a firm for scientific instrumentation, abandoning active research.

Mandò after his laurea joined Emilio Segré in Palermo. When Segré was forced to leave, in 1938, Mandò joined Bernardini in Bologna. Then there was the bracket of the war, including a period in a prisoners of war camp. Finally, he returned to Florence and contributed, with Simone Franchetti (successor of Tieri in 1949), to the development of the post-war institute. Arcetri was coming to life again, and this together with the rest of Florentine Physics, no longer concentrated on the hills through the activity of the CNR Microwaves Centre, a remarkably successful achievement of Nello Carrara (a "normalista" and student in Pisa with Fermi and Rasetti) and the outstanding scientific and didactical work of Giuliano Toraldo di Francia.

5.3. The "spirit" of Arcetri. – The time is now to leave the stage to the actors, picking up a few significant quotations from the notes (unpublished as yet) of the Round Table, December 4, 1987 (see sect. 5.1). The quotations are from Bruno Rossi, Edoardo Amaldi and Giuseppe Occhialini.

⁽³⁹⁾ G. Occhialini, private communication to A. B., 1987.

⁽⁴⁰⁾ G. Occhialini, unpublished document.

Bruno Rossi: *"When I think back to my past life I feel as if the years in Arcetri were a dream, a magic experience, which left a permanent mark on all my life: I think that Daria, Gilberto and Beppo, here with us, can understand, can interpret what I am trying to say, and also those would understand that are not here any more, Racah, Emo, Scandone, Righini, Beatrice Crinò ... We were a small group of young people, just "laureati", in part still students. We were very different from one another as for familiar tradition, attitude, tastes, character, but we were united by strong friendship ties and by a common commitment to science. Other factors contributed to the special atmosphere created by these human relations, to what Mandò defined "the spirit of Arcetri". First the almost paternal attitude of the director, professor Antonio Garbasso, who would do his best to facilitate our work, using his political authority. Second, the proximity of the Observatory and specially of Giorgio Abetti who ... somehow ... took us physicists under his protection. Last but not least, the fascination of Florentine hills which would reassure our spirit and would allow our mind to sweep over the contingent practical problems of our work. I arrived in Arcetri ... full of enthusiasm for the new life which was to begin and with the ambitious intention of undertaking some kind of research which would contribute in a substantial way to the development of science. I found ... Gilberto who had the same intention..."*

Edoardo Amaldi: *"Corbino in Rome and Garbasso in Florence played a truly important role. Corbino was a self-made man ... of great intelligence and a clear vision of the scientific and organizational problems of the country. Garbasso was a man of remarkable culture ... very different from Corbino ... [But he had the same] very positive attitude towards the new Physics which was being born in Europe [in those years] ... and had the same will and ability to help the young physicists who would enter in those fields and produce scientifically ... Contacts and exchange of ideas between the groups of Rome and Florence were kept through relatively frequent visits of the Florentines in via Panisperna and of the Romans in Arcetri ... The Florentines would invite us to present our results in the Seminar ... established through the initiative ... of Giorgio Abetti ... I was particularly impressed [by him], an exceptional person endowed with an uncommon charm, who would ask appropriate and interesting questions on any subject with unsurpassable grace and politeness ... I was coming from the Institute of via Panisperna, which was beyond doubt a very well functioning and attractive place. In Arcetri the atmosphere was very different: the interest for music and beauty arts would appear frequently during the work ... or in intervals such as that for having tea, which was prepared by Daria for everybody. An almost imperceptible romantic climate would waft in Arcetri, while in via Panisperna extra-scientific interests were almost exclusively mountain trips and nature ... and international contemporary literature, a field in which Rasetti surpassed everybody ... Also the research subjects were rather different, but all these diversities between Rome and Florence were a reason of attraction between the members of the two groups."*

G. Occhialini: *"Garbasso, Abetti and Persico ... these persons had in common very important qualities: they [belonged to the category] of professors and scientists who were loved and respected, with no fear, no feeling of awe in front of them ... [and furthermore] a common style, a common attitude towards what would be called Europe ... those aristo-*

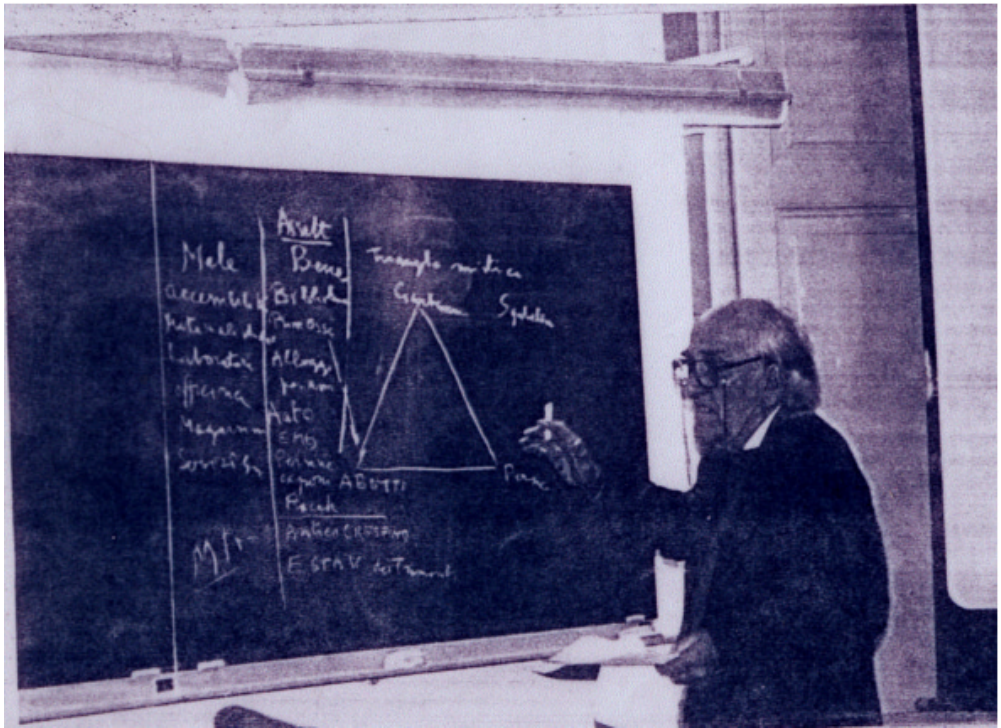


Fig. 1. – Beppo describing “male” (Bad) and “bene” (Good) of Arcetri (December 4, 1987) and the *mystic triangle* made of Abetti, Garbasso and Persico. The writing on the black board is as follows. Bad: accessibility, didactic material, laboratories, workshop, storeroom, general services; Good: library, proximity of the Observatory, accomodation for assistants, motorcars (Persico, Racah, Caponi, EMO: these persons would collect colleagues and students from downtown) Antico Crespino (a “trattoria” at walking distance from the institute), ecstasy of sunsets.

cratic sages probably had an influence on the members of the laboratory in a notable lack of aggressivity ... [Abetti's] Seminar would bring the name of Florence where it was unknown ... people came from everywhere, such as Hans Bethe, same age as Rossi, already involved in what was to become the Physics of fields ... the Seminar was a high-level club ... but it was not only for senior or junior researchers, but also for students who were striving to become researchers ... So, together with the regular reading of journals promoted by Persico, junior people were put in the condition to have access with up-to-date scientific information to such exclusive Institutes as Rutherford's Cavendish.”

The contribution of Occhialini was made specially amusing and touching by his “Table list of Bad (Male) and Good (Bene) in Arcetri” that he draw on the blackboard in his characteristic humorous way: one misses Beppo's sharp to-the-point commentary.

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* * *

The intention of the authors was to give an idea, however sketchy, of the cultural premises from which the group of Florence emerged with its peculiar characteristics. A parallel reference to the group of Rome appeared necessary to make clear similarities and differences, also because of the rich human and intellectual exchanges between the two groups in the few years of a surprisingly productive revival of Italian Physical Sciences, with results lasting in time.

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Giuseppe Occhialini and the history of cosmic-ray physics in the 1930s: From Florence to Cambridge

MARTHA CECILIA BUSTAMANTE

Equipe Rehseis, CNRS UMR 7596-Université Paris7 Denis-Diderot, Paris, France

1. – Introduction

Giuseppe Occhialini's stay in Cambridge at the Cavendish laboratory spanned three years, from 1931 to 1934, although he originally had left Italy for England with the idea of staying only three months. This "Cambridge period" turned out to be most important in his scientific life and established him as a confirmed researcher. The work he performed on cloud chambers, cosmic rays and the positron, with the Cavendish physicist Patrick Maynard Stuart Blackett made him one of the leading figures in the international scientific scene of the thirties. It contributed an essential part of our present-day physical knowledge.

What did Occhialini find at his arrival in Cavendish? How did he organize his life? In what scientific and instrumental context did he work? What could he do with the scientific background he had and within the new Cambridge framework? How did he interact with Blackett? How did he come to contribute major discoveries? Without pretending to completely answer those questions, I will try to recall significant aspects and moments of Occhialini's visit to Cavendish [1]. I will end by referring to Occhialini's return to Italy and to his trip to Brazil.

2. – To the Cavendish

It was in July 1931 that Giuseppe Occhialini eagerly joined the Cavendish laboratory. As a contemporary witness noted, in those days this laboratory was, "with its 'Nursery' and 'Garage'... a dingy, dirty and dismal sort of place built in the nineteenth century" [2].

It also was a place where “everything was improvised... the budget was minimal and expenditure was very tightly controlled. In general, at the Cavendish all the physicists had to make all their apparatus for themselves” [3]. Occhialini easily adapted to these conditions. He had a special gift for constructing devices and had a good training in instrumental work.

Occhialini went to the Cavendish thanks to a CNR (Italy’s National Research Council) fellowship that had been intended for Gilberto Bernardini, who instead had to serve in the Army. The idea of Occhialini’s visit resulted from a meeting that had taken place in Berlin between Bruno Rossi and Blackett. These two physicists had developed a friendship in this occasion. As Rossi was interested in Blackett’s knowledge of cloud chambers, they agreed to send one of Rossi’s pupils to Cambridge to learn this technique. Occhialini enthusiastically seized this opportunity, since, following the Roman physicist Emilio Segré, he had been deeply interested in imaging devices and dreamt of building a big cloud chamber [4].

Occhialini was a vivid and perspicacious man, who had engaged scientific studies with some reticence but quickly convinced himself it was the right choice. He came from the University of Florence, where in 1929 he was a fresh undergraduate with a degree in Physics based on a dissertation work in spectroscopy [1]. This work was supervised by his father Raffaele Augusto Occhialini, a well-known physicist and university professor, whose strong character played an important role in young Giuseppe’s intellectual and moral development. Occhialini then worked on Geiger-Müller counters and cosmic rays under the supervision of Bruno Rossi, a young and rising star who had joined the University of Florence. He was also initiated into the mysteries of the new quantum physics by Enrico Persico, another brilliant figure, schoolmate of Fermi and professor of theoretical physics at the same University of Florence.

After his graduation, Occhialini became a voluntary research assistant at the Institute of Physics located on the Arcetri Hill, a place Galileo Galilei had once called his home. He became a temporary research assistant the following year, and subsequently obtained a permanent appointment. That was a rather good situation for a young Florentine physicist in the late twenties. At that time the Institute of physics of the University was just emerging. The conditions were poor. The financial situation was precarious, despite the efforts of the director Antonio Garbasso. As Bruno Rossi, then Garbasso’s assistant, recalled years later, the Institute was always late in paying its electric bills. The only reason why electricity was not cut off was that the Institute director was the Major of the city [5].

At the Cavendish, Occhialini found himself in a very new and different world. This laboratory was much bigger than the Institute of Physics of the University of Florence. Founded in 1874, it had soon become a place of excellence within the context of a flourishing British scientific development. By the end of the century, the Cavendish laboratory had become one of the premier world centres in experimental physics. When Occhialini arrived, the laboratory was under the direction of Ernest Rutherford, a very prominent scientific personality with a prestige comparable to that of his predecessors James Clerk Maxwell, Lord Rayleigh, and Joseph John Thomson. Rutherford dominated the Cavendish both physically and intellectually [6]. One can imagine that Occhialini,

with his vivid nature and enthusiasm, was quite taken by Rutherford's strong personality. We will see, however, that it was Blackett who deeply and permanently impressed him.

In a short autobiography of 1974, Occhialini recalled the Cavendish at the time of his arrival. Cavendish scientists pivoted around Rutherford and his assistant James Chadwick. Blackett already was amongst the most distinguished ones. Occhialini also noted that many researchers came there from all over the world. The laboratory had become a place of pilgrimage for outstanding young physicists interested in radioactivity and atomic physics [7].

Occhialini enjoyed a friendly community through the academic clubs that met at the Cavendish and elsewhere in Cambridge. These clubs consisted in selected people that met for the purpose of political, intellectual, or scientific discussion. Probably thanks to Blackett, Occhialini was admitted to the Kapitza Club and participated to its session of 1932-1933. This club was a seminar group founded by the Russian physicist Peter Kapitza in 1922. "The original idea (of the seminar) was to provide a forum for unfettered discussion of current developments in physics, freed from ambitions that tended to characterize laboratory discussions, and for theoretical debate, for which there was no provision in the formal university system." Membership was limited and was decided by election. Occhialini thus joined a list of prestigious names. The club also had distinguished foreign guests such as James Frank in 1924 and Niels Bohr in 1925. Originally the members were almost all, apart from Kapitza himself, primarily theoreticians, Paul Dirac and D. R. Hartree among them. As the years passed, membership increased and the Kapitza Club began to include eminent experimentalists. Blackett himself was a member. Contrary to other academic Cambridge clubs, such as the ∇^2V Club, which was mainly for mathematical physics, the Kapitza club "was one in which the barrier between theory and experiment was all but obliterated" [8].

Occhialini was lucky to come to the Cavendish laboratory at the time he did, for Kapitza's stimulating presence at the club had little time left. As is well known, the Russian physicist went to visit his homeland soon after the summer of 1934 and was unable to return. When Occhialini arrived, the club had held 377 meetings, all of them with Kapitza in charge [8]. The club's activities consisted of informal discussions and readings of papers. Meetings took place in College rooms, often in Dirac's room in St John's College, and once a week, probably on Tuesdays and after dinner. One can imagine how stimulating these meetings were for Occhialini.

3. – Electric counters and cloud chambers

In the Cavendish Laboratory, Giuseppe Occhialini joined P. M. S. Blackett. It was maybe the first time the two physicists met. Occhialini was 24 years old and Blackett ten years older. Blackett was a member of Rutherford's teaching staff, having been appointed as a university lecturer. Occhialini and Blackett each had their own instrumental and experimental specialties. Occhialini was an expert on Geiger-Müller counters, Blackett was a world authority in cloud chambers.

Patrick Blackett was an experimentalist, a scientific “Jack of all trades”, as he characterized himself. Instruments and devices were for him the means through which he could illustrate his gifts. So, for a physicist like him, Occhialini’s arrival to Cavendish presented promising prospects. In Italy Giuseppe Occhialini had been trained in a very modern technique. Geiger-Müller counters had only been invented by H. Geiger and W. Müller in 1928. In Florence, Rossi had the idea of placing two counters in coincidence in order to detect cosmic ray events. Occhialini’s early research and first paper, the only one he had published by the time he went to Cambridge concerned this subject. There Occhialini presented a magnetic spectrograph for β -rays emitted by weak-radioactive sources [such as Rubidium and Potassium], a device used in one of the first applications that the Florence group made of the counter technique [9].

Geiger-Müller counters consist of a metal cylinder with a thin metal wire stretched along the axis and held by insulators. The cylinder is filled with a gas at a pressure of a fraction of an atmosphere and a potential difference is applied between the tube and the wire. An energetic charged particle entering the cylinder can ionize it and induce an avalanche of electrons that reached the wire until full discharge. As Bruno Rossi and his pupils in Italy quickly understood, Geiger-Müller counters are very sensitive, being able to react even to a single ionization anywhere in the tube. They were suitable for the detection of beta and gamma rays and were inexpensive. This is why the instrument was adopted by Bruno Rossi. It was just what a small laboratory like his needed [10].

In contrast, Geiger-Müller counters were not really in usage at Cavendish. However, as early as 1908 Rutherford and Geiger had collaborated in Manchester to devise an electric particle detector, which proved effective but not reliable [11]. Another detector of the same kind was used at the Cavendish towards 1930.

Blackett’s own speciality, the cloud chamber, was a typical Cavendish technique. C. T. R. Wilson, its inventor, was a Cavendish professor when he invented this device towards the end of the nineteenth century. Blackett had attended Wilson’s lectures on light just after the war [12]. The device resulted from the confluence of two distinct fields, ionic physics and meteorology. Wilson, who started his research in 1895, investigated the condensation of water drops in moist air and the reproduction of atmospheric phenomena: rain, hail, fog. Studying condensation phenomena he “intensified his efforts to see the drop formation itself” and thus invented a technique that rendered visible the path of ionizing agents [13]. The operation was based upon the property of ions to serve as centres for the formation of droplets from a supersaturated vapour. When a charged particle passes through a vessel containing a gas saturated with vapour, ions are formed all along the trajectory. By suddenly increasing the volume (sudden expansion), the vapour begins to condense around whatever ions are present in the chamber. A cloud of very minute droplets then appears showing the trajectory of the charged particle. This array of droplets can be observed visually and recorded photographically for quantitative work.

In 1923, Rutherford wanted Blackett to study nuclear reactions with the cloud chamber, hoping it would reveal more details of the nuclear collision process than the earlier technique of scintillations. This was a significant turn on Rutherford’s part. Indeed he had been using the technique of scintillations since the beginning of the century and

much of his Manchester work depended on it. This technique involved scintillations caused by the impact of the fast particles on a zinc sulphide screen and observed through a low-power microscope. Being visual, it predisposed Rutherford to use the equally visual cloud-chamber technique.

Blackett took over work of a Japanese student (Takeo Shimizu) who had already been looking for nuclear disintegrations with a cloud chamber. Blackett was a former naval officer with a strong education in the sciences and engineering arts. In the span of a few months in 1924 he managed to set-up a chamber which took half a million of photographs whereas Shimizu only had a few thousands. Blackett improved the suddenness and the repeatability of the expansion by means of a simple string device. He also arranged that the photograph was taken just as the expansion was completed. Among his numerous photographs he found a few representing alpha disintegration processes.

In the following years, Blackett devoted himself to the development of this automatic cloud chamber and to its application to the field of nuclear physics. By the end of the 1920s, he had become a leading figure among Cavendish physicists, and an international authority in cloud-chamber methods and nuclear physics. At the same moment, after almost ten years of collaboration, Blackett's collaboration with Rutherford came to an end [14]. So the way was free for him to switch over to cosmic radiation with the help of his Italian guest Occhialini.

4. – Cosmic rays

Perhaps Occhialini, who had been trained in cosmic-rays studies, suggested to Blackett to work on this subject. Or perhaps Blackett got the idea from Bruno Rossi during their Berlin meeting. Cosmic-ray physics was a relatively new branch of physics. It had not yet penetrated the Cavendish laboratory but had already received considerable attention in other centres.

Robert Millikan himself had given the name of cosmic radiation to the phenomenon discovered at the beginning of the century by the Austrian physicist Victor Hess. Millikan was a world authority on that matter. He had founded a laboratory in the California Institute of Technology in order to study the ultra-penetrating radiation as it was also named at the time. The idea was to improve observations on balloons with electroscopes and ionization chambers. Millikan's program, launched in the mid-1920s, required important resources. The observations were made out of doors, at sea level, at high altitude, on the top of the California mountains, and in the upper atmosphere. It was necessary to transport very fragile and heavy devices. In order to account for the wealth of results obtained in his powerful laboratory, Millikan proposed what he called the "birth cry theory", a set of more or less scientific conjectures about the origin and the nature of cosmic radiation. In particular, he believed that the cosmic rays could only be hard gamma rays. If the cosmic rays were charged particles, Millikan reasoned, they would not have sufficient energy to penetrate, as they did, the air and water equivalent of a few centimetres of lead. This hypothesis roughly agreed with empirical results.

Occhialini had another conception of cosmic rays to transmit to Blackett. Rossi and his students were aware of the experiment performed in Germany by Bothe and Kolhörster in 1929 that seemed to refute the gamma-ray hypothesis and instead suggested that the cosmic rays were mainly composed by charged particles. Part of their program was to confirm the latter hypothesis by improving the counter technique.

In their experiment, Bothe and Kolhörster had placed two Geiger-Müller counters one above the other a small distance apart and recorded the simultaneous pulses (coincidences) due to the passage of individual particles through both counters. They recorded the coincidences by connecting the wires of the two counters to two separate electroscopes. They obtained only doubles coincidences and the method was very cumbersome. In order to improve this technique, Rossi devised a neat valve circuit that easily recorded coincidences of any order [10]. It was mainly from this work that Occhialini acquired the expertise that he brought to the Cavendish.

5. – Collaboration with Blackett

Occhialini and Blackett together launched a new Cavendish project focused on cosmic-ray studies by means of cloud chambers. Physicists elsewhere had already demonstrated that cloud chambers were well adapted to the observation of the ultra-penetrating radiation. In 1927, Russian physicist Dimitri Skobel'tzyn, while investigating the β -rays from radioactive sources by means of a cloud chamber and a deflecting magnetic field, noticed a few tracks of unusually high-energy, "penetrating rays", which occurred in small groups [15]. He thus opened the door to programmatic observations of cosmic rays by using cloud chambers in magnetic fields. Pierre Auger in Paris, and Millikan and his student Carl Anderson in Caltech, launched such programs shortly afterwards.

To follow the way that Skobel'tzyn's observations had suggested was not an easy task for Blackett and Occhialini. In the area of cosmic radiation the received cloud chamber method had serious drawbacks. As it had a very short sensitive time and as the expansions were made at random, cosmic ray tracks were found only on a very small fraction of the photographs. Blackett and Occhialini's new set-up made use of Geiger-Müller counters for triggering the chamber, so that "particles of high energy took their own photographs". (See fig. 1 and fig. 2.) Occhialini brought to the project his expertise with counters and the coincidence method. The two physicists placed one Geiger-Müller counter above the chamber and another below, and arranged their coincidence circuit so that only particles passing through both counters, and therefore lying in the plane of the chamber, triggered the expansion. The average wait after the mechanism was set up was about 2 minutes. In a laboratory where electric counters were merely beginning to be used, it was a very innovative idea. It proves immensely successful one. The efficiency of the instrument in observing cosmic rays was improved by a factor of about 1000. We will see later which results they obtained.

No doubt, Blackett's background knowledge in cloud chambers together with Occhialini's knowledge in tube counters and coincidence method proved decisive to the outcome of the project. For instance, the circuit associated with the counters still presented

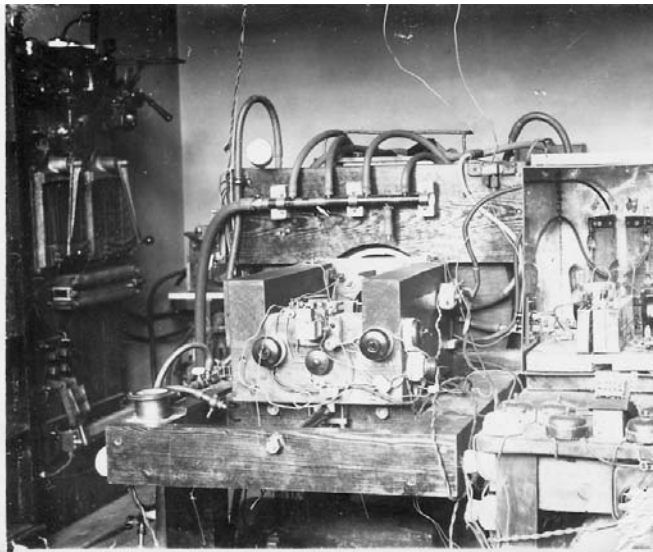


Fig. 1. – Blackett and Occhialini’s counter cloud chamber.

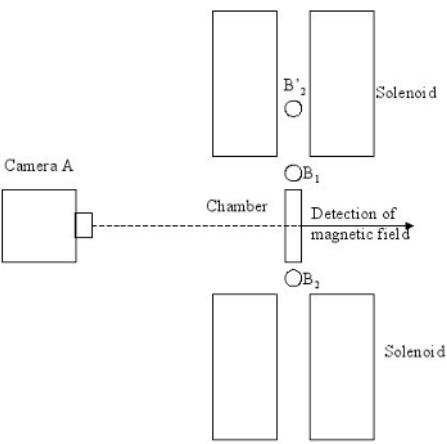


Fig. 2. – Blackett and Occhialini’s experimental set-up. B1 and B2 indicate the positions of the counters. (See P. M. S. Blackett and G. P. S. Occhialini, “Some photographs of the tracks of Penetrating radiation”, *Proceedings of the Royal Society, Ser. A*, **139** (1933) 699-727.)

difficulties and to make them work was not easy, furthermore, in the counter-controlled technique, the ray crosses the chamber before the expansion and distortion occurs before the drops are formed. The best way of dealing with this source of distortion is to make sure the ray has little time to diffuse before the expansion is complete. It requires the

chamber to expand promptly after the passage of the cosmic particle. Blackett made theoretical considerations about ion diffusion to know the time required to obtain tracks of no more than a given width.

Historical studies on the positron's discovery give us a detailed account of the work Occhialini and Blackett made at Cavendish with the novel chamber [16]. Fortunately, Occhialini could see his Italian grant extended (he also received an occasional financial support from Rutherford when he showed him the photographs of cosmic-ray showers). Blackett and Occhialini published four papers together, the two last of which were co-signed by James Chadwick. The first one is a letter to "Nature" dated 21 August, 1932 and entitled "Photography of Penetrating Corpuscular Radiation" [17]. This letter was the second Occhialini's scientific publications, appearing almost one year after the arrival of the physicist at the Cavendish. The authors describe the new cloud chamber triggered by counters and present a set of results. They do not talk about the several instrumental and experimental tests they must have done so the instrument would work as they wanted. They emphasize the speed of their technique: with the chamber placed in a magnetic field of 2000 gauss the average wait for the mechanism, as noted above, was about 2 minutes, at that time they obtained one hundred stereoscopic pairs of photographs, among them 59 showed the track of a single high-speed particle passing through both counters; 17 showed either multiple tracks of varying degrees of complexity, or else a single track passing through one and not through both counters; 24 showed no tracks. None of the straight tracks showed a measurable curvature thus indicating that their energy must have been greater than 600 MeV (if electrons) or 200 MeV (if protons). In 17 of the photographs showing multiple tracks, only about 10% were bent by the field, indicating energies of the order 2–20 MeV (if electrons) [17].

Blackett and Occhialini's second paper was communicated by Rutherford to the Royal Society five months later. It is an extension of the first work, with some very new results. It is a long article, 27 pages including fifteen photographs. This is the historical paper, "Some photographs of the tracks of Penetrating radiation" [18]. (See fig. 3.) As is well known, Blackett and Occhialini obtained photographs showing multiple tracks caused by showers of particles diverging from a region over the chamber. (See fig. 4.) Among them were tracks which the two physicists interpreted as being due to positively charged particles with a mass comparable with that of an electron rather than with that of a proton. By systematically analysing the curvature, range and ionization, they eliminated many other likely hypotheses and obtained evidence supporting the existence of positive electrons. They pointed out arguments in favour of the view that in the showers the main beam of downward-moving particles consisted mainly of positive and negative electrons in about equal numbers. At the same time, Blackett and Occhialini pointed out the relation between the particle they had observed and the anti-electron predicted by Dirac's relativistic theory of the electron.

As is also well known, Carl Anderson, Millikan's assistant at Caltech, preceded Blackett and Occhialini by a few months by presenting in a short article in the August 1932 issue of "Nature" experimental evidence for a positive electron [19]. In a non-triggered cloud chamber he had observed positive tracks corresponding to a mass much

Some Photographs of the Tracks of Penetrating Radiation.

By P. M. S. BLACKETT and G. P. S. OCCHIALINI, The Cavendish Laboratory,
Cambridge University.

(Communicated by Lord Rutherford, O.M., F.R.S.—Received February 7, 1933.)

[PLATES 21-24.]

1. *The Experimental Method.*

We have recently developed a method by which the high speed particles associated with penetrating radiation can be made to take their own cloud photographs.* By this means it is possible to obtain these photographs very much more speedily than by the usual method of making expansions at random. For when this latter method is used it is only on a small fraction of the photographs that a track will be found. The average number of photographs required to obtain one track will depend on the size and orientation of the chamber and on the effective time of expansion. The latter is not likely to be more than 1/20 second. From measurements with counters it is known that about 1.5 fast particles fall, from all directions, on 1 sq. cm. per second. Roughly consistent with these figures are the results found with cloud chambers. Skobelzyn† has obtained as many as one track every ten expansions, but in the work of Anderson‡ the number of tracks which were long enough to be suitable for energy measurements was only about 1 in 50 photographs. By our method, tracks are found on 80 per cent. of the photographs. We intend to give a full account of the technique of this method of photography in a separate paper, confining ourselves here to a rough outline only.§

A cloud chamber of diameter 13 cm. and depth 3 cm. is arranged with its plane vertical and two Geiger-Müller counters, each 10 cm. by 2 cm. are placed one above and one below the chamber so that any ray which passes straight through both counters will also pass through the illuminated part of the chamber, fig 2. An alternative arrangement is described on p. 719. The

* 'Nature,' vol. 130, p. 363 (1932).

† 'C. R. Acad. Sci. Paris,' vol. 195, p. 315 (1932).

‡ 'Phys. Rev.,' vol. 41, p. 405 (1932).

§ Mott-Smith and Locher 'Phys. Rev.,' vol. 38, p. 1399 (1931); vol. 39, p. 853 (1932) have found a correlation between the occurrence of these tracks and the discharge of a counter, and Johnson, Fleischer and Street. 'Phys. Rev.,' vol. 40, p. 1048 (1932) have used coincidences to operate the illuminating flash of a continuously working closed chamber.

Fig. 3. — Front page of Blackett and Occhialini's historical paper, *Proceedings of the Royal Society, Ser. A*, **139** (1933) 699-727.

less than that of a proton. Contrary to Blackett and Occhialini, Anderson did not refer to Dirac's theory. Although this theory was not easily accepted by Rutherford, Chadwick and other Cavendish physicists, Blackett and Occhialini, being familiar with Dirac's ideas and being in frequent contact with Dirac in the Kapitza club, did not hesitate to take it into account. As for the mechanism by which the showers might be produced, they suggested that the electrons were created during collisions and that non-ionizing links existed in the showers [18].

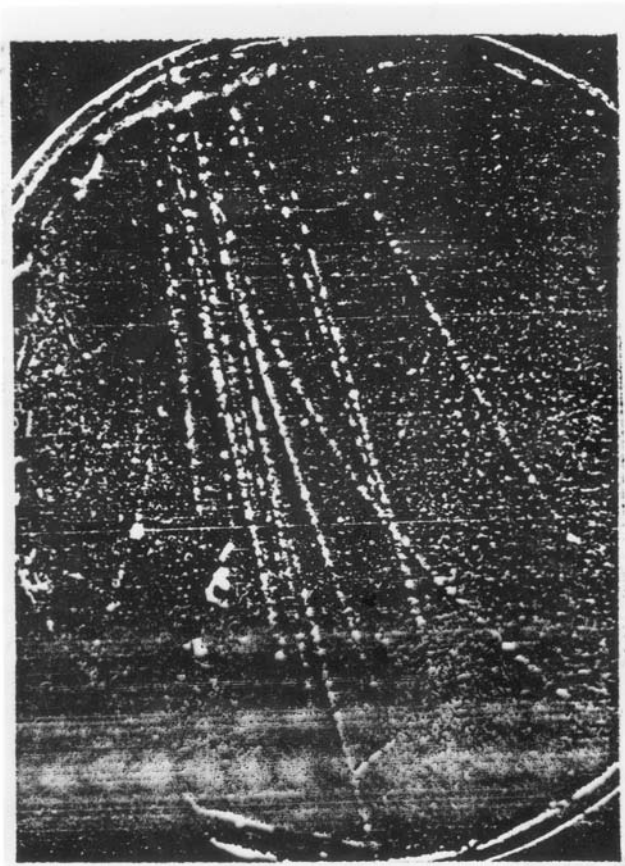


Fig. 4. – Stereoscopic counter cloud chamber photograph of a shower. Reprinted from P. M. S. Blackett and G. P. S. Occhialini, “Some photographs of the tracks of Penetrating radiation”, *Proceedings of the Royal Society, Ser. A*, **139** (1933) 699-727, Plate 22. Figures 2, 3, 4 are reproduced with kind permission of the Royal Society.

A few months later, Blackett and Occhialini oriented their search towards the production of positive electrons by means of radioactive sources. Chadwick joined them and brought his expertise on neutron sources [20]. The three physicists looked for positive electrons produced in the interaction of neutrons with matter. Their results appeared in

the April 1933 issue of “Nature”. A lead target (2mm thick) fixed inside the chamber was irradiated with a polonium source and a piece of beryllium, contained in a small container, placed close to the wall of the cloud chamber. The lead was thus exposed to the gamma rays and neutrons emitted from the beryllium. Measurements of the ionization and curvature of the tracks supported the view that positive electrons were being created in the lead target. No evidence was obtained about the manner of their production, whether caused by the neutrons or by the gamma-rays hitting the target [21]. In a matter of days, this question was settled by Curie and Joliot in Paris: their absorption experiments proved that the gamma-rays, not the neutrons, were responsible for the production of the positive electrons [20].

A second paper of Blackett, Occhialini and Chadwick, published in 1934, dealt with the positive electrons produced by the interaction of the nearly homogeneous gamma radiation from thorium C with matter. This was a natural extension of the earlier work using polonium as a source with beryllium. With the thorium C, they obtained 4000 tracks of positrons were studied [22]. This was the last Cavendish work to which Occhialini contributed.

Indubitably, Giuseppe Occhialini’s visit to Cambridge and his collaboration with P. M. S. Blackett were very successful. As he noted at the Memorial meeting for the English physicist held at the Royal Society in October 1974 and one of the very few non-scientific papers he wrote, it was “a wonderful time in Cambridge.” Occhialini thus befriended Blackett, a man he greatly admired. In his autobiography, he judges that besides his father, the person who most influenced him was Blackett. Occhialini spoke of Blackett’s clarity and integrity, his independence of mind and passionate sense of justice. Recalling his time at the Cavendish, he noted: “What not everyone had the chance to see was the passionate intensity with which Blackett worked. I can still see him, that Saturday morning when we first ran the chamber, bursting out of the dark room with four dripping photographic plates held high and shouting for all the Cavendish to hear *one of each, Beppe, one of each!*” [23].

The feelings between Blackett and Occhialini were reciprocal. Blackett recognized Occhialini’s scientific qualities as those on an experimentalist of the first order, with physical intuition and great skills and capacity at imagining devices. No doubt he also felt Occhialini’s brilliance, enthusiasm and charisma. In 1948 the physics Nobel Prize was awarded to Blackett, “for the development of the Wilson cloud chamber and his discoveries therewith in the fields of nuclear physics and cosmic rays”. Occhialini went unrewarded. The attitude Blackett had at this occasion sheds light on his feelings towards Occhialini. He sent a telegram to Occhialini telling him “Caro Beppe we are very happy but it would never have happened without you” [1]. To Occhialini’s father he wrote: “I would have been happier if Beppe had been honoured at the same time. For it was certainly his arrival in Cambridge which stimulated my embarking on the field of cosmic rays, which I have never left. And our work together in 1932-1933 was a real collaboration of the happiest kind” [1]. Actually, Occhialini’s contribution to the positron discovery did not go entirely unrecognized by peers. He was nominated for the Nobel Prize. In 1948 the prize was awarded to Blackett, but initial nominations of Blackett occurred

years before, in 1935, as they did for Anderson. On the occasion of a nomination of 1936, French physicist Louis de Broglie suggested one-half of the prize to Anderson and one-half to Blackett and Occhialini⁽¹⁾ [24].

6. – To Brazil

Occhialini came back to Italy in 1934. He returned to the Institute of physics of the University of Florence where he had a permanent position. He did some work on radioactivity [25]. He began also to teach as a professor in the faculty of architecture (1936) of the university. For his positron work with Blackett, he was awarded the Premio Sella dell'Accademia dei Lincei and the Premio Vallauri dell'Accademia delle Scienze di Torino [1].

However, at Arcetri the institute was no longer as before. Garbasso had passed away prematurely. Rossi held a professorship at Padua and Persico at Torino. Globally, things had changed in Italy. Owing to the rise of fascism, the political climate had become unbearable to Occhialini. He was a liberal and had become markedly anti-fascist, although he had adhered to the fascist party in the 1920s. In 1932, at the moment of the Spanish and Abyssinian events he had to stay in the party in order to save his job, but it was a tormenting decision [1].

In 1937 Occhialini accepted a professorship in Brazil offered by the Italian government and perhaps mediated by his father. He joined the University of São Paulo which had being recently created in 1934. The physics department was under the direction of Gleb Wataghin, an Italian-naturalized physicist, born in Ukraine. With Wataghin, whom he had already met during a visit to the Cavendish, Occhialini worked to develop Brazilian scientific activity. The first as an experimentalist and the second as a theoretician, they launched cosmic-ray physics in this country. This was an adequate research topic for a country like Brazil, as it had been for Italy, because it only required small laboratories.

Although the conditions were not to be as favourable as in Europe, Occhialini had a real opportunity to extend his scientific career. He held several chairs at the University of São Paulo, on general and experimental physics. Giulio Cesare Lattes soon became his most prominent student. Other gifted students of his were Mario Schönberg, Marcelo Damy de Souza Santos, Paulus Aulus Pompéia, some of them coming from the Ecole Polytechnique which had been annexed to the new university. Occhialini joined his forces to those of Wataghin in forming these young pupils and creating São Paulo's school of physics. They succeeded in creating an atmosphere of intellectual pleasure and excellence. They saw to it that the university had a good scientific library and followed Europeans trends in its scientific activities [26]. Although the task was not easy, Occhialini and his students built a controlled cloud chamber⁽²⁾. With Wataghin he shared an interest for

⁽¹⁾ During the Cambridge period, Occhialini and Chadwick visited together Parisian laboratories.

⁽²⁾ Interview of G. P. S. Occhialini by Martha Cecilia Bustamante and Antonio Augusto Passos Videira, Paris 1992.

large cosmic showers. With his students, Occhialini focused on the utilisation of counters of particles for absorption measurements⁽³⁾.

Occhialini, Wataghin, and their students contributed to the success of a symposium on cosmic rays held in Rio de Janeiro on August 1941. This symposium was held under the auspices of the Brazilian Academy of sciences, on the occasion of the visit of an American scientific delegation headed by Arthur H. Compton. Since the late 1920s Compton was a world authority on cosmic rays as was the case for Millikan, although they were working in an opposite direction because Compton, like Rossi, favoured the corpuscular hypothesis for cosmic rays rather than the γ -ray hypothesis. The delegation was travelling in South America to measure cosmic radiation as part of the program of research of the Ryerson Physical Laboratory of the University of Chicago where Compton held a chair.

The symposium was a large and successful one. The proceedings published by the Brazilian Academy reflect contributions by members of the American delegation and by specialists from several Brazilian academic institutions: the University of São Paulo (with Occhialini, Wataghin and their students), the National Institute of Technology, and the Naval School among others. Occhialini himself presented several papers: “The influence of a solar eclipse on the cosmic ray intensity”, “On the ultra-soft component of the cosmic radiation”, and “On the properties of cosmic radiation in the temperate and equatorial zones” relating absorption experiments that he and his students performed with counters in the laboratory or outdoors. In collaboration with de Souza Santos, he also reported on “Two useful gadgets for controlled Wilson chambers”, that is, an asymmetrical multivibrator circuit and a reliable valve system for controlled cloud chambers that improved over the conventional systems earlier described by Blackett [27].

Soon after this symposium, the international political situation degraded. In August 1942 Brazil entered the war, the group held by the two Italians at the University of São Paulo dissolved, and Wataghin left the direction of the Physics Institute. Out of consideration for Occhialini he could not take position against fascist Italy, for Occhialini’s father was living there and risked reprisals. But Occhialini became an enemy alien and had to give up his position. He took a pleasant refuge in the Itatiaia Mountains near São Paulo. There he lived in a meteorological hut, as an expert mountain climber and as a guide. Reportedly he wrote “an excellent [unpublished] guide book on that range” [4]. After the Italian armistice in September 1943, C. Chagas, Nobel-Prize winner in Medicine in 1921, invited him to the Biophysics Laboratory in Rio de Janeiro. During this stay in Chaga’s laboratory, Occhialini worked on techniques of photography and plate processing. At the end of 1944 he left Brazil and returned to England.

During his stay in Brazil, Occhialini’s largely contributed to the new scientific impulsion desired by the Brazilian government in the early 1930s. Yet the Brazilian episode, like the Cambridge one, was just another chapter in his scientific life. Others events, others circumstances and discoveries that significantly contributed to nuclear and cosmic-ray

⁽³⁾ Besides working on cosmic rays, Occhialini went on to be interested in studies on Rubidium β -Radioactivity. It is an extension of the work he had made in Italy from 1934 until he left.

physics were lying before him. Occhialini left Brazil for England because Blackett had invited him to join the British team working with Americans on the atomic bomb. Although he made the trip, he declined participation in that military research, owing to Italian nationality. Instead he went to Bristol to collaborate with Cecil Powell, again thanks to Blackett's help. As in his collaboration with Blackett, Occhialini once again left his mark. He was the decisive partner in the research work that led to the discovery of the pion in the cosmic radiation in 1947 and played a very important part in the development of the emulsion technique and its effective utilization by Powell's group. With Occhialini, nuclear emulsion technique came into great prominence.

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Occhialini's trajectory in Latin America

ANA M. RIBEIRO DE ANDRADE

Museu de Astronomia e Ciências Afins, MAST/MCT, Brazil

1. – Introduction

Giuseppe Occhialini treated mathematics with limited deference but was a master in conducting experiments, which revealed essential issues resulting from the observation of nature. His role in the training of the first generation of Brazilian physicists at the recently created Universidade de São Paulo (USP) was an important one. The study of cosmic rays, which at the time was considered an area of very high-energy nuclear physics, fascinated experimental and theoretical physicists at the university. Since cosmic rays are present anywhere in nature and since it is possible to detect them at any altitude, research in this area could be conducted in small laboratories at any location, and with limited financial resources. It was therefore well suited to Brazil's economic-financial reality and research could be conducted at a similar level to that of other countries.

It is significant that foreign professors have been present at USP since its creation in 1934. Competence was an important consideration in the hiring of the first foreign professors in Europe, but ideology and social relations interfered in the process. The Italian colony in the city of São Paulo was quite influential, so in order to protect the social and human sciences from fascist influence the organizers at the Faculdade de Filosofia, Ciências e Letras (USP) reserved the mathematics and physics courses for Italian professors. In this manner, pressured by his family to stay away from political movements in Italy, it was not hard for Occhialini to exchange the Università di Firenze for USP. Not only was his father friends with Gleb Wataghin, head of the Physics Department, but the process was made easy because of his scientific credentials, obtained after his collaboration with Patrick Blackett at the Cavendish Laboratory in Cambridge in the

perfection of the Wilson Chamber using the coincidence counter technique⁽¹⁾.

With his introduction of a new utilisation method of the Wilson Chamber at USP, he not only strengthened the group led by Wataghin but also became a leader in the experimental area and redirected the scientific career of at least two Brazilian students, Ugo Camerini and Cesar Lattes, with whom he later worked at the University of Bristol and the Centro Brasileiro de Pesquisas Físicas. Because of his charisma, coupled with his interest for art and literature, he made many friends in São Paulo and Rio de Janeiro, and also contributed to setting up the Laboratório de Física Cósmica de Chacaltaya in Bolivia.

2. – Physics at USP

In contrast with the precarious conditions in the Physics Department, by the end of the 1930's the two approaches to physics, experimental and theoretical, had already been consolidated at USP and research results were comparable to those obtained in European and U.S. laboratories. During Occhialini's five years at the institution (1937-1942) its dynamic scientific and intellectual environment was an uncommon feature in science in a peripheral country [8]. Aside from Wataghin and Occhialini, the theoretical physicist Mario Schenberg was emerging in the field after having worked with G. Gamov and others in the U.S. He states that Occhialini was very influential in his training [2].

In this manner the activities in theoretical physics were led by Wataghin and Schenberg—who offered courses in celestial mechanics, quantum electrodynamics, etc.—while the activities in experimental physics reached a much higher level under Occhialini's management. Unlike professors who taught in a traditional manner, they based their courses on seminars about recent themes and subjects published in international journals [9]. There was a close relationship between the two groups, both of which worked very hard. While on a ship headed for Italy in 1938, where Schenberg was going to participate in an internship with E. Fermi and later with W. Pauli, and where Occhialini was going on holiday, they wrote a paper together on the variation of the intensity of cosmic ray showers with latitude (table I, article 1). The following year Schenberg engaged in a theoretical study of the phenomenon of penetrant showers or the multiple and simultaneous production of mesons which Wataghin and his assistants, along with the experimental physicists Marcello Damy de Souza Santos and Paulus Pompéia, had shown experimentally. The group's work [10] resulted in an international cooperation between USP and the University of Chicago, the purpose of which was to measure and detect cosmic rays in the stratosphere. In 1941 an expedition was organised, in which the American physicist Arthur Compton took part, to measure cosmic radiation in the interior of the state of São Paulo using hydrogen balloons carrying Geiger counters [3, 4].

⁽¹⁾ With the exception of Occhialini, the biographies and the scientific trajectories followed by the pioneers of physics are registered in interviews. They are an important source of facts, for the accounts allow one to become acquainted with the professional environment and to identify the networks of scientific sociability. About Occhialini's activities in Brazil see: the interviews with G. Wataghin, M. Schenberg, M. Damy de Souza Santos, J. Leite Lopes and other Brazilian physicists [1-4]. About USP see also [5-7].

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 7. Occhialini G., Santos M. D. S., "Two useful gadgets for controlled Wilson Chambers", in *Symposium sobre Raios Cósmicos, agosto 4-8, 1941, Academia Brasileira de Ciências, Rio de Janeiro, 1943* (Imprensa Nacional) 1948.
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That very year the first international physics event to take place in Brazil was held at the Academia Brasileira de Ciências, the *Symposium sobre Raios Cósmicos* (Rio de Janeiro, 1941). Participants included Occhialini, Compton, Wataghin, José Leite Lopes, biophysicist Carlos Chagas Filho, mathematicians, students of the USP Physics Department and of the Universidade do Brasil (RJ) and military school professors. The Academia Brasileira de Ciências was the locus for scientific discussions; and the *Anais da Academia Brasileira de Ciências*, where Occhialini published seven papers, most of them in co-authorship with USP colleagues, was the oldest and the most important national journal [2], renowned in the international scientific community.

Even though Occhialini and Wataghin lived far from science production centers, they never ceased to keep in contact with Heisenberg, Niels Bohr, Heitler, Blackett, Dirac, Rutherford, Fermi, Max Born, Wolfgang Pauli, Ernest Lawrence, among other elite members of European and U.S. physics. This uninterrupted communication gave impetus to their students' advancement and made it easier for them to have access to foreign laboratories to complete their training. Wataghin and Occhialini's leadership also contributed to the maintenance of the tradition, popular at the time, in which experimental physicists made and improved their research apparatuses themselves. For this reason, because of the experience he gained at the Cavendish Laboratory and also because of his magnetic personality, Occhialini attracted theoretical physics students to his group. The experimental approach came into focus in a definitive manner with his development of a method in which two coincidence Geiger counters automatically recorded the passage of a particle by triggering a photographic camera that registered the vapour track left by the particle as it crossed the cloud chamber. Lattes says that he abandoned theoretical physics, Wataghin's and Schenberg's field, due to Occhialini, who taught Camerini,

Andréa Wataghin and himself to build and operate a small Wilson Chamber⁽²⁾.

The intensification of World War II inverted the situation. In Europe and the U.S., physics laboratories turned to the development of military technology, while at USP research practically came to a full stop: as a consequence of his Italo-Ukrainian origins, Wataghin was asked to resign from his post of director of the department; the majority of professors chose to secretly build artefacts for the Brazilian navy and army; and shortly thereafter Schenberg became engaged in party politics and was elected federal congressman for the Communist Party. Wataghin, Lattes and a few recent graduates were therefore the only ones who continued to dedicate themselves exclusively to cosmic ray research [2, 3].

When Brazil declared war on the axis powers in August 1942, Occhialini feared being deported to Italy and preferred to resign from USP to live incognito, possibly in the newly created Parque Nacional de Itatiaia. It is not known why he chose this region, what he did when he lived there and for how long he had been hiding there⁽³⁾. Aside from its exuberant beauty and numerous mountains, Itatiaia city is strategically located near the main road linking São Paulo and Rio de Janeiro, and a few German and Swiss families lived in this region. It is presumed he remained in this region until Italy's unconditional surrender in September 1943. He eventually left Brazil to co-operate with English war efforts, but before leaving he collaborated with biophysicist Carlos Chagas Filho at the Universidade do Brasil and gave a course on X-rays at USP, which enabled Lattes to use photographic film in physics [11].

Occhialini arrived in England on "(...) an invitation from the British Government to join the British Atomic Energy team, but with the success of the bomb trial in Arizona, policy changed and foreigners were excluded. We took on Occhialini in the first place partly to relieve the DSIR [Department of Scientific and Industrial Research] from an embarrassing situation, but the following spring we agreed to finance him from department funds [Physics Department of the University of Bristol]"⁽⁴⁾. In June 1945 he started to work with Cecil Powell at the small H. H. Wills Physical Laboratory. Powell, a pacifist, and also considered a leftist, did not take part in the war efforts and preferred to lead a modest research project.

3. – Reunion with the Brazilian physicist

Occhialini met Powell when he was working on neutron-proton scattering at approximately 10 MeV. Occhialini's unique ability to perfect research techniques helped him to convince C. Waller, a chemist at Ilford Ltd., to prepare emulsions plates with a silver density around six times higher than that normally used in nuclear physics. Many attempts were necessary before the plates acquired the desired density without increasing

⁽²⁾ Lattes' interview, see [3, 11].

⁽³⁾ It is hard to believe that Occhialini worked as mountain-hiking guide. The region was uninhabited and visited only by botanists.

⁽⁴⁾ Ref. [12], quotation on page 30.

the background residues, the undesirable grains, which masked the tracks of protons and other particles under study.

On the other side of the Atlantic, Lattes was impressed to receive Occhialini's positive prints of photomicrographs of protons and α -particles obtained with the new emulsion produced experimentally by Ilford. As he realised that this high-density silver plate was more powerful and more adequate for the study of cosmic rays than his small cloud chamber, he immediately wrote to Occhialini inquiring about the possibility of going to Bristol himself. Occhialini predicted that the new concentrated emulsion could lead to scientific discoveries, so he suggested Powell to invite Lattes [13].

It was not hard to "join youthful forces" at the H. H. Wills Laboratory. "Since 1946 the contribution by DSIR to the work of Powell has been substantial (...)">⁽⁵⁾, aside from the financial support provided by the Iron Steel Federation, Anglo-Iranian Oil, the Electrical Research Association, the Diamond Corporation, Eastman Kodak and the Royal Society. However, it would be difficult to find adhesions among junior England physicists who had been recruited for war and had been encouraged by the government to return to scientific activities. During those years the English paid little regard to experimental physics and to universities in the interior, since undergraduate courses had been redirected during WWII towards the training of electronics technicians. Bristol's main attraction was the theoretical physicist N. F. Mott [13, 11].

In January 1946, after Occhialini had seen a γ -ray photograph by Lattes of the slow-meson triggered Wilson Chamber, Powell made a formal invitation for him to join the Bristol group. Lattes, who typically acted quickly, immediately arranged to travel to distant and cold England, accepting the fact that he would live on a research associate scholarship, and knowing quite well that in the winter of 1946 a meal could consist of a bowl of soup and a slice of bread [14-16]. As is well known, laboratory-based development of new techniques attracts qualified researchers because of the possibility of obtaining promising scientific results, as well as a rapid return in intellectual investment in the form of *recognition*, *credit* and *scientific credibility*⁽⁶⁾. Lattes found Powell and Occhialini working on n-p scattering using common photographic emulsions, such as Powell himself had done long before. Having just left a group conducted by Wataghin's theoretical rigour and shaped by a quantum-oriented view of radiation and matter and by Occhialini's own experimental creativity in the subject of cosmic rays, Lattes stated that seeing a proton would require a lot of imagination. According to him, Powell was very conservative in science, but he allowed his subordinates much freedom. Compared to Lattes' work in USP, where he had been criticised for excessive dedication to theoretical physics, his work was now almost entirely experimental [14, 15, 17]. This motivation led Lattes to suggest that Ugo Camerini join the Bristol group. He was in fact investing in the future, so he could later work with cosmic rays, his favourite subject. He worked in-

⁽⁵⁾ Ref. [12], quotation on pages 31-32.

⁽⁶⁾ The meaning here is that used in the sociology of science, respectively, in Merton's, Bourdieu's and Latour & Woolgar's work.

tensively in calibrating Ilford's new plates in order to obtain range-energy relations [18]. He kept in touch with C. Waller at Ilford, convinced that the fading image problem might be solved by modifying the composition of the emulsions by adding borax, following the guidance of Johnny Williamson from the Chemistry Department. Later, Camerini collaborated in this investigation as well.

In Cambridge, studying reactions produced by the 1 MeV deuteron beam of the Cockcroft-Walton accelerator, Lattes was interested in light targets —D, Li, Be, B, F— in order to understand obscure issues relating to these reactions. He insisted in determining the range-energy relation for α -particles, protons and deuterons in the new nuclear emulsion. In the same experiment, he placed borax-loaded plates, which Ilford had prepared at his request [18]. As he predicted to Leite Lopes, the new emulsions called C₂ would soon be adopted in nuclear physics and in the study of cosmic rays: "(...) a real race is on the way"⁽⁷⁾ Lattes, Fowler and Cuer —the latter two Powell's students— managed not only to establish the range-energy relation protons through the analysis of tracks on those plates but also to make an important correction: the relation between the range in the air and in the emulsion is not constant⁽⁸⁾. Still with Cuer, he investigated the disintegrations produced by neutrons in the emulsion by loading it with D, Li, Be, etc. salts. In the theoretical domain, he was studying the action of nuclear particles in photographic emulsions, along with Peter Burton, a student of Mott's.

4. – The construction of the π -meson⁽⁹⁾

Whereas WWII led to the meeting of Powell, Occhialini and Lattes —all of whom engaged in scientific research rather than taking part in the war effort in their countries— the great battle to mobilise heterogeneous elements and to find mesons in cosmic rays started when Occhialini and Lattes met again. Together they transformed life on the fourth floor of the so-called *cigarette tour*, where the H. H. Wills Laboratory was located. Happy and carefree, they restarted cosmic rays research and became experienced in the use of photographic emulsions, as had Powell. As a result of this, said Occhialini, English physicists and technicians broke out of their torpor, having been frustrated and feeling guilty because they had not participated in war activities against nazi-fascism. They reacted when faced with the pair's intellectual excitement and were able to present civilised scientific contributions to the world⁽¹⁰⁾.

Obsessed with the idea of eliminating fading in the emulsions, Lattes and Occhialini decided to expose some plates at Pic du Midi. Occhialini was going on holiday in the French Pyrenées, where Powell and he had already exposed the old plates, so the Observatoire du Pic du Midi (2850m) turned out to be doubly convenient. Two researchers at the observatory, Hugon and Max Cosyns, helped conduct the experiment for approx-

⁽⁷⁾ See ref. [15].

⁽⁸⁾ See table II, articles 3 and 4; and [11].

⁽⁹⁾ This part relies mainly on [19], pages 23-53 and [20], pages 313-321.

⁽¹⁰⁾ See [13], page 7.

TABLE II. – *The discovery of the π -meson.*

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1. Powell C., Occhialini G., "Multiple disintegration processes produced by cosmic rays", *Nature*, **159** (1947) 93-94.
 2. Powell C., Occhialini G., "Nuclear disintegrations produced by slow charged particles of small mass", *Nature*, **159** (1947) 186-190.
 3. Lattes C., Fowler P., Cuer P., "A study of a nuclear transmutations of light elements by the photographic method", *Proceedings of the Physical Society*, **59** (1947) 883-900.
 4. Lattes C., Fowler P., Cuer P., "Range-energy relation for protons and α -particles in the new Ilford 'Nuclear Research' Emulsions", *Nature*, **159** (1947) 301-302.
 5. Lattes C., Occhialini G., "Determination of the energy and momentum of fast neutrons in cosmic rays", *Nature*, **159** (1947) 331-332.
 6. Lattes C., Muirhead H., Occhialini G., Powell C., "Processes involving charged mesons", *Nature*, **159** (1947) 694-697.
 7. Lattes C., Occhialini G., Powell C., "Observations on the tracks of slow mesons in photographic emulsions", *Nature*, **160** (oct. 1947) 453-456, 486-492.
 8. Lattes C., Occhialini G., Powell C., "A determination of the ratio of the masses of π - and μ -mesons by the method of grain-counting", *Proceedings of the Physical Society*, **61** (1948) 173-183.
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imately one month⁽¹¹⁾. All the plates were calibrated, that is the range-energy relation had already been determined, but some had been borax-loaded by Lattes. The hypothesis was that the anti-fading effect of the borax would allow the detection capability to last longer. Without the borax, on the other hand, the plates would suffer too much fading and would lose their detection capabilities in about one week [17, 22]. On the same evening of January 1947, when he returned to Bristol, Occhialini processed the emulsion and sent *Nature* a note praising the advantages of nuclear emulsions in the study of cosmic radiation (table II, article 1). The photomicrographs confirmed the hypothesis that at high elevations borax's antifading effect permitted the registration of a number of events and minute details, such as accompanying the trajectory of lithium-8—which Occhialini called *tarelo*—emitting a β -particle. The lithium-8 then decayed to beryllium-8, which in turn emitted two α -particles, which he called *martelo*. The detection of the neutron energy was part of the result. The photomicrography mosaic, which had been prepared by Powell, mobilized the entire laboratory into studying the normal low-energy events of cosmic rays. The work required many hours of exhausting and patient activity at the microscope. The microscopists were all women, some of which were physics graduates. A few days later, Marieta Kurz found a strange event: a line which was more crooked than that of a proton, less dense, with many changes in direction, and about one-eighth the mass of a proton. A similar event relating to a "double" meson (π - μ decay) was observed in the following days [17].

⁽¹¹⁾ This support is registered, *e.g.*, in table II, article 5. See also [21], pages 368-370.

Mass measurements were of two sorts. Occhialini and Lattes started refining the technique of grain counting and energy balance on secondary tracks in the observed reactions. Hugh Muirhead and another three students dealt with the problem by using multiple Coulomb scattering [14, 17, 18]. He believed that “the experience of our Brazilian friends in cosmic rays at this time was invaluable and on many occasions I heard the name Wataghin mentioned”⁽¹²⁾. Altogether, the methods were sufficient to guarantee that Yukawa e Anderson’s mesons were present (table II, articles 5 and 6).

“The excitement of the of the discovery of π -mesons was intense and the occasion was such that Occhialini, of undoubted rationalist outlook, could only express his feelings by going into the R. C. Cathedral to light a candle!”⁽¹³⁾, remembered Tyndall, the H. H. Hills director. Particle physics, so far subordinated to nuclear physics, was about to be formally recognised as an independent field.

There was one problem, however. How could the Bristol group guarantee their interpretation of the phenomenon if the number of analysed events did not allow the statistical reduction of errors in each of the methods used for mass measurement?

This difficulty reveals the uncertainty of the first results of scientific investigation, and the necessity of obtaining more events, for the discovery only acquires scientific value when singular observations become regular. An isolated event is insignificant, and the experiment must be carried out obstinately, with courage and vigour, before the evidence can be consolidated. Perhaps only an ex-student of Occhialini’s would have thought of exposing the borax-loaded plated at peaks higher than those in the Pyrenées, so as to detect the mesons more credibly.

Lattes went to the Geography Department in search of an accessible and safe location to repeat the experiment. A meteorology station in the Andes, 20 kilometres from La Paz and 5600 meters high, was located. The geographers probably had help from the Spanish meteorologists Mario Porto (director of the Irish Meteorological Service) and Duperier (Blackett’s collaborator in London), who knew Ismael Escobar, a political refugee from the Spanish Civil War responsible for Bolivia’s meteorological network⁽¹⁴⁾. Lattes had to act quickly, according to Occhialini and Powell, so that the Bristol group would not be left behind by D. H. Perkins’ group at the Imperial College of Science and Technology, a group which had already exposed nuclear emulsion plates at about nine thousand meters from Royal Air Force planes. But in the articles published in *Nature* (table II, articles 1 and 2) they had already revealed their addition of borax to the emulsions, so they could lose their leadership in the race, for in such cases the first to arrive becomes the winner.

Powell, spokesman for the group of twenty young scientists and technicians, had no trouble convincing the laboratory’s director, Professor Tyndall, to obtain funding for the journey from the British government and industries. After WWII, the discovery of new subatomic particles contributed towards a greater understanding of nuclear forces, possi-

⁽¹²⁾ See [18], quotation on page 15.

⁽¹³⁾ Ref. [12], quotation on page 32.

⁽¹⁴⁾ See [23], especially page 102.

bly leading to an increased political and military advantage for the promoting countries and for shareholders of companies directly involved in scientific applications. Lattes left on April 7, 1947, for Rio de Janeiro, from where he crossed Brazil to reach Bolivia's capital.

From La Paz, in the company of Spanish meteorologist Ismael Escobar, he departed for the meteorological stations. Despite the discomfort and adversities encountered at 5600 meters of altitude—precarious access, low temperatures and scarce oxygen—Lattes placed small piles of sensitive photographic plates at the second meteorological station. It was in fact a tiny and crude installation comprising four pieces of wood, lost amidst Mt. Chacaltaya's magnificent scenery of eternal snow. There the nuclear-emulsion plates received a number of particles thousands of times greater than in the Pic du Midi experiment.

One month later, at Escobar's house, he processed one pile of plates and found a complete "double" meson. Back in Bristol, about 30 mesons were found on the other plates. Even negative mesons were found, which at the end of their trajectory resemble the drawing of a star. After obtaining the list of the mesons' mass from counting the grains in the lines, Lattes' result confirmed the first experiment⁽¹⁵⁾.

The outcomes of the two experiments did not originate from natural evidence, primary observation or from an isolated action. A handful of multiple and interconnected factors propitiated the success of the two experiments: besides previous experience with cosmic rays accumulated by Occhialini and Lattes, and Powell's experience in nuclear physics using photographic plates, the discovery was due to the technical improvement of Ilford's so-called C₂ nuclear emulsion plates. They represented the result of combining Occhialini's skills in experimental physics, Lattes' clarity of thought and strong theoretical background⁽¹⁶⁾ and Powell's alliance with representatives of other institutions. "It was a reality of intense, arduous and continuous work of deep excitement and incredibly fulfilled dreams", said Occhialini⁽¹⁷⁾.

The process of construction of the meson is well-documented in six articles published in *Nature* and in two articles in *The Proceedings of the Physical Society*, the authors of which appear in alphabetical order, as was usual at the time (table II). Occhialini and Lattes did not wait for the acclamation of results in Bristol, which concentrated around Powell in 1950, after his own name⁽¹⁸⁾. Lattes surprised many when he decided to leave Bristol. In an apparent whim, he went to the Berkeley Radiation Laboratory with the intention of trying to detect mesons produced at the 184-inch Lawrence's synrocyclotron. Fifteen days after his arrival in the U.S., mesons produced artificially at this machine were detected by Lattes and E. Gardner. The Bristol group took almost one year to observe thirty "double" mesons, while in Berkeley they detected this same amount in

⁽¹⁵⁾ See [17] page 3. Lattes' measurement calculations are kept at the Wills Memorial Library.

⁽¹⁶⁾ See [24], page 11.

⁽¹⁷⁾ Ref. [13], quotation on page 7.

⁽¹⁸⁾ See Powell letters to N. Bohr and Rosenfeld Papers between 1948-1951 to apprehend Powell's movement.

one day, for they knew where the source of the mesons was, where their destination was and at what angle they reached the emulsion plates.

Lattes and Gardner's work caused great impact, for Lawrence transformed the scientific event into a press fanfare [25, 26]. The π -meson detection is registered in two articles: one published in March 1948, in *Science*, and the following year another in the *Physical Review*, the same year that Lattes returned to Brazil to invest his prestige in the creation of the Centro Brasileiro de Pesquisas Físicas (CBPF), the Conselho Nacional de Pesquisas (CNPq) and the Laboratório de Física Cósmica in Chacaltaya.

Also in 1948, Occhialini went to the Université Libre de Bruxelles to organize a research group to improve the method for developing thick emulsions uniformly (Ilford G₅). That same year he invited the Brazilian theoretical physicist Schenberg to work at the center for nuclear research at that university. Schenberg stayed there for five years, working on cosmic rays and statistical mechanics with Prigogine, Cosyns and evidently with his friend and colleague Occhialini [2].

5. – The return to Brazil

Since the mid-'40s, professors of physics and mathematics at the Faculdade Nacional de Filosofia (FNFI) of the Universidade do Brasil, influenced by Wataghin and Occhialini and motivated by Schenberg's and Damy de Souza Santos' performance, wished to bring teaching and research together, as was done at USP and universities abroad⁽¹⁹⁾. Nonetheless, before this could be done the proper conditions for scientific work had to be created: laboratories, a library, full-time work hours and technician support. The project gained momentum in 1945 with the creation of the Instituto de Biofísica by Carlos Chagas Filho, who supported the claims made by his colleagues at the FNFI. They invited Lattes and convinced the director to create a nuclear-physics chair and to include the subject in the curriculum. But without funds for the installation of the laboratories needed for Lattes' work, they concluded that the battle in favour of research at the Universidade do Brasil had been lost.

The year 1948, when this defeat occurred, was an exceptional year. Democratic ideas were spreading in the large urban centers, the Sociedade Brasileira para o Progresso da Ciência was created and Lattes was being given wide coverage by the national press. This group of professors at the FNFI then made their last risky attempt, namely to engage in science outside the university with private capital.

The *scientific capital* accumulated by Lattes was applied towards the creation of the Centro Brasileiro de Pesquisas Físicas in 1949 and of the Conselho Nacional de Pesquisas in 1951⁽²⁰⁾. Both institutions relied on political support from congressmen, ex- and future presidents of Brazil, the military, and government technicians interested in the

⁽¹⁹⁾ The level of the majority of foreign professors was very poor and Italian professors at FNFI were accused of excusing the fascist regime instead of engaging in science, see [27] and [19], pages 55-66.

⁽²⁰⁾ About the creation and the activities of the CBPF in the 1950s, and politics of the CNPq, see [19], pages 55-142 and [28].

production of nuclear energy in Brazil. As in the financial issues present in the game of political interests, the scientific patrons supported future negotiations, including those which took part in other arenas and joined forces to finance the CBPF, becoming its members. Alliances were inevitable. The physicists and mathematicians who founded the CBPF belonged to a generation whose social, political and philosophical options had been conditioned by WWII and therefore wanted to work on science in Brazil rather than on their careers at American institutions [14].

Support for the CBPF was not unanimous, however. In particular, some FNFI professors and competitors, such as USP nuclear physicists, particle physicists and cosmic ray physicists, regarded CBPF as a rival. Before, there had been a large difference between the level of courses offered in São Paulo and in Rio de Janeiro, so all the CBPF pioneers, with the exception of Hervásio de Carvalho, had attended USP.

The usual competition between colleagues and institutions present in science becomes more serious or more apparent when new disciplines, groups or areas are institutionalised, or when new research equipment is needed. Disputes typically occur between researchers belonging to established scientific fields and recent rivals, especially when the latter already start off with solid political support. This was precisely the case with CBPF, which emerged in cooperation with the FNFI Physics Department. The corresponding department at USP had already lost Occhialini, Lattes, Camerini and Schenberg, and still had to face the departure of Wataghin, who returned to Europe in 1949. It was therefore struggling to recover its previous productivity level. Scientific research is not any more rational and logic than other human activities, so it is easy to understand the reaction and competition at the beginning of the '50s, specially in the experimental area, when the first three researchers worked at CBPF, where the theoretical physicists Leite Lopes, G. Beck, J. Tiomno e R. Feynmann also worked.

The Brazilian delegate at Unesco, Paulo Berredo Carneiro (scientist and a founding member of CBPF), was enthusiastic about the repercussion of Lattes' work and was responsible in April 1951 for the success of the negotiations of the first Technical Assistance Treaty between Unesco and the Brazilian government. The treaty favoured CBPF by granting scholarships to visiting fellows, allowing Brazilian researchers to continue their training abroad and providing aid in the acquisition of research material and journals. This type of collaboration had become common at the time. Twenty such treaties had been made official with the Unesco Member States, eight of which benefited Latin America⁽²¹⁾.

Foreign investment and financing in underdeveloped countries after the war was scarce, so Unesco support allowed the rebirth of international cooperation in the realm of the exact and natural sciences. The so-called "Physics and Chemistry Science" field was the greatest beneficiary in the 1950-52 period, having received 27.78% (1950), 28.03% (1951) and 24.37% (1952) of Unesco's total budget⁽²²⁾.

⁽²¹⁾ See official letters in [29-31] rapport of Unesco's director [32], page 190; fellowships [33].

⁽²²⁾ See [34], page 43 and [35], page 46.

The first visiting researchers selected by Unesco arrived in 1951 to work at CBPF: Occhialini, who brought with him a nuclear-emulsion processor and twenty photomultipliers; the Dutch Gerard Hepp, a specialist in fine electronics at Philips Research Laboratory; the German Helmut Schwartz, a specialist in vacuum technology; and the Brazilian Ugo Camerini, who was considered a special case. Gert Molière, a physicist at the University of Tübingen, arrived in 1952. Aside from the Unesco scholarship, all of them received daily stipends from CNPq⁽²³⁾.

Paulo Carneiro visited CBPF in October 1951 and, fascinated with Camerini and Hepp's activities, he wrote to the general director making other requests: "La importance grandissante des travaux de CBPF dans le cadre de l'économie du Brésil me fait espérer (...) que vous donnerez votre apui"⁽²⁴⁾. CBPF's dynamism was reflected in the *Simpósio sobre Novas Técnicas de Física* (RJ e SP, July 1952), which had support from the CNPq and the Centro de Cooperação Científica da América Latina, a Unesco organization based in Montevideo. Of the 74 works presented in the Rio de Janeiro sessions by 32 Brazilian researchers or foreign researchers with links to national institutions, two themes predominated: particle accelerators and cosmic rays. Theoretical and experimental issues on cosmic ray physics were dealt with by Occhialini, Lattes, G. Molière and Sandoval Vallarte from Mexico⁽²⁵⁾. Occhialini impressed the participants in the event and was honoured with the "Medalha da Ordem do Cruzeiro do Sul", the most important distinction bestowed on foreigners in Brazil.

Nuclear physics, including the cosmic ray physics, field theory and quantum mechanics, was the first sub-field of physics in Brazil to receive systematic support from CNPq and the national budget. This happened because in the 1950s nationalist-developmental economic policies influenced the State's actions and experimental physics was within reach of technological applications. In return for the ambitious project of building cyclotrons, introduced by the generals who headed CNPq and wanted to produce nuclear energy, Lattes demanded that a laboratory be built in Mt. Chacaltaya.

6. – The laboratory on mount Chacaltaya⁽²⁶⁾

Since Brazil has no high mountains of its own, and Chacaltaya was considered a privileged location ever since *Nature* revealed its existence, it was the logical choice for cosmic-rays research. Lattes' scientific capital led to the guaranteed cooperation between CBPF and the Universidad Mayor de San Andrés (UMSA) in the construction of a "(...) centro de investigaciones, enseñanza y observaciones meteorológicas"⁽²⁷⁾.

Ismael Escobar, the director of the Servicio Meteorológico, was appointed director of the Laboratorio de Física Cósmica in January 1952. That very year American and

⁽²³⁾ See [36, 19], page 100.

⁽²⁴⁾ Quotation in [29].

⁽²⁵⁾ See [19], pages 135-137; [37], pages 211-212 and [35], page 62; [38, 39].

⁽²⁶⁾ This part relies mainly on [40-42].

⁽²⁷⁾ Ref. [41], quotation on page 14.

Brazilian physicists started travelling to Bolivia to expose plates on Mount Chacaltaya and Lake Titicaca. Lattes and Roberto Salmeron, who were, respectively, the director of research and head of the CBPF Divisão de Raios Cósmicos, went there to evaluate the conditions for research. Faced with many obstacles, Salmeron preferred to obtain his PhD in Manchester on a Unesco scholarship [43, 44], while Lattes dedicated himself to formalizing a UMSA/CBPF cooperation program.

CBPF was a powerful ally, given the converging interests in science and external politics. For the production of science in Brazil, Chacaltaya was ideal, and for Brazilian diplomacy science could be used as a form of state propaganda in which politics is often above scientific and technical decisions, even though the conjunction of interests in science and international relations was not in fact part of a larger political project by the state. The Foreign Ministry took advantage of a program of scientific cooperation to expedite the execution of international treaties, while the Brazilian physicists looked at the state as a guarantor of investment in scientific capital. At first the interaction between the partners in this scheme went well, but once objectives started to differ, the lack of equilibrium in this power relation became apparent and the scientists had to yield to the diplomats.

Events occurred quickly. Scientific instruments made in the CBPF workshops were taken to Bolivia and the UMSA and CBPF representatives signed a scientific cooperation agreement between the two countries. In brief, the agreement dictated that the CBPF could use the Chacaltaya installations for ten years and that it could erect buildings, which would become UMSA property right away; in return, it would offer physics and mathematics courses at UMSA and would give two annual scholarships in Brazil to Bolivian students specializing in cosmic rays [45-47].

The period when Lattes was leading the enterprise was one of intense activity. Much was accomplished: scientific politics, investments in infrastructure, engineering and science, while research groups from MIT, USP and the University of Chicago participated as well. The projects and investigations were compatible with the technical and financial possibilities and had precise objectives: determining the half-life of the pion; measuring the density and the energy spectrum of air showers, among which were the V-meson and other little-known particles; and determining the second maximum of the Rossi curve⁽²⁸⁾.

CNPq granted regular scholarships and aid for CBPF's activities, since the latter was not able to survive on aid from private enterprises. In fact, the help was even greater, for CNPq took care of all administrative expenses, technical and scientific personnel expenses, daily stipends, financial aid for researchers and their families, and granted scholarships to Bolivian students in Brazil. While the Laboratorio de Chacaltaya's infrastructure was being erected, there were five research groups: USP, MIT, Chicago (already mentioned), CBPF and Escobar himself. Interests converged, and Lattes interacted with all the groups, while Escobar dedicated himself to negotiations with the Bolivian authorities, substituting the allies of the government deposed in the 1952 *Revolução Boliviana*.

⁽²⁸⁾ See [37], pages 226-227.

Aside from dozens of Geiger-Müller detectors and highly sensitive electronic circuits taken to Bolivia, CBPF put up three buildings in Chacaltaya to house technical and scientific personnel during the week, laboratories and workshops. Adaptation by foreign researchers was difficult, because of the cold weather, the poor quality of the meals and the scarcity of oxygen, at half the amount present at sea-level. Aside from all this, the economy was fragile and the politics was unstable, not to mention other material and cultural obstacles. Bolivia had shortages in many things, such as electrical lighting, libraries, bread, meat, and the cinema, but the trip was considered very picturesque.

Under Camerini's and Lattes's leadership, Alfred Hendel, Theodore Bowen and four students were part of the CBPF group between 1953 and 1955⁽²⁹⁾. Lattes attempted to conciliate research with his positions as scientific director of CBPF and of the Cyclotrons Project, as counsellor for CNPq, and as professor at the Universidade do Brasil. Meanwhile the research group dedicated itself to setting up and testing the operation of the large Wilson Chamber built at the University of Chicago for Marcel Schein. Brazilian engineering students had the opportunity to learn a great deal of electronics with Brown and Hendel, but local conditions were inadequate for solving the complex daily problems related to the cloud chamber. Hepp went to Bolivia as an assistant, but also could not do anything. The observation of rare events, that is, high-energy events related to mesons or to the so-called Rochester and Butler V particles, was not feasible because of errors in the instrument's technical project. This experiment was conducted by other groups using similar equipment operating at sea-level, but only at high altitudes was it possible to observe the initial collisions without the occurrence of many secondary particles [48-50].

Occhialini participated actively in the development of the CBPF cosmic ray research program and was in Bolivia; he left in Chacaltaya the nuclear-emulsion processor he had built in Italy. The fact that after his departure nobody was able to operate it brings into evidence the distance between the periphery and a science production center, namely Italy. Technical problems created obstacles for the production of science by the CBPF group in Chacaltaya⁽³⁰⁾, and cooperation between foreign and Latin-American institutions in the 1950s was not sufficient for all phases of the process of creation of scientific knowledge to be successfully implemented.

Occhialini returned to Europe in 1952, while the senior researchers of the CBPF group exchanged Bolivia for the U.S., without abandoning cosmic ray and particle physics. Some adopted the new technique of exposing nuclear emulsions, consisting of sending equipment bearing plates to high altitudes in stratospheric balloons. Lattes performed this experiment at the University of Chicago in 1955; Camerini settled at the University

⁽²⁹⁾ A. Hendel belonged to UMSA but was hired by CBPF to administer the project; T. Bowen was a member of the cosmic ray group led by Marcel Schein of the University of Chicago, who collaborated with Lattes. Bowen impressed Brazilian students (*e.g.*, Fernando and Susana Souza Barros) because of his abilities as an experimental physicist and also because of his complete integration with the aimarás culture [48,49].

⁽³⁰⁾ UNESCO kept track of the agreement by means of detailed reports. See [34], pages 154-155; [32,51].

of Wisconsin in Madison; Bowen left the University of Chicago and went to the University of Michigan; and Hendel went to the University of Arizona.

Starting in 1958, CBPF decided to cancel the agreement with the Universidad Mayor de San Andrés. Conflicts resulted and CBPF has to step back when faced with pressure from the Foreign Ministry. Assuming a leadership position in Latin America was part of Brazil's foreign policy, as was the idea of revising the country's relationship with the U.S. Nonetheless, diplomacy was important for Brazil's interests and for those of the great European powers, and the country still considered pan-americanism more important than latin-americanism. The negotiation between CBPF and the Foreign Ministry reflected the existing tension between internal and relative autonomy in science and the pressure which is external to the production of scientific knowledge. That is, since science cannot be dissociated from other power systems and movements in society, the scientists yielded in order to guarantee financing for other research. In practice, the links with the Laboratorio de Física Cósmica de Chacaltaya were maintained until Escobar's final move to the U.S. As can be seen, countries with no scientific tradition face tremendous challenges to produce knowledge, even when internationally renowned scientists take part in the efforts.

7. – Final considerations

Far away from Brazil, Occhialini met his Brazilian colleagues many times at international events. Until recently he was remembered often by his colleagues for his intuition and creativity in the production of science and for his role in the institutionalisation of experimental physics at USP, as well as for his picturesque stories, his sense of humour and his talents in winter sports at Chacaltaya. Most people admired him. He not only made great friends, particularly Lattes, Schenberg, Camerini, Leite Lopes, J. Danon and Damy Souza Santos, but he also helped the career of many people who worked with him, directly or indirectly, at USP and CBPF. Aside from issues relating to his scientific credibility, which allowed him to be hired to work at the Universidade de São Paulo, at the H. H. Wills Laboratory, the Université Libre de Bruxelles and the CBPF, on a Unesco scholarship, three issues are prominent in Occhialini's trajectory in Brazil: the contribution made by foreign professors in the training of Brazilian scientists; the social networks of science; and the transference of techniques and scientific instruments from central countries to countries in the scientific and political periphery.

Occhialini generously lent his academic prestige to the scientific institutions through which he passed, transferring knowledge and therefore contributing to the creation of a new generation of Brazilian, English, Belgian, Italian, etc. physicists. As was shown, he co-operated closely with Brazilians in four institutions in the 1940s and 1950s, at the same time elevating their level significantly in the international science scenario. In this way he allowed for the introduction and utilisation of new methods, techniques and research equipment, and also managed to attract scientists of other nationalities and produce new knowledge. Like nobody, he knew how to *move* and *mobilise* the world in the laboratory, he knew how to obtain data and, in spite of the controversies, how to represent nature.

Finally, when examined from the perspective of social history, Occhialini's contribution to the development of science and the training and establishment of researchers in Latin America allows us to consider the value of science and its social functions in the contemporary world, whether we take into account the increase in material riches or the makings of science in different societies.



Fig. 1. – The physicists Joaquim Costa Ribeiro, Cesar Lattes, Giuseppe Occhialini and Lattes's wife, Martha Lattes, for occasion of the “Simpósio sobre Novas Técnicas de Física” (Rio de Janeiro, 1952). Credit: Archive MAST/ CNPq.

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Occhialini's scientific production between the two English periods

LEONARDO GARIBOLDI

*Istituto di Fisica Generale Applicata, Sezione di Storia della Fisica
Università degli Studi di Milano, Italy*

1. – Introduction

Occhialini's scientific production between his stay at the Cavendish Laboratory in Cambridge in the early '30s and his stay at the Wills Laboratory in Bristol in the mid '40s is maybe the less known. The recollection of his papers was at the base of a reconstruction of his scientific activity and showed his work in a standard kind of researches on radioactivity and cosmic rays by means mainly of G-M counters and Wilson chambers⁽¹⁾, showing a status of continuity with his precedent activity in Florence and Cambridge. It was a particularly striking fact to note Occhialini's technical ability in designing new kinds of simple devices to detect cosmic radiation in a situation of scarce financial and technological support.

2. – New kinds of instruments

One of the most important topics in particle physics had been the development of new and better detectors and counters. In particular, the sensibility of the counters had been developed along three objectives:

- 1) to detect single particles;
- 2) to detect simultaneously arriving particles (coincidences);
- 3) to get a prompt output of the device.

⁽¹⁾ On two devices by Occhialini and Damy de Souza Santos to be used with a controlled cloud chamber, see [1]

On the basis of a system of two electrodes with a potential difference between them, physicists developed different kinds of counters: Geiger point counters, Geiger-Klemperer proportional counters, Burcham-Lewis loop counters, etc. Cosmic ray physicists improved, in particular, the construction of G-M wire counters.

In order to have at proper disposal a counter sensible to particles arriving from one direction from a weak-emitting source, and with a large useful surface, Occhialini and Yolande Monteux⁽²⁾ suggested a new kind of counter [2], derived from the coaxial G-M counter, to be used with the anti-coincidences method. Since cylindrical counters had to be very small to discriminate among the direction of provenience of a particle, Occhialini and Monteux suggested then to substitute the cylinder with a series of parallel molybdenum wires, stretched on glass spikes, between two aluminium plates in a rectangular knock-down ebanite frame, in order to be able to detect particles within a large solid angle and with a good individuation of the direction of origin of the particles. Occhialini-Monteux counter was built in order to detect and study short-living weak-emitting β -sources with absorbers of different thickness.

One of the problems met with in the use of G-M counters lay in the necessity of using resistors of at least $1\text{ G}\Omega$, which clearly showed a lack of stability over long periods of time. Among the possible solutions advanced by some physicists, Cosyns' one consisted in the use of non-ohmic resistors [3], in particular saturated photo-cells. A practical difficulty in the employment of Cosyns' circuit was the high price of photo-cells having a sufficiently high resistance.

Occhialini developed a less efficient and simpler method [4], which was also cheaper than Cosyns' photo-cells. He replaced Cosyns' photo-cell with a thermoionic valve, used with its filament cold but illuminated from the outside so as to act as a photo-cell. Occhialini was able to see that his non-ohmic resistor was much better than an ohmic one, but it could not attain complete saturation.

Occhialini, Pompéia, and Saboya analysed a technical aspect concerning the stabilisation of tension in alternating current [5], a subject of great interest in the researches based on the use of G-M counters, being the instruments with the strongest need of voltage stability. Some problems concerning high-pressure counters could be solved, according to Occhialini, thanks to the properties of synchronous motors, or of particular induction motors, connected to an electrical network with an alternating current at constant frequency. Occhialini's method permitted to use an electric power larger than the one usable with other circuits. This method could thus be used anywhere there was a distribution network that, although it could have slow or fast variations of tension, had very

⁽²⁾ Yolande Monteux was the first woman who graduated in physics in Brazil. She was a member of the group of research in cosmic rays physics led by Gleb Wataghin and was, thus, a colleague of Damy de Souza Santos and Schönberg.

small variations of frequency, as it was in the case of São Paulo. A synchronous motor acted by an alternator could avoid variations of tension, the tension of the alternator being a function of the network frequency only. This looked to be a quite good solution for counters used in the São Paulo laboratory.

Occhialini and Damy de Souza Santos suggested in 1941 a new method of recording the arrival of cosmic rays particles [6]. A typical problem physicists met in their experiments on cosmic rays and radioactivity was the necessity either to record or count particles arriving at random and at a very high rate. A colleague of Occhialini at the Cavendish laboratory, Wynn-Williams, built maybe one of the most satisfactory devices for fast counting: the thyratron “Scale of Two” counter [7], that is thyratron tubes connected in such a way to construct a binary digital counter which gave reliable information of pulses due to particles arriving at a rate up to 2000–3000 per minute.

Occhialini and Damy de Souza Santos’ ingenious device was a cutter connected to a vibrator circuit and a gramophone. Their method consisted in recording, with the cutter, sharp and short electrical pulses produced by a multivibrator circuit on an acetate record operated at 78 r.p.m. No appreciable distortion concerned the shape of the engraved pulses in the tests, so that Occhialini and Damy de Souza Santos’ method seemed to be very useful for counting purposes, and for having a reliable picture of both the size and the shape of the electric pulses. A possible application of their device, because of this high fidelity reproduction, could be its use as an efficient and trustworthy tool for recording physiological phenomena. The main application of Occhialini and Damy de Souza Santos’ device had been four: the measurements of the variations of the intensity of cosmic radiation during the solar eclipses [8] of October 1st, 1940; the study of the statistical distribution of cosmic ray particles; the observations of echoes from the ionosphere; the determination of the disintegration constant of short-lived artificially-produced radio-elements.

3. – The study of cosmic radiation

In São Paulo, with the help of Schönberg, Occhialini worked on a project of researches on the ultra-soft component of cosmic radiation, that is, cosmic radiation whose energy is less than a few MeV. The typical studies on cosmic radiation generally concerned the high-energy component. The high-energy component was of the foremost interest since it was not possible to obtain artificially such energetic particles. Knowledge about the soft component was very less rich, and was limited to the observation of the existence of particles with energies typical of nuclear reactions. Studies on the soft component started in the late ’30s and were found to be strictly bound to three important problems:

- 1) the determination of the mass of the mesotron⁽³⁾;
- 2) the study of nuclear evaporation by cosmic rays collisions;
- 3) comparative measurements made with counters and with ionisation chambers.

(3) The mesotron was thought to be Yukawa’s meson; it is known nowadays as the muon.

Clay and Jonker were the first physicists to study the soft component of cosmic radiation [9], while the Italian physicists Bernardini and Ferretti found a large component of ultra-soft cosmic radiation at sea level. Occhialini and Schönberg researches [10] had the aim to complete Bernardini and Ferretti's results, by means of a telescope of two G-M counters in coincidence, with Cosyns' non-ohmic resistance photo-cells, movable to all possible zenith angles. Occhialini and Schönberg confirmed that the ultra-soft radiation had actually a small energy and was about $12 \pm 6\%$ of the total ionising radiation, but were not able to detect the zenithal effect, within the experiment errors. With the same kind of device, Occhialini and Schönberg tried also to study the neutral component of cosmic radiation, but their results were quite different from those obtained by Clay and Jonker, and Bernardini and Ferretti [11].

Since solar phenomena have an influence on the terrestrial magnetic field and since the intensity of cosmic rays at sea level depends on this field, physicists engaged in geomagnetic research were looking for solar phenomena acting on the intensity of cosmic rays observed at sea level. Occhialini and Damy de Souza Santos' aim [12] was to show the solar effect on the intensity of cosmic rays during the above-mentioned total solar eclipses on October 1st, 1940, that was visible from São Paulo. They actually used two different devices. The first one was a set counters, controlled by a multi-vibrator circuit, which had to count single pulses with, as a counter, a pick-up to record on gramophone plates its vibrations. The second device was a cosmic rays telescope, used to record the coincidences. The telescope showed a raise of 12–15% of the intensity of cosmic rays during the eclipses. The effect started sharply about three hours before the beginning of the astronomical eclipses, when the Moon's shadow was still thousands of kilometres away from the Earth.

Studies on the latitude effect had been made on the whole spectrum of cosmic radiation with ionisation chambers, and on single particles with counters [13]. Knowledge about the latitude effect of showers gave useful information on the soft component and, by comparison with the total effect, some indications on the hard component too. Theoretical studies on the soft component at sea level began in the late '30s. Observational results on the latitude effect for the soft component were discordant [14]. Occhialini's aim was to give his own contribute with a new set of measures made during a journey he made with Schönberg from Salvador do Bahia to Trieste, on January 3rd–19th, 1938 [15]. The detector was a set of three counters at triangle with different thicknesses of lead above. Occhialini found an equatorial variation of about 10% for single particles, and a much lower variation for the showers. He could thus deduce that soft showers and radiation did not show a noticeable latitude effect.

4. – The studies on gamma and beta rays

At the International Congress of Physics-Chemistry-Biology, held in Paris in October 1937, Occhialini submitted the results of his studies on the diffusion of γ -rays emitted by a source of thorium C'' (^{208}Tl) [16]. The aim of his work was to determinate the precise

nature of the radiation emitted under great angles by different elements irradiated by the 2.6 MeV thorium C'' γ -rays. The targets were made of carbon, aluminium, silver, and lead, and secondary radiation was detected by a radiation telescope. Occhialini confirmed the already known works on the study of such a subject. By comparison of the absorption curves for silver and lead, Occhialini directly saw the growing of anomalous radiation as a function of the atomic number, and the plateau typical of a Compton effect in heavy elements. A lead diffuser gave origin to four different kinds of radiation: the radiation corresponding to the Compton effect; a radiation corresponding to positrons annihilation at rest; a high energy radiation with a breakdown at 1.2 MeV; a small component very high energy radiation corresponding to positrons stopping and annihilation.

An interesting note [17] by Occhialini had again the γ -rays as the main subject. He was interested in the very hard radiation emitted by beryllium when irradiated by the α -particles emitted by a polonium source. After its discovery, the main aim was to measure exactly its energy. Occhialini tried an evaluation of the energy of beryllium radiation by the study of the photographs he took in 1933, with Blackett and Chadwick, with the Cavendish controlled cloud chamber in a magnetic field [18]. He chose only the tracks left by electrons whose origin was in the chamber or in a very thin plate able not to brake electrons originated inside it, so that to be sure of their energy. Unfortunately no photograph at all satisfied these conditions, but it was, however, possible to obtain useful information. From the selected tracks it was possible to see that most of the them had $E < 4$ MeV, corresponding to an energy for the beryllium radiation of about 4.3 MeV.

The discovery of the spontaneous decay of the mesotron was seen by the physicists as a mean to verify experimentally the different theories about the emission of β -rays. Schönberg advised Occhialini of the existence of a disagreement on the results of the β -spectrum of rubidium, as obtained by coincidence counters, or by a magnetic spectrograph. The β -spectrum of rubidium was particularly noteworthy for two reasons: the β -decay of rubidium does not involve the emission of γ -rays, and there are also lines of emission over the continuous spectrum. Occhialini decided to continue his studies on rubidium and exposed some considerations on the relative experimental technique [19]. Occhialini had already demonstrated that the magnetic rigidity of β -rays of rubidium was not higher than 1350 gauss-cm, corresponding to a maximum energy of the β -rays of about 140 keV. His colleague of Arcetri, Daria Bocciarelli, found similar values of magnetic rigidity (1296 gauss-cm) and β -rays energy (132 keV) [20], but other physicists found different (lower) results. Occhialini was particularly stricken by these so different values and tried to throw light on the problem. A careful analysis of the various sources of error and of the parameters of the energy spectrum confirmed Occhialini's previous results.

On the basis of this short analysis of Occhialini's scientific production during this period, we have to note his valuable skills in the production of new kinds of instruments, which could be produced in a laboratory with scarce financial supports, and his interest in the researches on typical problems in cosmic radiations and radioactivity studied in the '30s. Although the results cannot be compared in importance to those of other periods of his career, Occhialini always tried to be, with his students and colleagues, on the crest of the wave, with his passion and his deep interest in the contemporary most advanced problems in the physical science.

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Occhialini's contribution to the discovery of the pion. An interview by L. Gariboldi

WILLIAM O. LOCK

CERN, Geneva, Switzerland

LEONARDO GARIBOLDI

*Istituto di Fisica Generale Applicata, Sezione di Storia della Fisica,
Università degli Studi di Milano, Italy*

The paper “Processes Involving Charged Mesons” [1, 2] signed by Cesare Mansueto Giulio Lattes⁽¹⁾, Hugh Muirhead, Giuseppe Paolo Stanislao Occhialini, and Cecil Frank Powell⁽²⁾ was published in the May 24th, 1947 issue of *Nature*. In the introduction to this paper we can read the following announcement:

“we have found evidence of mesons which, at the end of their range, produce secondary mesons.”

The primary mesons, whose discovery was announced with these very words, were at first thought to be the long searched for pions, the particles responsible for the strong interaction predicted by Hideki Yukawa in 1935⁽³⁾, the secondary mesons being the muons discovered by Carl Anderson and Seth Henry Neddermeyer in 1937⁽⁴⁾ and identified with a particle different from Yukawa's meson by Marcello Conversi, Ettore Pancini and Oreste Piccioni in Rome [18-26].

⁽¹⁾ On Lattes, see [3-5].

⁽²⁾ On Powell, see [6, 7].

⁽³⁾ On Yukawa's meson, see [8-15].

⁽⁴⁾ On the discovery of the muons, see [16, 17].

The research of Powell's group⁽⁵⁾ at the Wills Laboratory in Bristol leading to the discovery of the pion had been the second fundamental contribution by Occhialini to the physical sciences, after his work at the Cavendish lab with P. M. S. Blackett in the early thirties⁽⁶⁾. The series of important results obtained by Powell's group using the nuclear emulsion technique, thus improving the physicists' knowledge of cosmic radiation, made Bristol a capital in these studies, "the big sun surrounded by little satellites", according to an expression used by Louis Leprince-Ringuet at the 1952 Rochester Conference and a capital in the network of universities and laboratories that joined together in European collaborations⁽⁷⁾.

The nuclear emulsions used at the Wills Laboratory had been experimentally produced by Ilford Ltd. and exposed on mountains at high altitude to detect the interactions due to cosmic rays. Powell had been interested in the use of nuclear emulsions from 1938 on, by a suggestion from Walter Heitler, and preferred them to the cloud chamber because of the superiority of their results, and began to increase their ability to detect particles other than low-energy protons.

In 1944, Blackett invited Occhialini, then in Rio de Janeiro, to work with the Americans on the atomic-bomb project. Blackett's suggestion was kept secret to avoid repercussions on Occhialini's relatives in Italy. Occhialini arrived in the United Kingdom on January 23rd 1945, but, because of his nationality, he was not permitted to participate in war research. He thus spent some time in London, working with Edward Appleton, and then a few days at the General Electric Company. He was then invited by A. M. Tyndall, the director of the Wills Laboratory, to join Powell's group.

In the meantime, Blackett, via the Cabinet Advisory Committee on Atomic Energy, set up an "emulsion panel", chaired by Joseph Rotblat, to support and encourage industrial firms to produce better nuclear emulsions, such as Ilford and Kodak for more sensitive emulsions and better precision microscopes such as Cooke, Troughton and Simms. Among the members of the panel were Powell (Bristol), Livesey (Cambridge), Perkins (Imperial College, London) and May (King's College, London). Occhialini was not a member of the panel but he had a considerable influence on its work [32].

Occhialini, Powell, and their collaborators developed new kinds of plates such as the Ilford "Nuclear Research Emulsions", able to record the tracks of particles of more than

⁽⁵⁾ On the Bristol school of cosmic-ray physics, see [27]. On the discovery of the pion, besides the other works cited in this chapter, also see [28, 29]. A fundamental book on this context is [30].

⁽⁶⁾ It is an interesting fact that both Blackett and Powell had been Rutherford's students at the Cavendish laboratory in Cambridge.

⁽⁷⁾ "Without doubt Powell's main contribution to cosmic ray studies was the development of the nuclear emulsion technique into a precise tool for identifying the characteristics of ionizing particles traversing them. Pre-eminent was his discovery, with colleagues, of the π -meson, or pion, a result of fundamental importance for the development of our ideas about nuclear forces [...]. The desire to observe the primary cosmic rays themselves led to the use of balloons to carry the nuclear emulsions to great heights; in turn this gave rise to big international collaborations, collaborations which played a big part in the eventual creation of CERN." Quoted from [31], p. 32.

six times the minimum ionisation. They were joined in winter 1946 by Lattes from São Paulo. The first exposures on the Pic-du-Midi were made by Occhialini, who, with the help of his friend Max Cosyns during a speleological campaign in the Bas-Pyrénées in August 1946, aided the transport to the Pic-du-Midi of a stack of Ilford C2 plates. The discovery of the first two “double meson” events happened probably in November 1946, although the publication was delayed until May 1947. The first observation was made by Marietta Kurz, and within a few days Irene Roberts found an event with the two tracks completely inside the emulsion⁽⁸⁾.

The search for a confirmation of the discovery of the π -meson made on the Pic-du-Midi took Lattes to the Chacaltaya meteorological station in Bolivia at high altitude (5500 m)⁽⁹⁾. The first plate was developed in La Paz and scanned in Rio de Janeiro and a complete track of the “double meson” was found. With the discovery of the pion, a further step was made in the study of cosmic radiation thanks also to Occhialini's ability in developing a new kind of physical instrumentation.

* * *

Dr Lock, in 1947 the group of physicists working at the Wills Laboratory in Bristol announced the discovery of the pion. The Group was headed by Cecil Frank Powell and the pion was a particle which had been predicted by Hideki Yukawa in 1935. The discovery of the pion and the contemporary discovery of the V-particles [35] were maybe the last major discoveries made in Europe before the 1973 discovery of the neutral current. Four physicists of Bristol signed the paper published in “Nature” in May 1947: Powell himself, Occhialini, Muirhead and Lattes. The pion was discovered among cosmic-ray particles, but Powell used nuclear emulsions also with particles produced artificially before 1947. Before the war he exposed emulsions to a proton beam and during the war Powell studied neutron spectroscopy. Why did they choose to study interactions of cosmic rays and not only artificially produced particles?

Lock: Because the intensity is greater. The number of interesting events you find in a plate was much higher the higher you went. In the original exposures you only find an event of interest very rarely, whereas when you are at mountain altitudes then the flux of incoming cosmic rays is very much greater. It was a question of intensity.

They thought they were ready to use nuclear emulsions with particles that they were unable to control, of course, both in energy, number, and flux, didn't they?

Lock: I'm not completely sure about this. I think the point is that because of the need to have a higher intensity of incoming particles you had to manufacture and fly balloons, The people who pressed for balloons in Europe were Muirhead and Camerini, with Powell originally being hesitant. I was there, so I can remember this very well. It

⁽⁸⁾ On the discovery of the π -meson, see also [33].

⁽⁹⁾ On cosmic-rays studies at Chacaltaya, see [34].

took some time to convert Powell to balloon flying, but then when he did then of course he was immediately very effective.

Why did Powell not agree with the use of balloons? In 1947, Bradt and Peters in the US used balloons with emulsions, and Donald Perkins flew them on an airplane.

Lock: Yes, that's right. A big company called General Mills was making balloons in America much sooner than in England, but you're right, Bradt and Peters were the key people. With the balloon flights an exposure of several hours was all that was needed. Again, I remember very well when we started very early on in the balloon flying game, being on the back of the motorbike of the technician of the group, Max Roberts, following the balloon in the sky, and after three or four hours, the laboratory sent a signal to cut the parachute off and down came the balloon with the emulsions. Of course these eventually ended up in the G-Stack in Italy. Originally, I think, Powell was at least hesitant but he didn't stop it, he didn't stop Camerini. We had the advice of a naval research man, Tom Coor, who was at that time attached to the U.S. embassy in London as O.N.R.L. representative.

Were they looking for the pion or was it a case of discovery by accident?

Lock: I think it was a discovery by persistence. It was just a question of going on to greater sensitivity, greater thicknesses and discovering more events.

With the 1947 paper, did they feel that the search for the pion was at its end or not?

Lock: No, I don't think so.

The discovery of the π - μ - e decay in 1948 maybe was an end point?

Lock: Yes. That was the first piece of research work that Muirhead and I did together. The electron decay, the spectrum of the electrons from the decay of the muons. Then you have to remember that the tau mesons with the 3π decay mode were then found almost immediately. Once the electron-sensitive emulsions were available, this 3π decay was then observed. Then came the discovery of $K_{3\pi}$. This meant that the doors were open to discover more things day by day, starting with this 3π decay. This was one of the first interesting events and it happened to be mostly within the layers of emulsion. You didn't lose the particles, they stayed in the layer. What we did for the electron decay was to use the Telepanto, which was Occhialini's invention, which was a precision

microscope, that could protect the image within the emulsion upon a big screen⁽¹⁰⁾. So grain by grain you would copy the grains of the emulsion and then we used a set square. We had a draughtsmen's drawing board and plotted the grains along the track of the electron or positron with a protractor and then we simply measured the segments of the electrons, and that was early in '49. There was a Como conference on cosmic-ray physics in '49, and I was the second speaker to give the first results of the decay spectrum of the electrons. I remember very well the only person who asked a question was Pontecorvo. Blackett was chairman of that opening session and he made me stop. I asked "Two more minutes, two more minutes!" And he gave me the two more minutes.

Did you know the result of the experiment of Conversi, Pancini and Piccioni?

Lock: I think the answer is yes, we did. Because, also, if you look in the paper by Davies, Lock and Muirhead [37] you'll see that other references are mentioned. Later, others in the States measured the decay spectrum with a falling cloud chamber, not a bubble chamber, and others used counters and we used emulsions.

Did you know the theory on two mesons of the Japanese physicists Tanikawa, Sakata and Inoue?

Lock: Not immediately.

Maybe after the discovery of the pion?

Lock: I think so, because you have to remember that the Japanese theories only became known in the West with a considerable time gap due to the war [38,39].

Can you describe the role played by Occhialini in the studies with nuclear emulsions, which was his particular contribution concerning the discovery of the pion?

Lock: I would say that there were two things. First of all, his enthusiasm and his long working hours and his commitment to the discovery of the pion was really very great. In particular, of course, this question of processing emulsions was not entirely new to him but in a major part it was his pressure on Ilford, pressure on Kodak, to come up with more ideas. His commitment to the physics, I would say, was very marked and it

⁽¹⁰⁾ "[...] visitors were more frequently shown a whole gamut of the events on a screen in the darkroom by a microprojection arrangement set up by Occhialini and christened by him "the Telepanto" from (he said) "Tele: I see, Panto: Everything!". In it the stage of the microscope was given a slow transverse motion by clockwork, and at the same time focusing in depth was put into regular slow oscillation, in order that the viewer could follow tracks dipping into the emulsion or out towards its surface." Quoted from [36].

is a pity that it was to some extent marred by the eventual conflict between Powell and Occhialini. They were rather different people.

What can you tell us about the development of thick emulsions? Who had particular ideas concerning the way to get thicker emulsions?

Lock: I think in a way it was self-evident that if you want to be able to measure things in emulsion you've got to have enough track length. It was realized early on, probably by Powell himself, that the way to the future was thicker emulsions. Because of this belief, naturally people pressurised the manufacturers to make them and then it became standard. Occhialini's major contribution was the elaboration of the essential two-stage "temperature development method for the processing of thick emulsions" [40].

The study of cosmic rays with nuclear emulsions implied a collaboration with the industry, with the industrial producers of photographic emulsions. Can you describe the collaboration of the group with Ilford and Kodak?

Lock: The collaboration took the form via what was known as the "emulsion panel" mentioned before. This consisted of representatives from industries and all the major laboratories using emulsions and wishing to improve them. This panel met, I think, only once a year, maybe twice. Livesey was the representative from Cambridge. You could see from the attendance list that this panel was a connecting voice of the different elements to put forward recommendations to the Government and to say: "please, send a contract to Ilford, give a contract to Kodak, give a contract to Cooke, Troughton & Simms, with time limits and cost limits". It was this informal panel, which existed for about six years, which was absolutely essential to make sure that things were done systematically, and whenever a new step was taken then of course this emulsion panel would follow what was being done. It was a very fruitful interaction between the industry and the scientific world.

And which was the role played by Joseph Rotblat, in particular? He chaired the panel?

Lock: He was the chairman of the panel. He and Powell were good friends, that's for sure. Rotblat started to write a book on nuclear physics, but he got so tied up with other things—I don't remember the details—that he never wrote it. When the Second World War broke out, Rotblat was in Poland and two days before the Germans invaded Poland, 1939 he came to England to work with Chadwick on the atomic bomb and he's one of the few, if not the only one, of atomic scientists who refused to work on the project and left. As I say, he left Poland, came to England, and two days later, after his arrival in England, Hitler invaded Poland. He never saw any of his family again, all were wiped out. He of course stayed in England, instead of going back to Poland, where he had had a permanent job. He developed his career on the Liverpool machine, the Liverpool accelerator. It's a tragic story, really. Poor Rotblat, but he was a very nice man, he was special.

How was the collaboration between Occhialini and Ilford and Kodak? For instance with Cecil Waller, with Berriman?

Lock: I don't know but he was not a member of the "emulsion panel", at least not as far as I could see. On the other hand, he must somehow have interacted with Powell, Waller and Berriman. But there's nothing official about it, not that I know of.

Were there any problems concerning industrial secrets?

Lock: Not that I know of, no.

As for the microscopes, what about the collaboration with the producers of microscopes, with Cooke, Troughton & Simms? How was this collaboration?

Lock: Again, I have no direct knowledge. It's a good study, this interaction as I said before between pure science and materialistic industries.

There were young physicists in Powell's group, some of them coming from abroad, for instance Lattes in '46, Camerini in '47 and so on. Who generally decided to call whom, and why? For instance, we know that Lattes was suggested by Occhialini.

Lock: Camerini was suggested by Lattes. I think what happened was that, quite early on, Powell's laboratory became a place people wanted to go to, so it wasn't so much a question of going to look for somebody, it was a question of making a choice between different people wanting to come. For example, when I graduated from Bristol and I had a good degree, I was given a choice of working with Powell and his group or working in the low-temperature group. So, because I knew already something about Powell's group and I'd been there as a summer student for a month or so, and I was already attracted by the idea of a multinational grouping of people, I chose Powell's group. I was following Muirhead who had come the year before. People just wanted to come, so there was no need for advertising for whom we wanted, we had plenty of them.

In 1947 how was Powell's group formed? How many people were there and with which role? How many physicists, for instance?

Lock: I think you only have to look at the people in the classical picture of the group: Powell, Occhialini, Muirhead, Lattes.

Let us say about ten physicists, professors and students?

Lock: I would say it was less to start with, I would say six.

And what about the scanners, the microscopists: how many people were there at the time?

Lock: Well, they steadily increased. One of them, my wife, is here. How many scanners were there? About fourteen at a guess. That was in 1950.

And in '47, maybe?

Lock: There were a few. There was Mrs. Powell, Marietta Kurz, Irene Roberts, Mrs. Andrews. If you look at a group photo taken in 1949, of the scanners, you see, there are twelve to fifteen scanners. It was quite a big group.

What training did the scanners have?

Lock: Nothing formal. They learnt on the job.

But were they trained to recognize particular tracks or not?

Lock: They knew what they were looking for and they had also to be aware of unusual things. To ask, not just think "oh, that's nothing". They always had to go to a physicist and ask. All the things there, it was the scanners who found them.

The scanners' contribution was recognized in the articles published at the time, and Powell wanted this fact. Can you tell why? After a few years, in the fifties, we did not have the name of the scanners on the photographs when they were published.

Lock: Oh, I see. I always thought it was a rule that they had to put all their names. Well, I think it was Powell's general policy to acknowledge the work done by putting the name of the person who found it, because of his attitude to life, that "people-were-equal" sort of thing.

Were most of the scanners women because of economical problems?

Lock: I don't know. Maybe it was considered a woman's job. On the other hand, Freddy Hertz, when he came to Geneva, had men scanners, he brought them from Rome. Don't forget that this was fifty-five years ago, when women were the workers but I think Powell was the driving force to name the scanners who found the events. There was a good atmosphere amongst the scanners. There were fourteen on the fourth floor at one stage when I was there. With fourteen of them you would think there'd be tension, but it was quite the opposite. Because we all made allowances for each other. We had some good parties together, scanners and physicists.

How was the life in the laboratory?

Lock: To some extent, of course, the cosmic-ray people were cut off from the rest because they were doing something rather different to low-temperature physics, or electronics or something like that. Also you happened to be on the top floor of the building, which physically put you at a distance. In order to help people mix between the different specialities there was the event of afternoon tea and that was another way in which people would meet each other and there was a good spirit in the laboratory because Tyndall had got together some very good people.

What about the time of work? Occhialini once wrote that you were often unshaved, maybe unwashed.

Lock: That's right. Yes, I think I was unshaved. I would say the life in the laboratory was very pleasant. There was a very friendly atmosphere. The fact that so many physicists married the scanners shows there was a good atmosphere. I think practically all of us are still together. I can't think of one marriage that has broken up.

How many rooms were there in the lab on the fourth floor?

Lock: Oh, I have no idea. Of course the physics building has changed now. The fourth floor was really in the roof, because the floor of the fourth floor was glass. If you're on the third floor working on the corridor, above your head the ceiling was glass. Powell's group was in this space, attic space, underneath the real roof, and actually that gave the length of free space to make the balloons, because otherwise how do you make a balloon? You need space. The rooms were more like plywood cubicles. For the research staff there was a dark room in the middle of the floor, then maybe eight rooms devoted to Powell.

And what kind of instrumentation had you got in the laboratory: microscopes, devices to process the emulsions? Can you describe it?

Lock: I think the important thing was to be well provided with microscopes, and there Cooke, Troughton & Simms filled the job very well. There were quite ten microscopes for the scanners, because there were four in the room where everyone used to come for tea, there were two where Peter Fowler was, two where Perkins was, two where Dainton was—and a projection microscope for Dainton, a Telepanto—there were eight microscopes on the glass floor, and then another four.

And did you process the emulsions by yourselves?

Lock: Yes, but not always. We had a dark room which was in the middle of the floor, part of the floor upstairs. At some stage, when we were at the very beginning of what we were doing, people used to sleep in the dark room on a simple bed in order to get up

in the night and change the fixing solution. I remember doing that. So it was usually the physicists who were involved.

How did you spend your time outside of the laboratory? Was there a life together?

Lock: There were the evening parties, there was the balloon flying which is not a pastime. We went on holidays together to the Dolomites. Powell and Mrs. Powell came with us, Goku Menon, Dainton, Mulvey. We all came to the Dolomites in 1950 and had a mountain guide for two weeks. This shows you Powell wasn't stand-offish, he was very much friendly with everybody.

Where did Occhialini live in Bristol? How was his life?

Lock: I couldn't tell you how his life was. But I think he had an apartment somewhere near the university. He was a great smoker. There were very few cigarettes in those days, it was just after the war, and he and Camerini used to pick up fag-ends, and then make cigarettes with the bits left over that the other people were throwing away. Very unhygienic. Occhialini was quite the expert at that.

What about the financial support? Who paid for this research?

Lock: The Department of Science and Industrial Research, which is obviously a government agency. It was for some time the Ministry of Supply. It was a bit complicated. I don't remember who the DSIR (Department of Science and Industrial Research) reported to. There must have been a minister above, there must have been some minister for science. Powell managed to get a block grant year by year on a well-planned basis, given the importance, perhaps, of Blackett in the wheels of government. For example, people like myself had a government grant. I had a fixed amount, and when I came to the end of three years I stayed on another year with Powell as a researcher, but again, paid out of his government budget. So there was no problem then with money, it was not easy perhaps, but the work was regarded as something important, as Powell was a Nobel Prize winner⁽¹¹⁾. That's another example the atmosphere. . . . When Powell got the Hughes Medal⁽¹²⁾ of the Royal Society, the year before he got the Nobel Prize, what he did was but take all of the physics group out for dinner to celebrate his award. When it came to the Nobel Prize, we had a party for all the physics department.

⁽¹¹⁾ Powell was awarded the 1950 Nobel Prize in Physics "for his development of the photographic method of studying nuclear processes and his discoveries regarding mesons made with this method."

⁽¹²⁾ Powell was awarded the 1949 Hughes Medal "for his distinguished work on the photography of particle tracks, and in connection with the discovery of mesons and their transformation."

Occhialini spent three years at the Cavendish in 1931-34 with Patrick Blackett, and Powell himself worked at the Cavendish with Wilson. Which are the similarities with the laboratory practice at the Cavendish and in Bristol? Can we speak of a Rutherford school coming from the Cavendish to Bristol? Was there a peculiar way to make physics, maybe?

Lock: Well, it's a difficult question, but I think the Cavendish was an old, established, rather conservative place. For example, you couldn't stay working in the evening after six o'clock under J. J. Thomson, and certainly when Powell was there as a young researcher, that was one of the problems. They could only do their work during the day time, they could not stay in the evening. That makes things a bit too formal and it was a bit stupid, really, if you're in the middle of an experiment and all of a sudden they shut the doors. So I think that was the attitude of Powell, that he was very open. Some people got a DSIR grant, just as I have said, Goldschmidt-Clermont for example, but others would come with their own funds, they would have no parachute. It was a question of being accepted by Powell—or not, as the case went. So there were quite a few cases of Indians who were not that well trained in physics, who would want to come and had to be refused in some way, although Powell tried very hard, I think, to have this international mix. So, for example, we had an Indian from Chandigarh, in Northern India. There was Menon, of course. Menon was, later on in his career, Minister of Science and Technology in India.

Which was the relation of your group in Bristol with Blackett's group in Manchester? Was there any collaboration?

Lock: Well, don't forget that I was only in Manchester a short time, seven months. Again, there was the problem of the Nobel Prize, that Blackett got the Nobel Prize⁽¹³⁾ and Occhialini didn't. Then, when he came to Powell, Powell got the Nobel Prize and Occhialini didn't. This caused bad feeling.

They were studying particles with cloud chambers.

Lock: Cloud chambers, that's right. But the emulsion people in Manchester... there were only two of us, John Major and myself. Blackett had set up a cloud chamber group with Barrister and Butler. At some stage he became convinced that the people should do emulsion work as well, and so there was in Manchester a very small emulsion group of about four young post-graduate students and three scanners. My wife was one of them. We had got married, but left in March '53. We went in September '52 and we left seven months later to go to Birmingham.

⁽¹³⁾ Blackett was awarded the 1948 Nobel Prize in Physics "for his development of the Wilson cloud chamber method, and his discoveries therewith in the fields of nuclear physics and cosmic radiation."

On your opinion, why did Occhialini leave Bristol?

Lock: I don't know for sure, but I think it was his unhappiness with being to some extent neglected, in a sense that he didn't share the Nobel Prize.

But he left in '48, two years before Powell was awarded the Nobel Prize.

Lock: Yes, he left in '48, before that. The very fact that the book "Powell and Occhialini" [41] was not credited to him in the alphabetical way —Occhialini and Powell, as it should have been, had quite a substantial effect on him, that he wasn't being recognized. In fact, I spoke to Freddy Hertz recently, who said that after being in Bristol and in Brussels Occhialini was really just unhappy that he didn't get a share of the Nobel Prize. So, I think he was perhaps too eccentric. Brilliant, but too eccentric. On the other hand, to work with two Nobel Prize people, Blackett and Powell and not be a laureate too. . .

Do you know why he chose to leave in particular to Brussels? He was invited by Max Cosyns to build a new group in Brussels, but maybe he could have chosen other destinations, maybe again Brazil or Italy?

Lock: Good questions, but I don't know the answer.

Do you want to say a last thing about the Nobel Prize?

Lock: It is something which still is widely recognized as being only given to people who deserve it. Of course, people's ideas as to what is good physics and what is bad, vary, but I think the Nobel Prize is worth keeping, it's been a recognition of merit and in the case of Powell, the Nobel committee realised that they were neglecting a field which was important and growing. I remember we were so excited when we heard that the Nobel Prize had been awarded, we thought we were the best in the world. We probably were, for a bit.

As a conclusion, would you like to tell us a personal reminiscence or comment concerning Occhialini in Bristol?

Lock: Well, just thinking it out, I think Occhialini was a good thing, because you need people to have wild ideas, not in agreement with everyday beliefs, and he got things done. I think Occhialini was just showing this enthusiasm for something new and he was ready to work all hours, day and night, in order to take the next step. What was the pion really, was it a low-mass meson or was it a high-mass meson? There were in fact two groups at one time using different techniques to find out what the mass of the pion was. You need a few people like Occhialini to push things and trigger things. The steady step after another approach is equally needed. So in a sense they were a good combination when they weren't arguing. There were in fact two Bristol groups working at the same time using different techniques to find the mass of the pion. Both techniques found an answer but the Muirhead group were the closest to the present-day value.

* * *

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Occhialini and the Université Libre de Bruxelles.

An interview by L. Gariboldi

ALBERTO BONETTI

Università di Firenze, Italy

LEONARDO GARIBOLDI

*Istituto di Fisica Generale Applicata, Sezione di Storia della Fisica
Università degli Studi di Milano, Italy*

Prof. Bonetti, Occhialini left Bristol in 1948 to begin a new scientific adventure at the Centre de Physique Nucléaire of the Free University of Brussels, a group led by Max Cosyns. You joined this group a few months later.

Bonetti: Yes, I did. In spring 1948 Giuseppe Occhialini returned to Italy after almost ten years of absence. He paid a visit to his father Augusto Occhialini in Genoa, where the latter was restoring good teaching and research conditions after the difficulties of the war. It was then that I met for the first time Beppino, not yet Beppo for me. He asked his father if there was a young physicist willing to join him in Brussels. He added that the young man should go there with a microscope. At the time I knew nothing about microscopes and nuclear plates (I was playing with ultrasounds and had seen only a few photos of tracks of charged particles which Beppo sent his father as greeting cards!). I accepted the proposal of my professor Augusto, and with some naïveté I accepted also to take with me the century-old brass-bright monocular making a fine display with its immersion objective in a cabinet among other less obsolete optical instruments (I wonder if my prof intended somehow to test me out). I arrived in Brussels, Université Libre, Groupe de Physique Nucléaire, on September 1st 1948, and was put right away in front of my microscope to look through some small plates. I did not see anything else than ill-formed blobs of grains, but everybody was happy: “Those are electron tracks!”, a first attempt to electron-sensitive emulsions.

How was the situation of the researches in physics in Brussels at that time?

Bonetti: I discovered there something new for me, open to the world. I went on trying to learn quickly what I had to do and to understand the way of life in that centre of research, not big, but full of enthusiasm and ambition —and also of pressure. So I have only a vague recollection of what was going on in other parts of the University building, and also in other parts of the country. In the Physics Department, apart from Max Cosyns, I remember Auguste Piccard preparing something about a next expedition, Madame Piccard helping her husband, and Paul Kipfer (a nice person full of recollections and anecdotes, as the one about himself explaining the principles of thermodynamics to an absent-minded Wolfgang Pauli just before this latter had to give an undergraduate lecture). The professor of Theoretical Physics was Jules Géhéniau, who had a part in the direction of the Centre. His wife, Madame Géhéniau, was the very helpful and charming secretary of the Group. Max Cosyns was an able director, ready to satisfy Beppo's requirements in setting up the Group, starting with microscopes and scanners. In particular he was able to provide some money for extra people, as was the case for me.

Which were the main researches at the Centre?

Bonetti: The science was Cosmic Rays and Radioactivity, but much effort was put in the quest for a reliable processing technique of nuclear emulsions, which was at the time a crucial problem. I had a part in it under the leadership of Beppo and Constance (Connie) Dilworth, the two scientists who worked out the "temperature method". The method was optimised through a previous study of the geometrical stability and possible deformations of the sensitive layer during the various steps of the processing, which would result in a distortion of the tracks. The study of the emulsion permeation and dilation in liquids of different pH was carried out by Edgard Picciotto, a geologist, and Adrienne Ficq, a biologist, both interested in the use of nuclear emulsions in their field of research. They succeeded in measuring precisely the thickness variation of emulsions soaked in solutions of different acidity and found that basic solutions produce a much larger swelling than acidic ones. Then Beppo found in an old photographic treatise that Amidol was the developer which allowed the lowest pH. Then again Connie was the one who got from the shelf a bottle of boric acid in order to prepare with sodium sulphite the buffer solution to stabilise the pH of the developer. Amidol with boric acid was a winning card of the Brussels group. The method was adopted by practically all the groups working with nuclear emulsions.

Was there a particular idea or contribution that led to the temperature method?

Bonetti: The basic idea of Beppo and Connie was that developers act very slowly at low temperatures; the second point was that the swelling time is much less dependent on temperature. I do not remember if Cosyns played a role in the method itself: while I was in Brussels he certainly contributed to the design and realisation of stainless-steel developing vessels of larger and larger dimensions and with temperature control devices. When

“Poohnicastro”, the first double-walled, quasi-optically flat, silver-plated, 45×45 cm vessel started working, Beppo told me: “Here it is! Now I have my own cyclotron.” After Cosyns left the Group, the construction of a large-dimension developing plant went on in Brussels under the direction of Beppo: it was the largest at the time. The “temperature method” was instrumental in the use of thicker and thicker emulsions and in the management of stacks of stripped emulsions of large dimensions. Usually our Ilford thick emulsions looked a bit brownish after development. Kodak emulsions looked a bit reddish. In the 1957 meeting in Strasbourg devoted to the use of nuclear emulsions, a Russian physicist, Konstantin Sergeevich Bogomolov, presented his own emulsions: 400 micron thick, Amidol-developed, they were crystal clear. But the amount produced was small and the thickness inconvenient. I do not know what happened later with Russian emulsions.

How was the collaboration of Brussels with Waller of Ilford?

Bonetti: Waller was the man who manufactured reliable, high-quality emulsions with almost perfect reproducibility, independently of the amount requested by experimenters. I could hardly understand a single word of his spoken English, so Connie revealed to me a secret of British society: “Waller’s family does not belong to the establishment. He is a self-made man, no public schools, so he speaks working-class English.” Waller was a valuable person and very correct in his relationship with his customers. The Brussels group had a very kind and open relation with him. I remember when one day Connie stood up from the microscope shrieking: “Fungi! Fungi!”. There was a sudden sensation of scare. She phoned Waller: “We have fungi!”. The answer was: “Tymol in the washing water”. We never had fungi again. The important step was the massive production of minimum-ionisation-sensitive emulsions which led to the discovery of the μ -e decay and of the K-meson by Bristol and of the Hyperons by Brussels-Genoa-Milan. A trick suggested by Bates and Occhialini [1] (still in Bristol) was to have the emulsions mounted on 1 mm thick glass plates: with a suitable objective it was possible to turn the plate and observe through the glass and even make two-plates sandwiches. Occhialini asked the British firm Cooke to produce a long-working-distance immersion objective to that effect. Later also Leitz and the Italian Koristka produced those objectives with magnification up to $100\times$. All these tricks turned out to be very useful with the advent of stripped emulsions, first proposed by Shapiro and Stiller of the Naval Research Laboratory, and then used massively by Bristol and the other nuclear emulsions groups. As it happened, I was the one who discovered Koristka because their rather rough binocular microscopes were cheap enough that we could buy them in Genoa at the very beginning, when I was back from Brussels, summer 1949. That serendipitous choice had unexpected consequences, because the important person in Koristka was Claudio Cantù, an old acquaintance and friend of Beppo at the time of Arcetri, twenty years before! The collaboration of Occhialini and Cantù led to the production of the outstanding MS-2 and MS-3 microscopes which made the glory of emulsions for more than ten years. . .

Which was Occhialini's particular contribution to the development of electron-sensitive emulsions?

Bonetti: No doubt that Beppo stimulated Ilford to improve the C3 emulsions which were the endpoint of a generation of emulsions with which Lattes, Occhialini and Powell obtained such important results as the π - μ decay. Beppo was not alone in the action of stimulus to Waller, but I believe that he played an important role in convincing Ilford of the soundness of producing the next-generation emulsions sensitive to minimum-ionisation particles. The first G5 were available in Brussels in spring 1949. Before going back to Genoa, I studied with Yves Goldschmidt-Clermont the range-energy relation for slow electrons. Then Giovanna Tomasini and I carried out a reliable statistics of the μ -meson decay in plates exposed to Cosmic Rays and developed by Brussels. We were dumb enough as to lose the possible identification of a K-meson decay in those plates. A K-meson was observed for the first time in Italy by Levi-Setti and Lovati in plates lent by Brussels.

How was the problem of emulsion distortion confronted?

Bonetti: A first step was the 1% glycerol bath which gave the processed emulsion very nearly the same thickness as the unprocessed one. Then Beppo invented the guard-rings method to dry emulsions with a minimum amount of distortions. In a drying room where there was a temperature-controlled air draft, the glycerol-treated emulsions were surrounded with strips of gelatine-coated, water-soaked plates whose thickness was brought to be much the same as that of the emulsions to be dried. This avoided the quick drying of the borders of the emulsions, which was the main source of distortions. Bristol proposed a nice geometric method to correct the false curvature of tracks introduced by quasi-regular shears in emulsions during drying.

The Brussels group also studied the magnetic deflection of tracks. What was Occhialini's role in it?

Bonetti: The seminal work by Bates and Occhialini [1] contained the suggestion of using the gap between sandwiched plates for magnetic deflection measurements. The first experiment was carried out by Yves Goldschmidt-Clermont and others in Bristol, and repeated by Merlin, Someda and others at the Cancano Lake in Valtellina (Italy). Goldschmidt-Clermont was a very attractive person. He had met Beppo in Bristol and played an important role in Brussels for his cleverness and for the kindness of his nature.

How was the relation with the producers of microscopes?

Bonetti: Beppo was in touch with Cooke, Troughton & Simms when he was in Bristol and obtained from them a first long-distance immersion objective. But he asked other firms to produce those objectives and also to produce microscope tables with precision

movements. Leitz produced very good long-distance objectives, while the ones produced by Koristka were not perfect, but less costly, and contributed to a lot of good work. But the interesting result of the collaboration with Koristka (*i.e.* Claudio Cantù) was the design and production of the large-table, high-precision microscopes for multiple-scattering measurements already referred to. The mathematics of the MS-2 spring movement, based on the principle of certain seismographs (I can't remember who had the idea), was worked out by a good friend of Beppo's, J. Plainevaux.

How many microscopes were in use in Brussels?

Bonetti: When I first arrived in Brussels there were perhaps six or eight, some Cooke microscopes, others of Belgian make, all conventional binocular microscopes. A critical point was the quality of the movement of the table in view of measurements of angles and of lengths, both important in the measure of the mean angle of "multiple scattering" of fast particles. When the collaboration with Cantù began, there was a kind of competition between the method of the sagitta, measured with a micrometer at regular distances along the track, and that of the angle between successive segments of the track, measured by means of an ocular goniometer. I think Beppo felt that the method of the scattering angle was not promising, while Cosyns insisted on it. Cantù even produced a maquette following Cosyns' ideas. At that time Riccardo Levi-Setti went to Brussels and Beppo assigned to him the task of comparing the sagitta and the angle method. In that work Levi-Setti showed his understanding, his critical sense and the skill of his hands. At the end he went to Beppo and was worried because he had found that the sagitta method was the better one. Beppo found the way to tell the truth without insulting anyone. Since then the angle method was abandoned and Cantù started designing microscopes optimised for sagitta measurements: a first attempt was the MS-1, which was soon dwarfed by the MS-2 and MS-3. For these ones Cantù built also an interferometric gadget for the control of the quality of the movement of the table.

Where did you usually expose your plates?

Bonetti: In the first years mainly on the Pic-du-Midi, sometimes on the Jungfrauoch. Then there were the first balloon flights started by Bristol in England, if I am not wrong, and followed soon by flights in Italy, Sardinia and Po Valley. It was the beginning of collaborations among several Italian groups, Genoa, Milan, Padua, in association with Brussels where the plates were processed. Some of the balloons were even produced in Padua by Gianni Quarenzi, one of the valuable young physicists whom Michelangelo Merlin (who had spent several months in Brussels in 1949) was leading to success in the emulsion group he set up in Padua. Physicists like Marcello Ceccarelli, Gianni Quarenzi, Milla Baldo Ceolin, began their career in Merlin's group.

Can you describe the Brussels laboratory?

Bonetti: For some reason my memories are vague as far the structure of the place, less vague about people and the spirit of the place. There was a common room which looked like a kitchen with a cowl, shelves with chemistry vessels, a sink to wash everything including one's hands, gas bottles. We spent our spare time there, having coffee and discussing our work in an often casual way. We used to speak French, sometimes English with Connie. There were also fervent discussions on subjects unrelated to physics. I understood certain peculiarities, I dare say oddities, of Beppo's behaviour thanks to his discussions with a psychologist, the husband of one of the girl-physicists: they indulged in arguing heatedly about the psychological traits of the Nazi leaders on a Freudian basis (the Nuremberg trials were a big issue at the time). The talk was also about music and literature. I consider myself lucky in my life because I found that many of the physicists I met in Brussels and elsewhere were not only good in their field, but also carriers of a "humanist" culture. Beppo was one of them. He had a touching knowledge of Shakespeare and enjoyed quoting by heart the most notorious passages, generally with a direct reference to what was going on. I mentioned already Madame Géhéniau, the nice and efficient secretary of the group. Madame Cosyns came sometimes to help her husband in the preparation of an expedition whose scope I don't remember. There was Picciotto with his bottles for the pH studies and his penetrating judgements about science—and people. There was Aldo Igiuni who was to become a good friend and a perfect fellow worker. There were two dark rooms, in one of which there was my microscope. In the microscope room two microscopes were for Beppo and Connie. There they observed a notorious disintegration star with big nuclear fragments. With the after wit, one of those fragments might have been a hyper-nucleus. I think it was the last important work done with C3 emulsions. I remember a large room divided into two parts. The upper part gave way to the dark rooms; the lower part, among other things, contained the processing apparatus before the construction of the "Poohnicastro". The apparatus was quite cheap: stainless-steel tanks from military kitchens and simple thermostatic systems with a contact thermometer and an agitator. The temperature method was obtained by passing the nuclear plates, contained with the relevant chemicals in sealed tubes, from a cold tank to a warm tank and viceversa. There was no cafeteria in the building. Sometimes we brought a sandwich from home, but there were reasonably cheap restaurants nearby. Merlin and I went often there.

Why was a new journal created, the "Bulletin du Centre de Physique Nucléaire?"

Bonetti: As usual it was a suggestion by Beppo, who asked Cosyns a mechanism to made known in a short time the relevant information concerning the technical aspects of nuclear emulsions handling and their applications. Probably Beppo had in mind the habit promoted in Arcetri by his "maestro" Garbasso who encouraged a quick publication of results. The Brussels Bulletin was a lucky initiative because it efficiently reached all those interested in the use of nuclear emulsions.

How many people worked in the laboratory?

Bonetti: When I arrived there no more than ten people including Beppo, Connie and Yves Goldschmidt-Clermont: some of them were guest researchers with temporary appointments, some of them students engaged in their graduation research. As far as I remember, Aldo Igiuni was the only attendant before becoming an outstanding technician. There were the microscope scanners, typically underpaid girls, following Bristol model promptly imitated by other emulsion groups in Italy and elsewhere (Connie began her career as underpaid microscope scanner!). The atmosphere was warm and open. Everyone who wished to work in the laboratory was heartily welcomed. I remember when my sister, a biologist, came to Brussels to try to use nuclear emulsions in biological experiments. Beppo provided a place and the material she needed, Connie showed her the use of liquid emulsions. Louis Vermaesen, a clever young physicist, came from Gand: Occhialini associated him to the “temperature development” work. I do not know why he left the group and put an end to his scientific career. Very little I remember of Stephen J. Goldsack from England: he worked with Connie and later was for sometime in Milan. Guy Vanderhaege had been a very proficient student and was a member of the group from 1949, I believe. He became the senior researcher in later years, when the group was engaged in collaborations at European level. In the last years of the collaboration of Milan with Brussels the senior researcher was brilliant and attractive Monique René.

How was the scientific relation between Occhialini and Connie Dilworth?

Bonetti: Connie wrote to me, in August 1948, a very formal invitation letter in French, dictated to her by some Belgian colleague not without amusement, as I discovered later. When a few days later I met her in the common room in Brussels I found that Connie was not only not formal but also very attractive: I liked her direct and clearly intelligent expression. She proved to be an exceedingly talented woman with an inbuilt intellectual authoritativeness, facing scientific issues with uncommon clarity and rigour. Connie had the ability to elaborate quickly and precisely Beppo’s ideas, as was the case when they were working out the “temperature development”. But Connie’s mind was also highly creative, I only quote here the constant sagitta method for the measurement of the mass of particles at the end of their range, and the first idea of the Lunar Occultation Satellite (HELOS).

Mario Schönberg, the renowned Brazilian theoretician, was also a member of the group.

Bonetti: Schönberg joined the Brussels Group for many months. He used to work by night, and the lights of his room were on until late in a cloud of smoke. Among the problems he treated I remember the ionisation-energy relation of high-energy charged particles, a work done partly in collaboration with the French theoretician Louis Michel. They studied in particular the ionisation rising after the minimum, a problem relevant to fast-particle measurements in nuclear emulsions. The problem troubled many people,

with various solutions, on both sides of the Atlantic Ocean. Schönberg solution was similar to that of Fermi's. Schönberg's presence in Brussels was important, because he was a reference for students interested in theoretical physics. He was one of those physicists endowed with a "humanist" culture, whom I referred to in the above.

When Occhialini was in Bristol, he called Lattes and Camerini from Brazil. Did he call other physicists from Brazil once in Brussels?

Bonetti: No, as far as I remember. The interaction with Brazil was the UNESCO mission of Beppo to Chacaltaya.

A noteworthy member was Aldo Igiuni. Why did it happen that an assistant began to work as a researcher?

Bonetti: Igiuni had no scientific training at all, but he was a particularly talented young man. He learnt quickly and with full understanding the techniques in use in the laboratory. He started his training helping Beppo in the tests on the processing of large plates, both normal and stripped. He never used a microscope, because that was not his task, but he was there as long as nuclear emulsions were used both in Brussels and in Milan. I do not remember exactly when Beppo persuaded Aldo to accept an appointment as technician in the Milan section of INFN (the National Institute of Nuclear Physics). The INFN was open-minded and ready to provide highly qualified technicians with an administrative and financial position similar to that of researchers. Aldo Igiuni was one of them. After almost ten years of nuclear emulsions Aldo followed Beppo (and Connie) in the space adventure, contributing to the design and realisation of balloon and space instrumentation.

Was the activity in the laboratory strictly ruled?

Bonetti: Our activity was regulated by our commitment and common sense. We spent the daytime in the laboratory, except those who had worked until late in the evening. Nobody took notice at what time any of us went to work.

How was your life outside the laboratory?

Bonetti: I mentioned already that Beppo had a remarkable literary culture and strong interests in politics, so he enjoyed meeting personalities visiting Brussels. The Institute of Italian Culture offered the occasion of encounters often at dinner time with writers such as Elio Vittorini or Giuseppe Raimondi and the discussions on political and/or literary issue were heated.

What about the financial support to the laboratory?

Bonetti: As far as I know, the laboratory was supported by the Free University. The director was responsible for the financial needs of the Centre. Beppo's requests were wise and sensible —and were granted to him.

How were the relations of the Brussels group with the Italian groups led by Occhialini? Let us begin with Genoa.

Bonetti: Beppo had just won, 1949, the competition for a chair in Italy when he was offered the direction of a laboratory at the Tata Institute, in India. He even wrote to his father that he would accept the offer. But in the end Beppo accepted the position in Genoa, and Bernard Peters went to the Tata in his place. Beppo kept his chair in Genoa for two years till the death of his father, 1951, and supported strongly the emulsion group which I put together following Brussels model. The microscope scanners were undergraduate students making their graduation research on plates lent by Brussels. Some of those students were good and enthusiastic. I mentioned already Giovanna Tomasini who came from a family of farmers and enrolled in the University of Genoa to become a secondary school teacher. When Beppo moved from Genoa to Milan in 1952, his successor Ettore Pancini appreciated the courage and commitment of Giovanna and appointed her as leader of the emulsion group, maintaining the collaboration with the group of Milan and Brussels which was still the provider of the plates. The work of Giovanna continued to the end of her career in important collaborative experiments at the CERN accelerators. The more prominent person emerging from Genoa in those years was Livio Scarsi, the first graduated student of Beppo with a thesis on particle pairs production in emulsions by primary cosmic rays. Livio followed Beppo to Milan, working first in particle physics and cosmic-ray interactions in emulsions, then in space physics. He was faithful to cosmic-ray research to the end of his life.

Occhialini had been invited to Milan by Polvani and Caldirola. On the other hand the physicists in Milan had already worked with emulsions.

Bonetti: Polvani's assistant Antonio Lovati had even invited the Canadian physicist Pierre Demers to learn from him the technique of producing nuclear emulsions in the laboratory. The attempt was stopped when Beppo, keeping his position in Brussels with the agreement of both sides, shifted from Genoa to Milan. Lovati, Levi-Setti and Martina Panetti were there, Livio Scarsi joined them, and so did I one year later. Connie came from Brussels with her daughter in 1953. So the Milan group took its shape for the next eight years, Beppo being the link with Brussels.

When Occhialini moved to Milan, in 1952, there was also the tragic death of Loubens caused by a broken winch during a speleologic excursion.

Bonetti: The death of Loubens was a tragedy which provoked a crisis in the Brussels Centre, also because of the responsibility of Cosyns in the design of the winch. The good relationship between Beppo and Cosyns came to an end. After much bitterness Cosyns decided to leave Brussels and went to Paris, and then to East Germany. The new director was Pierre Baudoux, who was able to largely heal the wounds and appease the climate to the point that the collaborations with the Milan group and at international level could go on until the late fifties. Let us close this sad chapter with a happy anecdote. When Beppo's 80th anniversary was celebrated in Arcetri, 1987, Jacques Labeyrie, a member of the excursion at the Pierre Saint Martin, told the story of how Beppo, who had been rescued at great risk after two days spent at the bottom of the cave, solved the problem of the defective winch. Beppo made with a suitable rope a seamen knot with which it was possible to pull up the cable of the lift letting free the damaged wheel. The winch could be repaired satisfactorily enough to save the life of the trapped speleologists. Once again, Beppo.

How were the relations of the Brussel group with the English groups?

Bonetti: Beppo was the link with Bristol and Manchester, thanks to his personal relations with Powell and Blackett. It was the time of the discovery of "new" particles and we were interested in knowing what the Manchester cloud chamber was producing at the Pic-du-Midi. The same for Bristol: great rivalry and high esteem in the quest of new objects. The result was the collaboration which led to the G-Stack balloon experiment through the commitment in particular of Michelangelo Merlin who, after his stay in Brussels, joined for some time the Bristol group.

And what about a relation with French groups?

Bonetti: We had a correct relationship with the group led by Louis Leprince-Ringuet in Paris. Of course, we were interested in the results obtained with their beautiful multiplate cloud chamber. There were also lasting friendly relations with his junior researchers. The friendship of Charles Peyrou with Beppo and Connie lasted to the end of their lives, after the scientific clash about the mass of what came to be known as the K_μ . The French had found the notorious 918 mass, at variance with the value 960 from the mass of the τ -meson and the average value, again about 960, of the mass obtained by Diworth, Occhialini and Scarsi (1954) through a compilation of measurements from many groups with nuclear emulsions and Wilson chambers. The G-Stack solved the problem.

The Brussels groups was part of the Free University. How important was it for you to work in a “free” university?

Bonetti: I happened to be educated without any reference to religious beliefs, in the spirit of 19th century “libre pensée”. So to live in a place where professors and students had to sign the “déclaration de libre pensée” was to me a rewarding intellectual and emotional experience. I enjoyed Picciotto explaining to me some amusing aspects of such an ambience. The persons I met were used to a rigorous and critical attitude towards both traditional and novel ideas. A challenging case was that of Lysenko. All over Europe the communist press presented reports on Lysenko’s “extraordinary discoveries” and the communist parties were under strong pressure, to say the least. The Belgian communist party was a small elite party, mainly of intellectuals. Among them Jean Brachet, the son of the embryologist Albert Brachet and himself biologist at the Université Libre, travelled to USSR to ascertain facts with his own eyes. Back to Brussels he exposed publicly the preposterousness of Lysenko’s ideas and the indecency of the Soviet government in supporting amateur Lysenko against well-known geneticist Vavilov. The matter was discussed with perfect freedom and open-mindedness, a not at all obvious outcome.

Occhialini was awarded a doctorate “honoris causa” by the Free University.

Bonetti: It was a recognition of Occhialini’s work on the occasion in which the Université Libre invited him to join Cosyns in the foundation of a promising research centre. From a rather eccentric personality one might expect some eccentricity. Indeed at the ceremony Beppo had to wear a toga, something which he did not care of, and carelessly he wore it, availing himself of the informal climate of the Université Libre. I reported to his father the ceremony describing Beppo, to his amusement, as “that short and rebellious Italian”...

A centre of nuclear physics in the immediate after-war is somehow associated to the atomic bomb. Were you conscious of such a problem?

Bonetti: I do not remember if we ever talked explicitly about the bomb. I cannot say why it was so, perhaps we felt we were short of arguments to rationally discuss such an issue, besides the emotional side of it. The cold war was at its beginning and many of the group (most were proud of having been resistant against nazis) were militant gauchistes, giving strong support to the movement of the “partisans de la paix”.

The Brussels groups, such as other emulsion groups, were large groups if compared to those in the ’30s. Scientific papers in the ’30s were signed by one or two scientists. Lattes, Muirhead, Occhialini and Powell’s 1948 paper was noticed also because it was signed by four scientists. A whole page was needed for the names of the scientists signing the G-Stack paper. Did you notice that something was changing?

Bonetti: The Bristol experience shows that Powell favoured the presence of the signatures of all those who had a part in an experiment. I wonder how much this was due to the influence of Occhialini, who was very careful in the recognition of the role played by each one and in the use of the alphabetic order of names. I think it is correct to say that the advent of visual particle detectors stimulated the formation of groups of researchers engaged in long-term projects. Nuclear plates were a model case: being a relatively cheap instrument, they required a lot of man power to get significant results in a fairly short time. Scanners were often clever people, but it was up to researchers to control the results of the observations, and especially the unusual events, with a systematic cross-checking. This favoured the formation of relatively large groups of researchers in a work which required not so much specialisation as efficiency. The next step was the coordination of several groups in large collaborations. The experience of nuclear plates passed first to the bubble chamber projects and then to the mammoth detecting devices of the big accelerating machines: they engage a lot of people not only because of the dimensions and the huge amount of data, but also because of the variety of specialisations requested by such complex projects.

We usually refer to physical studies made with the big accelerating machines as a "Big Science". Can we say that also in your case there was a transition towards a "big" science, even if of a different kind?

Bonetti: As already remarked, nuclear emulsions stimulated the formation first of large groups and then of large collaborations. In this sense the G-Stack was a first, if minor, version of Big Science.

Which was the importance of the G-Stack for all of you?

Bonetti: Ending the 1953 meeting on new particles and cosmic rays in Bagnères-de-Bigorre, Leprince-Ringuet described particle physicists as belonging to two factions, the accelerating machines faction, striving to realise in the laboratory higher and higher accelerating energies, and the cosmic ray faction, striving to reach higher and higher altitudes to exploit the high-energy tail of the cosmic-ray spectrum beyond the reach of laboratory machines. This was the attitude of those who in 1954 joined the efforts of six emulsion groups to attack the still extant problem of the decay scheme of charged K-mesons by means of a many-litres stack of stripped emulsions flown at balloon altitude. Also the G-Stack did not supply more than a few tens of useful events, but the sample was good enough as to give a substantially correct answer.

Was the Brussels group aware, from the beginning, of the importance of the researches they were doing?

Bonetti: I would say yes, everybody felt to be on the way of important results.

During the years Occhialini spent in Brussels, both Blackett and Powell were awarded the Nobel Prize, respectively in 1948 and 1950. How did Occhialini react to these awards and his possible exclusion?

Bonetti: Blackett was the only person, I believe, for whom Occhialini felt a kind of veneration. Ten years older, Blackett exerted a great influence on Beppo, both on the scientific and the human side. Beppo was in touch with Blackett when he was an enemy alien in Brazil during the war and was trying to go to England to join the war effort against fascism. Thanks to Blackett he succeeded in reaching England, but was rejected from direct involvement in the war effort (he was a mistrusted Italian!). Always through Blackett he joined Cecil Powell who, being one of the few communists in England, worked in isolation in Bristol. Beppo's intellectual and human esteem for Blackett was such that he accepted Blackett's prize as one accepts a welcome thing, something concerning a teacher and a friend. I never heard from Beppo a single jealous word about Blackett who, in his Nobel Lecture, acknowledged officially, I should say warmly, Beppo's contribution. That was not the case with Powell. Beppo's contribution to the discovery of the pion and more generally to the technique of nuclear emulsions was at least as recognisable as his contribution to the discovery of electron pair production and to the control of Wilson chamber. When Powell was awarded the prize, Beppo expected at least an acknowledgement of his part in a joint endeavour. The absence of a mention of Occhialini in Powell's Nobel Lecture was blatant. The sadness was great, to say the least, even disregarding the fact that Occhialini himself was not awarded a deserved recognition. It took a long time to overcome the bitterness, but in the end it happened, and this, in my opinion, was a part of the success of the G-Stack. After several years, Beppo once told me: "I met Powell. He feels his death (*sente la morte*)". Powell actually died a short time later, prematurely. Beppo succeeded in having a bench with Powell's name on it on the hills near Como, a cherished English habit.

Although Occhialini was formally a member of the Centre de Physique Nucléaire up to 1959, in november 1949 he already moved to Genoa as a professor, and divided his working time between Italy and Belgium. Why did he come back to Italy?

Bonetti: Beppo felt he could not disappoint his father after the long years of absence from Italy. Among other things, Augusto Occhialini had avoided major damages to the Institute of Physics in Genoa during the war, also against the attempts of Germans to take away the valuable instrumentation—he certainly longed to leave his place to his son. This is also why Beppo did not accept the invitation by the Tata Institute. But this is not all. Having been openly and strongly antifascist, Occhialini felt he had some kind of duty towards his country, and this attitude was in consonance with that of the other prominent Italian physicists with whom he was familiar from the time when Arcetri in Florence and via Panisperna in Rome gave rise to the "Italian School of Physics". As it happened, just after the death of Augusto Occhialini, between 1951 and 1952 Amaldi and Bernardini in Rome, Deaglio and Wataghin in Turin, Polvani and Caldirola in Milan, Rostagni and Dallaporta in Padua promoted the formation of the Istituto Nazionale di

Fisica Nucleare. The INFN was a highly successful structure around which the Italian research in Physics was organised in a modern way for many tens of years, outliving largely the founder-members. Occhialini accepted promptly the offer of Caldirola and Polvani (who was an ancient pupil of Augusto in Pisa before the 1st World War!) and joined the Milan section of INFN. Beppo's emulsion group in Milan played an important role in particle physics in the fifties and sixties. And then Livio Scarsi and Aldo Igiuni, and several younger members, followed Beppo and Connie in the new adventure of Space Physics after their 1960 sabbatical year with Bruno Rossi, the founder with Gilberto Bernardini of the group of Arcetri and the initiator of cosmic ray research in Italy.

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Elementary-particle physics at the University of Milan 1951-1956

RICCARDO LEVI-SETTI

The Enrico Fermi Institute and Department of Physics, The University of Chicago, USA

If history was made in the development of strange-particle Physics, under the guidance of Giuseppe Occhialini (Beppo), by the nuclear emulsion researchers at the Universities of Genoa and Milan, scant record is to be found in the literature under Beppo's name. Only toward the end of 1955 does Beppo's name appears, in the reports of the "G-Stack Collaboration" that he orchestrated. Although he would have been fully entitled, as well as his wife Mrs. Connie Occhialini-Dilworth, at first as Miss Dilworth, to co-author most of the publications of our group, neither of them did. In this brief summary of the new Physics we were fortunate to uncover, I take the liberty of relating the excitement of our discoveries also on behalf of my closest collaborators, A. Bonetti, M. Di Corato, B. Locatelli, M. Panetti, L. Scarsi, and G. Tomasini, the Genoa-Milan group. The list of publications of the group, covering the period 1951-1956 and reproduced here, was taken in its entirety from my CV. Thus Beppo's legacy is bound to transpire through the work of his pupils and apprentices, being myself one of them. The manner by which his guidance of our work manifested itself deserves a separate memoir of mine, to accompany this writing.

Cosmic rays were our only source of high-energy particles, and the higher the altitude of exposure of the emulsion stacks used in our work, the higher the chance of encountering new members of the particle zoo, due to the energy degradation suffered by the primary cosmic radiation in penetrating the atmosphere. How to expose the stacks at high altitude, by mountaineering and balloon flight expeditions became part of our education. We all learned to collaborate with small and large teams of colleagues, in the field and in our laboratories. Beppo taught all of us how to appreciate and endure the experience.

The story begins with my apprenticeship with Beppo at the Brussels' Centre de Physique Nucleaire during the summer of 1950. There I learned how nuclear emulsions

were processed, to yield uniformly developed and undistorted ionizing particle tracks throughout their considerable thickness (up to 1.2 mm), a prerequisite for the Physics to be. I was also introduced to the methods of particle identification by the measurement of $p\beta$ from the multiple scattering of their tracks [1,2] in conjunction with that of the rate of energy loss dE/dx revealed by various methods of track ionization density measurements. In the latter endeavour I benefited greatly from the help of Miss Dilworth and Yves Goldschmidt-Clermont, experienced members of Beppo's Brussels team, and like Beppo, of Bristol extraction.

By the time my leave of absence from the University of Pavia expired, at the end of the summer, Beppo was already casting the foundations of what became to be known as the Genoa-Milan group. How this materialized involved the introduction of new characters accompanied by a musical-chair like reshuffling of the cast affiliations. While still at Pavia, I was introduced by Beppo, who held a professorship at the University of Genoa, to Alberto Bonetti and Giovanna Tomasini, recently initiated by him to the art of nuclear emulsions. Beppo arranged for me to collaborate with Tomasini in a study of the energy spectrum of μ -meson decay electrons by multiple-scattering measurements [3], on plates exposed at the Pic du Midy in the Pirenées, at 3000 m, on loan from the Brussels lab. While this collaboration was in progress, I was offered a position at the University of Milan, which I joined in mid-1951. There, under the auspices of the Chair of the Department of Physics, Prof. Giovanni Polvani, and the support of the INFN (Istituto Nazionale di Fisica Nucleare), led in Milan by Prof. Piero Caldirola, I began to organize a nuclear-emulsion scanning and measuring lab, and took steps to get Alberto Bonetti to join me, followed soon afterwards by Beppo and by then Mrs. Connie Occhialini-Dilworth. By early 1952, the Genoa-Milan nuclear emulsion group had taken shape, joined by Mrs. M. Panetti, wife of a Physics faculty colleague, already present at Milan.

While the above infrastructure was under development, Tomasini and I were struggling to compete with our counterparts at Bristol and Dublin in the study of the μ -meson decay spectrum. Our nemesis at Dublin was C. O'Ceallaigh (the joke, in Italian, at the time was that they have Occhiali, we have Occhialini) and the Bristol group who reported some evidence of a high-energy tail in the μ -meson decay spectrum, extending further than the tail we found. As much as Prof. M. Schönberg convinced us that the tail we observed was still consistent with a spectrum cut-off at 55 MeV, and to be expected due the large errors in our multiple-scattering measurements, we were left with a lingering doubt. The emulsions used by our competitors, flown at balloon altitude, may have contained decaying particles heavier than μ -mesons, not present or less frequent in our plates exposed at mountain altitude. To dispel our apprehension, Beppo provided us promptly with plates flown for two hours at 20000 m, on loan from the Brussels Centre, to continue our μ -decay study. This move paid off handsomely, as we shall see shortly. At the same time Beppo undertook to organize a balloon launching expedition which took place in Sardinia during the summer of 1952, as a collaboration between the University of Bristol, the Brussels Centre, a number of Italian Universities, other European laboratories, and the Italian Navy and Air Force. Numerous stacks of nuclear emulsions were flown over many hours at an altitude of 27 km in egg-shaped gondolas hanging from giant polyethy-

lene balloons, under the guidance of the Bristol group headed by Cecil F. Powell, and tracked by radiotelemetry (to which I attended, in extreme summer temperatures). Some of the gondolas were recovered at sea in adventurous circumstances (*e.g.* amidst floating sea tortoises). Our labs became awash with plates to scan and discoveries to be made.

The newly acquired Sardinian plates yielded promptly our first discovery that K-mesons originated directly from strong interactions, rather than from the decay of even heavier particles. In a paper entitled “*Slow Heavy Mesons from Cosmic Ray Stars*” [4], G. Tomasini and myself described our respective findings. Shortly afterward, the Genoa-Milan group described [5] the first observation (by G. Tomasini, in the plates borrowed for the μ -decay spectrum) of a particle of “transprotonic” mass, called J, decaying into a fast secondary (we now would call it a $\Sigma^+ \rightarrow n + \pi^+$ decay), and the paper was cautiously entitled “*Observation of the Decay of a Heavy Particle*”. Back to scanning the 1952 Sardinian plates, on a lonely 1952 Christmas day spent at the lab, I had the venture of finding the first example of a $\Sigma^+ \rightarrow p + \pi^0$ decay. After struggling to demonstrate the direction of motion of both primary and secondary particles coming to rest, to exclude a dp reaction, the event (called J_p at that time) was accepted as a bona-fide alternative mode of decay of a Σ^+ hyperon, yielding a mass a few MeV away from the now accepted mass of this hyperon. (Within the uncertainty of the range-energy relation the energy of the stopping decay proton could be reliably determined, and so the Q -value of the assumed two-body decay reaction) In the spring 1953, Beppo arranged for me to be invited to present our evidence to the Royal Society in London, and I will never forget my emotion at speaking from that well-worn lectern (most likely of Newton’s vintage) in a chapel-like, mahogany-paneled lecture room. In July 1953, our events were presented at the Cosmic Ray Conference of Bagnères de Bigorre, where our hyperprotons were rebaptized (fortunately temporarily, it took Gell-Mann in 1955 to call them Σ) Ω_p and Ω_π and finally published in the Fall 1953 [6] in a comparison with additional evidence from other laboratories. This paper, entitled “*On the Existence of Unstable Charged Particles of Hyperprotonic Mass*” was awarded the “City of Como” 1953 prize by the Italian Physical Society.

In the summer of 1953, another International Expedition to Sardinia took place, and before the end of the year another discovery was made, the first two-body pion decay of a Λ -hypernucleus, in fact, an example of the decay reaction ${}_\Lambda\text{H}^3 \rightarrow \pi^- + \text{He}^{3++}$ [7]. Although the experimental evidence was unquestionable, our paper carried implicitly Beppo’s cautious approach by being entitled “*On the Possible Ejection of a Meson-Active Triton from a Nuclear Disintegration*”. In a subsequent note [8], the Λ binding energy was estimated at ~ 1 MeV, to be compared with that of the last neutron in H^3 , equal to 6.24 MeV. In the summer of 1954, I attended the Varenna Summer School, where Fermi gave a lecture series on pion physics (tragically his last summer). With much trepidation, I also gave a lecture on hypernuclear physics and noticed that Enrico was intrigued by my mention of a ~ 1 MeV Λ -binding in ${}_\Lambda\text{H}^3$. After my talk, he came over to me and told me: “*That is extraordinary, the Λ must have a wave function as large as this room....*”

At the beginning of 1954, several nuclear emulsion groups, primarily the Genoa-Milan, the Dublin-Bristol, and the Padua groups, were spearheading the study of strange particles and their interactions (they were yet to be called strange). It was pretty well established that we had to deal with charged K-mesons and Σ -hyperons which were produced in the interaction of high-energy cosmic rays with the emulsion nuclei, and the only access we had to neutral strange particles was confined to the Λ when bound in hypernuclei. While the knowledge of the mass and decay modes of the Σ hyperons was settled relatively quickly [6,9] (an event almost identical to our first $\Sigma^+ \rightarrow p + \pi^0$ decay, reported by the Padua group in 1954, confirmed the two-body decay assumption) the numerous decay modes of the K-mesons could only be attributed to one and only charged K after a protracted struggle. Thus each decay mode was at first assumed to originate from K-mesons of different identities, and the latter given different names. The first K-meson standard was provided by the decay $\tau^+ \rightarrow \pi^+ + \pi^+ + \pi^-$ reported earlier by the Bristol group and fully identified through its decay at rest of the three pions. Then came a number of K's decaying into one charged secondary and one or more neutrals. A two-body decay $K_\mu \rightarrow \mu + \nu$ was proposed by the cloud-chamber group of Ecole Polytechnique, another two-body decay $\chi \rightarrow \pi + \pi^0$ by the Bristol group, and finally the three-body decays. These included the $\tau^+ \rightarrow \pi^+ + (\pi^0 + \pi^0)$, first envisaged by Pais and Dalitz and observed at Rochester, the $K^+ \rightarrow \mu^+ + \pi^0 + \nu$ proposed by O'Ceallaigh at Dublin, and the $K \rightarrow \beta + \pi^0 + \nu$ first observed at Bristol.

The paramount difficulty, even using thick emulsions or emulsion sandwiches, was that the geometry of the decay events very seldom provided sufficient track length for an unambiguous identification of either the secondary energetic pions, muons and electrons by multiple scattering and g^* measurements, or an accurate mass determination by multiple scattering *vs.* range of stopping primaries. Although the latter measurements hovered by a few hundred electron masses around the known mass of the τ they were seldom accurate enough to establish that all the K-meson decay zoo represented alternative decay modes of the same particle.

This struggle and frustration is portrayed well enough in a number of reports by the Genoa-Milan group [10-12], describing successively increasing numbers of K-mesons, some of which could be attributed to specific decay schemes, but whose measured masses were not accurate enough to resolve the above impasse. Together with Beppo, we came to the realization that a radical solution of the impasse was called for, that of bringing to rest or to their fate in the emulsion all K-meson secondaries. This would allow their positive identification, the accurate measurement of their energies, and thus the precise determination of the decay Q -values, and ultimately of the K-meson masses. For this to be possible, the most energetic secondaries, the muons from the two-body K-meson decay (~ 20 cm long) would have to be traced through many emulsions of a very large emulsion stack. This was the motivation for an experiment never attempted before, the G-Stack (G for giant) Collaboration [13,14]. We owe to Beppo the orchestration of this major effort, which involved the coordinated participation of 36 physicists from 6 laboratories, an absolute first at the time. In the words of Cecil F. Powell, (the master of the spoken word, as referred to by Beppo) "*There are, of course, difficulties in*

establishing a successful collaboration. It takes time for standard procedures to be worked out, and to be put into general use. But, we have found that the advantages greatly outweigh the difficulties. In particular, we have not found the collaboration onerous or that it tends to inhibit the flow of new ideas. On the contrary, the friendly mutual support has seemed to us to result in an easy relation between the different laboratories which has tended to release and encourage individual initiative”.

The above was then the spirit in which we undertook the daunting experiment. In the Fall 1954 a 63 kg stack of 250 emulsion sheets 600 μm thick and 37×27 cm in size, was flown at a mean altitude of 27000 m for six hours over Northern Italy. Mobilizing an amazing number of Italian Ministries, Armed Forces, airport and communication facilities and organizations. Once again, I found myself in charge of tracking the balloon by radiotelemetry from the Milan airport. Due to a parachute mishap, the stack-carrying gondola, weighing a total of 140 kg, descended somewhat too rapidly over the Apennines, and about 10% of the emulsions were damaged in the impact with the ground. This however did not affect significantly the outcome of the experiment, and enough stack subsets were distributed among the collaborating laboratories. The fun began when a long K-meson secondary exited one stack subset and had to be expertly traced to one of the adjacent subset often abroad. Difficulties notwithstanding, the experiment was an outstanding success and contributed significantly to establishing the unique parent identity of the K-mesons involved in the zoo of six decay modes. These results were reported first in a comprehensive paper entitled: “*Observations on Heavy Mesons Secondaries (G-Stack Collaboration)*” presented at the 1955 Pisa International Congress [13] and further elaborated in a paper entitled “*On the Masses and Modes of Decay of Heavy Mesons Produced by Cosmic Radiation (G-Stack Collaboration)*” [14]. As previously mentioned, these were the only publications where Beppo’s name appeared among the authors of the Genoa-Milan group. Together with another discovery by our group, that the nuclear capture of negative K-mesons gave rise to the production of Σ^- and Λ -hyperons (the latter bound in hypernuclei) [15], the G-Stack Collaboration results gave impetus to Gell-Mann’s establishing the strangeness scheme and the “eightfold way”. This took place in momentous conversations between Beppo and Murray Gell-Mann at the Pisa Congress in June 1955.

On the front of hypernuclei, I became involved with reviewing the existing evidence twice [16,17], and more events from the Genoa-Milan group were described [18]. Another collaboration of the Genoa-Milan group, with Saclay and Brussels, led to a very precise measurement of the τ Q -value and hence of the K^+ mass [19]. Finally, the energy spectrum of the muon-decay electrons, reported earlier [3] from 278 measured events, was re-examined after the statistics were augmented to a total of 506 events [20]. In a detailed comparison with the theoretical spectra calculated by Michel for positive muons and by Porter and Primakoff for a mix of positive and negative muons, inclusive of radiative corrections, a fairly accurate value of the theoretical parameter ρ was obtained. This was regarded as a feat of the multiple scattering method of measuring particle $p\beta$ ’s in nuclear emulsions, to the point of allowing a rather sophisticated comparison of measurement with theory.

My association with the University of Milan, with Connie and Beppo, and my productive collaboration with the colleagues of the Genoa-Milan group, came to an end on July 1st, 1956, when I joined the University of Chicago. There, I could rekindle at first my interest in hypernuclear physics with emulsions exposed to separated K-beams from the Berkeley Bevatron and the CERN PS. It became a lot easier than cosmic-rays exposures to find hyperfragments. Beppo's legacy guided my carrier since that day.

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Giuseppe Occhialini in Milan in the sixties and beyond: His legacy for particle physics and his influence on young researchers and students

GUIDO VEGNI

Università degli studi di Milano, Italy

1. – Introduction

I intend to deal with two topics connected with Giuseppe Occhialini —“Beppo”. The first concerns the last nuclear-emulsion research in Milan and, in the early 1960s, the way in which he urged and endorsed the part of the “Milan emulsion group” who had decided not to take part in the new Cosmic Physics research projects, to continue their research in Particle Physics with new experimental techniques. The second concerns Beppo’s relationship with young people, his activity as an educator in science, but more generally in Life, in my personal experience as assistant close to him from the end of the 1950s until he retired —and beyond, until his death in 1993.

The first topic does not mean to be the history, an account, of Elementary Particle Physics in Milan in the 1960s. This will certainly be dealt with by other people in other places. As I have already said, I intend to limit myself here to the last nuclear-emulsion activities and the new research of the young Milan physicists of the “Occhialini group” who preferred to remain in the Particle Physics field at the time the Cosmic Physics group was set up.

2. – Notes on the last Milan research projects with nuclear emulsions

In the second half of the 1950s, the line of research with emulsion stacks exposed to cosmic rays in high mountains or at high-altitude balloon flights was completed. Amongst

these researches of the fifties, the G-Stack experiment, with its large collaboration, is probably the most famous: those results will be described elsewhere.

Afterwards the research proceeded with stacks exposed to particle beams produced in accelerators. I remember, among the many important results, the spin-parity measure of what was then called the “tau”-meson. The last work on this subject was obtained, with large statistics, in collaboration with Padua and Brussels groups, in a research made in 1956-57 on a G-5 emulsion stack, exposed to a K^+ -meson beam from the Berkeley Bevatron. The 0-spin parity assigned to the “tau”-meson in that measure definitely confirmed the so-called theta-tau puzzle. Two weakly decaying strange mesons, named the tau and the theta and later identified as the decays of the K-meson in three and two pions, respectively, appeared to be identical in any respect except in their decay: one decayed to a state of negative parity, and the other to a state of positive parity. This puzzle led to the hypothesis of parity non-conservation in weak decays and, successively, together with the result of the ^{60}Co experiment of Madame Wu *et al.*, to the important discovery that in nature, as suggested from T. D. Lee and C. N. Yang who received the Nobel prize for this, in the weak interaction, there is a violation of parity symmetry: as a consequence, left is not always equal to right.

In 1960-61 with an Ilford K-5 emulsion stack, exposed to the 300 MeV/c K^- -meson beam from the Berkeley Bevatron, were made studies of the capture of the K^- at rest with the emission of hyperons and the production of hypernuclei.

In the same period, researches were performed with emulsion stacks exposed to 14, 16, and 17 GeV/c π^- -meson beams at the CERN PS in Geneva. I vividly recall the experience of one of these exposures. Alberto Bonetti gave me the start and end signals with his arms from a window of the control room of the beams hall at the CERN PS. I was on a bridge above the beams with a colleague, and I executed the order to raise and lower the stack with respect to the beam by pulling ropes, a simple method in the midst of the refined technologies which the halls of the CERN were already full of. Also in this case the emulsions were an Ilford K-5 type and were the top of photographic emulsion technology for research in elementary particles. Nevertheless, production and, even more, the process of development of emulsion stacks had, in many respects, remained an art entrusted to the skill, experience and passion of the individual operators rather than a mature technique with reliable and repeatable results.

I remember some facts and anecdotes regarding the life of the group at that time. At the end of the 1950s, the processing plant used until to then in the laboratories of the “Université Libre” of Brussels was left definitively, and the activity was transferred to the plant built in the basement of the new building in Milan nicknamed the “Capannone”. This was built on land (the same land where, years later, the current Physics Department would be built) at the corner of Via Ponzio and Via Celoria. That area was still a mix of crops and football fields, dominated by the Cascina Rosa. This land was also the setting for Vittorio De Sica’s famous film “*Il miracolo a Milano*” in 1950. Occhialini was helped in the construction of the Capannone by both the University, in particular via Giovanni Polvani who had been mainly responsible for his call to Milan from Genoa in 1952, and

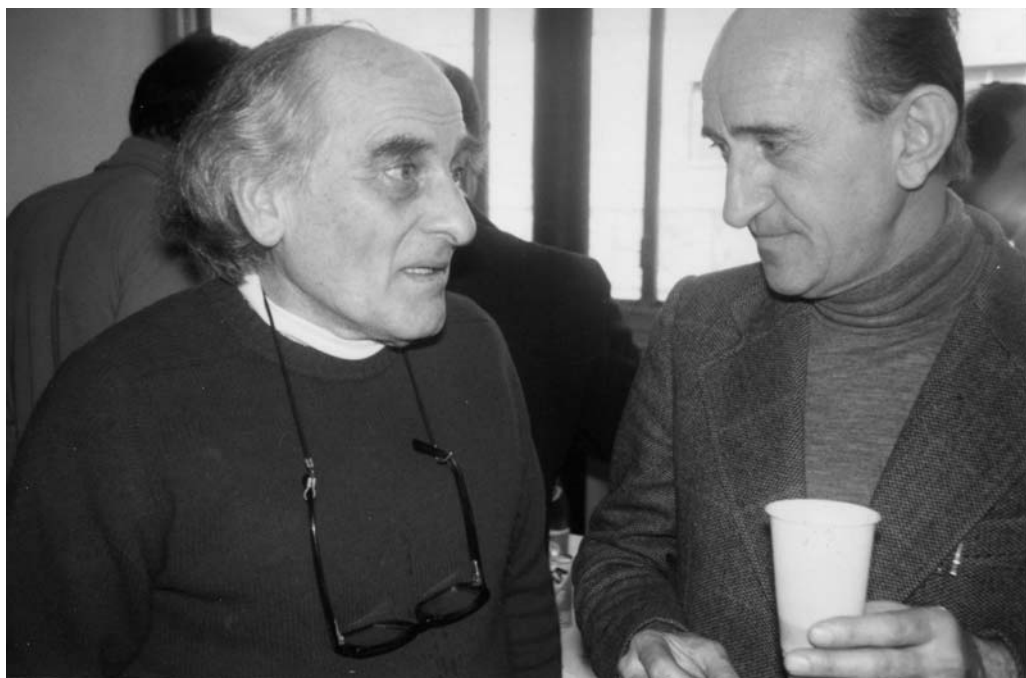


Fig. 1. – Beppo Occhialini and Aldo Igiuni in the laboratory, in the Capannone, in the early 1960s.

various private and public bodies, including the Koritska company. At the time, this company produced microscopes of the same name in the nearby Via Ampère. Doctor Cantù, a great friend of Occhialini and the group, was director and he was an invaluable collaborator in creating increasingly refined measurement microscopes, in accordance with the requirements of progress being made at the time in measurement methods.

Returning to the development process, I took part in one of the last stack development processes carried out in Brussels. I remember one of the most critical moments of this operation in which the two technicians Aldo Igiuni e Renato Ballerini —wonderful people and friends— were involved, under the direction of Beppo. I was mainly responsible for the never-ending and tiresome duties of washing the floors and all the equipment, and handling the plates in the tanks where they were immersed in developer at 5 degrees below zero. *“What difference does it make?”* Ballerini said to me, *“One day you’ll be the professor and have an easy life”* (sic).

One of the main question marks in the process was the formation of bubbles between the thick, jelly-like “stripped” emulsion and the glass it had been attached to, before development, at the end of the process. It was attached to the glass for ease of handling but mainly to have a rigid, transparent support which allowed the geometry of the

particle traces to be measured precisely, without deformations in measurements at the microscope. If the bubbles occurred too often, those plates were useless and precious information had to be abandoned. That time, in Brussels, an unacceptable number of bubbles appeared at the end of the first process. I remember a silent and immobile Beppo, furious, seated on a small stool in a corner of the room in the half-darkness as he watched us work. In the end, he got up and, still in silence, he started working with us, slowly carrying out all the cleaning operations, movement of the plates and checks on the temperatures at the different stages, etc. in a maniacal manner that we all tried to copy. However, it was clear that he was turning it over in his mind. At the end of that cycle, things were better, but not much. Then he said that he had decided to change the percentage of dilution in water of the cleaning liquid (I think it was “tiked”) used for all operations, but in particular, for the steel tanks in which the plate development process took place, gradually increasing the temperature. Eventually we celebrated the success!

The research carried out in 1960-61 on emulsions exposed to the $14\text{ GeV}/c\ \pi^-$ -meson beam at CERN, were aimed, moreover, at the first studies on the coherent production of new quantum states on nuclei. This line of research into the coherent interaction on nuclei, became one of the strong points of high-energy particle research by Milan physicists, in the next two decades. I would like to recall here that the physical concept inherent in this research was first proposed by the Russian theoretical physicists E. L. Feinberg and I. Pomerančuk [1]. In particular, Feinberg followed the development of the Milan researches with warm interest. The basic concept, in a corpuscular approach, is connected to a simple and elegant application of the Heisenberg uncertainty principle. If, in the interaction of an incoming particle with a target, the momentum transferred is very small, then the interested volume is consequently very large. Everything contained in that volume interacts as a whole; if a complex nucleus is the target in the volume, it behaves like an elementary particle with well determined and preserved quantum numbers. If a new state is produced in the interaction, it will have “filtered” quantum numbers. Consequently, new selectively produced particle quantum states could be studied in the various experiments of this kind. This research will be referred to once more below.

These experiments with stacks of emulsions exposed to beams of accelerators were the last in which the Milan “emulsions group” took part. Beppo Occhialini had not been directly putting his name as author on the scientific papers produced by the group for several years, despite continuing to exercise a strong influence on the strategic decisions, activity and guidance of the younger scientists... Direct continuity with past experience, also in scientific contacts, in particular the collaboration relationships with the other participant groups and the tutorage of new researchers was performed by Alberto Bonetti, then senior assistant to the chair of “Fisica Superiore” held by Beppo, and Connie Dilworth, who had collaborated intensively with Beppo in his research since 1948 and had, in the meantime, become his wife. The names of those taking part in this last research into nuclear emulsion, some new and some from the research with the emulsions exposed to cosmic rays of the 1950s, include M. Di Corato, A. Fedrighini, E. Quercigh, V. Pelosi, and A. E. Sichirollo, in addition to myself. I remember guests from other laboratories

such as B. Stiller, E. Villar and A. J. Herz, who took over direction of the group while the Occhialinis were in the USA in 1960. In addition to the particle research, Beppo promoted and supported other lines of research of an instrumental type, including, as I recall, the development of Emanuele Quercigh's "pulsed magnets", able to attain very high magnetic fields.

At the end of the 1950s, Beppo became more and more aware that the development of new techniques of observation and measurement in particle research, such as bubble chambers invented by Donald A. Glaser in 1952, or other complex detectors as the wires chambers, were destined to dominate the researches. These are now almost exclusively conducted at the accelerators, and leave less and less space for the technique of nuclear emulsions, destined to be used only in very particular situations in which, even today, their extremely high spatial resolution and the ability to be active and record physical events for very long times are essential. Livio Scarsi was the first subject of Occhialini's growing concern and he tells of an interview with him in 1957, when Scarsi was convinced by Occhialini to leave the research in emulsion and go to Bruno Rossi, at the MIT in Cambridge, USA, for a period of time [2]. Furthermore, the direction that the research in elementary particles was taking, and whose results were increasingly tied to laborious statistical analysis on the large numbers of events and often to be interpreted through complex theoretical formalisms, had probably become less congenial to the spirit of direct discovery of natural phenomena which had guided Occhialini in the research of the preceding thirty years. Therefore, along with the changes in the techniques of observation, he also decided on a drastic change in the field of research, passing from particle physics to cosmic physics. This was a choice which, at the same time, gave some form of continuity with all the previous researches carried out with cosmic rays. This is why other collaborators were later sent to American cosmic physics in Scarsi's footsteps and why Beppo and Connie stayed in Rossi's laboratory for a year, in 1960. Hence the foundation of the new cosmic physics group in Milan.

3. – The new elementary particles physics research activities of the former emulsion group

Despite the increasing commitment to space research, also of an organisational nature, at the national and European level, Beppo Occhialini was very interested in the migration to other experimental techniques, in particular bubble chambers, of those physicists in the group who intended to continue their research in particle physics. His interest was expressed in accordance with his character, in an apparently discrete and indirect manner but, in reality, rather drastic and pressing. In this, as in many of his other activities, Beppo's wife Connie supported him and acted explicitly; she also took part directly in the early research with hydrogen bubble chambers. The friendships and consolidated habit of the Occhialinis of collaborating with various physicists, particularly, but not only, French and Belgian, were used extensively to start the first research collaboration with photos of the CERN hydrogen bubble chambers. They also helped in sending physicists of the group for periods of study and training in the new techniques, at the CERN, where those

friends were, in the meantime, developing the organisation and activities of the Track Chamber Division. I particularly remember Charles Peyrou, who was the TC Division director, Yves Goldschmidt-Clermont, William O. Lock, Raphael Armenteros,...

In Milan the equipment for the nuclear emulsion research was gradually dismantled and the stereoscopic projector systems for the photographs of the bubble chamber installed. At long last, everyone understood what the trap doors in the ceiling of the large dark central room of the “Capannone” were for, a room which, up to then, had been used for microscope observation of the emulsion plates. Occhialini, with his typical intuition had had those trap doors made years before, when the Capannone was built, and they allowed now the projectors for the photos of the bubble chambers to be suspended at the correct height! At first, the scanning and measurements on the photos were done by hand, using templates to measure the momentum of the particles through the curvature of the track resulting from the magnetic field of the bubble chambers. Subsequently, semi-automatic methods were used with the so-called “*mangia spaghetti*”, whose measurements were recorded, with the scanning data, on punched cards, taken periodically, by car, to the Bologna Computer Centre for processing. These were the first timid attempts at introducing information technology to research, where, however, the data transmission network was anticipated by one or more, long, boring weekly trips by car on the Milan-Bologna motorway, with boxes of perforated cards on the outbound journey and packets of printed paper on the return. The first computer systems - for the activities of the scientific institutes in the basement of the building in Via Saldini - were activated in Milan in later stages. On the other hand, the CNAF (Centro Nazionale Analisi Fotogrammi — *National Photograph Analysis Centre*) was created in Bologna, which made automatic measurements on pictures of events we selected during scanning.

The results of the early research, made with film from the 81 cm Saclay bubble chamber, exposed to CERN PS proton beams, were presented at the 1963 Sienna International Conference. I recall that, among those taking part in this research, in addition to various physicists of the old emulsion group, already mentioned above, were Sergio Ratti, from the Milanese branch of activity in Wilson’s Chambers, John Kidd, an American guest who, among other things, had brought from the USA his red Moto Guzzi which he looked after meticulously and, later, Luciano Mandelli, who briefly participated to some research in emulsion before immediately moving to research in the bubble chambers, field in which he was the first of the group to write a thesis. The activities in hydrogen bubble chambers continued for the whole of the 1960s and beyond with other chambers, in particular the 150 cm British national hydrogen bubble chamber, and with data taken at several CERN PS beams. In those activities the collaboration with the Genoa group was particularly continuous, thus partly maintaining a tradition connected to the earlier research with nuclear emulsions. This new research was mainly dedicated to study of the properties of the new hadronic states, produced with the growth in energy of the incident beams. These will be dealt with in detail in another context.

As indicated above, one of Occhialini’s actions, at this stage, was to promote the stay of young researchers of the group and, in particular, that of his assistants to the Chair of “Fisica Superiore”, at the CERN, mainly at the Track Chamber Division. In 1963, it

was the writer's turn to go to the CERN for two years, where he was mainly concerned with research with photos from the 81 cm Saclay bubble chambers filled with deuterium. In addition to the first observation and study of the new hadronic multi-pion states, this was the opportunity to use the channel of coherent production on nuclei mentioned above, to study selected states of spin and parity in a very low background because of the target of deuterium nuclei. Emanuele Quercigh became a CERN Fellow at the end of 1964. He then joined the CERN research staff and was involved in several lines of physics, working first with bubble chambers and then with electronic techniques. His involvement with heavy-ion physics started in the early eighties, as soon as studies of this kind were initiated at CERN, and it is still continuing. Virginio Pelosi was at the CERN from 1965-67 and, when he came back to Milan, he continued the research undertaken in hydrogen bubble chambers before moving to other significant and much appreciated interests in the bio-medical field. In addition, he was a pioneer in the agro-meteorology field in Italy. Virginio died early, in 1982. Mico Rollier, the youngest of Occhialini's appointed assistants, did not come from the nuclear emulsions group and, once he became one of Beppo's assistants, he continued his research, brilliantly and mainly at the CERN, with heavy liquid bubble chambers with Ettore Fiorini, Tonino Pullia, and Pietro Negri etc. before passing to the construction of the HPC, the large Delphi electromagnetic calorimeter, until his precocious, sad death in 1985.

There is a little-known episode, which occurred in 1961, when Occhialini returned from the period spent in the United States. It had a long-lasting effect on some of his assistants and, in particular, on the writer. It was connected to a prototype silicon detector, which Beppo brought with him, in a small box that he opened for us in the corridor of the "Capannone", with that slightly mysterious air which he sometimes assumed. That detector had begun to be used in low-energy nuclear physics but Occhialini suggested to us that it could probably also be useful for high-energy particle research, "... *Think about it!*" The seed thus planted took several years before it bore fruits; Milanese physicists who had been given similar stimuli from different sources met and, at the end of the 1960s, silicon detectors were used for the first time during an experiment in high-energy coherent state production at the CERN. The experiment, in collaboration with groups from the CERN, Imperial College of London, Milan and ETH Zurich, was a research on the line mentioned above, coherent production, in diffractive dissociation of mesonic states, on different kinds of nuclei. It was carried out with a spark chamber magnetic spectrometer, exposed to a $15\text{ GeV}/c\ \pi^-$ -meson beam at the CERN PS. The Milanese group, of Gianpaolo Bellini, Mariella di Corato and the writer, included a "live" target, in addition to different "passive" nuclear targets, made of silicon detectors that gave better insurance that the nucleus was not broken, through the level of the electronic signal. P. F. Manfredi took care of the power and reading electronics of the detectors. Later, this type of experiment, on coherent production on live targets of silicon detectors was carried out by the afore-mentioned group, with other, mainly Milanese colleagues and the early contribution of various Italian technicians, with significantly higher statistical power and higher energy. I would like to stress that, in this experiment, as in the

previous one mentioned, two lines of scientific interest, particularly cultivated in Milan, were joined —the coherent interaction on nuclei and silicon detectors, both connected somehow to the cultural environment of the “Capannone” in the 1960s with Beppo Occhialini. The experiment was carried out throughout the 1970s at the Serpukhov Russian accelerator in collaboration with a group of physicists from Dubna, with the large spark chamber magnetic spectrometer of that laboratory, exposed to a 40 GeV/ c momentum π^- -meson beam. The great quantity of data collected and its particular “cleanliness” due to the live silicon target, allowed a detailed study of the properties of three and five particle states selected in that process. I love remembering that this scientific collaboration at the Serpukhov accelerator between Milan and the Russian group was promoted and supported by Y. Goldschmidt-Clermont I mentioned as being amongst Occhialini’s senior collaborators and who was, at that time, Co-Chairman of the CERN Committee Scientific-USSR State Committee.

After these experiments, the Milanese groups continued in more than one direction at the CERN, Fermilab and Stanford, to develop the silicon detector technique and to use it in their experiments. As far as I am concerned, I remember the role of the Milan group in proposing and building the micro silicon vertex detector in the Delphi experiment at the LEP $e^+ e^-$ collider at the CERN in Geneva. Delphi was the first of 4 LEP experiments and was also absolutely the first large worldwide experiment at a collider —with a silicon vertex detector inserted— to have worked at the start of the LEP in 1989. Different groups from the international collaboration contributed to its design, construction and development; for the Italian part, this was mainly in the laboratory in Milan. In the final LEP period, in 1999, more than 150000 silicon micro-strips were installed in the central Delphi barrel and more than 1.2 million pixels in the forward and backward small-angle part compared to the primary incident beams. This detector system was important in determining the geometry of all the particles produced, but had a determining role in the identifying and measurement of secondary vertices in heavy-quark particles, whose charm and, in particular, beauty, decay in a few tenths of a micron from the primary interaction vertex. Therefore, this had a positive influence on all the physical results correlated with those particles, both, generally, in the measurement of Standard Model parameters and the research into new particles such as the “Higgs” which has heavy-quark decay channels. Part of that micro-vertex detector is currently in the “Leonardo da Vinci” Museum of Science and Technology in Milan.

This undertaking in silicon detectors by students of Beppo’s students continues. Massimo Caccia in Como is the European coordinator of a vast European Community project for applications of pixel detectors, particularly in the medical field. Moreover, in Milan, the group originating in the DELPHI experiment, led now by Francesco Ragusa, is taking part in the construction of the pixel detector part of the central detector of the ATLAS experiment for the LHC proton-proton collider, which will start working in 2007 at the CERN. Chiara Meroni, who is contributing to this undertaking, co-ordinates pixel detector activity at a national level in the INFN. The pixel detector system in Atlas is two orders of magnitude as complex as that in DELPHI, and is, in the completion stage, consisting of 100 million detectors and the same number of electronic channels.

4. – Beppo Occhialini and the young people —educator in scientific research and, above all, in life. Memories of a student and assistant

Those who were often in contact with Beppo Occhialini were always struck, and often fascinated, by his very strong personality. In particular, this influenced scientific training, perhaps even more the human one, of his young students and collaborators. My relationship with Occhialini was firstly that of a graduand and student, then, for more than twenty years, that of assistant to his chair of “Fisica Superiore”, and finally, in the last years, simply that of a friend. In addition to the episodes and experiences shared with him, I want to bring back to mind some parts of his life which he narrated, to me and certainly to other young people who spent time with him, tales often told with the clear aim of transmitting a method, a way of acting, a teaching.

He told us one episode of 1932-33 when he was in Cambridge, in England, working with Patrick M. S. Blackett on the research which led to the discovery of the existence of the positron (Blackett received the Nobel Prize in 1948 for the developments, in the research, of the Wilson chamber method and the resulting discoveries). The announcement of their discovery was at the beginning of 1933, a little after the same discovery made by Carl D. Anderson (Nobel Prize in 1936 for this discovery) at Caltech, in California. Anderson cited in his article the fact that, while he was correcting the drafts, news reached him of Blackett and Occhialini’s discovery. What Beppo told us was that, in the evening, he and other young Italians, in the Cavendish Laboratory in Cambridge, gathered around Enrico Persico, the Italian theoretical physicist who was a few years older than Beppo, appreciated by many of us for the great clarity of exposition in his books. On those evenings, Persico explained the most recent developments in quantum theories to the youngest friends, including P. A. M. Dirac’s theory, which predicted the existence of anti-particles. Unlike Anderson’s article, which only spoke of the discovery of a particle which was the same as the electron, but with a positive charge, Blackett and Occhialini’s article explicitly cites Dirac’s theory and identifies the positive electrons or “positrons” they observed with the antiparticles predicted by Dirac. Then they proved the validity of the theory, also citing the mechanisms of creation and annihilation of particles-antiparticles.

The relations between Blackett and Occhialini were always very affectionate and explicitly linked to Blackett’s recognition of the then twenty-five-year-old Occhialini’s crucial contribution to the work of the discovery, as witnessed by Blackett’s famous telegram from Stockholm in which he said something like “...*you should have been here with me as well, Peppino*”. Peppino was Occhialini’s nickname when he was young, particularly used by his father, as Giovanni Polvani, former student of Occhialini’s father, Augusto, in Pisa, told us. The motivation of the Nobel Prize explicitly states, “... *for his development of the Wilson cloud chamber method...*” and it is well known that this development was mainly the use as trigger of the technique of the coincidences of Geiger counters for the Wilson chamber, which Occhialini had learnt from Bruno Rossi, in Florence, and had taken with him and adopted in Cambridge.

Later, relations with the English physicist Cecil P. Powell were not as warm. In 1947, with Powell and C. M. G. Lattes, Occhialini shared the discovery, in Bristol, of the π -meson in nuclear emulsions exposed at altitude, then identified as the particle predicted by H. Yukawa as carrier of strong interaction (in one of the works on this subject of the Bristol group there is also the contribution of H. Muirhead) . There was a great deal of expectation for this particle, after Pancini, Conversi and Piccioni definitively demonstrated that the “mesotron”, now called “muon”, could not be the Yukawa particle in their fine experiment in Rome, at the end of the Second World War. Powell won the Nobel Prize in 1950 for this discovery, but did not give any public recognition to his collaborators Occhialini and Lattes. The latter had come from Brazil, where he had been a young student of Beppo, during his stay in Sao Paolo. The motivation of the Nobel Prize was *“for his development of the photographic method of studying nuclear processes and his discoveries regarding mesons made with this method”*.

Bruno Pontecorvo summarised the situation with the quite famous sentence he wrote in his contribution to the Symposium for the 20th anniversary of Occhialini’s return to Italy, *“It would be very easy for me not to offer a toast to Peppino, but to any physicist, more or less in this way: I raise my glass with the hope that you can collaborate with Occhialini in some experiments. It is a practically certain way for you to win the Nobel Prize.”*

Rereading that, full of affection and esteem for Occhialini, I remembered a rather curious little episode in which I was witness to the rapport between our two physicists. I often went back and forth to Russia in the 1970s for our experiment, at the Serpukhov accelerator. Beppo wanted me to take a small gift to Bruno Pontecorvo and asked me to look in a “street market” for one of those cheese-graters that works like a coffee grinder. In reality, it was impossible to find it in the markets and we could only find it in Picowa, an extremely up-market shop in the centre of Milan. When I got to the Pontecorvo’s house in Dubna, Bruno was away for a few days and one of his sons, Gil, met me. I gave him this well-wrapped object, but I was too trusting of his knowledge of Italian and, later, when he passed through Milan, Gil came to dinner at our house and we laughed a lot because, thinking the package contained Parmigiano cheese, it was jealously guarded over in the fridge until his father returned and was, however, delighted with the thought of his distant friend, and I, too, shared in his thanks.

Returning a moment to the relationship with Powell, there is a probably little-known episode which says a lot about Beppo Occhialini’s nature. He had a wooden bench built with Powell’s name carved into it and then, with the laboratory technicians, transported it to Premana, a small village in the mountains above Lake Como. That bench was put on a path at the point in which Powell, who was there tracing an old English colony which had founded the processing, (which still continues), of steel objects, had a heart attack which killed him in 1969. Beppo’s reason was, *“...if that bench had already been there, Powell would probably have stopped to rest there.....”* and, therefore, we can conclude, the incident would have had a different outcome!

The habit, started in Cambridge, of meeting periodically in the evening to discuss physics freely among friends, continued throughout Beppo’s life. I do not believe there

is anyone among the Occhialini's friends who did not spend at least one evening at their home in Viale Argonne in Milan. The after dinner appointment was generally weekly, but also linked to any important guest passing through Milan. Famous physicists from all over the world, responsible for important experiments or frontier theories, often Nobel Prize winners, managers of large American or European laboratories, passed through, and young people were always welcome, both as a reference point for the meeting and also simply as participants. After the birth of their daughter Etra, known familiarly as Connie Pooh, the young people, in turn, had the task of going to check that she was sleeping soundly. There was good music, for some of us it was a chance to discover the first high fidelity systems, there was "Lugana" wine and large sheets of white paper on an easel, for listening to —and discussing— ideas, new theories, developments of techniques or projects. Clearly, this was the best way for Beppo Occhialini to confront and understand the new theories; he could lead discussions and suggest explanations in the ways he preferred, always bearing in mind the natural phenomena which underlay mathematical formulations, or might underlie them, without being intimidated by new formalisms, which sometimes are destined to future success, but sometimes are meteors soon forgotten. He greatly appreciated the theorists who had a good knowledge of experimental evidence and were able to assess its reliability. Obviously, as a great experimenter himself, he was always enthusiastic and wanted to investigate new experimental techniques in depth: as mentioned above, one of the many possible examples, was his interest in the new silicon detectors.

Beppo was very attentive, almost obsessively, to the orders of magnitude. He believed that experiments could not be prepared and theories could not be developed without having the orders of magnitude of all the quantities clear, even the most simple and obvious, from the start. This preoccupation of his was introduced, in particular, in his teaching and relations with students. I believe it to be well known that Occhialini was not at all fond of academic lessons, to the extent that he feared them. This could also have extremely awkward consequences for his assistants. We met after dinner in Viale Argonne and began to discuss how to develop the subject of the lesson of the following day; it continued until the small hours, thoroughly discussing all the alternative approaches to the forthcoming lecture and trying to predict the possible questions or objections of the students, in particular the "worst ones". Quite often, in the small hours, Beppo would reach the conclusion that, perhaps it was better that an assistant should take the lesson that time, and he said it looking meaningfully in the direction of the most senior assistant. It should be said on behalf of the students, that there could not have been a better preparation for a lesson than that one. Decades later, there are ex-students of the "Fisica Superiore" course who remember the lessons of the course with great appreciation, and the ex-assistant has to explain to them what Occhialini's role was in that success.

On the other hand, he was an assiduous participant of the course viva voce exams. Also in this, his preoccupation with knowledge of the natural facts which translate into laws and models was enormous. The future physicists had to know that the muons, produced at great heights in the atmosphere, arrived in such large numbers in the cosmic rays at our level, to the extent that it was part of the knowledge and he knew the law of

the relativistic dilatation of time like the back of his hand. In desperation, when faced by ignorance of the themes of modern physics, he resorted to knowledge of elementary physics and the most common orders of magnitude. I remember one exam where he even asked the difference between the densities of air and water. Faced by silence, he tried asking about those of lead and gold. *"But haven't you ever seen that clip of a comic film from the 1930s in which you see a thief running away from a bank, quickly carrying two suitcases full of gold? What was unreal about that scene?"*. I seem to remember that, in the continuing silence, Beppo quit the room, scowling in silence, leaving the final task of the failure and ritual explanations and recommendations to us assistants.

In reality, Occhialini feared the occasionally somewhat fanatical strictness and rigidity with which young panel members often conduct exams. With his indirect way of transmitting his will or reinforcing behaviour he liked in his collaborators. I remember that once, at an exam for the post of an assistant, he turned at the end to a young collaborator who had been on the panel with him, behind closed doors, and thanked him in an uncharacteristically warm way for the *"human way in which you had, at a certain point, helped the candidate who was in difficulty!"*.

Still talking about this indirect way of his of transmitting something to those near him, I think it is appropriate to tell a personal anecdote. Although I had spent a lot of time with him in non-work situations, made trips with him and so on, I only went to the cinema with him once. He suggested going to the cinema in Piazza Piola, near our university, and he took me to see the wonderful film *"Please don't bite my neck"* by Roman Polanski who plays the part of the professor's assistant Alfred, obsequious par excellence....! At that time, as Alberto Bonetti had won the chair, I was Occhialini's most senior assistant. I hope I was able to adapt to this lesson on behaviour on what to avoid in the relationship between assistant and professor, or, at least, I tried..... !

The last memory I want to relate of all that Occhialini told us, and which others will probably have heard, briefly touches on his thought and behaviour in public things. He was certainly anti-fascist, and lived in harmony with this conviction, in particular as the Second World War approached and during it. Nevertheless, he did not want to go away and live, as he could have done —and as he openly said— as an exile in a country at war with Italy. So he chose Brazil, even if that country later entered the war against Italy in August 1942, before the armistice. The time of the anecdote was when I told him that, to celebrate the last step of my university career, I had bought a Guzzi motor bike. I knew he loved motor bikes and he immediately showed it, adding however, that "motor bike" for him was synonymous with "BMW" *"...even if, as you know, I haven't always liked German things..."*. He told me then about what was probably his last motor bike journey in Italy. It was in the summer of 1937 and, while he was in Siena, he received a telegram from the Foreign Ministry calling him to Rome for the ceremony of the award of the permit to leave the country and go to Brazil for study purposes, officially for a fixed period of time, although he had already decided that, with Fascism, he would not return. He had just a few hours to get to Rome and so the only way to do it in time was by motor bike. The road was the Cassia which passed by Radicofani, full of bends and, at the time, not asphalted. He reached the door of the Ministry a little late, covered from

head to foot in dust; he brushed himself down a bit and cleaned his face with his hands, and while telling the story, he would repeat the gesture with his fingers on his cheeks. At last, someone hustled him into the room with other young people lined up and whispered to the person (the Minister?) who was speaking. The latter looked at him and added something like *“Greetings to comrade Giuseppe Occhialini you can see over there who, like a true representative of Fascist youth, has joined us with a courageous journey on a motor bicycle...”*. Occhialini looked at me laughing and said, *“You see...I was about to leave for ever”* and... made a gesture with his arms...

In the last few years, when he divided his time between Tuscany and Paris, I and my wife Anita met him by sometimes going to Marcialla, where he and Connie were happy to have guests. I met him just once, passing through Milan. That time, he no longer had the flat in Viale Argonne and came to us for dinner after which he had to leave from the Central Station. He went, travelling overnight, to see the widow of Pauli, who had been his great friend, in southern France. I insisted on accompanying him to the train; he had tennis shoes and his ever-present rucksack, the same one which, in times when it was an uncommon sight, he had taught us to use even for normal journeys, very handy.... When I realised that he would have made the long journey in an uncomfortable second-class carriage, I was worried, but he said, *“Don’t worry, I’m used to it. Until recently, when they still paid my business travel, I still travelled in second-class, and I have got used to it”* and then, as I was still perplexed, *“... OK, if I’m really uncomfortable, I’ll go through to the couchette car.”* I do not believe he did.

* * *

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Giuseppe Occhialini and CERN

EMANUELE QUERCIGH

CERN, PH Division - CH-1211 Genève 23, Switzerland

It is almost a paradox that Giuseppe Occhialini, Beppo for the older generation, never had any formal connection with CERN. Yet he had an influence on its creation and on its physics. His achievements provided some of the best arguments for having such a new laboratory, while his personality and his example had a lasting impact on many young and talented cosmic ray physicists who later made CERN a scientific success and an example of international collaboration. This was Beppo's first legacy to CERN, and to give an idea of how this happened I shall briefly try to evoke a few testimonies from the early fifties. In 1948 Occhialini was back in Italy, first in Genoa and then, in 1952, in Milan, where he soon formed a strong group for the study of high-energy physics using nuclear emulsions, a branch of research that he had first pioneered in Bristol and then in Brussels. At that time, the discovery of the pion was already well known beyond the scientific world: at school, it was our history, and not our physics teacher who first told us about the then newly observed particle, which he described as "the glue which keeps matter together". Most of the experimental activity on the properties of the pion, however, had already moved from cosmic rays to the synchro-cyclotrons of Berkeley, Columbia and Chicago. Nevertheless, cosmic ray physicists still had a few years of discovery before them exploring a new type of unstable particles discovered soon after the pion, and later, in light of the riddle between their copious production and their long lifetimes called "strange". The atmosphere was one of enthusiasm, and important discoveries were made that paved the way to the recognition of the quark structure of hadrons, as described in a somewhat lyric evocation of that period by Charles Peyrou. *"...here appeared for the first time a new flavour. Baryons were found which were not nucleons, not even excited states ones since they decayed much too slowly. Heavy mesons were found which had all the thinkable weak decays with all possible lighter particles (π , μ , e , ν). Here for the first time an entire new world, parallel but not exactly similar*

to the old one, was discovered without any guidance from theory" [1]. At that time, Peyrou was working with Bernard Gregory and André Lagarrigue in Paris at the École Polytechnique. Yves Goldschmidt-Clermont had moved from Bristol to Brussels, Donald Perkins was in Bristol, and Rafael Armenteros was at first in Manchester, as was Clifford Butler, and then in Paris. A few years later, all of them were to play a central role in the physics at CERN.

The main detectors employed in this endeavour were the counter-controlled cloud chamber and nuclear emulsions, which both owed a lot in their development to Beppo's imagination and inventiveness. The principal cloud chamber groups in Europe, from Manchester and Paris, had their chambers installed separately in an observatory at the top of the Pic du Midi de Bigorre (2867 m) in the French Pyrenées. Manchester had another chamber (whose control passed at CERN in 1955) at the Jungfrauoch (3460 m) in Switzerland. The choice of the Pic du Midi was another of Occhialini's ideas, as reported by Butler: "*Occhialini suggested to Blackett that it might just be possible to install and operate the old Blackett magnet at the astronomical observatory on the Pic-du-Midi in the French Pyrenees at a height of 2867 m. Occhialini and Cosyns from Brussels knew this observatory well and particularly its director J. Roesch*" [2]. The emulsion groups, who found it necessary instead to work together, were exposing their stacks of detectors at high altitude by means of hydrogen-filled balloons, in order to take advantage of the much higher intensities of cosmic rays in the stratosphere. These activities had also the support of CERN, which was sponsoring cosmic rays investigations as a cooperative European research endeavour [3-6]. Many of the flights were launched from Italy, and recovery was made at sea with the help of the Italian navy. The overall complexity of this task, forced various groups to combine their efforts, as described by Perkins: "*The balloon-flying expeditions in the Mediterranean had unforeseen and highly providential effects for high-energy physics in Europe. Many flights had to be made, with highly organized tracking and recovery procedures, and these were beyond the resources of a single university group. Large collaborations were formed, with balloon fabrication centred at Bristol and Padua, but preparation of stacks, radiosonde and tracking equipment, and so forth, (were) the responsibilities of many laboratories. The Sardinia expedition of 1953 involved twenty-two laboratories from twelve countries: Bristol, Brussels, Bern, Caen, Catania, Copenhagen, Dublin, École Normale, École Polytechnique, Geneva, Goettingen, London, Lund, Milan, Oslo, Padua, Rome, Sydney, Turin, Trondheim, Uppsala, Warsaw. The processed emulsion stacks were divided between laboratories. This implied that tracks in an event found in one group had to be traced through the emulsions to those of a second, third or even fourth group. Could this possibly work? It did, and it was a triumph of international collaboration and organization. The largest and final stack of emulsions in this program was the famous G-stack (G for gigantic), with fifteen litres of emulsions (63 kg) flown for six hours at 27 km over northern Italy in 1954*" [7]. The G-stack was flown by a collaboration between Bristol, Milan and Padua, which were joined in the analysis by Copenhagen, Dublin and Genoa. Its results provided definitive information on the decay modes of the charged kaon. A vivid account of the atmosphere during the G-stack analysis, is given by Cormac O'Ceallaigh: "*The organization of the*

results of the measurements and their presentation was again largely the work of the Italians and took place at Padova. I never will forget the fever and the excitement associated with that effort. Occhialini was in ultimate charge and strode up and down the scene like an avenging Jehovah or thundering Jove..." [8].

In addition to his flair for physics and his energy, Beppo had scrupulous standards in correctly attributing scientific credit to individuals and laboratories, standards that he expected to be followed by all collaborators. Such a concern was important for the G-stack, whose publications had more than thirty authors, a record for the time. It was later to be crucial for the extensive collaborations at the CERN accelerators, where many more people from different laboratories, nationalities and cultures, needed to work together. In conclusion, the emulsion groups had proven that international collaboration in physics was possible and scientifically rewarding. A few years later, their experience was to influence positively the development of the scientific programme at CERN.

Around the mid-fifties, the cosmic ray community realized that the future of particle physics did not rest with cosmic rays but with the new accelerators. At Brookhaven, the 3 GeV Cosmotron started operation in 1952, while plans were being made for alternating gradient proton synchrotron, the AGS. In 1954, the 6 GeV Bevatron became operational at Berkeley. In Europe, the provisional CERN Council, with Edoardo Amaldi as Secretary-General, had decided in 1952 that Geneva would host the future Laboratory [9] and its first two accelerators, a synchro-cyclotron, and a 28 GeV alternating gradient proto-synchrotron, the PS, which was to yield its first beam in 1959. The challenge lay in giving European physicists the tools to continue the study of elementary particles, using accelerators, which they had so successfully initiated with cosmic rays. The newly invented bubble chamber seemed perfectly appropriate for this study, and the emergence of the electronic computer was seen as a means of dealing with the flux of data. Many of the European cloud chamber and emulsion groups adopted the new technique. Peyrou took charge of the CERN bubble chamber programme, a natural continuation of his cloud chamber expertise. He fully appreciated the kind of international collaboration introduced a few years previously by the emulsion groups, and had the vision that particle physics needed large chambers built by a team of specialists, operated as a facility similar to accelerators, and producing millions of photos to be distributed to all the groups requesting them. He thus created the Track Chamber division. This had the task of building the chambers, operating them through all the stages, from the design of the particle beams to the development of the film, and of collaborating with the European groups in the preparation of film-measuring devices, software tools and, frequently, in the analysis of the physics. In this way, the bubble chamber programme played a major role in building up the existing symbiosis between CERN and the community of European high-energy physicists; a situation that today appears so natural. At CERN, Beppo was well known and highly respected. I remember Peyrou referring to him as one of the few exceptional men *"qui m'ont appris à peu près toutes les choses que je me targue de savoir, autant et même plus en dehors de la physique qu'en physique"* [10].

When, in 1959, the first particle beams from the PS became available, Beppo, together with most of his collaborators, had already moved his interests to space physics.

Nevertheless, with the intelligent help of Constance “Connie” Dilworth, he generously gave all possible support to those who instead wanted to continue with particle physics. This was Beppo’s last legacy to CERN. He assisted them in obtaining the necessary tools for getting started with the analysis of bubble chamber pictures, together with an adequate laboratory space, the “Capannone”. Perhaps most importantly, he supported their first steps at CERN, thus making the way easier for a number of young physicists who were later to make many effective contributions to the discovery of weak neutral currents and to the knowledge of hadron interactions.

* * *

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1960-1970: Milano and Gruppo Spazio

GIORGIO SIRONI

Dipartimento di Fisica “G. Occhialini”, Università di Milano-Bicocca, Milano, Italy

1. – Introduction

In Milan cosmic-rays studies are going on since 1932 [1] when, just a few years after the birth of the local University, Giovanni Polvani got the chair of Experimental Physics at the Science Faculty. His assistant G. Cocconi, just arrived from Rome, began observation using counters and cloud chambers installed in Milan and at mountain altitudes (Passo Sella on the Dolomites). With the help of V. Tongiorgi and G. Loverdo, Cocconi intended to study the composition of the secondary radiation produced in the Earth atmosphere and the nature of the primary radiation which arrives at the top of the atmosphere from the external space.

Cocconi activity continued in the '30s and part of the '40s, during the II world war. When he left Milan, in spring 1945, G. Tagliaferri, G. Salvini and A. Lovati took over. They were joined in the following years, among others, by S. Ratti, E. Fiorini and R. Giacconi. Observations continued in Milan and at observatories, some of which still in use, in the Alps: Passo Sella (2240 m a.s.l.), Lago d'Inferno (Valtellina, 2100 m a.s.l.), Testa Grigia (Western Alps, 3500 m a.s.l.), Lago dei Sabbioni (Val Formazza, 2466 m a.s.l.) or under mountain rocks (Montorfano on the Lago Maggiore, 240 m a.s.l.). The observations were intended to separate the hard and soft components of the Cosmic Radiation which arrives at the ground level and to recognize the particles present in these components. Studies of matter-radiation interaction and, hopefully, discovery of previously unknown particles were also among the observation aims.

The arrival of Occhialini in Milan in 1951 gave new impetus to the activity of the Cosmic Ray group. At that time Beppo, as Occhialini was called by his friends, was internationally known for his participation to the discoveries in 1932 with Blackett of

the electron-positron pairs [2] and in 1947 with Powell, Lattes and Muirhead of the pions [3]. He brought to Milan the nuclear emulsion technique for particle detection and included Milan in a network of international collaborations. The nuclear emulsions sent at stratospheric altitudes with balloons (famous was the so called G-stack with a volume of 63 l) recorded large numbers of interesting events. Studied with microscopes the tracks in the emulsions were an important source of information for the new field of elementary-particle physics. With Occhialini arrived, among others, Beppo's wife Connie Dilworth, Alberto Bonetti, Livio Scarsi and Aldo Igiuni, an invaluable laboratory technician, expert in developing nuclear emulsions. They joined the pre-existing Milan cosmic-ray group.

2. – Gruppo Spazio

The scientific yield of the 1950-1960 cosmic-ray activity was large. But as it was suggested by Le Prince Ringuet at the famous International Conference on Cosmic Rays held in Bagnere de Bigorre in 1953 crisis was approaching: bigger and bigger particle accelerators were arriving in the USA and in Europe and CERN was becoming a reality [4]. It soon became possible to produce interactions of known particles, of known energy with known targets, and carry on experiments better defined than it was possible with cosmic rays. So the interest for cosmic rays as sources of elementary particles faded everywhere. In Milan the original Cosmic Ray Community split in two parts.

Those interested in elementary-particles physics moved toward accelerators and kept interest only on cosmic rays of extremely high energies not yet available at machines.

Others focused their interest on the astrophysical aspects of the cosmic rays: composition, origin and propagation in the interstellar and interplanetary medium of the particles not yet modified by the interaction with the Earth atmosphere, the so-called cosmic-rays primary component. Previously astrophysical aspects attracted the attention of physicists like E. Fermi and B. Rossi, but were generally ignored by the official astronomers.

Unexpected support to the astrophysics party arrived at the beginning of the '50s when Alfvén and Herlofson (1950) [5] and, independently, Kiepenheuer (1950) [6] followed by Ginzburg and Syrovatskii (1964) [7] suggested that the powerful radio emission from galactic and extragalactic sources and from the interstellar medium just discovered by radio astronomers was synchrotron radiation produced by Cosmic Rays Electrons moving around the lines of force of a diffuse magnetic field. Because both the existence of interstellar magnetic fields and of electrons among the primary cosmic rays had to be proved (just a few years before, in 1948, Rossi had failed in detecting electrons among the primary cosmic rays) this hypothesis triggered new interest on Cosmic Rays and tightened the links between physics and astronomy.

Occhialini with Connie Dilworth and Livio Scarsi (Alberto Bonetti had left Milan for Bari) maintained a deep interest in the elementary-particle field but privileged the astrophysical approach. So between the end of the '50s and the beginning of the '60s in Milan from the original cosmic-rays group stem a group informally known as Gruppo Spazio (from the observing techniques used by its components). His activity lasted a

decade and ended at the beginning of the '70s when it gave rise to Laboratorio di Fisica Cosmica e Tecnologie Relative (LFCTR) of the Italian National Council for Research (CNR).

The choice between elementary particles and astrophysics was not easy. In 1961 when, as a student who had to prepare his thesis for the Physics Degree, I asked Occhialini to be my supervisor he suggested two research activities: double beta decay, a typical elementary-particles physics argument, and search of GeV electrons among the primary cosmic rays. After a few days I agreed with Beppo (at that time I never thought I could use such a familiar name, it took years before I became accustomed to do it), and chose cosmic-ray electrons. So I joined the activities of the Gruppo Spazio. Members of Beppo group were Connie Dilworth, Livio Scarsi, Gianni Degli Antoni and Giuliano Boella. Among the technicians there were Aldo Igiuni, Renato Ballerini, Piero Inzani and Edoardo Bardeggia. Invaluable secretary of the group was Nella Valoire. Gradually the group was reinforced with students who, after graduation, remained as young researchers like me, Emilio Rocca, then Cesare Perola, Oberto Citterio, Cesare Reina, Aldo Treves, Laura Maraschi, Giovanni Bignami, Gianfranco Quaranta and many others. Giuseppe Gavazzi, in 1975, was the last student who had Beppo as an official supervisor.

First research arguments were primary cosmic-ray electrons and neutron distribution in the Earth atmosphere. In 1961 the first weak evidence of the presence of electrons among the primary cosmic rays had been obtained by J. Earl [8] and P. Meyer and R. Vogt [9]). By the end of 1963 a collaboration between the Milano Gruppo Spazio of Occhialini and the Service d'Electronique Physique of Saclay, lead by J. Labeyrie, obtained statistically robust evidence that cosmic-ray electrons of energy above few GeV were present among the cosmic rays [10].

A result which definitely brought cosmic-ray physics to the attention of the astronomical community. It was possible to reach it in just a couple of years because Beppo with his group: i) created tight and efficient links between Milan and Saclay; ii) choose as the most appropriate detector a spark chamber with lead plates, controlled by a sophisticated coincidence system of plastic scintillators (when an electromagnetic shower developed through the plates the chamber was triggered and an image of the sparks recorded on film); iii) made chamber and electronics able to sustain the harsh environmental conditions of a stratospheric balloon flight; iv) prepared balloons launching facilities and system of data transmission necessary to bring payload of few hundred kg at 35-40 km a.s.l. and to keep it there for hours.

The first flight was made from Aire sur l'Adour (Southern France) on 5 November 1963. The payload transported by strong and unforeseen stratospheric winds landed few hours later on the Italian Alps during a storm and was covered by snow. A few days later however the experiment was recovered and brought to Milan.

The film with the spark chamber images, immediately developed and analyzed, gave in three weeks the first measurement of the flux of primary electrons of energy greater than 4.5 GeV. In December of the same year this result was presented at the International Conference on Cosmic Rays of Jaipur. In September 1964 it appeared on "Physical Review Letter". Measurements of the flux of primary cosmic-rays electrons were repeated

at higher energies and extended with observations of the East-West effect which gave the ratio between positive and negative electrons, essential to understand the origin of the cosmic electrons. The most complete campaign of observations was made in 1967 with balloons launched from Trapani, in Sicily and payload recovery in the Mediterranean Sea.

Besides positive scientific results, this activity triggered interest for long-duration balloon flights as low-cost competitors of space flights. Tests of trans-mediterranean and trans-atlantic balloon flights lasting from few days to a week were successfully made.

The experience, acquired in detecting electrons through the electromagnetic showers they produce, stimulated the interest of the Gruppo Spazio for the growing field of gamma ray astronomy. Through the same combination of precision and efficiency shown with the detection of the cosmic-ray electrons the Gruppo Spazio in collaboration with Saclay by the end of 1970, a mere two years after the pulsar discovery by radio astronomers, got evidence that the Crab Nebula pulsar produces also gamma-ray pulses [11].

Measurements of the flux of epithermal neutrons in the atmosphere and its dependence on the elevation was another line of research of the Gruppo Spazio [12]. They were made to evaluate the cosmic-ray albedo-neutron-decay as a source of high-energy protons in the inner radiation belts of the Earth. Initially observations were made flying detectors on meteorological and stratospheric balloons. To overcome the 40 km limit altitude, a rocket was then launched from Sardinia. Natural evolution of this project was the search of high energy neutrons from the Sun with detectors flown at high altitudes on stratospheric balloons [13].

So gradually Gruppo Spazio became acquainted with space techniques and toward the end of the '60s was ready for a series of experiments on artificial satellites: S79 and S209 (on the ESRO satellites HEOS A-1 and HEOS A-2), dedicated to studies of the low-energy (< 1 GeV) cosmic-ray electrons, S-88 and S-133 (on ESRO TD-1) for the detection of gamma-rays from the Sun and from the interstellar medium [14]. These early satellites were collections of different payloads, tightly packed on the same platform. So frequently experiments interfered and were disturbed by radiation locally produced by the cosmic ray bombardment hitting nearby massive pieces of equipment. The results were therefore poor, but helped to gain experience and paved the way to the second generation of space experiments prepared in Milan. The first of them was COS-B [15], a satellite completely dedicated to gamma-ray astronomy, based on a single detector, a spark chamber similar to those previously used by the Milano-Saclay collaboration. Prepared by a European collaboration (Milano, Saclay, Munich, Leiden and the ESRO Technical Centre, ESTEC, in Noordwijk) COS-B, originally proposed to NASA as the Caravane Collaboration, was finally selected by ESRO. It gave the first catalogue of gamma-ray sources [16] and a measurement of the diffuse background at energies above 30 MeV [17].

With the only exception of S-79 which was launched in 1968, all these experiments were devised by the Gruppo Spazio in the '60s but were launched in the '70 when the Gruppo Spazio had been superseded by Laboratorio di Fisica Cosmica e Tecnologie Relative (LFCTR) of the Italian National Council of Research (CNR).

3. – Gruppo Spazio, GIFCO and CNR

When at the beginning of the '60s accelerators became the main source of elementary particles, the Italian National Institute of Nuclear Physics (INFN) who in the '50s was the major funding agency, decided to abandon the Cosmic Ray field. A new source of funds had to be found, so Occhialini, Amaldi, Puppi, Castagnoli and Gratton applied to the National Council of Research (CNR) and joined their efforts forming the Gruppo Italiano di Fisica Cosmica (GIFCO) of CNR. Research units of GIFCO were created at the Physics Institutes of the Universities of Milan, Turin, Bologna and Rome and CNR became the main source of financial support for cosmic-ray studies. Soon to overcome the difficulties University had in providing grants and positions, CNR decided to provide also fellowships and temporary positions (the first two GIFCO researchers took position on 1st February 1967 and were assigned to Milan and Turin when the President of GIFCO was Carlo Castagnoli). In a few years CNR people outnumbered the University people so at the end of the '60s the GIFCO research units were transformed in Laboratories of the CNR. GIFCO maintained however a role of research coordination. The first Director of the Milan Laboratory was Beppo Occhialini. The official name Beppo chose for it, Laboratory for Cosmic Physics and Related Technologies (LFCTR), underlines the importance Occhialini already gave to links between Research and Technology. The laboratory was originally hosted at the Physics Institute. Part of the components of Gruppo Spazio became CNR personnel and the activity went on in strict collaboration with those who maintained the University affiliation. The laboratory gradually expanded and at the mid of the '70s left the Physics Institute because of the lack of room and because activities were becoming more and more disturbed by protest of students asking for a new University system [18,19]. At the beginning of the '80s LFCTR and the GIFCO Laboratories were transformed in Institutes, and became more and more independent from GIFCO. Finally at the beginning of 2000 all the Astrophysical Institutes of CNR, including those created through GIFCO, were transferred to the newly created National Institute of Astrophysics (INAF) and GIFCO closed. Roughly at the same time the great majority of the components of the Gruppo Spazio who had remained with the University left the old University of Milan and moved to the new University of Milano-Bicocca, where the new Physics Department was named Giuseppe Occhialini.

Besides creating GIFCO and CNR Laboratories, to foster space activities and provide the structures necessary to manage contracts with industry, Occhialini with Amaldi and others succeeded in convincing CNR to create initially a Service for Space Activities (SAS), then the National Space Plan (PSN), precursor of today Italian Space Agency (ASI). At the international level the same activity brought to the European Space Research Organization (ESRO), precursor of the European Space Agency (ESA) [20].

4. – Life at the Gruppo Spazio

Beppo actions were essential in providing the framework which made the activities of the Gruppo Spazio possible. A major commitment was promoting and improving the

competence level of researchers and technicians of Gruppo Spazio. Essential were:

- i) International collaborations with well-known research groups.
- ii) Stable presence of researchers from abroad (George Clark, John Bland, John Kidd, Martin Turner, Tony Dean, Constantinos Paizis, etc.) and frequent visits of well-known researchers. All the researchers stayed one or two years, some like Constantinos Paizis remained and are now Professors at Milan University, others like Tony Dean left but kept scientific collaboration with Milan.
- iii) Discrete hints to young researchers to spend a couple of years abroad and learn new techniques and research methods. Beppo himself and his wife Connie Dilworth, invited by B. Rossi, in 1960 spent a year at MIT to become acquainted with space observations. Livio Scarsi went in USA to study Extensive Air Showers, the writer went in Cambridge to learn radio astronomy, Cesare Perola went to Leiden, Aldo Treves to MIT, Nanni Bignami to Goddard Space Flight Center in Greenbelt, etc.
- iv) Arranging talks and seminars by known physicists and astronomers who visited Beppo and regular talks on astrophysical topics by a young astronomer of Merate, Margherita Hack (a habit maintained by Alberto Masani when Margherita Hack left Milan for Trieste).
- v) Supporting new observing techniques and methods of data reduction, from which two sub-groups derived. One dedicated to electronics was initially driven by Gianni degli Antoni and then by Giuliano Boella when degli Antoni interest concentrated on informatics. The second one studied automatic methods of pattern recognition, with Piero Mussio, Gianna Cioni and Anna della Ventura. Among the techniques considered particularly important by Beppo were the applications of superconductivity. So around 1965 he considered the possibility of building a superconducting magnetic spectrometer for analysis of the energy spectrum and charge sign of the cosmic ray electrons from balloons. This idea, now accepted by large space experiments like AMS, was at that time premature and abandoned after a few contacts with expert at INFN laboratories in Frascati.
- vi) Keeping friendly exchange of experience with industry people like Claudio Cantù of Koritska, the firm where the microscopes used for analyzing the nuclear emulsions were built or Ing. Scandone of Officine Galileo in Florence, expert of optics. Beppo also gave support to some researchers (among them Gianfranco Quaranta and Massimo Isola) who after a few year experience with the Gruppo Spazio decided to move toward aerospace industries. Beppo was also continuously looking for good technicians so along the years Aldo Igiuni, Piero Inzani, Renato Ballerini, Edoardo Bardeggia, Nino Dellera, Giuseppe Aloardi, Augusto Solazzi, Enrico Mattaini, Ennio Ronchi were discovered and recruited, among others.

As head of the Gruppo, Beppo was anxious that everybody was happy with his job. If somebody had to stay late in the evening in laboratory, it was common to see him arrive with a coffee cup, offering his help. On several occasions he set apart research urgencies to help somebody in the group who had problems or simply was particularly tired. For instance in 1963 during the search of the first electron payload, landed on the Alps in a snow storm, two members of the Milano-Saclay collaboration, apparently

lost, were forced to sleep in the open in a winter night. When the two came back he left them rest and recover for one day before asking information on the payload the entire group was anxiously searching. However when everything was fine he was absolutely firm in deciding. When a member of the COS-B team left Milan for two years at Goddard, the writer was asked to stop his activity and join the COS-B team until the experiment was ready for launch. Or when he judged that a technique like nuclear emulsions was definitely overtaken by more recent techniques like electronic counters he did not hesitate and cut proposals based on the old technique.

5. – Conclusions

If one now looks to the scientific papers produced in Milan after the end of the '50s, Beppo name never appears among the authors. This was a definite choice of Beppo. Busy with the group research policy and organization he erroneously thought that his scientific contribution was insufficient and declined invitation to sign the papers. But he never resigned from his role, everybody in the group accepted, of final judge of research proposals, contributions to conferences and papers submitted to scientific journals by group members. Getting his approval sometimes required days and days of discussion with him, in his room at the Physics Department or at his home. This procedure was generally exhausting, however the result was always extremely good: a document that obtained his approval rarely was rejected and comments or request by the official referee to whom the document had to be submitted almost certainly were favourable. So the invisible signature of Occhialini is present on all the scientific production of the Gruppo Spazio.

Cosmic Rays are still a main research field of peoples at today Physics Departments of the two Milan Universities. And INFN who at the beginning of the '60 decided to abandon the Cosmic Ray Physics is now coming back.

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La Collaboration Milano-Saclay-Palermo

BERNARD AGRINIER

Noisy sur École, France

LYDIE KOCH-MIRAMOND and JACQUES PAUL

Service d'Astrophysique, CEA Saclay, France

1. – Introduction

Le 2 décembre 2005, quand furent célébrés les quarante ans d'astrophysique spatiale au Commissariat à l'Énergie Atomique (CEA), bien peu de participants, hormis certains, parmi les plus âgés, savaient que les activités spatiales avaient pris racine à Saclay en raison de l'amitié qui liait Giuseppe “Beppo” Occhialini et Jacques Labeyrie, alors Chef du Service d'Électronique Physique.

Cette amitié a eu pour origine leur goût commun pour l'exploration des “gouffres”. Dans son livre “Les découvreurs du gouffre de la Pierre Saint Martin” [1], Jacques Labeyrie raconte les circonstances de leur première rencontre en Juillet 1949. Le lieu de cette rencontre se situe dans les Pyrénées, plus précisément à Licq-Athère, au coeur d'une région située à la limite du Pays Basque et du Béarn particulièrement riche en grottes. Jacques Labeyrie avait été invité à se joindre à un groupe de spéléologues dont Beppo Occhialini faisait partie. Ce fut le début d'une série de campagnes d'exploration de gouffres auxquelles ils participèrent tous les deux. Ces explorations sont le sujet du livre de Jacques, livre riche d'enseignements sur les grottes mais aussi sur les hommes, et un peu aussi sur la physique.

Au retour de cette première campagne, ils s'arrêtèrent près de Paris, au Fort de Châtillon (premier centre de recherche du CEA), pour visiter le laboratoire de Jacques. Lev Kowarski directeur du centre qui connaissait la réputation scientifique de Beppo, tint

à les raccompagner, et les discussions continuèrent dans la voiture jusqu'à l'Université Libre de Bruxelles où Beppo travaillait à ce moment là.

2. – Expériences en ballon stratosphérique

Beppo revenu en Italie, à Gènes en 1950, puis à Milan en 1952, arriva à Saclay un jour de 1961 invité par Jacques Labeyrie. Leur idée était de continuer l'étude des rayons cosmiques à l'aide de moyens spatiaux. Beppo proposait plus précisément de rechercher la présence d'électrons de haute énergie dans le rayonnement cosmique primaire. En effet le rayonnement radio du fond galactique présentait un spectre de forme "non-thermique", qui pouvait être dû au rayonnement synchrotron d'électrons de haute énergie en interaction avec le champ magnétique interstellaire. Ce n'est pas par le seul hasard que Beppo entra dans le laboratoire d'Yves Koechlin: on y étudiait les détecteurs et en particulier les chambres à étincelles. Beppo voulait utiliser ce type de chambre à traces. C'était l'élément central de l'expérience puisqu' on devait y détecter les électrons par effet de cascade électromagnétique dans des plaques de Plomb.

Par ailleurs les électrons interagissant dans l'atmosphère encore plus rapidement que les autres rayons cosmiques, il était nécessaire d'embarquer le dispositif expérimental le plus haut possible, à quelques g/cm^2 (ou hPa) de pression résiduelle, ce qui n'était possible à cette époque qu'avec des ballons stratosphériques.

Ainsi a débuté une collaboration Milan-Saclay (Istituto di Fisica dell'Università di Milano et Istituto Nazionale di Fisica Nucleare Sezione di Milano)-(CEA CENSaclay Service d'Electronique Physique), collaboration qui perdure encore aujourd'hui. La chambre à étincelles fut réalisée à Saclay et les compteurs avec leur électronique à Milan. Cette chambre était du type optique, on prenait donc des photos stéréoscopiques des traces; on pouvait photographier les "cascade showers"! Beppo nous prodigua bien sûr de nombreux conseils sur les films . . .

Pour ce qui est des ballons, le Centre National d'Etudes Spatiales (CNES) n'existait que depuis fin 1961, mais il commençait à développer des ballons et des vols stratosphériques sous l'impulsion de R. Régipa. Ces deux activités avaient été installées à Aire sur l'Adour dans les Landes, pas très loin des Pyrénées, où il nous fallait parfois aller récupérer le matériel descendu en parachute lors des vols d'essai.

Le premier vol "scientifique" fut lancé le 5 novembre 1963 à partir de la base de Aire sur l'Adour (voir Photo 1). Mais cette fois la descente du détecteur en parachute s'effectua par mauvais temps dans les Alpes, sur le Mont Margareis dans la région de Cuneo. Retrouver l'expérience était indispensable pour obtenir les données enregistrées sur le film. Beppo participa activement à cette récupération qui, à cause de la neige, ne fut pas chose aisée (voir Photo 2).

Le dépouillement du film nous permit d'observer les traces de 18 électrons "primaires". Cette mesure au delà de 4.5 GeV était peu susceptible d'être modulée par l'activité solaire, et on pouvait en déduire que les électrons représentaient environ 1% de l'ensemble des rayons cosmiques. Ce résultat fut communiqué par Connie Dilworth à la Conférence Internationale sur les Rayons Cosmiques (ICRC) de décembre 1963 à Jaipur (Inde). Un



Fig. 1. – Préparation d'un lancement de l'expérience de mesure des électrons cosmiques à Aire sur l'Adour en octobre 1963. De gauche à droite L. Scarsi, G. Sironi, B. Parlier.

mois à peine s'était écoulé depuis le vol !

La question qui se posa alors était de savoir si ces électrons étaient le résultat d'interaction des autres rayons cosmiques avec la matière interstellaire, via la désintégration des pions chargés; processus qui devait produire autant d'électrons que de positrons. Nous avons donc entrepris avec le même détecteur la mesure du signe des électrons, ou plutôt la mesure du rapport entre flux de positrons et flux total d'électrons des deux signes. Nous utilisâmes pour cela la modulation Est/Ouest de ces flux, due à la traversée du champ géomagnétique. Nos résultats, obtenus au cours de vols en France (1965) à des énergies supérieures à 4.5 GeV, et en Sicile (1967) à des énergies supérieures à 8.3 GeV, ont montré que les positrons ne représentaient que environ 10% des électrons des deux signes. Il devait donc exister des sources d'électrons "primaires".

Pour les vols effectués en 1967 à partir de la base de Trapani en Sicile, la Marine Italienne en Méditerranée fut très efficace, car la récupération se faisait en mer; et à cette occasion l'étudiant Milanais qui était "à bord" a appris que "un navire de guerre n'est pas une bicyclette"!



Fig. 2. – Récupération de l'expérience de mesure des électrons cosmiques dans les Alpes, le 9 novembre 1963. De gauche à droite G. Sironi, G. Occhialini, R. Faber et Anonyme du CNES.

Notre activité dans ce domaine s'est arrêtée après les vols de 1967. En effet, le prolongement de ce travail sur les électrons de haute énergie aurait nécessité l'emploi d'aimants supraconducteurs et de ballons gros porteurs et ces moyens étaient encore balbutiants.

Mais le dynamisme des équipes et la maîtrise technique acquise au cours de ces premières expériences nous firent envisager d'autres horizons.

Vers la fin des années 1960 l'Astronomie dans le domaine des rayons X était une réalité, et certaines des sources ponctuelles découvertes semblaient avoir un spectre en loi de puissance jusqu'à des énergies de la centaine de keV. D'autre part, suivant le même processus que pour les électrons, le milieu interstellaire pouvait aussi être la source de photons dont l'énergie se situait au delà de quelques dizaines de MeV, mais cette fois via les pions neutres.

Nous nous sommes donc lancés dans la détection des photons gamma de haute énergie, en utilisant maintenant l'effet de création de paire dans les plaques d'une chambre à étincelles.

C'est la même collaboration Milan-Saclay, à laquelle s'était joint Palerme (Istituto di Fisica, Università di Palermo) depuis que Livio Scarsi y avait été nommé Professeur, qui s'est attaquée à la réalisation d'un télescope à photons gamma, basé sur une chambre à étincelles de 850 cm^2 emportée en ballon.

Les pulsars avaient été découverts en 1967 dans le domaine des ondes radio et peu après dans celui des rayons X; le pulsar de la nébuleuse du Crabe PSR 0531 a donc été notre premier objectif. Mais c'est seulement au bout de trois ans, et quatre publications entre 1970 et 1973, que nous sommes arrivés à un résultat positif. Quelques autres groupes s'étaient lancés dans la compétition et tous les résultats étaient à peu près au même niveau.

Il fallait manifestement des moyens plus puissants, et la confirmation de cette première source gamma de nature ponctuelle fut apportée par les satellites SAS-2 (1973) et COS-B (1975). Aujourd'hui PSR 0531 bien que faisant toujours l'objet de recherches approfondies dans un domaine très étendu de longueurs d'onde, est devenu la source d'étalonnage naturelle pour les télescopes gamma.

Cependant les ballons stratosphériques furent encore utilisés entre 1983 et 1990 pour des objectifs plus "pointus", comme l'étude de la temporisation fine des pulsars gamma, avec l'expérience FIGARO. La collaboration s'étendait alors à sept laboratoires français et italiens.

3. – L'avènement de l'Europe spatiale et les expériences en satellite

Entre temps, Beppo avait participé à l'organisation d'un monde scientifique nouveau: celui des expériences embarquées sur les satellites scientifiques européens. Ces démarches débutèrent avec la création du futur Conseil Européen de Recherches Spatiales (European Space Research Organisation, ESRO), qui deviendra en 1974 l'Agence Spatiale Européenne (European Space Agency, ESA). Douze pays européens adhèrent à cette Europe Spatiale naissante matérialisée en 1962 par un livre bleu dans lequel était détaillé un projet de programme spatial. Beppo fut l'un des fondateurs et des animateurs du groupe cosmique (COS-group) de l'ESRO et étendit la coopération des cosmiciens à quatre autres groupes anglais, danois, hollandais et allemand. Jusqu'en 1982 aidée par leurs moyens nationaux respectifs, en plus de l'ESRO, de la NASA, et de l'INTERCOSMOS soviétique, cette association amicale des 6 laboratoires va réaliser une douzaine de grosses expériences spatiales pour découvrir et étudier les rayonnements à haute énergie qui arrivent sur notre planète.

En parallèle avec son rôle dans le développement de l'ESRO, Beppo impulsa la collaboration Milan-Saclay sur la recherche dans le domaine des rayons cosmiques galactiques et solaires à l'aide de satellites. Ceci se matérialisa à bord des deux "Highly Excentric Orbital Satellite" HEOS1 et 2, lancés successivement en 1968 et 1972; avec à bord respectivement les expériences S79 et S209.

Ces deux expériences comportaient un détecteur réalisé en Italie: un compteur Tcherenkov à gaz (azote à 15 bar) rendu directionnel par un jeu de miroirs, un calorimètre en verre au plomb, un détecteur de silicium et un scintillateur; quant à l'électronique rapide elle fut conçue à Saclay et réalisée par Matra. Les étalonnages auprès des

accélérateurs à électrons (Orsay, DESY ⁽¹⁾, Frascati) furent des aventures inoubliables, en particulier par l'amitié née entre les équipes. C'est ainsi que l'époque pendant laquelle devaient avoir lieu à Milan les essais d'intégration du modèle de vol de S79 fut aussi une période de conflits universitaire en Italie et le laboratoire était devenu inaccessible! "Miraculeusement" cependant, le matériel se retrouva à Saclay, ainsi que six ingénieurs et techniciens de Milan qui y œuvrèrent pendant un mois pour terminer à temps la réalisation du modèle de vol.

Ces instruments ont permis d'observer l'accélération par le soleil et la propagation dans l'espace interplanétaire d'électrons de 7 à 300 MeV de 1969 à 1970 avec HEOS1, puis de 1972 à 1974 avec HEOS2. Seul le spectre des électrons cosmiques n'a pu être mesuré. En effet la production locale d'électrons par les interactions des protons cosmiques dans la matière du satellite et de l'instrument lui-même dominait d'un facteur vingt le flux cosmique.

Un modèle théorique rendant compte de l'accélération des électrons dans la chromosphère solaire, de leur stockage dans la basse couronne et de leur diffusion dans l'espace interplanétaire a été construit. Cela a été possible grâce au fait, obtenu en particulier par Beppo, que les scientifiques disposaient de l'ensemble des données sur les particules nucléaires, le plasma et le champ magnétique interplanétaire, mesurées avec l'ensemble des instruments à bord de HEOS1 et 2. La collaboration Milan-Saclay a aussi pu montrer que les électrons solaires pénètrent la magnétosphère terrestre via la reconnexion des lignes des champs géomagnétique et interplanétaire.

À la fin des années 1970, l'impulsion donnée par Beppo durant la phase de définition du projet "Out of Ecliptic" se matérialisa par la proposition d'une expérience, fruit de la collaboration entre Milan, Saclay et l'Université de Kiel, qui fut embarquée à bord de la mission ESA/NASA "ULYSSES" lancée en 1990 et toujours en opération en 2006.

L'instrument était capable de mesurer le spectre des électrons cosmiques entre 3 MeV et 6 GeV ainsi que celui des protons et noyaux d'hélium cosmiques, grâce à un détecteur Tcherenkov à aerogel de silice, développé dans les années 1970 à Saclay pour l'expérience franco-danoise C2 sur HEAO-3.

Deux surprises et une découverte apparurent dès le premier survol du pôle sud du soleil en 1994. Alors que l'on s'attendait à avoir enfin accès aux électrons cosmiques du milieu interstellaire d'énergie inférieure au GeV, on s'aperçut que le gradient de ces électrons en fonction de la latitude solaire entre pôle et équateur était quasi nul, ce qui impliquait une surprenante homogénéisation des particules dans l'héliosphère, grâce à leur forte diffusion perpendiculairement aux lignes de champ magnétique. Par ailleurs on découvrit, sur le trajet d'ULYSSES près de Jupiter, des jets d'électrons joviens, très collimatés jusqu'à environ 1 AU de Jupiter, ce qui impliquait au contraire une très faible diffusion des électrons joviens, perpendiculairement aux lignes du champ magnétique jovien.

Ce projet a mis à l'épreuve l'enthousiasme et la patience des équipes car le lancement

(¹) DESY pour "Deutsches Elektronen -Synchrotron".

par la navette Discovery eut lieu treize ans après le début des études et huit ans après la fin de réalisation des modèles de vol!

Dans le domaine des rayons gamma, à l'instigation de Beppo la collaboration franco-italienne Milan-Saclay s'est rapidement élargie à d'autres laboratoires. Le Max-Planck-Institut für Extraterrestrische Physik (MPE) à Garching, près de Munich, dirigé à l'époque par Reimar Lüster, fut le premier à rejoindre la collaboration en vue de réaliser un détecteur de rayons gamma dans le cadre de l'appel d'offres lancée par l'ESRO au début de l'année 1966 pour deux satellites scientifiques "lourds", dénommés TD1 et TD2, simplement parce que leur futur lanceur devait être la fusée américaine Thor-Delta. Pour des raisons budgétaires, seul TD1 fut finalement mis en orbite en mars 1972, avec à son bord l'expérience d'astronomie gamma S133, fruit de la collaboration Milan-Saclay étendue à Munich. S133 bien que fonctionnant correctement fut cependant "submergée" par le "bruit" gamma généré par les expériences voisines trop proches et trop nombreuses sur le même satellite. Elle n'en parvint pas moins à mesurer l'émission gamma galactique au delà de 100 MeV.

En 1969, toujours sous l'impulsion de Beppo, l'ESRO approuvait la réalisation d'un satellite entièrement dédié à l'astronomie des rayons gamma de haute énergie. Son objectif principal: traquer les photons gamma que produisent les protons du rayonnement cosmique, d'où son nom, COS-B ⁽²⁾. Consacré sans réserve aux rayons cosmiques, COS-B profitait bien sûr du crédit de Beppo, grand nom du mariage entre physique des particules et rayonnement cosmique.

Pas moins de cinq instituts européens s'étaient associés pour fournir les cent kilos d'appareillage scientifique embarqués à bord de COS-B. Cette coopération internationale—la collaboration "Caravane"—comprenait bien sûr l'Institut de physique cosmique de Milan, avec à sa tête Beppo en personne. Mais il y avait aussi l'Université de Leiden, aux Pays-Bas, et son laboratoire d'astrophysique dirigé à l'époque par Henrik van de Hulst, un des pionniers de la radioastronomie: n'avait-il pas prédit aux heures les plus sombres du dernier conflit mondial que les atomes d'hydrogène confinés dans les nuages interstellaires devaient émettre un rayonnement radio à vingt et un centimètres de longueur d'onde? "Caravane" rassemblait aussi le MPE à Garching, l'Université de Southampton et le Service d'Électronique Physique—le futur Service d'Astrophysique—du CEA à Saclay.

C'était une équipe bien disparate, rassemblant des jeunes diplômés issus des quatre coins de l'Europe, chacun s'ingéniant à privilégier le mode de pensée propre à ses racines. Ce cocktail aurait pu être explosif, ce fut au contraire un de ces creusets miraculeux où une bande de jeunes physiciens croyait fermement bâtir l'Europe, tout en échafaudant la première expérience européenne d'astronomie gamma. Et puis quel parrainage! Beppo, van de Hulst, un seigneur de la physique, un pape de la radioastronomie! La suite des opérations fut menée très rondement. On ne trainait pas à l'époque pour conduire à son terme un projet satellite. Il y eut pourtant cette somme de coups durs qui sont le pain

(2) Ce fut en effet le projet "B" qui fut retenu parmi les deux alors en concurrence.

quotidien des entreprises spatiales. C'est ainsi qu'au bout de quelques mois, l'Angleterre refusa de financer sa part ...

Bonne fille, l'Europe spatiale reprit alors à sa charge la contribution anglaise, et se paya même le luxe d'embaucher le physicien anglais qui participait aux travaux de notre petit groupe dans un laboratoire de son centre technique de Noordwijk, en Hollande. À la même époque, la France ruait aussi dans les brancards, fustigeant l'Europe spatiale naissante de n'avoir en chantier que des projets scientifiques comme COS-B. Les autorités françaises voulaient en effet à tout prix que l'Europe s'engage avec plus d'ardeur dans des programmes plus commerciaux, comme les télécommunications ou la météorologie, dont les États-Unis avaient alors le monopole.

Insensible aux coups de tabac qui secouaient les sphères dirigeantes, l'équipe COS-B poursuivait le développement et la mise au point de cet étrange dispositif expérimental, que même les plus enthousiastes de l'équipe n'osaient pas qualifier de télescope. C'est vrai qu'une fois terminé, COS-B ressemblait plus à une grosse boîte de conserve qu'à un observatoire astronomique. Son dispositif de détection avait plutôt l'allure d'une véritable expérience de physique nucléaire en miniature. Le domaine de l'astronomie gamma est si loin du visible que les premiers à s'y aventurer n'étaient pas des astronomes. C'étaient plutôt des physiciens qui cherchaient au ciel le seul laboratoire capable de leur accélérer des particules comme aucune installation terrestre ne pouvait —et ne peut toujours pas— le faire.

Mais qu'importe! COS-B était capable de détecter les photons gamma de haute énergie, et même d'en déterminer grossièrement la direction d'origine. Pour mieux s'en convaincre, l'équipe COS-B l'avait exposé des mois durant à des faisceaux de rayons gamma que fabriquaient les électrons relativistes accélérés au DESY, non loin de l'estuaire de l'Elbe, dans un quartier chic à la périphérie de Hambourg. Avec ses objectifs purement scientifiques, COS-B ne menaçait en rien les intérêts commerciaux des États-Unis dans l'espace. Ce fut donc une Thor-Delta américaine qui le propulsa le 8 août 1975 sur une orbite très excentrique dont l'apogée se situait à environ cent mille kilomètres de la Terre. La semaine suivante, toute l'équipe COS-B se retrouvait à Darmstadt pour mettre l'instrument en marche et en vérifier le bon fonctionnement.

COS-B débuta sa tournée des champs célestes par un premier pointage en direction de la nébuleuse du Crabe. Ce débris d'une supernova ayant explosé quelque part dans la constellation du Taureau tenait alors la vedette au début des années soixante-dix. Les radioastronomes ne venaient-ils pas d'y découvrir un des premiers pulsars? Aussi, dès sa mise en service, en août 1975, COS-B fut sommé de chasser le pulsar, alors que ses pères fondateurs l'avaient conçu avant tout pour traquer les rayons cosmiques de la Voie Lactée. En fait, rien ne le prédisposait à la recherche d'objets "ponctuels", une activité qui sera toujours pour lui un peu contre nature.

COS-B avait été ajusté pour détecter les photons gamma d'énergie supérieure à cinquante mégaélectronvolts émis par les chocs entre le rayonnement cosmique et la matière interstellaire. Le même appareil était donc tout à fait capable de découvrir les astres propres à accélérer les faisceaux de particules relativistes aptes à rayonner de tels photons gamma de haute énergie. Mais à puissance rayonnée égale, un astre émet beaucoup

moins de photons de haute énergie pour la simple raison que chacun d’eux emporte un beaucoup plus gros paquet d’énergie. La détection des étoiles rayonnant ce genre de photons nécessite donc des temps de pose exorbitants, surtout pour un appareil de petite taille comme l’était COS-B, avec sa surface sensible d’à peine cinq cents centimètres carrés.

Tout n’était pas pourtant si négatif, car la myopie de COS-B était compensée par un champ de vision considérable pour un instrument d’astronomie. Grâce à sa technique de détection par chambre à étincelles, son regard englobait d’un seul coup un champ du ciel d’un diamètre angulaire de plusieurs dizaines de degrés. COS-B pouvait ainsi balayer toute la Voie Lactée en moins d’une douzaine de pointés. Il ne s’en priva d’ailleurs pas, et parvint à produire la première carte digne de ce nom de l’émission gamma de la Voie lactée, apportant ainsi la preuve que les protons du rayonnement cosmique sont largement répandus dans tout le disque de la Galaxie.

L’exploitation scientifique de COS-B marqua une nouvelle phase de la collaboration Milan-Saclay, avec la venue à Saclay de plusieurs étudiants milanais. Ces échanges se poursuivirent bien au delà de COS-B, dans le cadre de nouveaux programmes d’astronomie gamma comme SIGMA et INTEGRAL. Fut ainsi perpétué l’esprit de la collaboration Milan-Saclay, bien au delà de la disparition de Beppo en 1993, personnalisé aujourd’hui par l’embauche au CEA de deux anciens étudiants du laboratoire de Milan.

4. – Conclusion

Anecdotiquement, à Saclay en 1990, à l’accélérateur “Saturne”, fut étalonné l’un des détecteurs du Satellite Italien d’Astronomie X “SAX”. Ce satellite désormais nommé “Beppo SAX” a permis pour la première fois la localisation de ces mystérieux “sursauts gamma”. En dehors du fait que Beppo avait été l’un des promoteurs de SAX, c’est maintenant une pratique courante de donner le nom d’un personnage célèbre à un satellite scientifique dès sa mise en orbite. Mais pour beaucoup de jeunes, “Beppo” reste un nom mystérieux. Nous espérons que cette ultime remarque sur le manque de reconnaissance attaché au surnom de Giuseppe Occhialini ne sera bientôt plus de mise. Puisse ce texte y contribuer.

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Beppo Occhialini and the birth of Space Physics in Italy and in Europe: Personal memories

LIVIO SCARSI(*)

Istituto di Astrofisica Spaziale e Fisica Cosmica, Palermo, Italy

INAF and Accademia Nazionale dei Lincei, Roma, Italy

1. – Introduction

The entering of Beppo Occhialini into Space Physics has not been driven by chance but, in a way, it was largely expected following a logical line of continuity of his life in science; that is what I think to have learned in the more than 40 years in which I had the venture to be acquainted with him. I will try to make the point recalling personal memories of what I heard or got to know indirectly for the years from 1927 to 1950 and directly starting from 1950, year in which with him as supervisor, I obtained the Degree in Physics at the University of Genoa. Mine does not want to be an accurate biographical reconstruction, but just a recollection of episodes and facts of life: possibly some dates or places could be not precisely quoted, but with no impact however on the essence or interpretation of the situations referred.

1.1. Background. – The attraction of Beppo for the challenge of discovery coupled to a spontaneous mastership and rigour in scientific research made him naturally tuned and ready to catch the magic of the new and innovation.

It was this way at the beginning of his career in Arcetri in 1927 where as a student of Bruno Rossi he was involved in experiments to explore the nature of cosmic radiation;

(*) Livio Scarsi passed away on 16 March 2006, before his text was completely revised. Only minor adjustments have been made by G. Sironi.

the detection technique employed was that of Geiger-Müller counters assembled in the fast coincidence Rossi circuit. After graduation Beppo was sent to Cambridge, UK to learn about the Wilson chamber technology; there, teaming with P. M. S. Blackett, he realized a triggered Chamber, the breakthrough detector coupling the visual properties of track formation to the discriminating trigger logic of the Rossi circuit. The first exciting result turned out to be the clear picture of the electron-positron pair phenomenon (1932); although the priority for the discovery of the positron is usually accredited to Anderson, Blackett and Occhialini deserve the credit for the first glamorous direct evidence of the pair event signature for the matter-antimatter production.

An intriguing trend emerges in the complex history leading to the identification of the π -meson, next milestone in Beppo life. After a period of restless commuting between Arcetri and Cambridge in the tense years preceding the Second World War, Occhialini in 1937 joined Gleb Wataghin as a Visiting Professor at the University of São Paulo in Brazil, officially to bring modern research themes in Physics. but in reality to escape from the oppressive fascist regime: an additional case to the hemorrhagic loss of Italian physicists in 1938-39 because of the racial laws (Fermi, Rossi, Segré, Pontecorvo and others). With the evolving of world events and Brazil entering war against Italy in 1941, Occhialini saw his Visiting Professor status turned abruptly in that of "enemy alien" and to avoid confinement into a concentration camp he sought refuge hiding in the Itatiaia Mountain. When in September 1943 Italy signed the Armistice with the Allied Powers and he turned again free, he decided to leave Brazil to join Europe and engage in active participation to the war effort against the Nazi-fascism. Taking advantage of the old ties and friendship with Blackett, during the war in charge of a high position in the British Admiralty as responsible of the Office for strategic studies, Beppo came back to the UK in 1944. His request to be immediately employed in a war operation program, was however opposed by the military authority cautious about the involvement in a war-sensitive frontline of a citizen of a country like Italy still divided in 1944 about the choice of the side where to stand in the conflict. They managed to confine him in a peripheral outfit in Bristol; there a research activity rather disconnected from an active war effort was going on under the direction of Cecil Powell, scientist accredited with limited clearance, because suspected of sympathy for Communism and of frequentation of leftist movements. Powell was conducting a program on low-energy nuclear physics using photographic plates to visualize the track of the heavily ionizing products of nuclear interactions. This world turned out to be disappointingly dull for Beppo who was nevertheless attracted by the promising method of detection: he anticipated that the standard photographic plates used in radiography if upgraded to a sensitivity high enough to reveal relativistic particles and with thickness of the sensitive layer increased from few to hundreds of microns could provide a powerful tool for the investigation of the phenomena present in cosmic radiation, still an intriguing topic at the top of his curiosity. When stacked to reach sensitive volumes of the size of liters nuclear-emulsion layers could substitute the triggered cloud chamber as an unrivaled high-definition/high-density optical chamber, continuously sensitive, passive, compact and robust. Along these lines, Occhialini started a vigorous program to advance the photo-emulsion tech-

nology, involving George Wallace Chief Chemist at Ilford Ltd., the industrial Company in UK leader for production of photographic material. Progress was immediately fruitful with the production of the Ilford C2 plates, first step in the plan to reach the threshold for minimum ionization detection, enabling in 1947 the discovery of the π -meson through its π - μ decay [1]. The effort was finally fully rewarded in 1949 by the Ilford G5 emulsion sensitive to the minimum ionisation with ~ 20 grains/100 microns density tracks of relativistic electrons ($E > 0.5$ MeV) and protons ($E > 1$ GeV); the new emulsion was reproducible, stable and with sensitive thickness reaching 1mm. With its superior quality compared to the poor standard of the competing product by the US Eastman Kodak, G5 soon became protagonist in the golden era of discovery of the strange particles population with exposure to cosmic rays in the upper atmosphere with balloon flights (1951-1955). The decline in favour of bubble chambers and electronic devices coincided with the coming in operation of the large particle accelerators.

1'2. Foreground. – As a student of the last year of the course in Physics at the University of Genoa, I first met Beppo in 1949, when he had just come back to Italy after having spent abroad most of his creative life. His last stopover had been at the Université Libre in Bruxelles where he transited from Bristol in 1948 staying for a couple of years to set up a research center oriented to the development of the technique of nuclear emulsions. Also the permanence at the University of Genoa turned out to be a short transient before the settlement, this time definitive, in Milan in 1951 (see the contribution by Riccardo Levi-Setti). During his staying in Genoa Beppo succeeded in forming around Alberto Bonetti a research group in cosmic rays; I was one of the elements enrolled, after graduation. The work of my thesis: “Cosmic Ray electromagnetic component at the Pic du Midi” was essentially addressed at validating the G5 in the measurement of the energy spectrum of gamma and electrons above 10 MeV in the secondary cosmic-ray component [2], based on the multiple-scattering measurement on the minimum-ionization tracks of the free electrons or of the branches of the gamma materialization pair.

In 1952 I followed Alberto Bonetti to Milan to join Occhialini's group. Other testimonials in this book will refer about the Beppo activity in the years from 1950 to 1960 dedicated to the investigation of the elementary particle world with nuclear emulsions exposed in balloon flights: in those years Milan became, with Padua and Bristol, a center of excellence for the research in the field with a series of fundamental contributions culminating with the epochal experiment G-Stack (Giant emulsion stack) [3] which provided the first systematic classification of heavy mesons and hyperons before the intervention of the super accelerators. G-Stack closed the era of cosmic ray used as a particle beam freely produced by nature. The Tau-Stack [4] and the K-Stack [5], which followed, were exposed to artificial beams at Brookhaven and Berkeley.

In the last 1950s Beppo addressed increasingly his attention to CERN inducing to believe that Geneva will become the pole of his activity in the years to come: I remember the frequent meetings and alive exchanges with Charles Peyrou in his visits to Milan. The chapter by Emanuele Quercigh in this book will be certainly a mine of precious information on the subject.

Personally, in 1957 I left Milan and the world of elementary particles to reach MIT with a Fulbright Fellowship; I was joining Bruno Rossi and John Linsley in the experiment planned to investigate the extreme end of the cosmic-ray energy spectrum ($E > 10^{18}$ eV) with the giant Extensive Air Shower Array at Volcano Ranch, near Albuquerque in New Mexico. My switching of interests was not properly the result of a personal choice of mine, but induced by a strong suggestion, difficult to oppose, by Beppo who stated on that occasion: “My young friend: better for you to get out from here and go to hunt for the freedom of creativity before being trapped in a monoculture world where you will end to be just one of too many”. To tell the truth the situation was a bit more complex: an instability had occurred in the international relationship and his saying possibly was referred more to himself rather than to me.

2. – The beginning of the Space Era

2'1. *Boundary conditions in USA.* – I arrived in Cambridge, Mass., at the end of September 1957 just in time to watch at the TV on the 4th of October the USSR opening the Space Era with the launch of Sputnik. For the USA establishment it was quite a shock producing a quasi-hysterical reaction in university campuses and research institutions, beside naturally the impact on the defence and industrial outfits.

At MIT the activity stirred up by the event was particularly noticeable:

- J. R. Killian, President of MIT, was nominated special adviser to the White House for space policy.

- Bruno Rossi became member of the National Space Science Committee.

- I remember to have been enrolled in a “crash course” of technical Russian language, mandatory for all MIT staff members and research fellows.

- Bruno organized a series of conferences and seminars devoted to space-related topics.

- Practically all the Rossi Research Group (fourth and fifth floor of Building 26 in the Campus of Cambridge) was redirected to a new space-oriented program. John Linsley and myself exceptionally remained assigned to the “ready to go” Extensive Air Showers Volcano Ranch Project funded by NSF. We moved to New Mexico in Spring 1958.

In Italy and in Europe the emotional wave produced by Sputnik acted also as a tsunami. At first Beppo apparently seemed to maintain unchanged his attention to the world of CERN; in 1960, on the other hand, he and Connie decided to spend a sabbatical semester at MIT to investigate with Bruno Rossi and his collaborators the conditions for starting in Milan a research program in Space Science.

In September 1960 I travelled to Cambridge from New Mexico to discuss with the Administration of MIT my status and evaluate the implications arising from prolonging indefinitely my residence in the US. At the end of the year in fact my Fulbright Fellowship was expiring and with it my G5 Visa subject to a clause imposing to the bearer the re-entry in the country of origin in force of a bilateral treaty with the US intended to protect Italy from the loss of scientists of potential interest in areas strategically important. To overrule the G5 clause a special procedure had to be implemented, to demonstrate that a

mutual advantage would derive by the bypass of the regulation: a similar case was dealt with the year before for Riccardo Giacconi who happened to be in a situation similar to mine.

While in MIT, I participated to work meetings with Beppo, Connie Dilworth, Bruno Rossi and the Seniors of his group (Herb Bridge, George Clark, Bill Kraushaar, Stan Olbert, Martin Annis and others) to discuss objectives and possible scenarios for a space activity in Milan and in Italy. The group of Rossi was mainly interested in two lines: the interplanetary plasma and the extraterrestrial gamma rays above 50 MeV coming from discrete sources or from the diffuse component from the decay of π^0 -mesons produced by the interaction of cosmic rays with the interstellar matter. The group was planning experiments for the Explorer series of satellites and developing the instrumentation required. Herb Bridge, in charge of plasma physics and assisted by Stan Olbert for the theoretical aspects, was busy to assemble a space-qualified detector based on a modulated-grid Faraday cup: the instrument was successfully flown aboard the Explorer X spacecraft in the spring of 1961 and, for the first time, it provided direct measurements of the density and velocity of the solar wind. The gamma's area was covered by Bill Kraushaar and George Clark who had chosen for the telescope a phoswich system with alternated NaI and CsI slabs, screened by a plastic scintillator anticoincidence; the predicted gamma luminosity at $E > 50$ MeV of the galactic disk was based on calculations provided by Morrison of Cornell University and Hayakawa of Nagoya. Explorer11 and OSO-3 opened the way to the gamma-ray astronomy of today.

2'2. *Space in Italy.* – As a result of the several brain storming sessions in MIT, Occhialini reached the determination to enter into the space game with an important and ambitious program, relevant also at European scale.

To start moving in the new direction, he decided to gradually leave the activity in CERN encouraging those in Milan still interested in accelerator physics to converge into the effort coordinated by the Istituto Nazionale di Fisica Nucleare.

For the newly conceived space group, he chose a “light, flexible version” with a limited number of elements motivated to operate with him and Connie: I was eventually convinced to leave the US and to come back to Italy as one of the founding members of the group. For the initial programs it was convened to begin with a home activity based on balloon and rocket experiments aiming at specific aspects of cosmic-ray phenomenology, while establishing a systematic network of collaborations and exchange of persons between Italy, France and US Institutions with MIT as a privileged partner.

Connie Dilworth and Alberto Bonetti were immediately involved in stages at MIT with the plasma program, together with Alberto Egidi coming from Rome: this initial step was largely responsible for the Italian participation in space plasma experiments within ESRO first and ESA afterwards.

Back in Milan Occhialini entered into the space affairs with his usual impetus making a strong declaration of scientific interest. Responsibility for space in Italy was at the epoch attributed to an “ad hoc” committee (CRS — Commissione Ricerche Spaziali) headed by Luigi Broglio, Professor of Aeronautical Engineering at the University of Rome

and, in the same time, Lieutenant General in active duty of the Italian Air Force, with Edoardo Amaldi from the University of Rome and Giampiero Puppi from the University of Bologna as components. Luigi Broglio, in his double role of University Professor representing the interests of science and of Air Force Officer representing those of the defence was in fact acting as a privileged partner for NASA with which he enjoyed direct links; for the civilian side in this way he bypassed “de facto” the National Council of Research institutionally delegated to such affairs. This situation was only partly justified by the government with the role of Italy in the Cold War strategy of the North Atlantic Treaty Organization by hosting US Nuclear Head Jupiter launch sites deployed in the East coast of the Italian peninsula.

Occhialini first managed to insert Milan as a third pole added to Rome and Bologna to be directly represented in the CRS obtaining access to the funds needed to support the research activity in the new field.

The second move, for which Beppo has been the driving architect, was the action to shift the responsibility for science research in space to a body independent of military interests. The results obtained were quite remarkable, ranging from the identification of CNR as the only institution responsible for the program, its execution and to represent officially Italy in the international context. A National Space Commission was established with members nominated by CNR in agreement with the ministries involved: education/research, industry and treasury; Beppo naturally become an influential member. CNR, with Servizio Attività Spaziali (SAS) (of which I was in charge of the Direction for the first period) provided the central supporting structure. Funding was assured by special laws approved by the Parliament in the context of general programs elaborated by the Space Commission. The operational structure in Italy was originally framed in four groups located in Bologna, Milan, Rome and Turin, at first connected to the local sections of INFN and then (1964) formalized in four independent CNR units coordinated by a reference body, GIFCO, acronym for Gruppo Italiano di Fisica Cosmica; the number increased to five in 1980 with the transformation into an independent institute of the unit of Palermo, up to then operated as a section of LFCCTR of Milan.

The Laboratorio di Fisica Cosmica and Tecnologie Relative (LFCCTR) was the institute located in Milan and directed by Beppo. The LFCCTR program focused on the investigation of primary cosmic-ray electrons and of the extraterrestrial gamma radiation ($E_\gamma > 20$ MeV). It is worth to notice the emphasis on the denomination of Fisica Cosmica and the attribution of the extraterrestrial gamma rays to a component of cosmic rays anticipating the modern connotation of gamma-ray astronomy. A detailed account about GIFCO and the related institutes can be found in contributions by other authors in this book.

A characterizing feature of the new organisation was the separation of roles between GIFCO and the hybrid structure under the control of Broglio. This structure, which assumed the name of San Marco Project, was formally attached to the University of Rome, but enjoyed complete independence for programs and operation; funding were coming through “ad personam” allocations by the government, with personnel and equipment largely provided by the Air Force and other Defence Institutions. San Marco was in

charge of the equatorial facility at Malindi, Kenia for the launch of Scout rockets and the handling of the associated Tracking and Telemetry ground station. San Marco conducted a number of Scout missions (San Marco 1 from the NASA site at Wallops Island in 1964, the following 4 in 1967, 1971 1974 and the last, delayed until 1988, from the Kenya launch site) for the investigation of the upper atmospheric density profile with a drag measuring device named “Broglia’s balance” invented by Broglia himself and developed by his staff. On the basis of bilateral agreements San Marco launched on behalf of NASA the first three of the SAS-Small Astronomical Satellite series: SAS 1 on December 1970 (named UHURU after launch) carrying the first X-Ray Astronomy mission; SAS 2 dedicated to gamma-ray astronomy on November 1972; SAS 3 operated by MIT/GSFC on May 1975, again dedicated to X-ray astronomy; another Scout launch regarded the UK X-ray astronomy program.

No mission in collaboration or within the program of GIFCO was however ever performed: the few attempts made failed essentially for the difficulty in finding a common ground for the approach to the problematic. Broglia was moreover hostile to the involvement of the national industry preferring the philosophy of the “do it yourself”.

Just to quote an anecdote, I recall Beppo defining sarcastically the budget allocated to San Marco in units of painting cost needed to fight rusting on the off-shore sea floating platforms, instead than in Lire or AU (Accounting Unit adopted by ESRO to express the costs normalized by the exchange rates towards the various European currencies).

2’3. *Situation in Europe.* – At the onset of the years ’60s Europe was discussing how to combine efforts in space up to then limited to initiatives at national level, and to converge into a common organization [6] in analogy with CERN, the Centre Européen pour la Recherche Nucléaire, established for building the large accelerator facilities required by the elementary-particle physics. Negotiations between the interested European countries were going on in Paris under the umbrella of COPERS (COMmission Préparatoire Européenne pour la Recherche Spatiale). Occhialini was representing Italy in some of the key Working Committees and I usually accompanied him, with the role of Secretary/Assistant, at meetings in the “Salles de Reunion” of a “Belle époque” style Hotel in Rue La Perouse, near the Etoile in Paris. Beppo was very active and he emerged soon as one of the key-men of ESRO (European Space Research Organization) created by COPERS and charged by the February 1964 Convention of providing Europe with a science program in space able to compare with that of NASA in the US or that of the corresponding structure in the USSR. The glue to join European efforts for a common policy in space was identified in science objectives as far as possible not conditioned by nationally driven industrial or economical interests.

The governing structure of ESRO was conceived with a Council at the top, representing each member country at the level of the Ministry of Research (or equivalent), with the authority of the final approval of the program. This was assembled and proposed by committees charged for analysis and validation of its specific aspects: SPC (Scientific Program), STC (Technical), IPC (Industrial Policy), FC (Financial). The SPC was endorsed with the special status of submitting to the Council the final version for approval:

the Council was not entitled to introduce modifications, but, in case of non approval, they would return the plan to SPC for reformulation. The SPC delegate of each member country was indicated by the respective government on political ground, and he was assisted by advisers for the scientific issues.

The ESRO headquarter, hosting the general direction and the administration and where the council and the other committees deputed to govern ESRO hold their meetings, were located in Paris; the technical center ESTeC in Holland at Noordwijk; the center for operations ESOC in Germany at Darmstadt and the Institute for Space Research (originally oriented to the study of the interplanetary plasma), ESRIn in Italy at Frascati near Rome. The ESRO General Director was the British cosmologist Herman Bondi assuring the prevalence of science over purely nationalistic or industrial/finance oriented interests.

Occhialini was an influential adviser to the Italian SPC delegation. I remember the show going on around the meeting table at key sessions: at points in the agenda for which the official Italian spokesman assumed a position conflicting with that considered by Beppo as objective and immune by external influences, at the call for vote he visibly denounced his dissenting position by moving backward his chair from the border of the table.

ESRO was not involved in rocket supply: a parallel organization ELDO (European Launcher Development Organization) (1964) was instead completely devoted to the design and construction of launchers involving national industries. For satellite launches, waiting for the planned launcher EUROPA, ESRO relied on US Scout's and Thor-Delta's from the US facilities of Cape Canaveral in Florida or Vandenberg in California. Rather incomprehensible for outsiders was the complete absence of interaction between ESRO and San Marco with its launch base for Scouts, on the other hand used by NASA for its small astronomical satellites program. "Strange" if we consider that L. Broglio was at the epoch vice-president of ESRO, but "coherent" considering the conflict between the European collaboration and his personal plans with regards the sharing of national funds devoted to space: It was exactly the contrary to the attitude of Beppo Occhialini.

3. – Occhialini presence in space

From the very beginning, Beppo Occhialini impacted at the highest level on the Space Science strategic choices in Europe and in Italy which he considered strictly interconnected; his strong personality and undiscussed scientific authority have been determinant to define roadmaps and to establish the supporting structures in both cases.

3'1. *Europe and ESRO: The COS Group.* – In the convention agreed for ESRO (converted into ESA (European Space Agency) after the merging with ELDO in 1975) the "raisons d'être" and terms of reference were identified as:

"The purpose of the agency shall be to provide for and to promote, for exclusively peaceful purposes, cooperation among European states in Space research and technology and their Space applications, with a view to their being used for scientific purposes and for operating space application systems".

The objectives listed:

- Exploration of the natural phenomena occurring on Earth and its near environment and in the extraterrestrial space bounded by the Solar System (“near outer space”);
- Observation of the “far (deep) outer space” extending to the Galaxy and to the extragalactic regions. The “outer space”, following a suggestion from UK, was defined as the region extending above the altitude in the Earth atmosphere inaccessible to vehicles supported by air buoyancy in the absence of reaction propulsion: this limit was fixed nominally at 50 km a.s.l.
- For each of the main areas of interest identified (ionosphere, Sun and Solar System, interplanetary plasma and magnetic field, cosmic radiation and trapped radiation, astronomy and astrophysics) a Science Working Group was established composed from up to 10 members chosen between European scientists expert in the field. The membership was not conditioned by specific rules answering to political or national representation, but essentially on the basis of “clara fama”.
- The platforms considered for the observations were: rockets, artificial Earth satellites and deep-space probes not Earth bounded.
- Stratospheric balloons were excluded by ESRO, contrary to the policy of NASA, remaining as an optional choice by some European countries like France and Italy.

The procedure adopted to define the program was following the line:

- a) ESRO, having made public the general framework of its policy, issued a permanent Announcement of Opportunity with no fixed deadline.
- b) Research groups belonging to member states submitted proposals concerning one of the areas on interest detailing the essential characteristics of the mission required.
- c) The ESRO Secretariat proceeded first to a screening of the proposals submitted for technical feasibility and compatibility to the institutional rules; those found acceptable, were edited to produce a standardized document (pink cover tagged R for experiments requiring the launch of a rocket; blue cover tagged S if requiring a satellite or a deep-space probe) and assigned for evaluation to the competent Working Group. The ESRO “ad hoc” Working Groups were six representing the various disciplines and identified by acronyms: ATM (ATMospheric physics and chemistry), ION (IONospheric and auroral phenomena), SUN (solar physics), PLA (moon, planets, comets and the interplanetary medium), STAR (STARs and stellar systems), COS (COSmic rays and trapped radiation).

The outcomes of the working groups revision were then transferred to LPAC (Launching Program Advisory Committee), composed by 4-5 members again chosen with over-national criteria, to assemble a viable program for consideration of the ESRO Executive Boards and final approval by the Council.

The flight activity of ESRO was initially, up to 1967, relying on rocket payloads for the Belier/Centaure (French) and Skylark (UK) then available to the organization, with rather modest performance and essentially dedicated to the investigation of the near Earth environment (upper atmosphere and ionosphere) and X-ray astronomy.

Satellite-based missions begun in May 1968 with the first of 4 scouts launched in polar orbit (ESRO 1 to ESRO 4 in 1972) with payload of ~ 100 kg dedicated to ionosphere and plasma physics studies. In the same period of time two missions based on Thor-Delta launch HEOS-1 and HEOS-2 were performed to explore the magnetosphere properties and to measure the primary cosmic-ray electrons flux with payloads in near polar orbits with ~ 1000 km perigee and ~ 20000 km apogee. The highly eccentric orbit series, choice widely adopted by ESRO (and after by ESA) was dictated not only by observation requirements but primarily by the advantage of orbit coverage using a single ground telemetry/telecommanding station.

Beppo chaired the COS Group from its constitution to 1969 when he became member of the LPAC.

COS turned out to represent the real backbone for Europe in the worldwide context for the exploration of space. Its competence was spread over the region, practically still virgin, of the incoming photon spectrum of extraterrestrial origin (with the exception of the Sun) above the UV including therefore such rich potential gold mines as the X- and gamma-ray astronomies. This on top of the traditional field of the corpuscular cosmic radiation. The ground was ideal for a generation of scientists with a deep experience in particle physics and the technology associated. A comparative overview for the fields covered by the other “ad hoc” groups showed a panorama with lesser opportunities in Europe:

- the “in situ” exploration of planets in the Solar System was dominated by NASA and the USSR space machinery; ESRO, beside lacking launchers adequate to interplanetary missions could not count on an effective network of deep-space tracking/TTX stations;
- for domains related to the Earth ionosphere, the radiation belts and the magnetic-field structure, the exciting days of discovery had leaved space to the less exciting requirement of systematic detailed measurements to model in principle known and predictable phenomena;
- a breakthrough in optical-UV astronomy could come from the realization of a powerful no-diffraction limited telescope (of the class of Hubble put later in orbit by NASA with a participation by ESA); the efforts of ESRO have been limited to the rather modest UV instrument on board of TD1 in 1972.

The COS membership of that time was representing an important gotha for the European pioneers in space science: just to quote some names beside Occhialini, Peters, Lund (Dk), Elliot, Pounds, Quenby, Rees (UK), Labeyrie, Koch (F), Lust, Pinkau (D), Dilworth(I) and many others shifting during the years. Scientific secretary was a brilliant young ESRO staff from Austria, Joannes Ortner master in public and diplomatic relations. I was involved in COS from the very beginning, first as an assistant to Beppo and as a regular member from 1965.

In the 1960s and the first half of 1970s ESRO policy progressively changed from a phase of episodic approaches to that of successful, well coordinated programs assuming the leadership in several fields.

The role of the COS Group under the Chair of Beppo has been determinant in this transformation⁽¹⁾.

The pressure for an increase of the opportunities offered both in number of experiments and in complexity of the missions, posed to the LPAC and subsequently to the Council, together with the adoption of larger payload capacity the problem of the choice between the sharing of the resources between a number of experiments (“streetcar” solution) or the concentration on a single experiment with multiple users (“observatory” solution).

The “streetcar” approach was adopted for TD1 launched in 1972 with two main experiments: a UV telescope and S133 representing a first attempt to open gamma-ray astronomy to Europe. S133, proposed by the COS Group and based on a triggered spark chamber visualizing the electron pairs materialized by photons with $E > 50$ MeV was the result of the joint effort by three Institution: University of Milan (represented by G. Occhialini, G. Boella, L. Scarsi), “Max Planck Instituts für Extraterrestrische Physik in Garching bei München (represented by R. Lust and K. Pinkau), CEA-Saclay (represented by J. Labeyrie, Y. Koechlin, B. Parlier) from which the name MIMOSA, with MO standing for Munchen or Monaco in Italian and French. The interference between the UV telescope and MIMOSA prevented the identification of cosmic gamma sources because of the heavy gamma background produced by the interaction of energetic particle of cosmic rays with the neighbouring mass of the UV instrument.

3'2. ESRO: “Street car” vs. “mono-experiment” scientific satellite concept. – At the end of the decade 1960-70 the period of first orientation and creation of the basic structure for ESRO was to be considered completed, with the incumbency of drawing a definite design for the future activity. The complexity of the experiments proposed on one side and the increasingly growing list of requests on the other were definitely out-phasing the capacity of a Scout class launcher. The Europa II under development by ELDO offering performances superior to those of the Thor Delta was expected to become operational soon despite the difficulties encountered in its development and it was included as a reference launcher for the science program. The situation was imposing a definite and urgent solution for the alternative “street car” vs. “mono-experiment” scientific satellite concept.

In first line the attention was focused on the reactions and positions assumed by the COS group and naturally Beppo Occhialini was a central figure in the debate.

The TD1 result, marred by interference between the various payload components, had a negative impact on the “multi-experiment” approach; the “mono-experiment” ap-

⁽¹⁾ I remember one of the very first meetings taking place at late evening in the hall of a hotel in Rome: on the agenda the grading of rocket and satellite proposals. Beppo with skill and authority succeeded in building an agreement on the priority list, but his masterpiece was the solution of the crisis provoked by the frantic search of a pair of black shoes for one important group member wearing a yellow one strictly unacceptable by the protocol in the formal meeting of the Vatican Academy of Science scheduled the next morning and having in the program a visit of the Pope.

proach encountered on the other hand the political difficulty of the forced convergence of European groups distributed in widely different scientific and economical contexts.

In TD1 the S133-MIMOSA Collaboration involving Italy, France and Germany showed a first positive step for gamma-ray astronomy. A complement could be represented by the proposal S-111, a more ambitious project submitted previously to S133 and derived by the Milan-Saclay groups experimenting in gamma astronomy with a spark chamber telescope developed in balloon flights [7-9]. A definition study involving an enlarged European collaboration was undergoing in view of a mission proposal to the attention of the COS Group.

At the same time X-ray astronomy (Ken Pounds from Leicester in front line) was strongly lobbying for support and opportunity, putting forward the results obtained with rocket flights both in UK and in USA and the attention received by NASA; the British groups were elaborating a satellite mission devoted to X-rays to be inserted in the ESRO Program.

The COS Group, under pressure by the contrasting X- and gamma requirements, was facing a delicate choice in setting up a satisfactory solution. Beppo was engaged at his best in a frenetic activity of knitting and smoothing. In this period Beppo left the chair of the COS Group to become member of the Launching Program Advisory Committee (LPAC) and his role at the chair of the COS Group was taken by Connie Dilworth.

While the panorama appeared reasonably well defined for the main areas outside that concerning COS with a satisfactory equilibrium reached about the allocation of resources and definition of realistic programs, the issue on the table of COS, and from it the issues investing LPAC, was critical for the impact on the Science Community and fundamental about the future of ESRO itself. As a first attempt a COS mission was considered by coupling a gamma-ray telescope as a major component to an X-ray telescope complementing the payload. The proposed design however resulted technically unacceptable because the level of resources required was largely over that available.

After debating at length, finally the concept of single experiment [10-12] emerged with a revised version of COS-B, an experiment fully dedicated to gamma-ray astronomy for $E_\gamma > 50$ MeV, approved by Council in 1969. The launcher designated was the Europa, but to avoid too strict conditioning by the risks attached to the troubled life of ELDO the mission design had the provision of a back-up solution with a Thor Delta launch from the Vandenberg Base in California. COS-B is noticed as a masterpiece of diplomacy succeeding in assembly together around a project five major institutes scattered through Europe. The consortium nicknamed by Beppo Caravane, evoking a crossing in the desert, comprehended the Physics Institute of the University of Milan, the CEN/CEA at Saclay, the MPI für Extraterrestrische Physik at Garching, the University of Leiden, the ES Lab of ESRO (replacing the Physics Department of the University of Southampton after the withdrawal of UK from the project) and a final addition of the Physics Institute of the University of Palermo. The technical management was under responsibility of ESTeC while the scientific management was assumed by a Steering Committee composed by the heads of the institutes involved: G. Occhialini for Milan, J. Labeyrie for Saclay, R. Lust for MPE, H. Van der Hulst for Leiden, E. Trendelenburg for ES Lab with a chairman

nominated with yearly rotation between the members.

COS-B was due for launch before 1972 anticipating SAS 2 from NASA, but it was substantially delayed because of difficult navigation in the ESRO waters, the cancellation of ELDO and the negotiations ongoing for the redefinition of an European Space Organization.

The launch date, with a Thor Delta, was on 9 August 1975, the first mission of the newly born European Space Agency (ESA): it has been a tremendous success giving to Europe the leadership in gamma astronomy “... *avec sa moisson de résultats exceptionnels obtenus en 7 ans de fonctionnement 1975-1982.*” (quote from B. Agrinier).

With COS-B, the line of “mono-experiment” satellites dedicated to “important and breakthrough missions” as advocated by Beppo and the COS Group was definitely adopted by ESRO, leaving the smaller size projects to individual nations or multilateral agreement.

The next mission sponsored by the Group to complement COS-B and dedicated to X-ray astronomy, followed as a first priority, concretized with the launch of EXOSAT in 1983.

In 1972 ESRO reorganized the Science Advisory System by concentrating the competence to two main committees: the Astrophysics Working Group (AWG) and the Solar System Working Group (SSWG) in charge of the Sun and the Planetary System; a new Fundamental Physics Advisory Group was added under the chair of Herman Bondi. I remained on the loop few more years as a AWG member in 1972-1974 and chairman in 1975. With the constitution of ESA the epoch of the “Mandarins”, as Beppo was defining the protagonists of the pioneering era in Space Science, who designated their own successors in the composition of the ruling committees, was terminated. During my chairmanship I negotiated with the ESA directorate for science the new rules for the membership of the AWG reaching the compromise of membership for three years with renewal by ESA for half of the members the other half being designated by the standing group. I must say that certainly there was a gain in organizational planning, but at the expenses of the irreversible transition from creative oligarchy to somewhat guided democracy.

3'3. *The space in Italy: the birth.* – The formal birth of a national interest in Italy for a Space program can be assumed to coincide with the set-up in Rome of a commission for space research, CRS, in September 1959, under the initiative of E. Amaldi representing the University of Rome and of L. Broglio, Dean of the Faculty of Aero-Space Engineering in Rome, but, possibly more relevant, at the epoch Lieutenant Colonel, in active duty, of the Italian Air Force. The CRS received for starting activities, the equivalent of some 400000 euros shared between the CNR and the Air Force. While the “Basic Science program” was just starting to identify its guidelines, the evident interest of the Air Force in High Atmosphere Physics, supersonic and ballistic flight, connected to other defence areas justified a relevant flow of funds, personnel and facilities by the government to the segment referring to Broglio in the two structures of the CRA, by him directly controlled at the Airport of Roma Urbe, and to that of the Armed Forces Missile Range

of Salto di Quirra in Sardinia of which he was in charge for the section of Research and Development. It was therefore natural that L. Broglio was nominated President of the CRS; Amaldi's role was to identify the strictly scientific body of the Commission which could represent the main national groups potentially interested in space for astrophysics, cosmic rays, ionosphere, physiology, propulsion and which was composed by: M. Boella (Rome), N. Carrara (Florence), R. Margaria (Milan), G. Puppi (Bologna) G. Righini (Arcetri-Florence).

For reasons, for me difficult to understand also because in those years I was abroad, Beppo Occhialini was left outside; officially because he was heavily involved with the "ground physics" of CERN, but more realistically because too far from interested frequentations of the halls of political power in Rome and with a personality allergic to the games of diplomatic marketing. That was true for both the national and the international scene.

From 1961, year in which I started to follow by near Beppo in the space venture my personal memory allows to draw a picture of "Italy in space" outside that available in official sites and reporting "oleographic reconstructions". I feel free to express appreciations and judgments which could appear rather harsh or severe, they are real facts at the best of my knowledge, free from contamination by subjective interests or resentments.

The presence of Edoardo Amaldi has been essential in the very first instances in recognition of his authority in the Italian and international scene. I quote from [6]: "The original idea of creating a European organisation for space research is owed particularly to the initiatives undertaken in Europe by Edoardo Amaldi and Pierre Auger in the post Sputnik period. The idea took shape during 1958 and 1959, through the debate and correspondence that Amaldi maintained with Italian and foreign colleagues. The project for an organisation for space research with no military ties of any sort, sprang from this debate". However: "The setting up of the Commissione Ricerche Spaziali—CRS in September 1959 marked a change in Italy's attitude [13]... The role played by Amaldi, who had been the first to conceive and support the project, was reduced while Broglio took on Italy's representation as president of the CRS."

With the coming into being of ESRO with the signature of the convention on June 1962 Amaldi's presence gradually faded out.

I think the influence of Broglio on "Space Science" in Italy has been not only modest and provincial, but at the end dangerously negative; he was self-enveloped in a kind of solitary idea of making Italy a "space protagonist" competing in the international scene with a national program conceived with no sense of vision. Unfortunately he was for a long time strongly supported by "Earmarked special laws" and funding "ad personam" by the Cabinet of the Prime Minister and the Ministers of Defence and of Foreign Affairs, enjoying the privileged status of special advisor to the Defence.

He practically was following selfishly his own plans with little or no consultations with the world of science, even the colleagues in the CRS. At meetings his interventions were systematically and exclusively coinciding with progress reports on the San Marco activity accompanied occasionally by movies of Scout launches. The positions taken by Broglio on basic issues turned out to be on the looser side or at the best much different

from his declared intentions.

– During the definition of the Scientific Program of ESRO in 1961-62 he was strongly opposing the idea of carrying at European level scientific experimentation with sounding rockets or small satellites, leaving this segment exclusively to a national level while assigning to ESRO only missions with large dimensions. This is explainable by the direct negotiations going on at the epoch between the CRA directed by Broglio and the Air Force/Navy firing range at Salto di Quirra in Sardinia for the sounding rockets and with CRA and NASA for an equatorial Scout launch platform off shore in Kenya (originally foreseen in Somalia) managed by a combined CRA-Air Force collaboration. The Range at Salto di Quirra, started with three Nike launch campaigns (1961-63) for a CRA-NASA Aeronomy investigations; it resulted of marginal interest not only for ESRO as institution which shifted the activity to the north site ESRANGE at Kiruna in Sweden, but also for Italian scientists. ESRO activity, based essentially on Skylark and Belier-Centaure spanned from 1964 to 1972.

– Similar venture for the Equatorial facility in Kenia: it regarded practically CRA and NASA internal agreements and programs based on the San Marco CRA program and the launch of the SAS (Small Astronomical Satellite) series for US Scientific Groups and one in UK. ESRO signed a special agreement for installing a tracking station operated autonomously by ESOC/ESTeC for support of Ariane launches from Kouru. ASI installed the TTX station for Beppo-SAX again operated by Telespazio. The final end was for CRA to cash a kind of fee.

All tentatives of Italian scientific missions non directly related to CRA failed for difficulties in finding common interest or agreement.

– The action imposed on ESRO by Broglio, supported in this case by Amaldi, of placing in Italy (in Frascati, near Rome and the CRA) the European Space Laboratory for Advanced Research (ESLAR), later called European Space Research INstitute (ESRIN), was accepted with some resistance by the other European partners in the convention signed in 1962. In reality Italy never provided suggestions or guiding lines for the institution so strongly advocated. ESRIN, at the origin directed towards Plasma studies, changed orientation few times, always with lukewarm interest by Italian Researchers and Research Institutions; today the main interest is toward Earth Observation and Environmental studies.

The position of Broglio as vice-chairman of ESA from 1978 to 1981 turned out to be flat and addressed to what is usually identified normal administration.

Nevertheless when CNR took charge of the management of the space research activity, despite the opposition of most of the members, Beppo was contrary to the exclusion of Broglio from the new committee substituting the ancient CRS. To explain the determination of Occhialini to maintain Broglio inside despite his “rawing against” and his the complete absence of coordination or common planning with CNR. I remember the private statement by Beppo: “Better to have him sitting around the common table and watch him trying to understand his moves instead of leaving him to act as a free lancer undisturbed”.

About the role of the government in general and of the central authority deputized

to deal with the economy, science and technology, military and diplomatic aspects of the involvement in “space”, I think that there has been a predominant sense of “the obvious” and desire to follow common trails instead of looking for innovation and opportunities offered by the new frontier. The Space Office set up by the Ministry of Research was rarely at the level required by the importance of the arguments on the table. The relevant choices were made following a kind of general governmental instruction: “look around and follow the wind; the real think is to show a visible participation in the national and, still more important, in the international scene”. The composition of official delegations at international level (COPERS and ESRO for example) were often questionable choices of political lobbies in Rome: I remember the frustrating behavior of retired Air Force Generals whose competence could be traced to the fact that air navigation has been considered to be the nearest domain that you can image to approximate “Space” and its implications. I had personal experience of meetings in which I was charged of assisting Italian delegates for technical interpretation but I had to provide language translation when the discussion was going on in English and the instantaneous translation service offered by ESRO resulted cumbersome to follow on the spot argumentations.

Remarkable exceptions beside the very effective presence of Occhialini in LPAC in 1969-1971 in defining guide lines, have been:

- The action of Ing. Mancinelli and his collaborators of the Ministry of Industry and of Dr. Fossa Margutti from the Ministry of Research in shaping the text of the law approved in the middle '60s which set the first organic plan for Italy participation in space.
- As ESRO Council chairman in 1971-72 Prof. Giampiero Puppi did a remarkable job by successfully negotiating the first package deal for Europe in space.

3'4. *The space in Italy: Milan activity.* – Beppo, following his typical pattern in these occasions, started with brain storming sessions dominated by his dialectics and his capacity of extracting the best and the worst from each of us. The first objective was the identification of a suitable line of attack motivated by scientific interest and on the same time affordable with our baggage and potential of experience, knowledge and network of collaborations. The choice landed on two experiments, having in common the path from an initial phase carried out with stratospheric balloon flights involving the development and testing of new technology together with a first approach to a measurement in space.

a)– Earth Albedo Neutron flux produced in the atmosphere by the interaction of the incoming cosmic radiation with nuclei of the air [14,15].

Neutron decay was suspected to be responsible for the population of the charged particles trapped in the Van Allen Radiation Belts. The observations were carried out with ^{10}B -enriched scintillation detectors flown first with meteorological balloons from the Milan Linate Airport and successively with stratospheric floating balloons in Sicily and in US to obtain the vertical profile of the neutron flux as a function of atmospheric depth up to 5 mb residual pressure and its latitude dependence (1962-64). A direct measurement of the albedo flux was obtained with the successful launch from the Sardinia Salto di Quirra Range of the rocket ESRO B1, in July 1966, a “première” for an Italian Institute

in the field of scientific research in space in the European context. The experiment also pioneered the technique of “in situ” exploration of the region above 40 km of altitude (< 0.1 mb residual atmospheric pressure) inaccessible to balloon flights; in ESRO B1, the rocket head at maximum altitude (in our case 65 km) ejected the external protection shield releasing the payload for a parachute-assisted descent allowing a slow speed crossing of the interval 0.1–5 mb and a normalization to balloon flight results for deeper penetration in the atmospheric layers.

b) Primary cosmic-ray electrons [16,17] and gamma rays [7-9].

While the Earth albedo neutron flux measurement was intended mainly as a preliminary acquaintance with operation in space, the investigation on the minor components of cosmic rays like the primary electrons and gamma rays formed the main theme of research chosen for the Space Group in Milan. Beppo has always been attracted by the flavour of adventure and fascinated by the novelty of discovery instead of by the satisfaction of pushing more decimal values to an already existing rough value.

Both research themes required a detector with a high discrimination power and capable of stable operation in the presence of heavy background; the requirements had strong similarity with those recalling the performance of the triggered cloud chamber of some 30 years before in the hunt to the electron pairs generated by the materialization of photons. The spark chamber, defined as the cloud chamber of the poor people, at the beginning of the decade 1960 appeared as the best to fulfil the expectations.

A French group in Saclay was at the epoch developing a spark chamber suitable for the adventure; the French-Italian collaboration established in 1961-62 played a fundamental role in the evolution in Space research also at the European level.

Beppo used to visit the Laboratoire du Pic du Midi (~ 2600 m.a.s.l.) in the French Pyrénées chosen as site for exposing the nuclear emulsions to cosmic radiation. There he established strong friendship ties with the French physicist Jacques Labeyrie, a student of Frédéric Joliot Curie, sharing with him the passion for speology and the exploration of underground Caves (memorable that of Pierre Saint Martin near the Pic du Midi, also for the tragic accident occurred during an expedition involving Occhialini, Labeyrie and Max Cosyns from Bruxelles).

This activity in Milan was continued as Gruppo Spazio. A chronicle of what happened, in particular the story of the Milano-Munich-Saclay (MIMOSA) Collaboration will not be repeated here. It is given for instance by Agrinier *et al.* in this volume. I will simply add that for the new collaboration derived from MIMOSA and extended to all of Europe, Beppo suggested the name of Caravane, reminiscent of a concept of individuality of components amalgamated by the drive to reach a common objective.

4. – End of an epoch

For COS-B Beppo participation has been determinant in all phases of scientific, technical and political construction, in the easy moments and in dark ones. I recall some exemplificative episodes, at progress meetings of the Steering Committee to monitor the status of the program.

A first at ESTeC in defining the deadline for freezing the design of the flight model: Beppo considered imperative to wait for the results of the background estimate resulting from the analysis of the balloon flight, against the opinion of the ESRO Project Manager who considered that the delay introduced would be unacceptable for the timeline dictated by technical and programmatic conditions. The contraposition introduced dangerous oscillations at various levels in the organization but at the end the line sustained by Beppo of prevalence of scientific points over those related to the financial political aspects was accepted.

A second one in Munich at the MBB main contractor premises. A dispute between Beppo and Ernest Trendelemburg about the lifetime of the spark chamber (estimated by the Director of ESLAB to not reach the year of operation) ended with the bet: “Bavarian Beer for everybody”. The pessimistic Trendelemburg paid (happily!) the beer.

Still in Munich, when in a meeting where the load of difficulties cumulated appeared to sink the boat, he invited to proceed without hesitation by stating: “Don’t panic: to save our faces we can still count on a blow up of the Thor Delta on the Launch Pad”.

The impact of COS-B on Europe has been fundamental in demonstrating the feasibility and validity of enterprises carried out at continental scale by fusing together resources and capabilities distributed in countries with different traditions, culture, government, laws and industrial structure: the extremely successful operation of the mission in the 8 years of active life (the satellite was finally turned off because of exhaustion of the filling gas mixture for the spark chamber) was an important fact for demonstrating the potential of a Europe united in frontier field.

The launch campaign for COS-B started in spring 1975 at the Vandenberg Western Range in Lompoc, California; Giuliano Boella and myself shared the responsibility for Milano/Palermo for the assistance and final “blessing” to the payload at the site: Giuliano has been in charge of the first period while I took care of the second half lasting most of July until a few days after launch, which occurred on 9 August 1975. While there, few days before the lift off I was informed by Giuliano that Beppo had suddenly, for “personal reasons” left the direction of LFCTR and cut any connection with the active involvement with COS-B. Giuliano was replacing him at the direction of LFCTR and in the COS-B Steering Committee position. I took his place in the “diplomatic roles”, concluding my COS-B involvement as a Steering Committee Chairman in 1983 at mission end.

The length of time of Occhialini involvement in space, beginning in 1960 can be considered formally closed in 1975: 15 years of achievements and profound imprinting on Europe and Italy.

The “exit” can be attributed only partially to external factors but it is almost certainly related to parts of his personality to be tracked down to the more profound regions. I never had clear access to the real “pages” involved. It is difficult in fact to inspect the imagery of a man like Beppo who in his life often approached the heights of excellence but systematically was left outside the choreography of the “Hall of Fame”. In the last period, all the publications concerning the major steps achieved collectively do not include his name following his desire to be considered more like a dictate.

My personal interpretation (and, as far as I can guess, Giuliano Boella shares the same feeling) is that the roots are to be tracked down to the complex situation built up around the activity of the COS Group and then of the LPAC. Although Beppo and Connie always showed a remarkable coherence and synchronism on line of action in their role respectively of chairperson of the COS Group and of component of the LPAC, their strong personalities inevitably must have introduced some contrast in their personal relations. In Milan, while Beppo was indisputably the leader, Connie represented the solidity of the group: “one of us” instead of “one above us”.

With the completion of the battles to reach positive reality in the turmoil of primordial ESRO coinciding symbolically with enclosing COS-B at the top of the Thor Delta on the launch pad showing for the first time the Logo ESA, Beppo decided that the period of stress and unconditioned dedication was over and now the time was arrived for a life devoted to the world inside.

We always remained very close but he constantly refused to enter into any official temple of science or even to discuss about. He shared his time between Paris, Provence and Tuscany at Cannarecchi near Siena in the country house bought with Connie. I tried repeatedly to invite him to Palermo and involve him in the life of the group I was in care of, but with no success for many years.

In 1980 the group in Palermo was formally recognized by CNR as an independent institute (IFCAI) in the GIFCO network: I was nominated Director assisted by a Scientific Board selected between scientists of international standard. I asked Connie to investigate with Beppo on his reaction to be proposed to CNR for Chairman of the Board. Few months later, at the IAU Congress 1981 in Bologna, Connie confirmed that he would accept and Beppo served in the Scientific Council of IFCAI from 1981 until his departure in 1993. He never missed a session, always informed, generous in suggestions and sharp in criticisms: he was appreciated by the personnel, proud and gratified for his involvement in the management of the institute. In his last years, to avoid to Beppo the burden of the travel to Palermo, we managed to have the Council sessions in Pisa hosted by local CNR Institutes and once even in the Airport Ceremonial premises.

I talked to him by phone last time while he was in the hospital in Paris in December 1993. It has been hard to accept the loss of a man who gave to others so much without book-keeping or interested calculation on the possible payback. Always deep in irony and caustic, Beppo was often expressing his thinking by parabolas appearing incomprehensible to many newcomers; sometime it was better to skip the decoding to avoid embarrassment or even worst.

It will take time to adequately replace personages like Beppo: the epoch of Lions seems to be over.

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An Italian school of high-energy astrophysics: A personal view from the sixties to Beppo-SAX

GIUSEPPE CESARE PEROLA

Dipartimento di Fisica “E. Amaldi”, Università Roma Tre, Italy

1. – Introduction

Years after their discovery (V. F. Hess in 1911), thanks to the penetrating and energetic particles they produce in the atmosphere, cosmic rays attracted the attention of physicists as a natural laboratory to investigate the existence of new particles, which could not be produced with the artificial accelerators of the time (mainly in the years 1930-1950). Beppo Occhialini was one of them, and his ingenuity in exploiting this potential is described elsewhere in this book.

Being cosmic rays (CR) not only the most energetic particles produced in the Universe, but also the only ones that reach Earth from outside the Solar System, it was quite natural for some physicists to feel attracted by the problems posed with respect to their span in energy, thus the acceleration processes, and their chemical composition. Problems which, to be put correctly in a theoretical context connecting acceleration and putative astronomical sites, required a new approach in experimental physics. Beppo and his wife Connie Dilworth became two of them, whatever their original motivations. In the early sixties it was clear that CR are mostly made of hadrons, that their energy spectrum can be described with a power law, that at energies below the GeV their flux is modulated by the solar activity, and that the most extensive air shower (thanks to the work by J. Linsley, B. Rossi and L. Scarsi [1,2]) could only be produced by primaries with energy up to at least 10^{19} eV. And it was also clear that detailed measurements on composition could only be obtained by lifting experiments on balloons to altitudes such that the primaries could be directly observed. The main, and potentially the more promising goal of the Cosmic

Ray Group they founded in Milan University, Institute of Physics, was very challenging: to measure directly the absolute flux of the leptonic stable, and charged, component of the CR, that is the electrons. The instrument used was a spark chamber, with a camera onboard to register the track images. The challenge follows from the simple fact that energetic electrons are produced in abundance by the hadronic component interacting with the atmosphere, starting immediately in the latter top layers. Even more challenging was the attempt, made after obtaining a solid result on the electrons (irrespective of their charge), to separate electrons from positrons: which meant the first time a terrestrial physicist would go for antiparticles in the cosmic-ray primaries. This story is narrated in detail elsewhere in this book, the reason it is recalled here is that, for some of the beginners, either involved in the experiments or circling closely around them, in a quest for their own way to original research, this activity represented a unique stimulus to pierce through a new open window on the cosmos. It is on the other hand gratifying to see that, after so many years, under the name of “astroparticle physics”, the track is still used with determination, and of course also with novel types of motivations and resources.

In retrospect those days appear to have impressed a turn in the stand of Italian astronomy and astrophysics, whose effects, both internally and on the international scene, were to become wholly evident years later. It may look as a chance temporal coincidence, but this is probably not the case, that at the same epoch, in five or so years, late fifties/early sixties, in Bologna University, another physicist, Marcello Ceccarelli, initiated the successful tradition of radio-astronomy in Italy, first with a prototype telescope to measure the diffuse radiation from the Galaxy, then with the Medicina Cross to investigate, with a cosmological spirit, the extragalactic radio sources. The connection between the CR electrons with the non-thermal galactic radio-emission is immediate; that between radio-galaxies and relativistic electrons elsewhere in the Universe is only one little conceptual step further.

Beppe liked new challenges, and was contagious in that. When Space beyond the reach of balloons became accessible, he went with his collaborators into another venture, gamma-ray astronomy. That happened when the X-ray window was successfully opened, to a large extent thanks to one of his most renowned students, Riccardo Giacconi, and to the most worshipped of his senior fellow physicists, Bruno Rossi. The inspiration came from the ability of the spark chamber, used to image the electron tracks, to detect the pair produced by an energetic gamma-ray. What was to become one of the most successful missions of the late ESRO, then ESA, namely COS-B, was conceived around 1968. The contributions of Livio Scarsi and Giuliano Boella (along with Bernard Agrinier, first class collaborator since the CR experimental activity, carried out in cooperation with the Serv. Electr. Physique group in Saclay, managed by J. Labeyrie) are considered invaluable for the success of this international enterprise.

Beppe's heritage in high-energy astrophysics might have had a narrower spread angle, were Connie not there to take on her shoulders also a different and most difficult task. Namely to start up, and take daily care, of a small group (starting with one and reaching a total of five members well within the first half of the seventies) that was to be initiated (by a non-professional!) into the realm of astrophysics. It is fair to say that she tactfully

led most of them to go in the direction of their preference, but it would be totally unfair not to recognize that her firm assignment in this educational mission was decisive for their scientific careers.

The author of this Chapter was the first one collected in Connie's group, and his experience, in 40 years now, is mostly on the observational-theoretical side. Thus, the following paragraphs will inevitably reflect some of his personal biases, not to mention his sheer ignorance of some parts of the "true and complete" story: he apologizes to everybody for that. They are devoted to a bird's eye recollection of names and some of their most significant contributions, put in international context, that sprung from this rather peculiar "laboratory". Where single personalities were respected as they were made by nature, in fact one would better say, stimulated to be and grow solidly so: but always as members of a "large" family, despite all sorts of temporal and spatial oscillations.

2. – A start with cosmic-ray electrons, CRE

In experiments on any sort of diffuse radiation, to master the background is always a key issue. In the case of the primary, relativistic CRE, if measured at very high balloon altitudes, the most tricky form of background is made of electrons and positrons of similar energy produced in abundance by the primary CR hadrons. There is no secure way to discriminate against the latter, because as the primaries are reduced by energy losses in the atmosphere, they are replaced by the secondaries. Thus in 1965 Livio Scarsi engaged the one that had just completed his thesis on the origin of the leptonic component of CR in the Galaxy (under the supervision of Connie) into the time consuming exercise of using all the information available at the time from accelerators on the production of mesons. The paper [3], originally conceived for a conference, represented the occasion for the collaborator to make the best use of the culture on CR collected during his thesis. From the abundance of the light rare-earth elements, the amount of matter traversed by the parent heavier CR, and presumably by the most abundant protons, during their lifetime in the Galaxy, can be measured. With no information at the time on the isotopic composition of those elements, no independent constraint was available on the lifetime, hence the matter traversed is degenerate in the lifetime and in the average density of the medium. However electrons are very sensitive to synchrotron losses, as well as to the so-called inverse Compton losses on the cosmic microwave background (discovered in 1965), the latter in particular being comparable to the former and independent of where the particles have been wandering. Therefore, on the one hand, the traversed matter provides a measure of the electrons and positrons of secondary origin produced in interstellar space, on the other, the shape of their spectrum, the positrons in particular thanks to their undisputable secondary origin, can provide an estimate of the lifetime, in the end on the effective residence volume in our Galaxy. From where they and the other CR would eventually leak and be lost [4, 5]. It is fair to say that these papers, published on a journal highly regarded among physicists, but not particularly familiar to astronomers, collected very few references in later publications on the most respected journals of the astrophysical community. Except for those published in the USSR, thanks

to the abstract in Russian that used to accompany the papers in "Il Nuovo Cimento". Similar sort occurred to a paper [6] where the point was made that the measurement of the gamma rays of interstellar origin, which stem from the same processes leading to the secondary electrons, would provide the direct estimate of the production rate of the latter, thus allowing one to bypass the argument associated with the amount of traversed matter. The same point was later reissued with more strength [7].

No matter how original these contributions might have been (even if possibly relevant, they had in practice very little consequence in the community, as explained above), they are recalled here with some emphasis, because for the writer of this contribution they represented a natural start of a rather long journey in radio-astronomy, with studies on radio-galaxies and their physical environment, especially within clusters of galaxies. This journey could hardly have begun, and developed in time with some success, without the experience made at the Leiden Observatory (1968-1970) thanks to an ESRO fellowship, and later in a long-term collaboration with Dutch scientists, and with Italian colleagues operating at the Institute of Radio Astronomy (IRA) in Bologna. All of this will not be covered here, except for two papers [8,9] which dealt with the new idea that double-sided radio galaxies in clusters of galaxies, when their wings are bent into an almost parallel configuration, resembling a tail, morphologically reveal their dynamical interaction with a gaseous intracluster medium, likely the same which could be conceived as responsible for the extended X-ray emission discovered by the UHURU satellite. On the latter issue, the writer cannot resist to mention another paper [10], which was the outcome of a two months (1971) visit to the Institute of Theoretical Astronomy in Cambridge, and the deep friendship established with the late M. Reinhardt while sharing the same office and sense of solitude. Michael was after the strength of a hypothetical intergalactic magnetic field, and his fellow happened to be aware of the existence of a radio-halo in the Coma Cluster, as well as of the detection by UHURU of an extended X-ray emission from the same cluster. Inverse Compton on the cosmic microwave background could, at the time, still be considered an alternative explanation for the X-rays, and for the first time the point was made that one could immediately derive the intracluster magnetic field strength, possibly a compressed intergalactic field. The X-rays turned out later for sure (1976-1977, detection of the highly ionized iron line with the satellites Ariel 5 and OSO 8) to be of thermal origin, but the point remains valid, the problem is to be able to observe the Inverse Compton, inevitable contribution, on top of the thermal one. A hot topic in current days.

It is time to recall the beginnings of another story, with its roots in the CRE measurements. Giorgio Sironi, after being directly involved in the data analysis of the experiments, was attracted by the possibility to combine their results with the existing measurements of the non-thermal radio emission from our Galaxy, to obtain an estimate of the strength of the interstellar magnetic field [11]. The idea was not new, but the data on the electrons were the most reliable at the time. The experimental situation on the radio emission was not that good, he realized, and therefore decided to embark himself in a project that led him eventually to the Cavendish in Cambridge (1970-1971) and to carry out his own observations [12]. Characteristically, he liked to play building at least part of the instrumentation, an attitude which later became his almost exclusive

way to do research. When it was suspected that underneath the non-thermal emission from the Galaxy might lie subtle signatures of the cosmological story in the spectrum of the microwave background, he started a long term project, made of steps of increasing sophistication and with international collaborations. He never felt, at least seemingly, frustrated by the tantalizing goal, and achieved most of his scientific reputation as an “experimental cosmologist”. Quite a jump, and a sharp diversification with respect to his beginnings; moreover a very good example of how the independence in personality was patronized in Beppo’s group, despite the demand of scientific manpower, in the early seventies, for the COS-B experiment-mission.

Perhaps the last offspring of the attention to CRE in Beppo’s group is Giuseppe (Peppo) Gavazzi. After graduating in 1973 with Giorgio, he found an interest in the writer’s line of research on radio-galaxies [13], where he made quite a long experience by concentrating on the radio properties of cluster of galaxies. The greatest benefit he brought home, at the end, was the close link he could this way establish with Dutch radio-astronomers. Combining the continuum with the hydrogen line was a step suggested by the role played by the ambient on the hydrogen content of spiral galaxies, most evident well inside clusters. He managed to systematically pursue investigations on line and continuum to encompass also the filamentary structures which apparently connect the clusters on the large scale. Ongoing stripping of interstellar gas from galaxies in a supercluster is described in [14], a good example out of his numerous publications.

Another student of Giorgio, Tommaso Maccacaro, graduated in 1975 on a topic which had nothing to do with CRE, but rather with an excess photoelectric absorption in the X-ray spectrum of the Crab Nebula, tentatively attributed to interstellar gas in its molecular phase [15]. His early career will be recalled in the proper place, sect. 5.

3. – Gamma rays and astronomy

The discovery of radio pulsars was a revolutionary event, in that the missing link, as it appears nowadays, between the evolution of stars and the fatally prompt and violent explosion that marks the end, for some of them, of their lifetime, was finally discovered to exist. When a pulsar was found in the Crab Nebula, the celestial laboratory of high energy astrophysics up to that time all of a sudden became more comprehensible. That pulsar was discovered in radio, in the optical and in the X-rays, with exactly the same period. This time signature can be used to beat the background and to obtain a pulsar signal by folding the data in phase, even if the signal is modest. This was the idea behind the balloon flights that, under Scarsi and Agrinier leadership, the Group made around 1969 of a still photographic spark chamber to search for the Crab pulsar in gamma rays. The positive result, albeit statistically marginal and controversial for a while [16,17], was amply confirmed there after.

While COS-B was in preparation, the idea arose that a “pulsar synchronizer” onboard might help to find objects akin to the Crab pulsar. To the writer recollections, then abroad and shipping on a different river, it remains rather obscure the extent to which the discovery of “accretion powered” X-ray pulsars played a role in that decision. Surely

in Connie's group the attraction exerted by these new phenomena was strongly felt, and one can recognize, in the participation to the analysis of the experimental data from the balloon flights mentioned above, the origin of the theoretical and observational interests developed in the early seventies by Laura Maraschi and Aldo Treves. They had joined the group after graduating in 1966 and 1968, respectively. Aldo had graduated with Beppo, Laura with a member of the theoretical physics group (upper floor): shortly afterwards, she experienced a sort of neutron capture, following a suggestion of Boella involved at the time in a rocket experiment to measure the possibly prompt arrival of neutrons from solar flares. This experience led her to descend from the upper to the lower floor of the Physics Institute for good. But her first publication [18] had nothing to do with neutrons, it was devoted instead to possible contributions by the CRE to the hard X-rays from the Milky Way, that were at the time being observationally investigated, among others, by colleagues in the Bologna group led by D. Brini.

COS-B was launched in 1975, its life terminated in 1986, and the international team (the Caravane Collaboration) in charge of the data analysis had matter to work on full steam for a decade. The results obtained [19-21] may appear in current days almost totally outshined by those from the mission Compton-GRO (1991-2000). It is fair to say that COS-B stands like UHURU in the history, respectively of gamma and X-ray astronomy. The main features that could be discerned in the map of the galactic disk were isolated sources embedded in the diffuse emission from CR inelastic collisions, with some of the structures tentatively identified with massive clumps of molecular hydrogen bombarded by the cosmic rays. In the high galactic latitudes, extragalactic domain, the very long exposures required to obtain a significant signal prevented most of the sky to be explored. Only the quasar 3C 273 was detected, with more luck the nearby blazar 3C 279 might have been detected as well. But fate decided that this particular blazar preserved its outbursting activity for Compton-GRO, and the now apparently obvious, and predictable, case of blazars being among, if not the strongest extragalactic sources in gamma rays is, admittedly, a failure of the imagination of the members of Connie's group, but not only theirs.

Worth a career, probably the most renowned is the one bright source in the galactic plane that had no obvious counterpart, and, as if it were a ghostly presence, it was baptized "Geminga". Giovanni (Nanni) Bignami, who had been a student of Beppo, took up the challenge in a way that someone might call fanatical: he tried all opportunities to find the counterpart, albeit not alone in the chase to this subliminal object. He succeeded first to find a good candidate with the Einstein Observatory (1983), later to discover the presence of a significant proper motion in a very faint optical counterpart (1993). By measuring the proper motion, hence a significant limit on the distance, the luminosity could be roughly estimated. In between these two steps, the idea that Geminga could be a neutron star stuck in the air until Jules Halpern and Steve Holt (1992) discovered, with the German satellite ROSAT, a pulsation in the X-rays, which guided Nanni to immediately recognize its presence in the COS-B data (with no firm precognition, the gamma rays collected from Geminga, with their too sparse temporal occurrence, could not by themselves yield to a Fourier transform or anything the like). The fascinating

story and the astrophysical implications have been described in great detail, and reviewed by Nanni and his wife Patrizia Caraveo in [22]. There a “fraternity” of Geminga is evoked in the title itself, as a special type of isolated neutron stars, of which several more have thence been recognized: may be more of them are lurking among the still unidentified gamma ray sources discovered by Compton-GRO. In 1993 Jules and Nanni earned the Rossi Prize for their discoveries.

4. – Bricolage

The first close encounter in Beppo’s group with X-ray astronomy took place sometime in 1967, when Bruno Rossi visited Milan and presented in a seminar the first intriguing results from rocket flights. That occasion stuck in the writer’s mind for two reasons, the obvious one and the other which reminds us of a habit that resisted, in that form or others, for quite a while: he was given the task to take notes, reshape them in a written report and circulate the latter within the group.

After the early discoveries by UHURU (1970-79), the international community attained the clear vision of the role that accretion plays onto compact objects in binary systems, the compacts being neutron stars and white dwarfs, and possibly black holes, the holy grail of general relativity.

In his two years at MIT (1971-72), working with Bruno Coppi, Aldo Treves attempted first to apply the unipolar inductor model, developed by Goldreich and Julian for the radio pulsars, to the X-ray pulsar. He obtained a result [23] that, although not the explanation of the observational evidence, remains an important contribution to the pulsars electrodynamics. Then he turned to accretion onto a highly magnetized neutron star, that was becoming popular wisdom. In collaboration with Baan, he was one of the first to introduce the concept of the Alfvén surface, where the magnetic stresses begin to dominate the dynamics of the matter inflow [24]. Back from MIT, it was Aldo’s enthusiasm on these matters to influence Laura, and the new entry Cesare Reina, to devote most of their attention to accretion disks, one of the most significant results being their work on the role played by radiation pressure on the accretion dynamics [25].

Another important new entry, that brought into Connie’s group other sorts of motivations, was Massimo Tarengi. He graduated with Aldo and Laura, Connie supervising, in 1970. The subject of the thesis was the source Sagittarius A at the centre of our Galaxy, a sort of prophetic start for the other line of research, on Active Galactic Nuclei (AGN), which in due time caught up very naturally with the previous one, thanks to the paradigm that was to become rather quickly accepted at large, namely that AGN are powered by accretion onto supermassive black holes,.

It is hard to understand how two so different characters like Massimo and Cesare could scientifically flourish, in the same group, and develop their individual attitudes very successfully in the end. To grasp some understanding, it requires that the ambient set-up be described in a few words. In the years 1970-1975, for lack of space in the Physics Institute, Connie’s group was implanted into a small (three rooms and a kitchen) apartment in a popular building in Via Aselli, about 400 meters from the institute, half-

way between the latter and the flat where the Occhialini family lived. Connie's style helped to keep within this limited space a very domestic type of cohabitation, so that everybody felt part of one and the same family, and the mutual interactions were the most open and frank. This spirit remained when the group eventually moved back to the institute, to take possession of a two floor, small building in the only empty corner of the institute campus. And the mind goes to the dear figure of Elvina, Connie's secretary with her office in the kitchen, who soon became an invaluable support for all the other fellows. The rare gift of intuition, combined with peaceful efficiency, made her the only irreplaceable member of the family. She could type the handwritten notes almost without a direct assistance: only who has ever come across an handwriting by Aldo can appreciate this virtue.

Before leaving the follow-up of this story in Connie's group, more appropriately, to the next two sections, it is interesting to outline briefly how things went with Cesare Reina and Massimo Tarenghi.

Reina's thesis (1971), under Connie suggestion and supervision, dealt with the number counts of X-ray sources in the galactic disk from the first UHURU catalogue, and with the correlation between photoelectric absorbing column and optical extinction. Both topics eventually went into publications. The one most deserving to be mentioned here, completed with the help of Tarenghi, is devoted to one of the very first quantifications of how much extinction by dust is associated to the atomic photoelectric absorption, in our Galaxy [26]. Cesare then became involved in the calculations on accretion. During the whole of this experience, he realized that phenomenology is a pleasant way to do research, calculations can offer a great satisfaction at the end of the process, but all in all that was not sufficiently gratifying to his aesthetical and ideological taste. For sure he must have been inspired by the fondness with which Aldo liked sometimes to embark himself in more profound speculations in General Relativity. As a matter of fact, he leisurely started to play with a mathematically more formal approach to scientific problems. (When he talked about Killing vectors, the writer recalls himself left as a S. Sebastian with a wordless mouth.) The drift was apparently slow, but so effective that in 1986 he won a chair of full professor in Mathematical Physics.

Tarenghi loved observing with telescopes, and was the only one with this inclination in the group. He had the satisfaction to do nothing really better than observing when he went in 1973, for two years with an ESRO/ESA fellowship, to work with Bill Tifft in Tucson. There was nothing in Tucson that could distract Massimo from the telescopes, except for the desert, as the writer realized during a short visit to him. This experience, to a large extent, concentrated on the idiosyncratic approach of Bill to the controversies on the real nature of galaxy redshifts, which took in his hands the form of a discrete pattern in the velocities of spiral galaxies in clusters, especially when endowed with a significantly high radio emission [27] (the writer remembers contributing targets from his studies on radio emission from clusters of galaxies). For Massimo that was a great professional experience, and he came back with quite a different level of self-confidence in his own talents. It was also the start of a scientific career with important observational contributions on clusters of galaxies, the large scale distribution of matter in the Universe

and AGN. But Massimo had a dream, not only observe with, but construct telescopes himself. The dream turned into reality after his move to ESO in 1977, where he became in 1983 Project Manager of the New Technology Telescope (NTT), and where, to cut the story short, he is at present (2006) the Director of the ALMA project, a vast array of antennas being deployed in the Atacama desert (Chile) for millimeter astronomy.

5. – Active Galactic Nuclei and jets

A combination of interests and competences present among the members of Connie's group led to developments, first somewhat hesitant at the beginning of the eighties, then more and more systematic, on the subject of Active Galactic Nuclei and their physical properties. The preliminaries to these developments can be found in the previous sections, and they combined in a very productive way both the attitude to deal with observations and the imaginative curiosity necessary to play with astrophysical models, which Connie fostered, and assisted with kind criticism. A turning point was the enthusiasm which Massimo Tarengi put in convincing his fellows that the first announcement of opportunity for the ESA-NASA satellite International Ultraviolet Explorer (IUE), issued before its launch in 1978, should not be left unanswered. This was perhaps not too difficult for targets belonging to the galactic compact X-ray sources, a little more difficult, given the predicted sensitivity of a satellite mainly designed for galactic objects, and the lack of familiarity (or sheer ignorance, to put it bluntly) with the ultraviolet band, for extragalactic targets. Perhaps one of the most valuable outcomes of this experience was the initiation to a multi-wavelength approach, which was rather soon recognized as a necessary tool for the physical understanding of many celestial sources, especially those including thermal and non-thermal effects of various sorts. The more so when short term variability complicates their spectral dynamics.

The proposals submitted concerned the most diverse types of AGN, namely BL Lac type objects, Seyfert galaxies (among which NGC 4151), the quasar 3C 273, and the most challenging of all, the jet in M87. For BL Lacs the expectations were basically that one could extend the observation of a featurless, variable non-thermal continuum. The same was for the M87 jet. A little bit more complicated was the case of the Seyferts and of 3C 273, because none of the fellows in the group was all that familiar with the spectra of photoionized matter. On the last subject a great opportunity was offered (patronized by Bob Wilson, the father of IUE), when the numerous proponents sharing the same, as obvious as bright, targets, were forced to join their efforts: under this condition a generous quota of observing time was allocated. It so happened that an international european collaboration was born (coordinated by M. H. Ulrich) to work together in perfect harmony through the following AO's, and to pursue a project, which became the more ambitious the better IUE demonstrated its ability to obtain spectra of excellent quality in a few hours of exposure also for the brightest objects of this class. The writer reminds this venture with great pleasure, and gratitude for the science he learned (with some rewards, he hopes) from personalities like A. Boksenberg, M. H. Ulrich, A. Elvius, G. Bromage, J. Clavel, the late Mike Penston, the wittiest of all. After the publication

of a long discussed paper on 3C 273 [28], where the result that appeared at the time most exciting was the discovery of a consistent set of absorption lines in the warm phase of the galactic interstellar medium, the collaboration concentrated on NGC 4151. Of the results obtained in several campaigns, three are possibly outstanding. The first [29] was the rudimentary application of what is now called “reverberation mapping” to a temporal string of spectra, when the object varied and behaved in the most obliging mode it could. The outcome was the first dynamical estimate of the mass of the putative black hole at the centre of this galaxy. The “mass” published was incorrect because someone had used a factor-10 wrong value of the gravitational constant: the mistake was soon spotted, after publication, by a british amateur astronomer, whose kindness in writing to the authors is a model of fair play, that is rarely found among the often nervous professional referees. The second came out from a campaign, whose spectra at first sight everybody regarded as very disappointing: the object had declined to the lowest intensity ever recorded, its lines had slimmed down to the equivalent of a type-2 Seyfert galaxy, and stayed so all the way through. But later inspection showed, other features remaining still, that two narrow lines, asymmetrically placed in the wings of the CIV line, kept varying erratically in intensity and by quite significant amounts. As no atomic explanation of this transient property has ever been found, the suggestion that a jet-counterjet equivalent of the galactic SS433 was present in this galaxy at the time remains a most intriguing possibility, as proposed in [30]. The launch of the ESA X-ray astronomy satellite EXOSAT in 1983 offered the opportunity to make simultaneous observations with IUE, and the campaign just mentioned belonged to this part of the project. The outcomes are described in [31]. Among them, the writer likes to remind one which caused a furious debate with M. Penston, during one of the regular meetings. If the UV flux is plotted against the X-ray flux, there appear to be two regimes, at low X-ray fluxes the two correlate very well, at high X-ray fluxes no correlation can be found. This is the way the writer had liked to present, and comment, the “evidence”, Mike pretended that the existence of a correlation must always be tested on the whole set of data, in which case its significance vanished. In the end the writer gained the consensus of the other co-authors, and Mike eventually started wondering, and drawing cartoons, on what the “two states” behaviour could tell us about the geometry of the accretion process.

It must be recalled, in this context, the painstaking amount of work done in analysing the EXOSAT data, with particular care for the background, by Luigi Piro, which graduated in 1983 with the writer (who had moved to Rome University in 1980). He availed himself this experience when, in the years 1988-89, he went to Japan and worked in RIKEN with Masaru Matsuoka on data from the *Ginga* satellite, and co-discovered the existence of a new spectral component in AGN, namely the now called “reflection bump” from optically thick, circum-nuclear matter, which emerges above about 10 keV [32]. Work on EXOSAT spectra of NGC 4151 [33], which confirmed the existence of the correlation between the X-ray spectral index and the flux first shown in [31] (in contradiction to the alternative fostered by Ken Pounds group, who eventually convinced themselves of the opposite with their *Ginga* data) was also the start experience as a post-graduate for another pupil of the writer, Fabrizio Fiore, who graduated in 1986, with the assistance of

Luigi in data analysis. And who later went for long periods first to RIKEN, then to the Center for Astrophysics in Cambridge, Mass., to increase his experience and reputation.

The story with M87 [34] deserves a place *per se*. With IUE it was mandatory that one of the proponents be present in VILSPA and take responsibility in the acquisition of the right target, as displayed from the optical small telescope onboard. Tarenghi went there, knowing that the jet would be invisible, prepared to guide the operator in the offset pointing manoeuvre, starting from the maximum in the bright bulge of the Galaxy. It was the first time they had tried a thing like that, one can imagine the burst of felicitations when, after about 7 hours exposing, the satellite returned a faint, featureless spectrum: it was the jet! Another exposure was made about one year later, Christmas Eve, that helped to eliminate any doubt about a narrow CIV feature in the first spectrum, a mere residual from a previous exposure of a bright star, that had stuck into the detector despite the wiping out procedure. The publication included also the spectrum of the M87 bulge, one of the first examples of an ultraviolet excess in the innermost stellar population of an elliptical galaxy.

The results on the highly variable BL Lac objects are best illustrated with the outcomes on PKS 2155-304. In the first IUE observation it was fortunately caught in one of its bright states [35]. As expected, a featureless spectrum was observed, that could be represented with a power law smoothly connecting with the optical and infrared spectra acquired in a similar state. That was a confirmation of the synchrotron origin of the extremely wide, from the radio to the far UV, continuum of these objects. The collection of several spectra taken with IUE showed that the power law is subject to variability in slope, while the combination with an optical spectrum, taken almost simultaneously with one of the UV observations, demonstrated that the continuum becomes steeper at higher frequencies, a signature of the increase in the energy loss rate, hence a decrease in lifetime, of the electrons responsible for the emission [36]. Extension of these properties to the X-ray band was found combining EXOSAT with UV and optical observations, clearly indicating that up to 10 keV at least the continuum has the same physical origin, while the temporal behaviour in the three bands led to the conclusion that the respective emission regions, probably distinct, must be connected [37]. Similar results were obtained on other objects as well (including, in addition, the measurement of the redshift of one of them, PKS 2005-489 [38]), but what seems no less important is that this activity stimulated in the group a long term investigation on the physics of the relativistic jet, which the astrophysical community at large, on these and other observational grounds, recognized as the outstanding property of this class of objects. Which was to be enlarged to include, under the name of “blazars”, also a subclass of quasars, characterized by violent variability and a high degree of (variable) polarization.

This theoretical activity, mainly inspired by Laura, is neatly summarized, in its international context, by Gabriele Ghisellini [39], one of her pupils who graduated in 1983. There one can easily appreciate the relevance of the contributions that came from the group, which had been slowly growing through the never interrupted pedagogical cultivation of new young entries. Among them, and to become a distinguished expert on this very subject, Anna Lisa Celotti, who graduated with Laura in 1989. Probably the

first important paper [40] was the one published in 1985, where a physical model of the jet, apt to reproduce the continuum from radio to X-rays, was proposed, which included the radiative role of the Inverse Compton process (synchrotron-self-Compton) in a self-consistent way. How the people most involved mastered the matter is demonstrated by the prompt interpretation of the gamma-ray outburst in 3C 279, detected by Compton-GRO, in terms of anisotropic synchrotron-self-Compton emission in a relativistic jet [41]. A fair example of how the observational activity always went along with the theoretical one, in the particularly demanding aspect of a multi-wavelength, temporally coordinated approach required by these objects, is offered by the paper [42], where the combination is discussed of X-ray and ground-based observations, organized in the occasion of a large flare in 3C 279 detected by Compton-GRO. The confrontation with the data led the authors to consider, as equally viable, an alternative to synchro-self-Compton, that is Inverse Compton on ambient photons through which the jet is propagating, which are present in very large quantity in the very small volume where most of the action recorded in the spectra of typical AGN takes place.

The last point naturally introduces, in this synthetic description, another line of research, whose roots go back to the early works on accretion disks mentioned in sect. 4. As standard accretion disk models do not immediately predict a hard power law emission in the X-rays (this is true for super-massive black holes in AGN, as well as for stellar mass black holes, like Cyg X-1), the idea that has so far survived various criticisms and observational tests appeals to a process, called “comptonization”, where photons, trapped in a gas with a kT larger than their energy, emerge from it with a power law spectrum extending up to 4 kT. How to provide this very hot gas is the problem. A prototype model, which is very often referred to in the literature, was proposed in [43] by Laura and another of her pupils, Francesco Haardt (graduated in 1991): no matter how that happens, if a consistent fraction of the accretion power is extracted in the form of hot gas, which like a corona (whether homogeneous or not, see [44]) sandwiches the standard disk, the photons emerging from the latter are duly comptonized and shaped into a power law. To mention just one case, the model was properly tested on a high-statistics spectrum of NGC5548 [45], when it became available over the range 0.3 to 200 keV from a Beppo-SAX long exposure. The satellite to which sect. 7 is devoted. It is appropriate to mention here also the outcome of a suggestion by Luigi Piro. Back from Japan, he threw on the table the idea that someone might further investigate the observable expectations from the reprocessing by thick, circum-nuclear matter in AGN. That became immediately the subject of the PhD thesis, that Giorgio Matt concluded, under the supervision of the writer, in 1993. The paper [46] contains part of it, and has become one of the most referred to in the field, for its results on the combined effects of Compton reflection and iron fluorescence, and the inclusion of the relativistic effects to be anticipated if the thick matter were to coincide with the accretion disk.

A rather different track was followed by Tommaso Maccacaro. When he went to Leicester in 1976-77, to work on ARIEL V data in the group led by Ken Pounds, he got involved in a thorough analysis of the sample of sources which could be identified with Seyfert galaxies. Being aware of the experience the writer had made on the bi-variate

(radio-optical) luminosity function of radio-galaxies, he invited him to take care of the chapter dealing with the first estimate of the fraction of galaxies endowed with a AGN property in the local universe [47]. This collaboration had a *sequitur* in a study with the Einstein Observatory of some objects, that at the time were called Narrow Emission Line Galaxies. A project accompanied by IUE observations made together with Jaque-line Bergeron. The X-ray data were analysed when Tommaso had moved, in 1979, to the Center for Astrophysics (CfA), Cambridge Mass., where he would stay till 1989. And the results showed the presence in these galaxies of a photoelectric absorption substantially larger than typical of the type 1 Seyferts, thus clarifying their intermediate nature between type 1 and type 2 [48]. It was during his first visit to the CfA for this project, that the writer vividly remembers Tommaso plunging himself in what would become his most original, and long term project. At the time the satellite HEAO-1 had provided the most complete all-sky survey that could be obtained in X-rays with a collimated instrument. Incidentally, another offspring of the School, Giuseppe Piccinotti, which graduated in 1975 with Costantinos Paizis, was then in Goddard to work on the final HEAO-1 A-2 catalogue [49]—after this experience he decided, when back to Italy, to devote himself to economics, a precursor in a type of career which has become the yearning of some modern physicists. In order to bridge the gap between this type of surveys, which are limited by “confusion” to the brightest objects, and the very deep, but also very narrow angle, surveys that could be performed with the imaging telescope aboard the Einstein Observatory, Giacconi had stimulated Tommaso to embark himself in a very painstaking job, namely to spot the serendipitous sources which happen to fall in the image of fields centred on pre-selected targets. To detect them above a prescribed significance level, distinguish point-like from extended (galaxy clusters) sources, attribute to each of them the right statistical weight, and combine the whole into a proper counts-versus-flux diagram required the implementation of a novel strategy. The first version of the so-called Medium Sensitivity Survey appeared in 1982 [50], by a curious coincidence exactly in the same issue of the *Astrophysical Journal* where Piccinotti’s paper was published. It is fair to say that this strategy has been systematically used by several authors thereafter, when other imaging X-ray telescopes ventured to explore the X-ray sky.

6. – Compact galactic sources

Why should B-emission stars have to do with hard X-ray sources? The persistent Massive X-Ray Binaries, pulsating in X-rays, are neutron stars wind-fed by OB stars. The discovery of the first transient X-ray source goes back to 1967, and was made during rocket flights. In the middle of the seventies astronomical circulars announced identification of some hard X-ray transients (which differ from the soft ones) with Be stars. In a conversation with Ed van den Heuvel, Laura became acquainted with the long time speculations that for several decades had accompanied the bizarre behaviour of these massive stars, and simultaneously with the characteristically prompt reaction of Ed to associate that behaviour (large and sporadic mass losses) with the transient nature of the associated X-ray binaries. A seminal paper followed soon [51].

In 1975 the discovery of the first X-ray bursters took place. Located mostly in globular clusters and in the bulge of the Galaxy, they are either persistent or transient X-ray binary sources, with light curves characterized by large, recurrent spikes in their emission, which take about 10 seconds to die away, and reach after the sharp rise a luminosity close to the Eddington limit for a neutron star. Laura spent a month or so in MIT in 1976, and vividly remembers, in Walter Lewin's room, the impressive sequence of such events on charts with light curves collected with the SAS-3 satellite. The idea, strongly supported by the black-body shape of their spectra, with the temperature declining after the maximum from some 30 million degrees, came to her mind that they might be due to thermonuclear flashes, occurring when a critical density in the matter accreted on the surface of a neutron star is reached. It is unfortunate that this idea, put on paper in collaboration with A. Cavaliere, left trace only in [52], but it is gratifying that the credits are correctly recognized in the book "Exploring the X-ray Universe" by P. A. Charles and F. D. Seward (1995).

The theoretical interests in interpreting the novelties popping up in the X-ray window, were to be associated in the late seventies, and to remain so thereafter, with a direct involvement in observations. That transition was instigated, and made possible to a large extent, by the access to space telescopes, IUE first and later EXOSAT. The collaborative presence of Lucio Chiappetti, who graduated with Aldo in 1979 just on IUE data, and of Enrico Tanzi, was invaluable in this respect. Among the first results obtained, it is worth mentioning at least three, the first on the prototype black hole candidate, the other on the prototype of a class of cataclysmic variables, the third on a non-pulsating neutron star. Repeatedly observed with IUE, the optical counterpart of Cygnus X-1 showed strong absorption lines, whose equivalent widths were modulated with the orbital phase: a clue for the ionization effects on the wind blowing off the massive companion by the X-rays from the accreting collapsed object [53]. AM Her is a strongly magnetized white dwarf accreting from its companion in a binary system. Unlike the case of more compact objects, the ultraviolet emission is more directly associated with the accretion process, thus the seven IUE spectra, showing strong emission features on top of a well-defined, power law continuum (sub-relativistic cyclotron emission), represent a very important constraint for detailed modelling of the system physics and geometry [54]. The far UV spectrum of Cyg X-2, a Low Mass X-ray Binary, turned out rich of strong emission lines, attributed to the atmosphere of the non-collapsed star in the binary system, heated up by the X-rays. The nature of the collapsed object was still controversial at the time, and the authors correctly argued in favour of a neutron star instead of a white dwarf [55].

A favourite of Aldo has been for quite a while the black-hole candidate LMC X-3 in the Large Magellanic Cloud. Not surprisingly, perhaps, his pupil Tommaso Belloni, which graduated in 1987 working on observations of that object, has since remained a lover of black holes. The study of a binary system containing a black hole can aim at different objectives. Among them, of the up-most importance is to pinpoint the aspect angle of the accretion disk (equivalent to that of the orbital plane). In a paper [56] which combines IUE, optical and infrared observation this angle is inferred from the shape of the broadband spectrum. In another paper of the same year [57], a number of X-ray observations

made with EXOSAT and spanning two years are used to investigate the variability and, most important, their ensemble is confronted with expectations from a specific comptonization model: the positive test is used to derive the physical parameters of the model.

Another offspring, who graduated with Aldo and Laura in 1981, that the writer would like to mention, is Monica Colpi. She apparently experienced the critical spark going off during a stage in the United States. There, while working with S. L. Shapiro and S. A. Teukolsky, she got enthusiastically involved in rather curious subjects—such as “exploding neutron stars” [58]. Since then, her interest in fundamental physics at work in astronomical systems, especially those including collapsed objects, progressed through valuable publications and, perhaps more important in the spirit of the School, excited the enthusiasms of her pupils: these pupils, however, belong with many others to the generation of the great grandchildren, which lies beyond the horizon of this script.

7. – Beppo-SAX

Under the leadership of Gianfranco Spada, Livio Scarsi, Giuliano Boella, with the collaboration of the Dutch SRON (Johan Bleeker) and the ESA SSD (Brian Taylor), the proposal of a “Satellite per Astronomia X”, SAX, was submitted in 1981 in response to a call by the Italian PSN (ASI later), and was selected for its phase A. The Italian group of scientists were mainly from CNR institutes, with a few additional ones, from universities, that were part at the time of a CNR funded Gruppo Italiano di Fisica Cosmica, GIFCO, a long-lived creature of Beppo. The writer, one of the latter, was convinced by Livio to be among the proponents, although he was not that enthusiast about the original set-up of the mission. He changed his mind when, during phase A, a coded mask instrument was replaced, on paper, with concentrator telescopes. These required a technological know-how that was only partly available, and it remains a great merit of Giuliano (as Director of the CNR Istituto di Fisica Cosmica e Tecnologie Relative in Milan, the first successor of its founder Beppo) that he managed to convince, and support Oberto Citterio to leave aside his beloved activity in optical interferometry, and to plunge into this new venture. The participation of the MPE in Garching b. Munchen, directed by J. Truemper, was officially recognized for the assistance in the testing of the optics. For Oberto, that was the beginning of a long story, which led him to become in due time an international authority on X-ray grazing incidence telescopes. All the activities have been followed by a Steering Committee, appointed by the PSN/ASI, chaired by Scarsi, with Bleeker as vice-chairman.

In brief, the payload [59] included three narrow field instruments (NFI), the first a set of four concentrators with spectroscopic and imaging capabilities, three with a MECS (PI Bruno Sacco) in the focal plane, 1.3 to 10 keV, one with a LECS, 0.1 to 10 keV, to be provided by SSD (PI Tony Peacock); the second a collimated high-pressure GSPC, 4 to 120 keV (PI Giuseppe Manzo); the third a collimated phoswich, PDS, 15 to 300 keV (PI Filippo Frontera); at right angles to the previous instruments, two (originally three) coded mask, proportional counters, Wide Field Camera (WFC), 2 to 30 keV, to be provided by SRON (PI B. Brinkman). To be placed on a near equatorial, circular orbit, SAX could then investigate, in optimal conditions for the particle-induced background,

the celestial sources across an unprecedentedly wide, 0.1–300 keV, energy window, and at the same time to watch the sky for transient events with the WFCs. The anticoincidence shield of the PDS would play an active role as a monitor of Gamma Ray Bursts (GRB).

The writer main task was to optimise and keep up to date the scientific objectives and, in doing so, to stimulate the scientific community at large in preparing itself to take the best advantage when SAX would be placed into orbit. One of his battles, in this regard, aimed to convince ASI to create a Scientific Data Centre, which should take care of the analysis software and of the assistance to the users. The Centre eventually materialized, just in time, as a small group led by Paolo Giommi (who graduated in Milan with Bignami, and had made a long experience in ESA since the launch of EXOSAT), who could count on the most effective cooperation of Fabrizio Fiore and Matteo Guainazzi (who graduated and took his PhD with Renato Robba, one of the earliest pupils of Livio in Palermo). The international success of SAX is certainly, to a fair extent, due to the connection between the SDC and the Ground Segment, which could not have been as functional, as it turned out to be, without the years long efforts of Guido Di Cocco and of Lucio Chiappetti, with Barbara Negri (who graduated, 1982, in Rome with Livio) assisting from the ASI institutional side. And to the exceptionally competent role of Mission Scientist as played by Chris Butler, both before and after launch. A rather demanding role, whose importance will be presently apparent, was assigned in 1992 to Luigi Piro, as Deputy (of Giuliano) first, then Project Scientist in 1996.

The project grew with an unpredictably high number of thorns, the writer moods had their ups and downs, he still remembers the encouraging words of Yoshuo Tanaka: life will be different, believe me, when the thing will go into orbit. When at last SAX was launched, April 30, 1996, his life had in the meantime already turned bitterly different. Beppo, who had been watching at the development of the project with sympathy and apprehension, had gone for ever. Connie had chosen to live segregated in her small estate in the Chianti region.

After the launch, the mission was soon baptized Beppo-SAX.

About ten months after launch, Beppo-SAX was glorified by the international community. GRB 970228, registered by the GRB monitor, was, after the link during the passage over the Ground Station in Malindi, found as a timely spike in one WFC, which gave the positioning for the imaging telescopes to start, about 8 hours after the burst, collecting the first photons ever of a GRB afterglow [60]. The good positioning allowed for the detection, 21 hours after the burst, of a faint afterglow also in the optical by Jan van Paradijs and his coworkers [61]. More of such events followed, the optical identification with distant galaxies as their place of origin finally settled the 30 years debate on their distance, hence on their luminosity, which turned out to be prodigiously high. To many people looked as if the SAX team had overlooked this chance for a long while. Anyone can easily find out that this is not the case. Given the presence of a GRB monitor onboard, almost the exact number of GRB whose position could be pinpointed with the WFCs (just a matter of arithmetic) in two years can be found in [62], the presentation of the scientific objectives, which, along with that of the state of the project at the end of phase A, were given to the international community in Rome, December 1983. In the

early nineties the NASA project of the Explorer HETE jumped into the scene, and in the writer mind the new situation distracted attention. When in 1996 HETE had its bad luck during the launch, the readiness of the team, full time concentrated on the preparation of the program for the leading instruments of the mission, was admittedly below standard with regard to GRB's. It is certainly a great merit of the Project Scientist Luigi Piro, together with Enrico Costa and Filippo Frontera, to have made operational the right strategy, in the minimum time allowed for by the restricted man-power available. For this success, in 1998 the SAX team, and Jan van Paradijs were awarded the Rossi Prize.

Designed to study in detail, with the NFI, spectra and their time variations over the band 0.2–200 keV, and to monitor the sky (the galactic plane and the galactic central region in prime mode) with the WFCs, Beppo-SAX has obtained a great number of very significant results, that could not be summarized here. A symbolic choice is therefore briefly mentioned, that to some extent links with the activities described in the previous sections. Four results illustrate the benefits of the full energy band, the other two refers to the best effort exploitation of the telescopes.

The X-rays from blazars, according to models based on the discovery of their large, and highly variable output in gamma rays, represent the transition region in their spectra between the synchrotron and the Inverse Compton dominated regimes. No one before had seen in reality how a blazar spectrum might look when the two regimes intersects. The first good example was caught by Beppo-SAX in a high state of ON 231 [63], when the spectrum showed a rather sharp transition in slope at about 4 keV. The sharpness of the transition speaks by itself in favour of a more homogeneous model of the jet than used to be considered before. This result suggests how it will eventually become possible to discriminate between synchro-self-Compton and Compton on ambient field photons, when a much more sensitive (necessarily imaging) instrument than the PDS will be available, hopefully in the near future, to probe the transition from high to low states, or viceversa.

The discovery in AGN, with the *Ginga* satellite, of a further component in their spectra, above about 10 keV, due to Compton reflection from thick matter, had its limit in the band at high energies, and its quantification suffered from the inability to detect and measure a high-energy cut-off, empirically described with an exponential factor, predicted by comptonization models to lie above about 50 keV. A dedicated survey of the brightest Seyfert type 1 AGN [64] led to a surprisingly neat result, thanks to the PDS sensitivity, where the presence of the cut-off was measured and its previously unresolvable degeneracy with the amplitude of the reflection component was objectively resolved. This fact helped in a more stringent test of the comptonization model mentioned in sect. 5, when applied to AGN [65].

Among Seyfert galaxies, those classified as type 2 suffer from optical, as well as from X-ray obscuration. The most extreme cases are those where the optical thickness of the obscuring matter is such that, in addition to the photoelectric effect, Compton scattering reduces dramatically the output also at very high energies. In such cases the evidence, in the X-rays, of a AGN is left to the presence of a pure reflection continuum, with the very intense accompanying Fe line fluorescence. Discovered with the Japanese ASCA satellite, their very hard spectrum beyond 10 keV could eventually be observed with Beppo-SAX.

The case of the Circinus Galaxy is a good example of particular interest [66,67], in that a small fraction of hard photons, percolating through the screen, were detected, and the actual thickness of the latter, not just a lower limit, could therefore be determined.

The existence of a cyclotron absorption line was discovered in the accreting X-ray pulsar Hercules X-1 by a German group led by J. Truemper with a balloon flight in 1977. The importance of this discovery is straightforward, in that it gives immediately the strength of the magnetic field of the neutron star close to its surface (in that case three trillions Gauss). A design goal of Beppo-SAX had been the search of such lines and of their overtones. In the pulsating transient X0115+63 up to four harmonics were discovered, as illustrated in [68].

The quality of an X-ray telescope is often judged on the basis of its resolution, expressed as the Half Power Diameter (HPD) of the spot where half the photons from a point source are confined. When energies above a few keV are reached, the wings of the point source image, where the other half goes, are almost as important: at such energies the wings can be dominated by the scattering due to the micro-roughness of the reflecting surface. In this respect (but also in the overall construction design of the optics) Beppo-SAX was superior to the Japanese satellite ASCA, a property which restricted the effects of confusion, and allowed a better distinction between extended and point sources. This bonus was exploited to count sources in the band 5–10 keV, a spectral region where the current paradigm on the discrete nature of the X-ray background (XRB) predicts the contribution of the absorbed AGN, type 2, to emerge, at least as equally important as that of the type 1 AGN, which in their turn dominate around 1–2 keV. Up to 20–25% of the XRB in that band could be resolved, with follow-up optical identification confirming the mix of type 1 and type 2, roughly as expected [69,70]. This result gave way to a collaboration, HELLAS, which is still pursuing that goal by using the much more sensitive and sharper images from the *Chandra* and Newton-XMM satellites.

Another quality of the telescope images, the very low particle induced background, represented a good opportunity to extend the study of clusters of galaxies to their low brightness outskirts. Silvano Molendi, who graduated with Laura on the physics of AGN, had been deeply involved in the calibration of the MECS in orbit, and quickly realized that he could benefit from a number of cluster observations if he took a systematic approach. Very significant results were obtained [71], and to the surprise of most experts he could show that, despite the much lower collecting area, Beppo-SAX had been comparable in performance to Newton-XMM when the lowest brightness levels are reached. A warning that a very low background, for this type of investigations (*e.g.*, the temperature gradient at large distances from the cluster centre) is at least as important, possibly even more important than a very large collecting area.

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Beppo and space research in Italy

GIULIANO BOELLA

Dipartimento di Fisica “G. Occhialini”, Università di Milano-Bicocca, Italy

In the year 1959, Beppo decided to evaluate the real potential of space research as a means to conduct research in the field of cosmic-ray physics. With this aim he decided to spend some time, about one year, at MIT in the Department of Cosmic Ray Physics, chaired by his friend professor Bruno Rossi (formerly his young tutor at Florence University!). That was also for Beppo the occasion to evaluate the potential of the new electronics techniques for the detection and measurement of cosmic rays: at that time in MIT these techniques, adapted to the space environment, were in strong growth. When back in Milan, Beppo decided to leave the photographic nuclear plates and was resolute to start a line of experimental research using electronic detectors, to be flown onboard stratospheric balloons and artificial satellites. Beppo asked Livio Scarsi to come back to Italy and around him he built the new research group, oriented to the study of cosmic rays from space. As a starting activity, Beppo provided a research contract for a detailed study of the neutron component of the cosmic radiation in the Earth atmosphere.

In 1961 Jacques Labeirye, director of the “Service d’Electronique Physique” of Centre d’Etudes Nucléaires (CEN-CEA) de Saclay, showed to Beppo a prototype of a spark chamber of small dimensions, suited to be used for cosmic-ray research onboard sounding balloons in the Earth atmosphere. It is worth remembering that few years before Beppo suggested to his long-date friend Labeirye to develop an imaging detector, capable to visualize the tracks of cosmic-ray particles with fast resolving time. On that occasion Labeirye also announced that France was building a facility, at Aire sur l’Adour (Landes), to launch balloons of new conception (tetrahedric shape).

Then Beppo selected two lines of research for his young research group, to be conducted with sounding balloons in the Earth atmosphere, as a preparation to the future space missions. The first line was devoted to the study of the neutronic component of the secondary radiation, with the aim to evaluate the albedo flux of neutrons produced by

the interaction of the primary cosmic radiation with the Earth atmosphere. The second line, in collaboration with the French colleagues of Saclay, was devoted to the study of the primary electrons on the cosmic radiation with evaluation of the e^+/e^- ratio. The initial research group was formed by Livio Scarsi, Gianni Degli Antoni, Giuliano Boella, Giorgio Sironi, Guglielmo Giannelli as a consultant, and the technicians Piero Inzani, Aldo Igiuni, Renato Ballerini, Pierluigi Dell'Era.

Quite soon it was evident to Beppo that the amount of involvement necessitated of personnel and funds, not directly available at the university level. He did not consider the INFN as the right place for space activities. He decided to evaluate the possibility that a sector, dedicated to the cosmic-ray physics, could grow in the Consiglio Nazionale delle Ricerche (CNR). With this idea, Beppo initiated a discussion with Edoardo Amaldi (Rome), Giampiero Puppi (Bologna), Carlo Castagnoli (Turin), Livio Gratton (Rome): these colleagues, in various Italian universities, had almost the same problems of Beppo. All of them did consider Beppo's proposal valuable. Amaldi, President of the Comitato Scienze Fisiche del CNR, did agree with Beppo ideas and acted to create a CNR structure, that initially did seem to be adequate. In 1963, two Groups were constituted in CNR, the Gruppo Italiano di Fisica Cosmica (GIFCO) and the Gruppo Nazionale Astronomia (GNA), reporting to the Comitato Scienze Fisiche. These groups joined sections, located in the universities of origin.

At the beginning of 1962, some European countries did create the COPERS, a committee that would propose a communitary program for space research. Beppo, for personal scientific credit and for the personal network of contacts with European colleagues operating in the field of cosmic physics, did act in favor of a significant presence of Italy in this committee, that would have originated firstly ESRO, then ESA. In 1962 and in the next years Beppo made a sequence of actions, with Ministero dell'Industria (ing. G. Mancinelli) and then with Ministero per la Ricerca Scientifica (dr. M. Fossa-Margutti) to get a deeper and deeper involvement of Italy within ESRO.

In these times the research activities in progress in the Sezione GIFCO of Milan gave good results and did demonstrate that the scientific and technological skills of the personnel of the group were already at a quite good level, ready to undertake more significant programs in space, beyond the activities with sounding balloons. In 1964, ESRO accepted the Milan proposal to extend the measurements on atmospheric neutrons above the balloon altitudes, by using a rocket to evaluate *in situ* the neutron albedo component. The experimental apparatus, flown on a rocket in 1966, was completely built in the Milan laboratory, in collaboration with small specialized industries for specific machining.

The success of the rocket experiment did show potential and limits of an experimental apparatus, built in house. Beyond a certain dimension of the experiment it is necessary to make use of industrial contracts both for manpower and laboratory resources, setting in the mean time a close cooperation between the research and industry personnel. Then the need for a more flexible and powerful research structure, ready to interact with industry in the frame of contracts for research and development, was evident. The same problems did arise in the Sezioni CNR of Rome and Bologna, both successfully involved in space research. Then a coordinated action was raised to upgrade from Sezioni CNR,

essentially university structures, to structures directly dependent on CNR. These new structures could have an increased budget, higher autonomy, possibility to get some personnel both at scientific and technical level, and possibly more space and equipments. Therefore the leaders of GIFCO acted together within the CNR to get the upgrade from Sezioni CNR to Laboratori CNR. The action had success in 1969. In that year and in the subsequent years the number of units of personnel did significantly increase, by incorporating the young graduated students who were already involved in the research activities. Oberto Citterio, Gabriele Villa, Dario Maccagni, Francesco Perotti, Laura Maraschi, Enrico Tanzi, Giovanni Bignami, Patrizia Caraveo, Paolo Cortellessa, Anna Della Ventura joined the research group. The technical staff was strengthened by the arrival of Enrico Mattaini, Edoardo Bardeggia, Alberto Rancati, Ernesto Franchini, Bruno Falconi. From the beginning, Beppo did also invite young foreign post-doc to cooperate in the research programs: among them we must remember John Bland, Martin Turner, Tony Dean. In the university a small group of young graduates made parallel theoretical research on the same topics with Connie Dilworth as a head and a supervisor. Among them we remember Cesare Perola, Aldo Treves, Cesare Reina.

In this configuration the Milan group was able to carry on significant space research programs, originated by its initial activities. Under the leadership of Beppo and with the relevant contribution of Connie Dilworth and Livio Scarsi there were conceived and proposed to ESRO some research programs to be carried onboard satellites for the study of the primary cosmic radiation. The selection bodies of ESRO did accept three research proposals for the installation of experiments onboard European satellites. An experiment, named S79, for the measurement of primary cosmic electrons in the energy range 10–100 MeV, built in collaboration with Saclay, was selected to be installed onboard HEOS-A1. Two other experiments were selected to be installed onboard TD1. The experiment, named S88, was dedicated to the study of the gamma-ray emission from the Sun. A more ambitious experiment, named S133, proposed by the MIMOSA collaboration (Milan, Munich, Saclay), was developed as a pilot program to study the gamma-ray emission at energies above 20 MeV. This project used as central detector a spark chamber with vidicon readout, an evolution of the spark chamber used previously in the balloon experiments for the study of cosmic-ray electrons in the joint Milan-Saclay program, adapted to operate onboard a satellite despite the potentially strong electromagnetic disturbance.

The manufacture of the experimental apparatus for these complex experiments has been done in cooperation with some Italian factories, which started to open development lines dedicated to space instrumentation. A strict collaboration was necessary between the industry personnel and the research personnel because of the complementarity of the scientific-technical competence. The formal agreement was stated by signing an industrial contract between CNR and industry. But the contracts, financed by PSN (“Piano Spaziale Nazionale” of CNR), had to be managed with enough flexibility by the scientific group, who had the scientific competence for the validation of the project. The slowness of the bureaucracy of CNR did endanger these programs. It was then necessary to find a new way of managing the cooperation with industry, by assigning more autonomy to

the scientific groups. Again Beppo, Amaldi and Puppi acted together on the CNR management to modify the situation. It was finally agreed to upgrade the scientific bodies to Istituti of CNR, with increased management autonomy. This happened in 1972. In Milan the Laboratorio became IFCTR (Istituto di Fisica Cosmica e Tecnologie Relative). Obviously, Beppo was appointed as Director of the new Istituto.

The S133 project onboard TD-1 did demonstrate that it was possible to install a spark chamber onboard a spacecraft, even if it suddenly discharged a high electrical power: it was possible to control the radiated power. Moreover S133 had indicated the need of a sensitive area of at least 1000 cm^2 to measure gamma rays and a sophisticated triggering system to select the good events and to limit background contamination at a reasonable level. The complexity of the detection system suggested the choice of a dedicated satellite, with a single experiment fully oriented to the gamma-ray study. Beppo was the originator of such a mission. Together with the French and German colleagues, who participated in S133, he succeeded to convince Dutch and British colleagues to present a joint proposal to ESRO. According to the rules of ESRO, a special project was selected, financed by the countries involved in the proposal. Finally in 1969, the COS-B project was approved. Within ESRO a Steering Committee was appointed to monitor the COS-B project evolution. Within this committee Beppo gave a substantial contribution to the project, as it has been recognized by all the participating scientists, in the whole development period up to launch in summer 1975. As an example, Beppo forced ESRO/ESA to fly the first development model of COS-B onboard a stratospheric balloon in the Earth atmosphere, to verify and eventually ameliorate the event triggering system; but especially to directly evaluate the effect of the cosmic-ray background, to be ready in time to the flight data processing in an efficient way and to ensure the best result of the mission.

In July 1975, about few weeks prior to the launch of COS-B, Beppo suddenly leaves the Direction of IFCTR for very personal reasons.

At the end Beppo was quite happy that the cooperation between the research institutes of CNR and the Italian space industry had originated a significant bulk of technical and scientific competence: on this basis the Italian institutes were in the condition to participate in important space research missions in large European collaborations. For sure, it is mainly due to Beppo that in Italy, initially in the CNR Institutes, the scientific and technical knowledge was born and has grown up to such a level that can make Italy compete well in the international community of space research.

Brief summary of the LFCTR/IFCTR history

GABRIELE E. VILLA

IASF-INAF, Milano, Italy

1. – Introduction

The CNR (Consiglio Nazionale delle Ricerche) was founded in 1923. Its task was to organize and finance researches in various fields of science. During the years, few institutions, created within CNR to organize the activities in specific fields, became independent, like INFN, Istituto Nazionale di Fisica Nucleare, and ENEA (previously CNEN), Ente Nazionale per l'Energia Atomica (now Ente Nazionale Energie Alternative).

Until 1963 CNR was essentially an administrative body, which financed various projects. The few CNR researchers usually worked in groups based in universities.

2. – The origins

Before 1963 the research in astrophysics and cosmic physics was carried out in university groups, like the Gruppo Raggi Cosmici in Milan, BoRiSpa (Bologna Ricerche Spaziali) in Bologna and Gruppo Astrofisica Spaziale in Rome.

In 1962-3 CNR created GIFCO (Gruppo Italiano Fisica Cosmica) and GNA (Gruppo Nazionale Astronomia), with the aim to promote and coordinate the research activities in the field of cosmic physics. The promoters of this initiative were profs. E. Amaldi, C. Castagnoli, G. Puppi and G. Occhialini. Sections of GIFCO were established in Turin, Milan, Bologna and Rome, under the directorships of profs. Castagnoli (Turin) Occhialini (Milan), Brini (Bologna), Gratton and Amaldi (Rome).

With the injection of new researchers and fellowships, an intense activity in space research started, particularly in the framework of ESRO (now ESA) and NASA programs.

This activity required a substantial amount of funds and a clear and stable institutional framework. In 1969 CNR allocated funds and structures, transforming GIFCO sections in CNR laboratories, each with a consolidated annual budget, its own scientific staff and structures: Laboratorio di Cosmogeofisica in Turin, Laboratorio di Fisica Cosmica e Tecnologie Relative (LFCTR) in Milan; Laboratorio Tecnologie e Studio delle Radiazioni Extraterrestri (LTesRE) in Bologna, Laboratorio Astrofisica Spaziale (IAS) and Laboratorio Fisica Spazio Interstellare (IFSI) in Rome. Directors were, respectively, C. Castagnoli, G. Occhialini, D. Brini, V. Castellani and E. Amaldi. Milan had also a section in Palermo with L. Scarsi as the responsible; this section became, in 1981, an independent laboratory, the Laboratorio di Fisica Cosmica con Applicazioni di Informatica (LFCAI).

Few years later the Laboratories became Istituti, with a different legal configuration and direct contracts by Servizio Attività Spaziali of CNR (SAS) which later became Piano Spaziale Nazionale and then ASI (Agenzia Spaziale Italiana).

3. – The LFCTR/IFCTR

The newly created LFCTR under the directorship of G. Occhialini, was an aggregation of expert researchers: Connie Dilworth, Giuliano Boella, Oberto Citterio, Gianni Degli Antoni, Cesare Perola, Livio Scarsi, Giorgio Sironi and technicians, all coming from the University and INFN, and a group of young researchers with very few years of experience in space research: Giovanni Bignami, Anna Della Ventura, Gianna Cioni, Giancarlo Conti, Paolo Cortellessa, Paolo Di Benedetto, Dario Maccagni, Laura Maraschi, Francesco Perotti, Enrico Tanzi, Massimo Tarenghi, Aldo Treves, Gabriele Villa.

The activity in the years after 1969 was frenetic, there were quite a number of projects both in stratospheric balloons and in Satellite which were keeping everybody very busy.

Since 1965, people from abroad were joining the Laboratorio, such as John Bland, Tony Dean, Constantinos Paizis and Martin Turner. The atmosphere was of sheer enthusiasm, the Project office led by Renato Ballerini with Andrea Bussini and Alberto Conte, the mechanical workshop led by Edoardo Bardeggia with Enrico Mattaini, Pier Luigi Dell'Era, Giuseppe Aloardi, Augusto Solazzi and Alberto Carzaniga, the electronics workshop led by Giuliano Boella with Aldo Igiuni, Piero Inzani, Ernesto Franchini, Bruno Falconi, Giulio Barbaglia and Alberto Rancati, were constantly under pressure, and they were producing not only components but also entire experiments to be flown in balloons or spacecrafts or for astronomical observations from the ground.

In 1967 Livio Scarsi moved to Palermo at the chair of Fisica Superiore at the Faculty of Science. He brought to Palermo the attitude and the interest towards the experiments and the technological developments present in the Milan group founded by Giuseppe Occhialini, and when in 1969 the CNR created LFCTR, a section was set up in Palermo under the responsibility of Livio Scarsi. At the origin the Palermo group was formed by a small number of young researchers: Gaetano Gerardi, Arturo Russo, Rosolino Buccheri, Bruno Sacco and technicians: Gaetano Agnetta, Renato di Raffaele, Guido Vizzini, for the electronics and mechanical workshops and for the data handling department.

Until 1981 the Palermo group was formally a section of the Laboratorio di Fisica Cosmica e Tecnologie Relative of Milan. The Palermo section of LFCTR contributed to most of the missions which were going on like COS-B, EXOSAT, Spacelab-1, Beppo-SAX and balloon gamma-ray telescope in collaboration with the French group of SAP CEN of Saclay. Then in 1981 the Palermo section became the IFCAI.

The experiments carried out in those years by LFCTR/IFCTR were of two kinds, units to be flown in stratospheric balloons and units for the scientific satellites. They were produced with a big H/W contribution of the Laboratorio/Istituto and as far as the ones in balloons are concerned they were made almost entirely in-house.

In the stratospheric balloon category we had: solar neutrons, cosmic positrons, Gamma-10, Dotty (X-rays), MISO Gamma, FRAMISO (X-rays), X-Pallas; some of these experiments were dedicated not only to scientific observations but also to test new technologies for future satellite applications.

In the second category, as far as the satellite experiments are concerned we had: S79 (ESRO HEOS A-1, 1968—cosmic electrons), S209 (ESRO HEOS A-2, 1972—cosmic electrons), S88 (ESRO TD-1, 1972—solar gamma rays), S133 (ESRO TD1, 1972—celestial gamma rays), COS-B (ESA, 1975—gamma rays). The last one was a dedicated mission to explore the Universe in the high-energy gamma-ray domain and was the first of the big ESA Observatories.

Some of these missions were going on almost in parallel, all the people were extremely busy, all the workshops were at the limit of their capacity and a lot of pressure was also on the Data Analysis Group led by Gianni Degli Antoni with Gianna Cioni, Anna Della Ventura, Piero Musso, Gico Sechi.

A parallel line of theoretical and observational studies led by Cesare Perola, Laura Maraschi, Aldo Treves was well established in collaboration with the Department of Physics at the University and other young scientists made their thesis in the Istituto and joined the group, like Marco Bersanelli, Patrizia Caraveo, Lucio Chiappetti, Bianca Garilli, Giuseppe Gavazzi, Paolo Giommi, Tommaso Maccacaro, Sandro Mereghetti.

In those very years there was a major involvement of Enrico Tanzi and his group on the long-lived mission IUE and in the collaboration with the IKI Institute in the effort of keeping alive the mission Spectrum UV.

In 1980 the European Southern Observatory commissioned to IFCTR the realization of a prototype to demonstrate the feasibility of the active control for the primary mirror of the 3.6 meter new technology telescope. Citterio, Conti and Mattaini built a support with 78 mechanical actuators to correct the shape of a thin (18 mm) 1 meter diameter mirror. The project was completed successfully and this technology was transferred to the NTT telescope which is operational since 1989 at La Silla observatory in Chile. The nineties saw the beginning of an important observational activity using the ESO telescopes (participation to the ESP, ESO Slice Project, a redshift survey of a “fair sample” of what is now considered the local universe). From this survey the VIRMOS project stemmed in 1995. This French-Italian collaboration (co-I D. Maccagni) proposed and built a multi-object imaging spectrograph operating at visible wavelengths for the VLT, VIMOS, which became operational in Paranal in 2002, and is conducting with it a

redshift survey (VVDS, VIMOS VLT Deep Survey) aimed at studying the evolution of the galaxy properties since the Universe had 10% of its present age.

In the VIRMOS project, which originally also included a multi-object spectrograph operating in the near IR, IFCTR was responsible for the MMU (Mask Manufacturing Unit), into which L. Chiappetti, G. Conti, E. Mattaini and E. Santambrogio put all their competences and capabilities, and for the Instrument, Observational and Data Analysis Software, designed and implemented by a multinational group led by B. Garilli. The VVDS is still on-going and is producing important results on the high-redshift universe with the contribution of about ten IASF Milan researchers, many of which are young post-docs.

At the beginning of the '80s, one of the Institute activity consisted in the observations of the Cosmic Microwave Background (CMB) thanks to the work of Giorgio Sironi and to the group of young scientists he created. In the period 1982-1992, the CMB group of "IFCTR" performed a number of observations of the low-frequency cosmic-microwave background from a number of sites, among which Alpe Gera (Sondrio), Campo Imperatore, White Mountain (California) and the Amundsen-Scott Base at the South Pole, always in collaboration with the Dipartimento di Fisica group and the Lawrence Berkeley Laboratory group.

Then, at the beginning of the '90s, the CMB group at the "Istituto Occhialini" gave its contribution to the proposal and development of the Planck mission that later became an ESA mission in 1996. Still today, the development of the Planck mission is one of the Institute's activities in collaboration with the Marco Bersanelli group at the Department of Physics.

From the '80s to the present days there has been a great development of the observational/interpretative activities based on Guest Observer programs of satellite missions, mostly in high energy: Einstein Observatory (NASA), EXOSAT (ESA), Rosat (DARA), ASCA (Japan-USA), Rossi-XTE (NASA), but also with the Hubble Space Telescope and ground-based optical and radio observatories. One of the major topics, stemmed from the COS-B results on radio pulsars and unidentified gamma-ray sources, is the study of galactic compact objects containing neutron stars and black holes (Bignami, Caraveo, Mereghetti, plus many graduate and PhD students who were then forced to continue their career abroad, due to the CNR questionable and short-sighted recruitment policy). In particular, important results have been obtained thanks to multiwavelength studies of isolated neutron stars-optical/X-ray counterparts, proper motion and parallax of Geminga, discovery of other radio quiet neutron stars, recognition of the new class of anomalous X-ray pulsars. The latter are now believed to be the neutron stars with the highest magnetic fields (10^{15} Gauss) observed in the Universe.

In 1980 Gico Sechi started the Computing Science Research Group, whose aim was to implement *in situ* abilities and competences of systems that could be used by the institute in future missions. In the following years an intense activity of research and didactics was done, national and international collaborations with universities and research groups were activated, research contracts with ASI and ESA were obtained and technological transfer activities towards industries like ST-Microelectronics were done. For over ten years the

group has been developing an intense research activity on the design of computing systems for scientific applications. More recently, the interest has been focused on the study of high-performance fault-tolerant computing systems for space applications and the exploitation of programmable logic devices, specifically SRAM-based Field Programmable Gate Arrays (FPGAs), which have been chosen as reference implementation technology.

In the period 1980-90 we had also three important missions:

Exosat (ESA, 1983), the first X-ray mission with some imaging capability. IFCTR was responsible for GSPC (Gas Scintillator Proportional Counter); the experiment was developed in collaboration with the Mullard Space Science Laboratory and the Space Science Department of ESA. Our contribution was particularly in the area of testing, integration and calibrations.

Spacelab-1 (ESA/NASA, 1983), on this Shuttle mission (STS-9) there was a prototype of the EXOSAT GSPC which was due to be flown years before EXOSAT but, due to the Challenger disaster, it was flown in the same year. During the 11 day flight, the GSPC, which was mounted on the external pallet and pointed vertically with respect to the flight vector, performed drift scans across the sky obtaining a detailed map of the X-ray BKGND at the Shuttle altitude.

Zebra (1990), the very first prototype of an imaging gamma-ray telescope using position-sensitive NaI bars, a pseudo random mask and a three-axis pointing. The first and only Zebra flight was not successful due to problems with the stratospheric balloon and the pointing system but the technology developed for this mission paved the way to the definition of other space missions like LEGRIT and GRASP.

But after this exciting period the recession was behind the corner, funds were not available any more with the same easiness and at the same level, the “istituto” had to leave the university (1984), INFN and university people could not follow the “istituto” in the new location and we lost them, they were the eldest and consequently the most experienced scientists and technicians of the institute. The mechanical workshop and the electronics workshop due to the big loss of personnel, had a very harsh time to continue the development of space H/W and sometimes industries were involved in what once was our field, the development and construction of flight hardware. The space missions were also becoming more expensive, more demanding in terms of resources, weight, power, dimensions, complexity, electronics and S/W, sometimes far behind the then limited resources of the institute but, with the injection of young personnel through ASI contracts, the institute was still able to be involved in the most important ESA missions.

We are now near the end of the '80s and the beginning of the '90s, apart from the development of the Zebra experiment, which has been a test-bed for the space mission GRASP (which in turn generated INTEGRAL), we are now in the new space era of the big space observatories and the IFCTR is at the front line with XMM and INTEGRAL even if our participation has been without a major in-house H/W involvement. A big departure from this trend has been represented by the Beppo-SAX (1996) ASI mission. For Beppo-SAX, IFCTR, in collaboration with ITESRE and IFCAI, developed, in-house, a new technology. We produced the first electroformed shells to demonstrate the feasibility of

nickel mirrors and their replica from a super-polished mandrel. A complete prototype of one telescope was produced, the mounting and fixing technology was demonstrated for the concentric shells, environmental testings were performed to demonstrate the capability to survive the launch, scientific calibrations were organized and the programs for data analysis were produced. Finally all the technology developed by IFCTR was transferred to the industry to produce the flight model (a good example of technology transfer from research to industry). This mirror technology was then used by industry to produce the mirrors of the X-ray telescope for the Jet-X mission and for the ESA XMM-Newton X-ray Observatory. Due to problems with the Russian partner, the mission Jet-X was never flown and the X-ray mirror module has been used for the SWIFT mission.

Beppo-SAX has provided fundamental contributions to the study of various classes of celestial sources. The most important result, the one that will go down in history, is the identification of gamma-ray bursts. Thanks to the combination of the GRB monitor and the WFC on board Beppo-SAX it has been possible to scan a sizeable fraction of the whole sky and to localize GRB with a few tens of arcmin precision. Rapid re-pointing of the narrow field instruments, allowed to achieve a position of about 1 arcmin within ten, twenty hours from the burst. Follow-up observations with large ground- and space-based optical telescopes provided the first redshift measurement of GRB hosts. These measurements proved beyond any reasonable doubt that GRBs are of extra-galactic origin, thereby putting an end to a twenty year long controversy. Beppo-SAX was also responsible for other important contributions which regrettably did not enjoy the same exposure. Amongst these are the broad-band characterizations of nearby AGN which allowed to constrain physical models for these objects; the extragalactic surveys, which thanks to the low background and good PSF of the MECS experiment, allowed to resolve 30% of the cosmic background into sources, mostly AGNs. Finally one should not forget the observation of galaxy clusters which provided measurements of temperature and metal abundance profiles of the intra-cluster medium at a better resolution than those obtained with Beppo-SAX's competitor ASCA.

For XMM, an Italian P. I. (Principal Investigator), G. F. Bignami (IFCTR), was selected by ESA as responsible for the European Photon Imaging Camera (EPIC). A big team of scientists, engineers and technicians was assembled in the Institute, it was named the EPIC System Team (EST). EST was responsible for the management of the EPIC Collaboration composed by 10 scientific Institutes in four nations, Italy, Germany, France and UK. The main tasks EST had to perform were the production control of the Data Handling Electronics, the AIV (Assembly Integration Verification) of all the EPIC experiment, the P. A. (Product Assurance), the Q. A. (Quality Assurance) of all the equipment produced by the collaboration, the Calibrations and the final delivery to ESA. From September 1988 (A. O., Announcement of Opportunity) to 10 December 1999 (launch of XMM), EST had full control of the activity and succeeded in the delivery of the experiment to ESA on-schedule for the launch. During the assessment and calibration phases in-orbit, EST gave support to ESA to trim the experiment's parameters and reach the best configuration for the X-ray sources observations. A reduced EST team is still in operation for the on-board S/W maintenance.

An offspring of the Zebra project, consolidated in the collaboration with Southampton and with the French group on SIGMA/Granat, is the experience on imaging in gamma rays with coded mask telescopes. This was crucial for the GRASP proposal and later for the participation to INTEGRAL (2002), where one of the most important contributions of IFCCTR has been the system for the rapid localization of the INTEGRAL gamma-ray bursts. This has been the first to distribute GRB coordinates with accuracy of 2-3 arcmin within 30 seconds from the GRB occurrence, allowing for the first time the kind of rapid follow-ups now routinely achieved with Swift.

AGILE (1997). The space program *AGILE* (Astro-rivelatore Gamma a Immagini LEggero) was proposed in June 1997 to ASI within the limits of the Program for Small Scientific Missions as a powerful and cost-effective space mission dedicated to gamma-ray astrophysics (30 MeV–50 GeV). The mission was selected in December 1997 for a Phase A study that ended in October 1998. Subsequently, ASI selected (June 1999) *AGILE* as the first satellite of the Program of Small Scientific Missions. The Mission is a cornerstone of the ASI Piano Spaziale Nazionale formulated in mid-2002 and approved by the Italian Ministry of Research. *AGILE* is currently in Phase C/D: the planned launch period is the first half of 2006.

The *AGILE* instrument is designed to detect and image photons in the 30 MeV–50 GeV and 15–45 keV energy bands, with excellent spatial resolution, timing capability, and an unprecedentedly large field of view covering 1/5 of the entire sky at energies above 30 MeV. Primary scientific goals include the study of AGNs, gamma-ray bursts, galactic sources, unidentified gamma-ray sources, diffuse galactic gamma-ray emission, high-precision timing studies, and quantum gravity testing. *AGILE* is planned to be operational during the year 2006, and will be the only mission entirely dedicated to high-energy astrophysics above 30 MeV to support multiwavelength studies during the period 2006-2008.

IFCCTR has a leading role in the *AGILE* Mission. It was the institute coordinating both scientific and technical activities of all scientific institutes (INFN and universities) involved in the mission. Moreover, it hosted the *AGILE* System Team which coordinated technical activities of both scientific institutes and industrial partners. All this activity has been carried out in close collaboration and coordination with the national industry which developed an excellent level of technology and experience.

4. – Conclusions

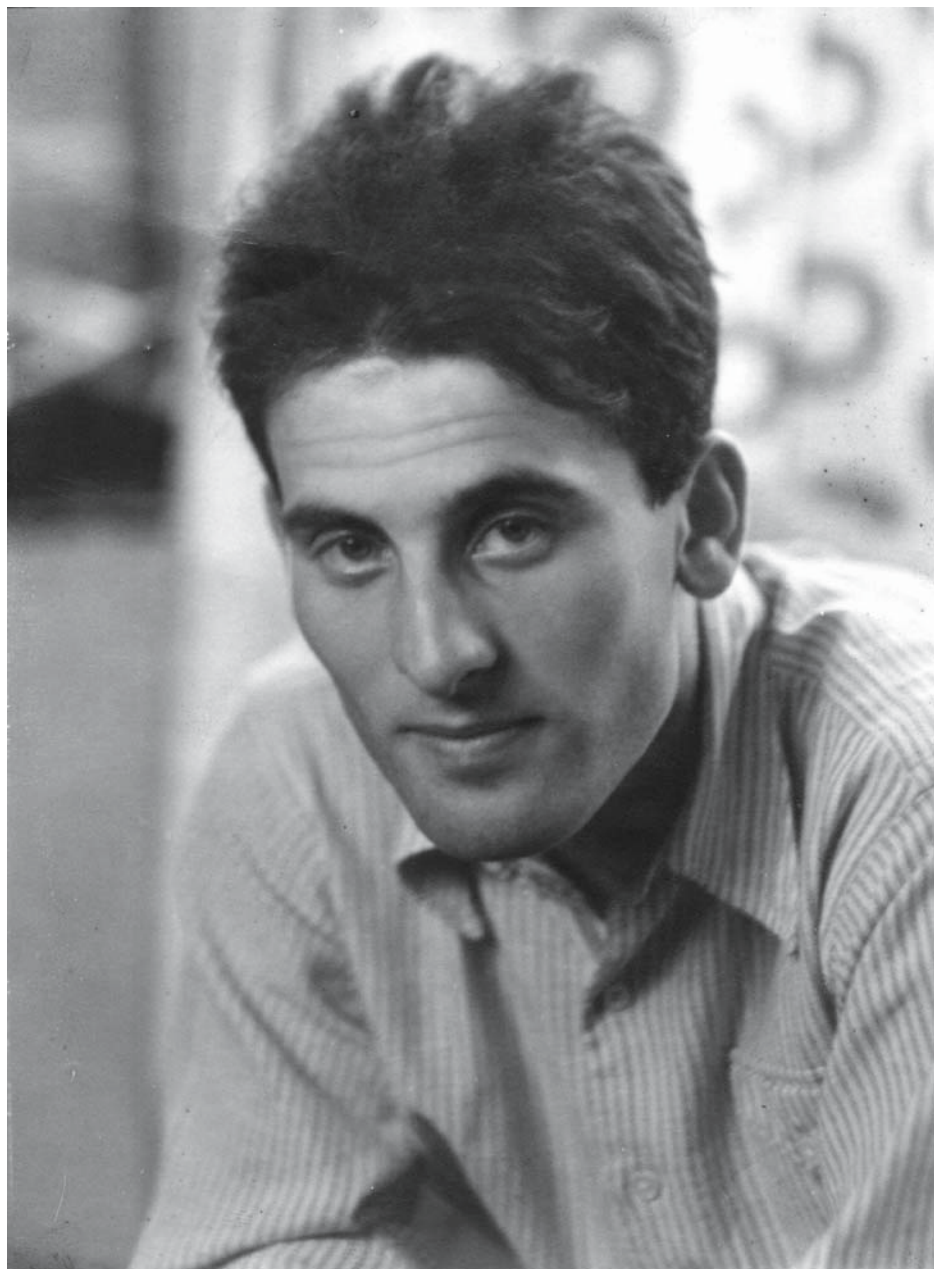
Coming back to the LFCCTR/IFCCTR history and to Giuseppe Occhialini, he held the position of Director from 1969 to 1976, then the position was taken by G. Boella (1976-1994), by E. G. Tanzi (1994-1998) and finally by G. E. Villa (1998-2005). In 1992 the Institute was dedicated to G. Occhialini changing the name from IFCCTR to IFC-G.Occhialini.

On January 2002, due to a major CNR reform, the four CNR Astrophysical Institutes in Milan (IFC), Bologna (ITESRE), Rome (IAS) and Palermo (IFCAI) became a single

Institute, the IASF, with headquarters in Rome and three sections (Milan, Bologna and Palermo).

Then on January 2003, the Minister of Research decided to restructure all the research institutes: IASF left the CNR together with IFSI (Istituto Fisica Spazio Interplanetario) and IRA (Istituto Radio Astronomia) and joined the twelve astronomical observatories in the new INAF (Istituto Nazionale AstroFisica). On 31 August 2005 INAF decided to restructure its research structures, IASF was separated again into its four original components and now the “old” G. Occhialini Institute in Milan is again an independent institute with a new Director, D. Maccagni.

Notwithstanding all those changes of names, directors and affiliations, the “spirit” of the old LFCTR has remained in the “surviving” researchers and technicians of the old times and they tried and are trying (quite successfully) to transmit it to the young generation.

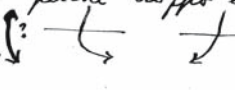
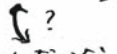


A young "Peppino" Occhialini (circa 1925).

9 marzo 1933

Carissimo babbo -

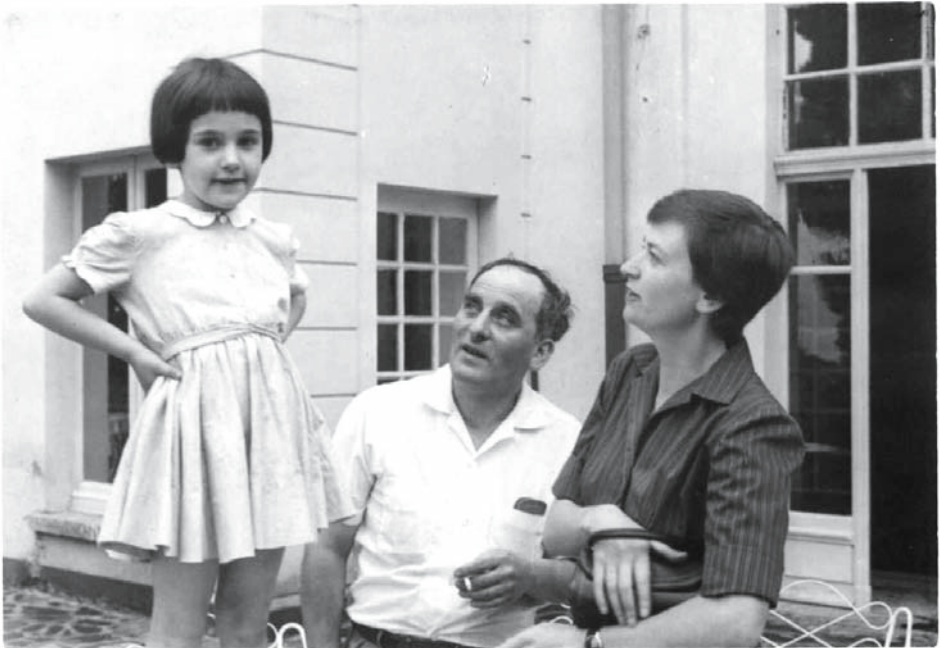
Ho ricevuto il clauaro - Tante grazie
 Ho spedito oggi 4 scatole di lastre due
 contengono fotografie di CTR Wilson, una
 fotografie di collisioni con neutrons, una fot.
 di cosmic rays. Sono 9 e altre 12 le manderò
 domani alcune non pubblicate - Serviranno per
 la tua conferenza - Qui ho dovuto tante volte
 ripetere le stesse cose che ormai sono so la lezione
 a memoria perciò l'ordine che vorrei consigliare è
 questo: Esperienze di Skobelzyn, Anderson,
 Kunze - Efficienza $1/50$ fotografie - Bisogno di
 una eff. maggiore - Difficoltà - 9 mesi di tenta-
 tive; Appareato - fotografie - Fotografie che mostrano
 i raggi cosmici ^{1 ogni 2 minuti} - le più semplici - Particelle
 scaraventate all'indietro evidenza di due fotografie
 (venanno mandate). Metodi per riconoscere
 nuova carica - ionizzazione — non m.H.?

Deviazione → 1 deviazione in campo magnetico
 2 Perdita energia - in gas no perché troppo energ-
 tiche - meno una lastra piombo - 
 3 Punto radiante 
 4 secondari ^{diver} 
 dovrebbe essere 90 senza relatività ma quando la massa
 delle part primarie diventa → ∞ → ∞  esempio
 fotograf

Queste tre evidenze sono contenute in fotografie
e conducono a confermare ipotesi di Anderson.
Mostrare tabella ionizzazione protone ed elettrone.
Noi arriviamo a separare elettrone da protone
con sicurezza fino a $100 \cdot 10^6$ dalla curvatura
e l'ionizzazione del protone a $100 \cdot 10^6$ è
tale che non è possibile di sbagliarsi -
Mostrare l'evidenza di qualche secondario
che è nella camera e che mostra una
ionizzazione assai più grande di quella
di elettroni veloci - ha deficienza di
ionizzazione nelle tracce curve così che
non è così istintuale. Mostrare a prova
anche la fotografia con i due protoni -
Disintegrazione Cu - Pb -
- Sembra che anche la sorgente
di Neutr. dia elettroni positivi ma per
carità non lo dire ancora a nessuno
Baci

Peppino

Aspects of Beppo way of life



Beppo Occhialini with his wife Connie Dilworth and their daughter (Connie Pooh).



Connie Dilworth (1924-2004). Beppo's wife collaborated actively with Beppo and played an important role in stimulating astrophysical interests among components of the Milano Cosmic Ray Group.



Beppo and Connie using unconventional tools to carry on preliminary test for a new experimental solutions (Bristol, 1947).

Participants to the 8th Solvay Institut Conference (Bruxelles 27/9 - 2/10 1948).



Front line : J.D. Cockroft, M.A. Tonnelat, E. Schrödinger, O.W. Richardson, N. Bohr, W. Pauli, L. Bragg, L. Meitner, P.A.M. Dirac, H.A. Kramers, Th. De Donner, W. Heitler, E. Verschaffelt.

Second line: P. Scherrer, E. Stahel, O. Klein, P.M.S. Blackett, P.I. Dee, F. Bloch, O.R. Frisch, R.E. Peierls, H.S. Bhabha, J.R. Oppenheimer, G.P.S. Occhialini, C.F. Powell, H.B.G. Casimir, M. de Hemptinne.

Third line: P. Kipfer, P. Auger, F. Perrin, R. Serber, L. Rosenfeld, B. Ferretti, C. Moller, L. Leprince-Ringuet.

Back line: G. Balasse, L. Flamache, L. Groven, O. Goche, M. Demeur, J. Errera, Van Isacker, L. Van Hove, E. Teller, Y. Goldschmidt, L. Marton, C.C. Dilworth, I. Prigogine, J. Geheniau, E. Henriot, M. Vanstyvendael.



Beppo in Milano in 1950 with collaborators. Left to right: Beppo, Riccardo Levi-Setti, Livio Scarsi, Bice Locatelli and Alberto Bonetti.

— ECO galleria —



L'ECO DELLA GALASSIA

PERIODICO DI INFORMAZIONE DEL GRUPPO SPAZIO

Anno I - N. 2

6 dicembre 1968

• UNICUIQUE SUUM •

EDIZIONE STRAORDINARIA

GRANDIOSO SUCCESSO DEL GRUPPO SPAZIO: S 79 IN ORBITA

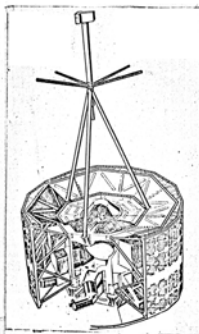
Cape Kennedy, 6 dicembre

E' STATO UN TRIONFO! UNA BELLE PREVISIONE, COME PER TUTTE LE IMPRESE DEL GRUPPO SPAZIO che non siano sotto la direzione di Di Benedetto e Polais, ma questo nulla toglie alla bellezza dell'impresa e alla legittima soddisfazione di tutti i componenti del più prestigioso gruppo dell'Istituto (il Gruppo Spazio appunto, per chiarire).

Dopo un interminabile mese di collaudi a terra, strabilmente presieduti dall'italo-giuliano Mario MacCagny, ieri alle ore 20.00 un possente Thor-Delta Abarth ha portato in orbita il satellite Heos-A, che portava nel suo grembo la più importante esperienza di Fisica Cosmica di tutta la storia dell'umanità S 79, per la quale stuoli di tecnici si sono sacrificati in propria gioventù in folli notti sulle sale degli acceleratori di tutta Europa. E' ancora presto per dirlo, ma i risultati di questa impresa saranno certamente tali per cui non noi, ma solo la Storia potrà essere degno giudice. A noi resti solo l'ineffabile gioia di poter un giorno raccontare ai nipotini raccolti intorno al focolare che anche un giornaliero e un avventuriero possono contribuire al progresso dell'umanità.

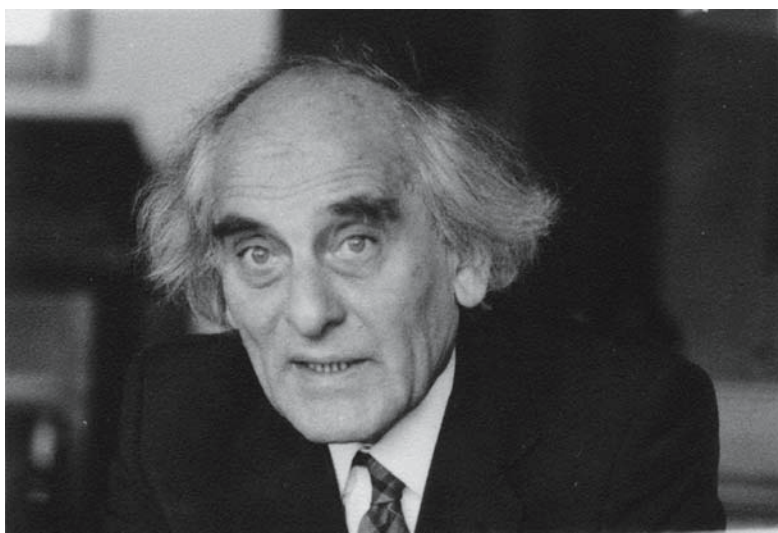
(Vedere a pag. 2 le reazioni da tutto il mondo alla notizia dell'impresa)

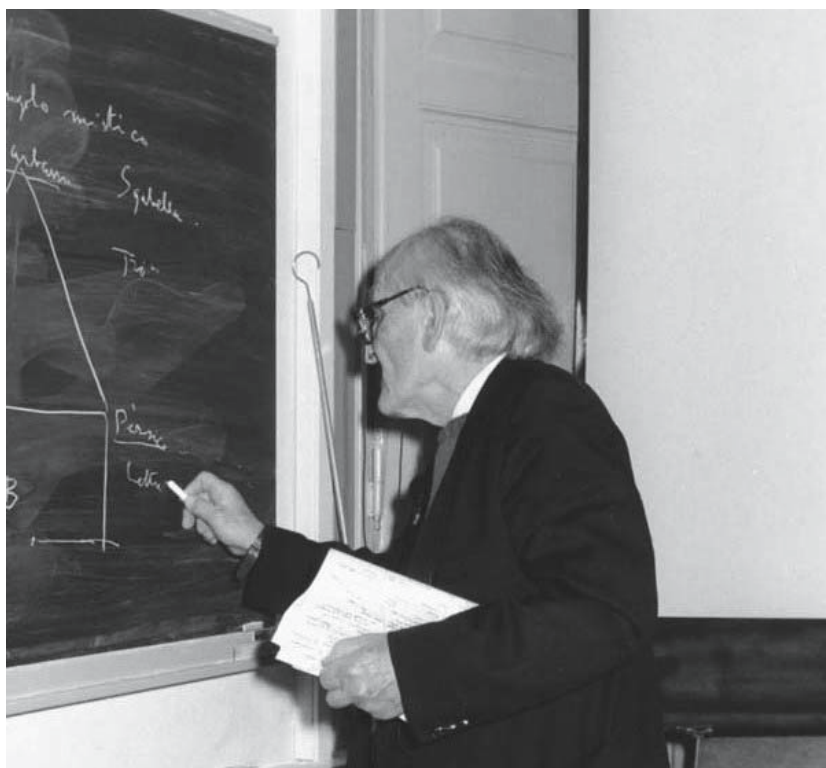
IL SATELLITE



Il satellite Heos-A: nella foto è chiaramente invisibile l'esperienza S 79

Beppo in a cartoon from "L'eco della Galassia" of December 6th 1968, a playful newsletter prepared by technicians and young researchers of Milano Gruppo Spazio, commenting the successful launch of the space experiment S-79 on the ESRO satellite Heos-A.





Beppe Occhialini in Arcetri at celebrations for his 80th Birthday in 1987.

**PRESENT DAY PERSPECTIVES IN RESEARCH FIELDS
PIONEERED BY OCCHIALINI**

Are diamonds for ever, or do protons decay?

A tale of the unexpected^(*)

DON H. PERKINS

Department of Particle Physics, Denys Wilkinson Building - University of Oxford, UK

1. – Baryon conservation

Our picture of the universe, and of how it developed from the Big Bang some 14 billion years ago, is based on the idea that the everyday matter with which we are familiar is eternal and everlasting. It does not spontaneously appear or disappear. This permanence of matter was given quantitative expression, first by Weyl, and then by Stueckelberg in 1938 and Wigner in 1949. They postulated a conserved “heavy charge”, or what today is called baryon number. The word “*baryon*”, introduced by Pais refers, as the name implies, to a heavy particle (a fermion of spin angular momentum $1/2$, in units of $\hbar/2\pi$) such as a proton or neutron in the atomic nucleus. A proton or neutron is assigned a baryon number $B = +1$, while their antiparticles, the antiproton and antineutron, which have exactly the same mass as the corresponding particle but the opposite sign of the electric charge and magnetic moment, are assigned $B = -1$. Then the law of baryon conservation states that the total baryon number $\sum B$, namely the *difference* in the numbers of baryons and antibaryons, is constant. For example, the total baryon number of our universe is about 10^{79} .

So, although baryons are conserved, they *can* be created or destroyed, but always as a particle-antiparticle pair—for example, a proton-antiproton pair—with net baryon number of zero, and never singly. Many heavier types of baryon are known and can be created as pairs at high-energy accelerators, but they are all highly unstable and decay

(*) Written version of the Wolfgang-Paul lecture given at the University of Bonn on 26/10/04.

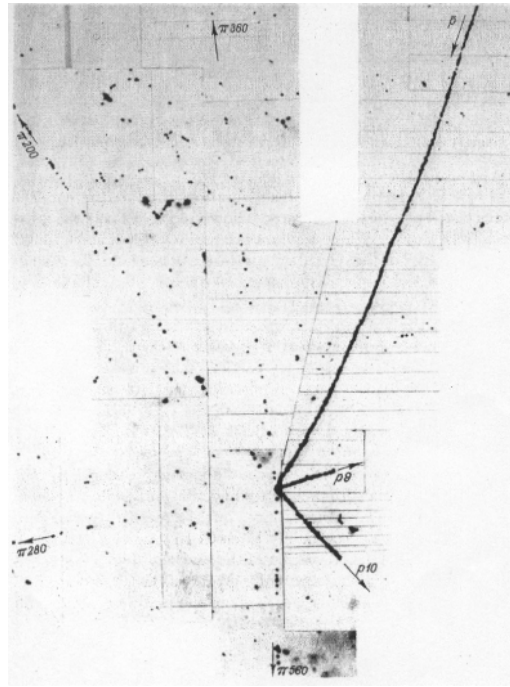


Fig. 1. – Example of the annihilation of an antiproton in nuclear emulsion, with the production of four charged pions producing tracks of minimum ionisation.

very rapidly to protons or neutrons. Figure 1 shows an example of the annihilation of an antiproton, taken in 1955 at the Bevatron, the first accelerator with enough energy to create proton-antiproton pairs, at the Lawrence Radiation Laboratory at Berkeley, California. It employs the technology of the nuclear emulsion, which had of course been developed more than 10 years before by Occhialini and Powell, among others. The track of the antiproton is shown in emulsion, as it slows down and finally annihilates with a proton in a nucleus. The annihilation energy, $2Mc^2$ (where M is the proton mass) appears in the form of lighter particles which are created.

Four charged pions appear, with total energy of about $1.5Mc^2$. In addition neutral pions, which leave no tracks, will also be created and account for the remaining annihilation energy.

The other constituents of matter are the light particles, called *leptons*, of which examples are electrons and neutrinos. Leptons are also conserved. For example, electrons carry a lepton number $L = +1$ and the antiparticle, the positron, has $L = -1$. Again leptons can be created, as particle-antiparticle pairs. The first evidence for e^+e^- pair creation in cosmic-ray experiments with cloud chambers was found in 1933 by Blackett and Occhialini at Cambridge. A converse process is the annihilation of a positron-electron pair

to gamma radiation, and this has practical medical diagnostic applications, in positron emission tomography (PET).

How good is the law of baryon conservation? Many years ago, Maurice Goldhaber of the Brookhaven National Laboratory pointed out that the existence of life alone sets stringent limits on matter stability. For, if baryons are not conserved, a proton for example should be able to decay, and the products of this decay, such as pions, positrons or gamma rays will cause radiation damage. Consider one gram of your body tissue. It contains about $3 \cdot 10^{23}$ protons (half of Avogadro's number). When and if a proton decays, the mass energy $Mc^2 = 938$ MeV will appear in the form of decay products, and much of that energy will cause cell damage. To pick a number, suppose that the mean lifetime of the proton is 10^{16} years. Then in one year, the radiation damage per gram will be of order $3 \cdot 10^{23} \cdot 1000/10^{16}$ MeV ~ 500 rads (or 5 grays or 5 Joules per kg). This is about one thousand times the natural dose of radiation of about 0.3 rads per year, coming from radioactivity in the ground and from cosmic rays. Such a high level of radiation would certainly lead to extinction of all advanced life forms on Earth.

So our very existence shows that the proton lifetime is at least one million time longer than the age of our universe (14 billion years). It is reasonable therefore that when you ask a jeweller what is the best thing to buy, he will usually recommend a diamond, because "a diamond is for ever". But he is only telling you half the truth. Not just diamonds, but *everything* is for ever, if indeed you regard such immensely long times as a good approximation to eternity. However a physicist would not agree with that: there are many radioactive isotopes with much longer, but finite and measured mean lifetimes, up to at least 10^{23} years. Nevertheless it seems that the limit from life would seem to be sufficiently long that, from the point of view of the development of the universe over only about 10^{10} years, we could forget about possible matter instability. So what is the problem?

1.1. Problems with the Big Bang. – The Big Bang model of the development of the universe postulates that it commenced as a tiny fireball of almost infinite energy density, which expanded and cooled, and is still expanding today. At a very early stage of that expansion, all possible types of elementary particle would have been created as pairs by the radiation, and be in thermal equilibrium with it. For example, proton-antiproton pairs would have been created by, and annihilated to, gamma radiation, according to the reversible reaction



Provided the mean thermal energy was large compared with the particle mass energy, that is $kT \gg Mc^2$ (here T is the absolute temperature and k is Boltzmann's constant) the numbers of protons and antiprotons would have been comparable with the number of gamma rays (to within numerical factors of the order of unity, to do with spin multiplicity). But as the universe expands and cools, eventually one reaches the point where $kT \ll Mc^2$. Then the rate of the back reaction starts to drop sharply, since it is only those gamma rays of $E_\gamma \gg kT$ in the high-energy tail of the Planck distribution which

have enough energy to create a proton-antiproton pair. Likewise, as the temperature decreases, the density $N(p)$ of protons and antiprotons in the Fermi-Dirac distribution falls off very rapidly, because it includes a factor $\exp[-Mc^2/kT]$. Hence the annihilation rate $\Gamma = \sigma v N^2(p)$, where σ is the annihilation cross-section and v is the relative velocity, falls off exponentially as T decreases. This has to be compared with the expansion rate given by the Hubble parameter H , which would also be decreasing, but varying only as T^2 in the early universe, dominated by radiation. Inserting the appropriate values for these parameters it is easy to show that $\Gamma < H$, when $kT < 20$ MeV. For lower temperatures than this, thermal equilibrium is lost and the proton and antiproton numbers are “frozen out”, as the probability of annihilation tends to zero. The remnant densities are then calculated to be

$$(2) \quad kT = 20 \text{ MeV}, \quad N(B)/N(\gamma) = N(\bar{B})/N(\gamma) = 10^{-18}.$$

Thus the proton-antiproton annihilation has been nearly, but not quite, complete. Simply because the universe is expanding, this tiny remnant of baryons and antibaryons, only 10^{-18} of the original number, would survive to form the material universe. This Big Bang prediction has to be compared with the actual measurement of the baryon to photon and baryon to antibaryon ratios

$$(3) \quad N(B)/N(\gamma) = 10^{-9}, \quad N(\bar{B})/N(B) < 10^{-4}.$$

Here, the ratio of baryons to photons is known from the observed abundances of ^2H , ^3He , ^4He and ^7Li which were formed in the first few minutes of the Big Bang, when protons and neutrons combined to form these light elements in primordial nucleosynthesis. Of course, some of these light elements get absorbed in the formation of stars and then destroyed in subsequent thermonuclear reactions, but the vast bulk has remained intact and can be measured today. The first stage is the formation of deuterium, according to the reversible reaction



where the deuterium concentration in turn gets “frozen out” at a temperature corresponding to the particular value (3) of the baryon-to-photon ratio. Subsequent fusion reactions lead to the production of helium and lithium. The striking result from (3) is that, contrary to our prediction (2), there seems to be an almost complete absence of antimatter in the universe. This is made apparent for example by studying the primary cosmic radiation, consisting of high-energy nuclei of all the known atoms, from hydrogen to uranium. Figure 2 shows an example of a nucleus of chromium ($Z = 24$), observed in nuclear emulsion flown on a balloon in the stratosphere (again a picture taken from the Occhialini era at Bristol in the late 1940s). The initial track density is typical of a relativistic particle, with ionisation of $(24)^2$ of that of a singly charged particle. As the nucleus slows down, it begins to gather electrons, first into the K -shell when its velocity becomes comparable with that of the K -shell electrons, followed by L -shell electrons and



Fig. 2. — Track of a chromium nucleus of the primary cosmic rays, coming to rest in nuclear emulsion flown in a high-altitude balloon. Notice that the track tapers as the velocity of knock-on electrons decreases as the nucleus slows down, and it also begins to collect successively, first *K*-shell and then *L*-shell, etc. electrons.

so on. The track shows a characteristic taper as both the velocity and the net charge of the ion decreases, and finally comes to a stop, as a chromium atom. However, if this had been an anti-nucleus, it would mean that, at the end, 50 antineutrons and antiprotons would have annihilated with 50 neutrons and protons in the emulsion, leading to an explosion of pion production 50 times as intense as that in fig. 1. Although many thousands of such primary cosmic-ray nuclei have been recorded, such annihilation events have never been seen. These nuclei are in fact messengers from quite distant regions of space. From the amount of fragmentation which they have undergone, in collision with

interstellar matter, we know that they have been in transit for typically 3 or 4 million years, so they can certainly originate from the most distant parts of our own galaxy or indeed from nearby members such as M31 (Andromeda) in the local cluster of galaxies. Nor on wider scales is there any evidence for antimatter, in the form of the intense radiation which would be observed if a galaxy of matter were to pass through a region of antimatter. This pronounced asymmetry between matter and antimatter on the cosmic scale, when all laboratory experiments on Earth indicate an (almost) complete symmetry between matter and antimatter, is indeed still one of the principal unsolved problems in cosmology.

1'2. The Sakharov criteria. – Many years ago, Andrei Sakharov (1967) wrote a seminal paper and in it spelt out the three conditions necessary for such a baryon asymmetry to develop. They were

- a) A new type of interaction which violated baryon number conservation.
- b) A condition of non-equilibrium.
- c) CP and C violation in the interactions concerned.

The first condition is obvious, if one starts off initially, as one assumes, with baryon-antibaryon symmetry in the early universe: the alternative would be to assume an initial baryon number of 10^{79} , which seems very artificial. Of course, in laboratory experiments, there is no direct evidence for such an interaction. The implication is that it must occur at a very early, very hot stage of the universe, at an energy far above what one has been able to achieve in the laboratory. Secondly, the interactions responsible must take place when the system is out of thermal equilibrium. In equilibrium, the density of particles of a particular type depends only on the temperature and the mass. The famous CPT theorem states that all interactions are invariant under the combined operation of charge conjugation or particle-antiparticle conjugation C , of the parity operation or inversion of the space coordinates P , and of time reversal T . One consequence of CPT invariance is that the mass of a particle is identical to that of the antiparticle, so different particle and antiparticle densities can only arise in a system which is out of equilibrium. Finally, invariance under the C and CP operations is also a requisite for an asymmetry to develop. In 1964, it was found that the weak interactions were not quite invariant under the CP operation. The decay of a particle called the neutral kaon was studied, and the ratio measured of the two decay modes

$$(5) \quad \frac{\text{Rate } K_L \rightarrow e^+ + \pi^- + \nu_e}{\text{Rate } K_L \rightarrow e^- + \pi^+ + \bar{\nu}_e} = 1.0033 \pm 0.0001.$$

The decay products in the denominator are the antiparticles of those in the numerator, and hence are related by the C operation. However, while both the positron and the electron have half-integral spin, with two possible eigenvalues $\sigma_z = \pm 1/2$ of the spin projected along the direction of motion, in weak interactions at high energies it is found that only *one* of these components is present. Positrons and antineutrinos have $\sigma_z = +1/2$ and are “right-handed”, while electrons and neutrinos have $\sigma_z = -1/2$ and are “left-handed”. Thus the numerator and denominator are related by the CP operation,

exchanging a left-handed particle for a right-handed antiparticle. (For the pions, with zero spin, the operations CP and C give the same result.)

So, ratio (5) indicates that CP symmetry is indeed violated in weak interactions. This has obvious ramifications for distinguishing between matter and antimatter on a cosmic scale. For imagine that one is in communication with an intelligent being in a distant part of the universe. If one can agree on the identity of the electron, as the lightest charged particle, one could send a message that the electrons on Earth are negatively charged, while positrons (antimatter) are positive. Back comes the question “What do you mean by negative charge?” Of course, “positive” and “negative” are just names, and all observable quantities would have been the same if we had decided to call electrons positive and protons negative. However, CP violation now allows us to unambiguously define the positron as the light charged particle in the more prolific (by 0.3%) decay mode of the neutral kaon.

2. – Grand unification and proton decay?

In Sakharov’s paper, he also concluded that if a baryon asymmetry existed, then in principle protons should decay, his estimate of the lifetime (via exchange of particles of the Planck mass) being 10^{50} years! In the *interim*, other and perhaps more cogent reasons have been given for expecting proton decay. Before going into these, let us recall the properties of the basic quark and lepton constituents of matter, see the following table:

Quarks		Leptons	
Symbol	$Q/ e $	Symbol	$Q/ e $
$u = \text{up}$	$+2/3$	e	-1
$d = \text{down}$	$-1/3$	ν_e	0
$c = \text{charm}$	$+2/3$	μ	-1
$s = \text{strange}$	$-1/3$	ν_μ	0
$t = \text{top}$	$+2/3$	τ	-1
$b = \text{bottom}$	$-1/3$	ν_τ	0

They occur in three families, each containing a quark of charge $+2/3$ and one of $-1/3$, plus a lepton of charge -1 and a neutrino of zero charge. The material of our world is made up of the lightest u and d quarks of the first family, constituting protons ($= uud$) and neutrons ($= udd$) of atomic nuclei, plus the electrons e to form atoms and molecules. The second and third families are of heavier quarks and leptons, which decay rapidly to the lighter particles of the first family. These heavier “flavours” can be produced in high-energy accelerators and doubtless they were present and active at a very hot, early stage of the Big Bang, and were therefore important players in shaping its development. However they are essentially absent from our present, cold universe. The muons μ , which are heavy

unstable versions of the electron with a mean lifetime of $2.2\ \mu\text{s}$, are produced as secondary particles by high-energy cosmic rays as they traverse the atmosphere, and a few hundred of them pass through everyone each minute. They form part of the natural dose of radiation which we constantly receive, and it is likely therefore that they contribute to the natural gene mutation rate. So they could be important to biological evolution, although compared with the electrons their number is absolutely negligible. Each neutrino has a “flavour”. In a weak interaction, an electron neutrino ν_e can transform into an electron e , but never into a muon or tauon. Equally, muon and tau neutrinos can transform only into their respective flavours of charged lepton. In any interaction, the flavour number is conserved. These elementary constituents interact through three types of fundamental interaction. In quantum terms, each interaction takes place via the exchange of a characteristic boson (spin 1) particle. The strong interactions are between the quarks only. To separate two tiny quarks in a nucleon, each less than $10^{-24}\ \text{g}$, you would have to exert a tremendous force, of about 14 tons weight. In comparison, the electromagnetic interactions between all charged particles are orders of magnitude weaker, while the weak interactions, taking place between all the particles listed, charged or neutral, are many orders of magnitude weaker still. The extreme weakness of the weak interaction can be gauged from the fact that roughly ten million million neutrinos from the Sun pass through the human body *every second*. You are unaware of them because they do nothing! Much feebler, however, than any of the above three interactions is that of gravity. One learns this from earliest childhood, in simply falling down. During the fall one experiences the gentle accelerating force of gravity, and only on hitting the ground does one realise the much greater strength of molecular (electromagnetic) interactions.

A major development in 1973 was the discovery of neutral weak currents, in experiments at CERN in the Gargamelle bubble chamber. In this discovery, physicists from the University of Milan played a very important role. The results proved that the weak and electromagnetic interactions are *unified* and have (within numerical factors of order unity) the same intrinsic coupling strength. In quantum language, forces are carried by virtual, integral spin particles (bosons) which are exchanged between the colliding particles. The different interactions just differ in the *range* of the interaction, equal to the Compton wavelength h/Mc of the associated bosons, which have very different masses in the two cases. At very high energies, corresponding to very close collisions, the two interactions would be of similar strength. In 1974, Georgi and Glashow proposed a further step, to a grand unified theory (GUT) whereby strong, weak and electromagnetic interactions would be unified into a single interaction and all give similar interaction cross-sections at some very high unification energy.

Figure 3 shows the expected relation between a parameter called $\sin^2 \theta_w$ of the electroweak model, and α_s , the strong coupling parameter between the quarks. The grand-unified scheme predicts that these variables should be related by a curve, the position on it being determined by the value of the unification energy. The early results on neutral current couplings compiled in 1981 gave a world average value of $\sin^2 \theta_w$ consistent with the so-called “minimal SU(5)” model of Georgi and Glashow, and the results indicated a unification energy of about $10^{14}\ \text{GeV}$. Later and more accurate 1990s data, from the LEP

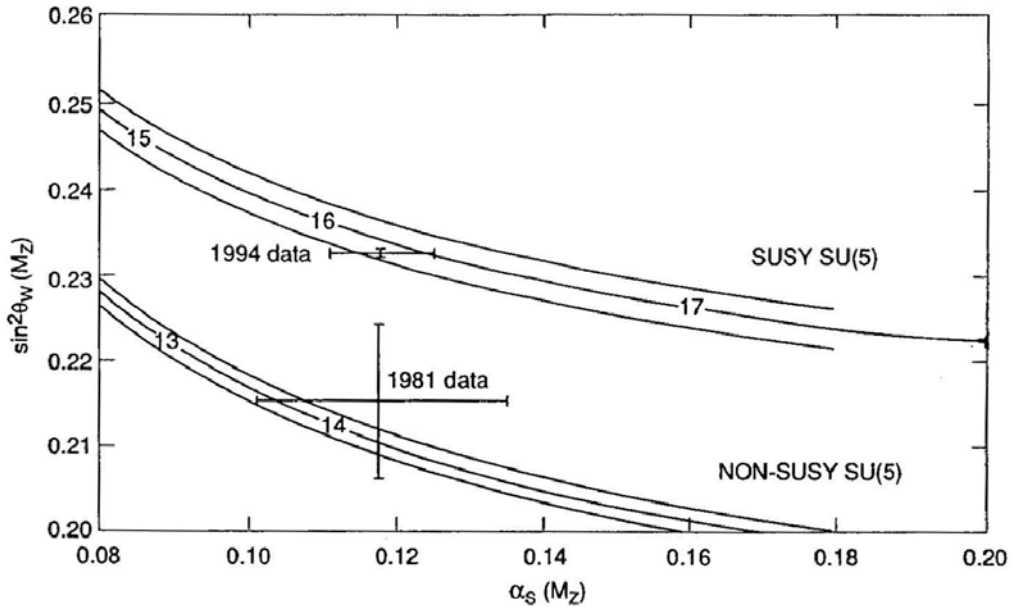


Fig. 3. – Plot of the electroweak parameter $\sin^2 \theta_w$, determined from the relative cross-sections for neutral- and charged-current events, against the strong-coupling parameter α_s . The curves show the predicted relation between these two parameters, as a function of log of the unification energy in GeV, for the non-supersymmetric SU(5) grand unification scheme, and for the supersymmetric version (after Dimopoulos 1994). In 1981, the experimental results gave a world average consistent with the non-supersymmetric model, predicting a proton lifetime of 10^{30} years, or one decay per kiloton per day. More recent and more accurate results from the LEP e^+e^- collider were, however, much more consistent with the supersymmetric scheme.

electron-positron collider and from other experiments, were in conflict with the model, and in better agreement with a supersymmetric version of the grand unified theory. However, the experimental situation in the late 1970s did support the Georgi-Glashow model. At the GUT scale of 10^{14} GeV, quarks and leptons would then have the same unified coupling and thus be able to transform one into another, via the exchange of massive leptoquark bosons X and Y , with charges of $4/3$ and $1/3$ of the elementary charge. Even at very low energies, this meant that *protons could decay*, since the constituent quarks could transform to leptons via the exchange of *virtual* X, Y bosons. Thus

$$(6) \quad p \rightarrow \pi^0 + e^+$$

was a possible decay mode. Because the lifetime is proportional to the fourth power of the free-boson mass (equal to the unification energy), it is very long, about 10^{30} years for the above value of the GUT energy. Although a very long time, this meant that, in a kiloton of material, one proton would decay every day. When physicists realised this, during the early 1980s, there was something of a rush for the mines and the mountain

tunnels, with kiloton-size detectors. Again, a group from Milan pioneered these early experiments. Proton decay would of course be not only of interest in itself but also provide a unique test of grand unification, as well, presumably, as impacting on the universal baryon asymmetry. Deep underground experiments were of course necessary to shield out the cosmic-ray muons.

Unfortunately, after many years of searching with ever more sophisticated and sensitive detectors, proton decay has not yet been observed and the lower limit on the lifetime, of about 10^{33} years, is orders of magnitude longer than the original prediction. Yet all was not lost! Twenty years ago, when proton decay experiments started, one of the outstanding problems was how to detect decays in the presence of a persistent and ineradicable background, which came from the interactions in the detector of neutrinos from the decay of pions and muons produced by collisions of primary cosmic rays (protons and heavier nuclei) in the atmosphere. These neutrino interactions had typical energies of order 1 GeV —similar to the energy release in proton decay— and occurred at a very comparable rate (~ 0.5 per kiloton-day). So it became necessary to study and understand the features of this background, for without that, one would never be able to lay claim to having discovered proton decay.

3. – Atmospheric neutrino oscillations

Fortunately, this “background” has become a real gold mine of new physics, with the entirely unexpected discovery of neutrino flavour oscillations. Such oscillations had actually been predicted by Maki *et al.*, and by Gribov and Pontecorvo in the 1960s. They proposed that although neutrinos interact as *flavour* eigenstates, they propagate through space as a superposition of mass eigenstates. These different *mass* components will travel at slightly different velocities, so that phase differences between the amplitudes develop, corresponding to an oscillation of neutrino flavour. So, starting off for example with a beam of neutrinos of a particular flavour, say ν_μ , as it travels through space it could transform into a beam which, as far as its interactions are concerned, becomes a superposition of ν_μ and ν_e flavours. The relative amounts of each depends on the distance travelled and on a parameter called the mixing angle. The “obvious” way to search for such effects was with the intense and controlled beams of neutrinos from accelerators, and physicists had been doing just that for over 20 years. However, their efforts met with absolutely no success. Because the differences in masses of the neutrino mass eigenstates turn out to be very small —fractions of an electron volt— the oscillation lengths for neutrinos to convert from one flavour to another, for energies in the GeV range concerned, are of order of 100s of km. The accelerator experiments had failed simply because the baselines were much too short, typically 100 metres only. Physicists were fooled in the sense that, drawing a parallel with quark flavour mixing, it was believed that the problem was that the mixing angles (rather than the mass differences) were small, whereas in fact they have turned out to be quite large.

Here it should be explained that a handful of atmospheric neutrino events had first

been observed as early as 1963, as a by-product of the measurement of the depth-intensity relation of cosmic-ray muons. However the measured rate seemed, within the large errors, to be roughly consistent with that expected and little interest was shown in pursuing the matter further. The numbers obtained in the early 1990s, in this case as a by-product of the proton decay searches, were more substantial and the effects observed in fact started out as “anomalies”. From the decay schemes of pions and muons in the atmosphere

$$(7) \quad \pi^+ \rightarrow \mu^+ + \nu_\mu, \quad \mu^+ \rightarrow e^+ + \nu_e + \bar{\nu}_\mu$$

and similar decays for the antiparticles, it is clear that, in the GeV energy region, where essentially all charged pions and muons undergo decay in flight in the atmosphere (as opposed to nuclear interaction in the case of the pions, or penetrating down to sea-level in the case of the muons), there will be two muon-type neutrinos or antineutrinos for every electron neutrino/antineutrino. So the ratio of interactions with muon *vis à vis* electron secondaries should be 2:1. In fact, the ratio was smaller, only about 0.7 of that expected, and this result had been confirmed by five experiments on three continents by the early 1990s. Subsequent experiments with a much larger, 50 kiloton detector called Superkamiokande, based on detecting the Cherenkov radiation emitted by relativistic charged particles traversing the water target, showed that the ratio of observed-to-expected absolute rates was consistent with unity for electron events (due to ν_e interactions), but less than unity and dependent on zenith angle for muon events (due to ν_μ). The zenith angle of the charged lepton determines the path length of the neutrino from its point of production (since the charged lepton is projected forward close to the direction of the original neutrino). Those neutrinos coming from directly overhead will have a typical path length of 20 km from the point of production in the stratosphere; those coming in more or less horizontally, around 200 km; whilst those coming vertically upwards have travelled around 12 000 km from the atmosphere on the far side of the Earth. These neutrinos travel through the Earth with only a tiny, 0.1% chance of absorption. So the weakness of the weak interaction is being exploited to investigate oscillations over a very long baseline. As shown in fig. 4, while the downward muon rate is in agreement with expectations, the upward travelling muons occur at about half the expected rate. Although there are three flavours of neutrino (ν_e , ν_μ , ν_τ) and three mass eigenstates m_1 , m_2 , m_3 (but only two independent mass differences), it turns out that the oscillations in this case are effectively between just two states, ν_μ and ν_τ , since electron neutrinos seem unaffected. Then, if we start with a beam of muon neutrinos, the oscillation probabilities are given by the formulae

$$(8) \quad P(\nu_\mu \rightarrow \nu_\mu) = 1 - \sin^2(2\theta) \sin^2(1.27\Delta m^2 L/E),$$

$$(9) \quad P(\nu_\mu \rightarrow \nu_\tau) = 1 - P(\nu_\mu \rightarrow \nu_\mu),$$

where θ is the unknown mixing angle, E is the neutrino energy in GeV, L is the neutrino path length in km, and the difference of the squares of the masses, $\Delta m^2 = (m_2)^2 - (m_1)^2$ is in $(\text{eV}/c^2)^2$. From this formula we see that the flavour oscillates as L varies. However,

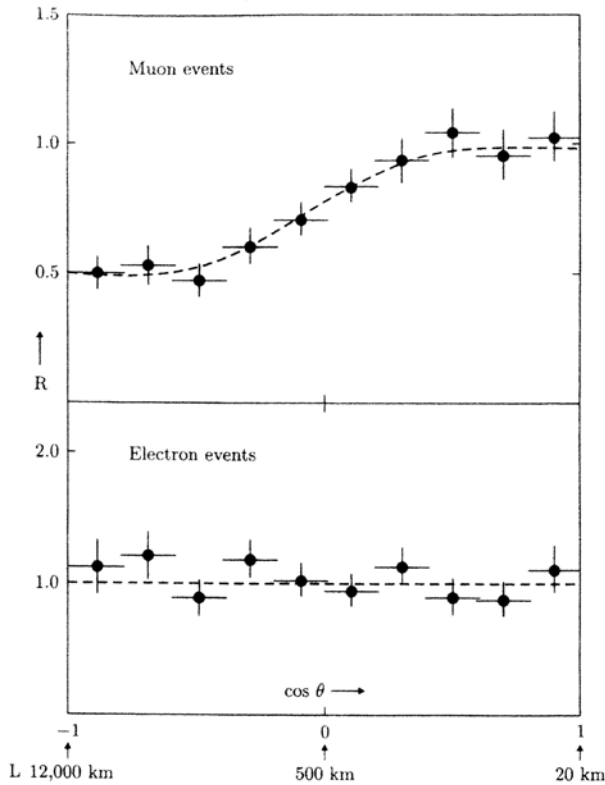


Fig. 4. – Zenith angle distributions of muons and electrons recorded in the Superkamiokande water Cherenkov detector, due to the interactions of atmospheric neutrinos. The quantity plotted is the ratio of numbers to those expected in the absence of flavour oscillations. The electron events are consistent with no oscillation, while the muon events produced by muon neutrinos coming vertically upwards through the Earth show a maximum oscillation signal, due to $\nu_\mu \rightarrow \nu_\tau$ conversion.

since the experiments integrate over a range of energy E and of path length L , one obtains only averaged effects. In particular, for large L , such that $1.27\Delta m^2 L/E \gg 1$, the mean value of the second $\sin^2$ term will be $1/2$. The fact that the intensity for upward muons is half that expected then shows that $\sin^2(2\theta) \sim 1.0$, that is there is maximal mixing with $\theta \sim \pi/4$. Inserting appropriate values for E and L , one finds that a value of $\Delta m \sim 0.05 \text{ eV}$ fits the data. The surprising thing about these results is not simply that neutrinos oscillate but also that the mass differences—and presumably the masses also—are so small in comparison with the masses of all other known fundamental particles. Analogous results on oscillations have also been found with solar neutrinos, again with tiny mass differences.

The reason for such small neutrino masses is unknown. First we must digress briefly regarding the nature of the neutrinos themselves. There are two possibilities. So-called *Dirac* neutrinos, like electrons, are postulated to occur as distinct particles and antiparticles, except that each has one of the two spin projections missing. Thus neutrinos have spin component $\sigma_z = -1/2$ only, while antineutrinos have $\sigma_z = +1/2$ only (a relativistically invariant description only if neutrinos are massless). On the other hand, for so-called *Majorana* neutrinos, particle and antiparticle are identical, and the “antineutrino” is that component of a single particle with $\sigma_z = +1/2$, while $\sigma_z = -1/2$ is the state associated with the neutrino. The suggestion has been made that there are two Majorana particles for each neutrino flavour, one of small mass (similar to that of leptons and quarks) and one of very large mass M_M . The small-mass particle can mix with the massive one in what is called the “see-saw mechanism”, which forces down the light mass below that, m_D , of the other light (Dirac) fermions. In fact the approximate formula is $m_L = (m_D)^2/M_M$, so that if, for example, we take a typical Dirac mass of 10 GeV, a massive Majorana mass of 10^{12} GeV would give a light Majorana mass of 0.1 eV. In this sense, the observed neutrino masses can be regarded as evidence for new physics on some much higher energy, grand-unification scale.

4. – Neutrinos and baryon asymmetry

Let us finally return to our main theme, the universal baryon asymmetry. As stated by Sakharov, one needs a situation of non-equilibrium, and the baryon asymmetry could arise in (at least) two different ways. One is that foreseen by Sakharov and later by Georgi and Glashow, namely the decay out of equilibrium of the massive X and Y bosons of their GUT model. These decay to quarks and antiquarks, and CP violation results in a *quark-antiquark* asymmetry at the GUT scale. Unfortunately, it is found that this asymmetry could well be wiped out by interactions on the electroweak scale.

The other possibility, proposed by Fukugita and Yanagida in 1986, is that first of all, a *lepton-antilepton* asymmetry is generated by out-of-equilibrium decay of massive Majorana neutrinos, again as a result of CP -violating processes at the GUT scale. In fact, for Majorana neutrinos of three flavours, there will be three CP -violating amplitudes. In most models, the lepton asymmetry ΔL is equal to the baryon asymmetry ΔB (as in the proton decay in (7) above). The proposal is then that the lepton asymmetry is converted into a baryon asymmetry in non-perturbative interactions (instanton effects) taking place at the much lower energies (~ 100 GeV) of the electroweak scale. However, these models are all very speculative, and there is at present no other experimental evidence to support them. We do not even know yet whether neutrinos are Majorana or Dirac particles. So, in conclusion, the universal baryon asymmetry is not understood at present, but it is a fascinating aspect of our cosmos, and its origin remains a mystery and a challenge for the future. We must also remember that the problem has arisen in the first place because, over 70 years ago, Patrick Blackett and Beppo Occhialini first observed the creation of matter-antimatter pairs in a cloud chamber.

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Present appeal in pion decay studies and applications

ANTONIO BERTIN and PIETRO FACCIOLI

Istituto Nazionale di Fisica Nucleare, Sezione di Bologna, and Dipartimento di Fisica dell'Università di Bologna, Italy

ANTONIO VITALE

Istituto Nazionale di Fisica Nucleare, Sezione di Bologna, Dipartimento di Fisica dell'Università di Bologna, and Fondazione Giuseppe Occhialini, Fossombrone (PU) Italy

1. – Introduction

1'1. *A personal recollection.* – One day by the end of February, 1983, being puzzled by a scientific question, two of the authors visited Beppo and Connie in their country house. The professional point having been settled, they were warmly invited to stay for lunch. Soon after unveiling that it was the 40th birthday of one of them (A.V., while the other's one (A.B.) had been celebrated dining with Bruno Pontecorvo a few years before), the conversation leant rather on the human side of the host's personal recollections, although being limited by something like a fire circle drawn at the defense of privacy.

Beppo's speech was in all ways fascinating; it looked somehow driven by his shining blue eyes, childishly sweet in contrast with the dramatic features of his physiognomy, which might have stimulated a painter like Van Gogh; he was capturing the attention both when he was serious (he seemed to have been significantly impressed by a recent viewing of the Ingmar Bergman picture *The Strawberry Place*) as well as when he was joking (*It is not easy to settle in Hell, but it's even harder to remain there in spite of the devils*, he stated referring to some uneasy work relations we were talking about).

In front of friendly queries, he reluctantly gave just a few details about his academic career and work, with an ironic trend to underestimate both (*I got my first fellowship because another guy had been obliged to give up since he was under the flags... My*

teaching activity was carried out exclusively through research and examinations. One student told me that he would have been happy if I had held my University lectures with the same skill I had revealed during the examination were some of his statements).

Going on in the dialog (which the authors soon understood might remain unique in their acquaintance) also more involving questions came out. After obtaining the promise of keeping the conversation confidential (*otherwise... and he mimicked firing a gun*), Beppo began to speak about his life and scientific experience as one does with close friends. It was a kind of unexpected gift, by receiving which the interlocutors got the flattering feeling that the man was getting the advantage on the scientist in Beppo's conversation, because he considered them as two friends rather than two colleagues who as well might have been his own students from the age point of view.

In this part of the meeting, he took care of saying more than once that he had got from his scientific life more than he deserved. Needless to say, in spite of his will, on this point he left us less than convinced.

1'2. Purpose of this contribution. – Experimental research with cosmic rays allowed to attain, immediately after World War II, two outstanding discoveries: Conversi, Pancini and Piccioni [1] observed the non-hadronic behaviour of muons, which thereby were shown to interact with nuclei too weakly in order to be identified with the Yukawa mesons; Lattes, Muirhead, Occhialini and Powell [2] discovered the pion through its $\pi \rightarrow \mu\nu$ decay in nuclear emulsions, by which it became definitely clear that the muons in cosmic rays are of secondary origin.

It is generally recognized that these two results are at the origin of modern research in high-energy physics, both due to the significance of the physics results and under the standpoint of experimental techniques. Aside of the discoveries of the positron [3] and of cosmic-ray showers [4], they may be also considered as the milestones of the subnuclear world knowledge before the advent of particle accelerators.

Once artificially produced pions were made available by particle beams, systematic investigations of the different properties of the pion were carried out, including measurements of its lifetime, decay branching ratios and static properties (for updated results, see ref. [5]).

Two results of exception soon obtained by studying the pion decay were the observation of parity non-conservation in the pion-muon-electron decay mode [6] and the experimental evidence in favor of the V-A form of the weak interaction [7] obtained by studying the pion decay into an electron and a neutrino [8].

The following decades yielded in particle physics such significant experimental discoveries as the ones concerning the existence of quarks [9] and of the $W^\pm$ and Z^0 vector bosons [10], experimentally supporting the electro-weak unified theory [11]. From the theoretical point of view, the Standard Model was elaborated as the so far most powerful frame for describing particle interactions.

Quite a long way has nowadays been covered, in other words, both in knowledge and in technologies, with respect to the quoted discoveries of fifty years ago. The purpose of this contribution is to recognize the present interest of Beppo's scientific legacy connected

to pion decay along i) fundamental physics, with main reference to the determination of elements of the Cabibbo-Kobayashi-Maskawa (CKM) matrix [12] and ii) one possible technological direction, where pion decay is involved via the use of the released muon (with reference to the muon catalytic action of nuclear fusion [13]). The scheme seems to us quite suitable to honor a scientist who gifted the scientific world with such important results both on the scientific and on the methodological standpoint.

2. – The pion beta decay

2.1. General remarks. – Within the framework of the Standard Model (SM), the quark mass physical eigenstates do not participate as pure states in the weak interactions. The transformation connecting the two bases of mass and weak eigenstates is the CKM matrix, which represents a special interest in particle physics for the objective of a unified description of weak-interacting mesons, baryons and nuclei.

By convention, the charge $+\frac{2}{3}$ quarks (u , c , and t) are chosen to be pure states, and the flavour mixing is described in terms of a 3×3 matrix operating on the d , s , and b quark states:

$$(1) \quad \begin{pmatrix} d' \\ s' \\ b' \end{pmatrix} = V_{\text{CKM}} \begin{pmatrix} d \\ s \\ b \end{pmatrix} = \begin{pmatrix} V_{ud} & V_{us} & V_{ub} \\ V_{cd} & V_{cs} & V_{cb} \\ V_{td} & V_{ts} & V_{tb} \end{pmatrix} \begin{pmatrix} d \\ s \\ b \end{pmatrix}.$$

Therefore, d' , s' , and b' are partners of u , c , and t , respectively within the weak isospin doublets.

The CKM matrix must be unitary, which excludes a number of yet unobserved processes not compatible with the SM of electroweak interactions. Therefore, an accurate determination of the matrix unitarity provides a self-standing test of possible deviations from the SM itself.

2.2. Status of experiment. – The most studied element of the matrix V_{ud} (which is the dominant term of the matrix first row and column) assumes an important role in the relevant unitarity tests. Looking at non-rare processes, it can be determined from the neutron decay rate and from superallowed Fermi (SF) transitions in nuclear beta decay. Some of the earlier results obtained from neutron decay, however, were not consistent with those from nuclear SF decays [5], and separately provided $|V_{ud}|$ values suggesting deviations from unitarity [14]. In its turn, this was partially due to the indetermination with which the ratio $\frac{g_A}{g_V}$ of the axial-vector to the vector weak-interaction coupling constant was known [15].

The alternative way is to get a determination of $|V_{ud}|$ from pion beta decay. On the one hand, this would bypass the controversial aspects of the theoretical description of nuclear decays, providing an unambiguous value of $|V_{ud}|$ without requiring the knowledge of additional parameters (such as the axial-vector coupling constant in the case of neutron decay); on the other, this measurement could yield a precise result, although being hindered by the low branching ratio of the process ($R_{\pi\beta} \cong 1 \times 10^{-8}$).

In fact, within the framework of the Conserved Vector Current (CVC) hypothesis [16] and quark-lepton universality, the rate of the decay $\pi^+ \rightarrow \pi^0 e^+ \nu_e$ is related to the V_{ud} CKM matrix element via the relation

$$(2) \quad \Gamma_{\pi\beta} = \frac{G_\mu^2 |V_{ud}|^2}{30\pi^3} \left(1 - \frac{\Delta}{2M_+}\right)^3 \Delta^5 f(\varepsilon, \Delta)(1 + \Delta),$$

where G_μ is the Fermi coupling constant, $\Delta = M_+ - M_0$, $\varepsilon = (\frac{m_e}{\Delta})^2$, and M_+ , M_0 and m_e are the masses of the π^+ , π^- and electron, respectively; the Fermi function f is defined as

$$(3) \quad f(\varepsilon, \Delta) = \sqrt{1 - \varepsilon} \left[1 - \frac{9}{2}\varepsilon - 4\varepsilon^2 + \frac{15}{2}\varepsilon^2 \ln \left(\frac{1 + \sqrt{1 - \varepsilon}}{\sqrt{\varepsilon}} \right) - \frac{3}{7} \frac{\Delta^2}{(M_+ + M_0)^2} \right].$$

The dominant source of error in eq. (2), apart from $|V_{ud}|$, is the pion mass difference Δ , which is known from a precise measurement [17] with an error which reflects in $\Gamma_{\pi\beta}$ at a rate $\frac{\delta\Gamma_{\pi\beta}(\text{exp})}{\Gamma_{\pi\beta}} \cong 5 \times 10^{-4}$; theoretical uncertainties, which are due to the combined radiative and short-range physics corrections, are upperly bound to $\frac{\delta\Gamma_{\pi\beta}(\text{th})}{\Gamma_{\pi\beta}} \leq 1 \times 10^{-3}$. Therefore, the pion beta decay represents an excellent tool to get a precise experimental determination of $|V_{ud}|$.

From 1963 to 1985, the rate of the pion beta decay was determined in several experiments [18]. The most precise one [19] was performed by detecting in-flight π^+ decays, yielding $\Gamma_{\pi\beta} = (0.394 \pm 0.015)\text{s}^{-1}$, which was by an order of magnitude less precise than the theoretical yield (2).

Still at the beginning of the present decade, therefore, it was felt [14] that a new and more precise value of $\Gamma_{\pi\beta}$ (with an experimental uncertainty ideally reduced by a factor of 10 in order to be comparable to the theoretical error) would be essential. Actually, a dedicated experiment was started in the late 90's at the Paul Scherrer Institut (PSI, Villigen, CH) detecting pion decays at rest, in order to achieve a better control of systematic uncertainties.

The experiment recently resulted [20] in a sixfold improvement in accuracy with respect to the previous measurement [19], with the values $\Gamma_{\pi\beta} = [0.3980 \pm 0.0015(\text{stat}) \pm 0.0019(\text{syst})]\text{s}^{-1}$ and $R_{\pi\beta} = [1.036 \pm 0.004(\text{stat}) \pm 0.005(\text{syst})] \times 10^{-8}$.

The $|V_{ud}|$ value calculated by comparing these results to the relevant theoretical predictions like eq. (2) [$V_{ud} = 0.9728 \pm 0.0030$] is in excellent agreement with the PDG 2004 value [$|V_{ud}| = 0.9738 \pm 0.0005$] [5] obtained by averaging the complete data set on SF nuclear transitions [21] and all the recent results on the decay of highly polarized neutrons [22], which have a better precision having been obtained by more refined techniques and improved detectors.

As far as the pion beta decay is involved, therefore, the present evidence is fully in favor of the SM of the electroweak interactions. Despite his rebellious temper, Beppo discovered a tool which so far is contributing to confirm an established and appreciated model of particle interactions.

Although this achievement represents to date the most accurate test of the CVC hypothesis and Cabibbo universality in meson environment, the accuracy of the result is expected to improve by further refining the experiment simulation/analysis and increased statistics.

Here we should like to underline that the interest for a more accurate measurement of $|V_{ud}|$ remains relevant. The value of $|V_{ud}|$ and the one of the adjacent CKM matrix element $|V_{us}|$ are tightly related to each other by the unitarity condition

$$(4) \quad U_{uu} = |V_{ud}|^2 + |V_{us}|^2 + |V_{ub}|^2 = 1,$$

where the contribution of the very small element $|V_{ub}|$ can safely be neglected.

In other words, the measurements of $|V_{ud}|$ and $|V_{us}|$ represent two independent, indeed the only two, constraints on the Cabibbo angle θ_C , or, in the three-generations language, on the Wolfenstein parameter λ , which defines the magnitudes of all CKM matrix elements and their hierarchy. Moreover $|V_{us}|$, after $|V_{ud}|$, is the second most precisely measured CKM matrix element, determined by measurements of the $K \rightarrow \pi \ell \nu$ branching ratios as well as by data on hyperon semileptonic decays.

The comparison between measurements of $|V_{ud}|$ and $|V_{us}|$ represents, on the one hand, a test of unitarity (eq. (4)). On the other hand, when the value of the latter element is assumed, they can be combined to increase the precision in the determination of the crucial parameter λ .

In both perspectives, the current determinations of $|V_{ud}|$ and $|V_{us}|$ are not free from contradictions. For example, by combining a subset of the neutron decay data with all but the last $K \rightarrow \pi \ell \nu$ measurements or with one of the global analyses performed on hyperon decay data, one would discover a significant violation of unitarity.

The most recent PDG values of $|V_{ud}|$, as already mentioned, and of $|V_{us}|$ as well [23] are the results of definite choices of experimental and theoretical inputs. In the latter case ($|V_{us}| = 0.2200 \pm 0.026$) the final error has been artificially increased to reflect an inconsistency between new and old measurements of K decays, while the hyperon decay data have been discarded due to the difficulty of interpreting them consistently within the SU(3)-symmetry framework. In this way, the unitarity condition is marginally satisfied: $U_{uu} = 0.9967 \pm 0.0015$.

In all methods, a significant or dominant fraction of the error is of theoretical nature, being associated with the calculation of the relevant radiative corrections and form factors, and the need for more theoretical work is generally recognized. The historical tendency towards a value of U_{uu} smaller than 1 can therefore be attributed to an underestimate of theoretical and/or experimental systematic errors.

An alternative, and more interesting, possibility is that this tendency is the signal of new degrees of freedom which actually spoil the unitarity of the CKM matrix. This perspective attributes a special importance to new, independent experimental inputs like the one expected from an improved measurement of the pion beta decay. Even in the (most likely?) scenario in which unitarity is preserved, a new measurement providing an indication of the underestimated uncertainties would ultimately lead to a more accurate

determination of one of the key parameters of the CKM matrix, reflecting positively also on the constraints on the physics beyond the SM.

3. – Muon catalyzed fusion (MCF), fifty years later

3'1. General remarks. – The centenary of Beppo's birth will occur fifty years after the already quoted discovery [13] of the fact that the nuclear fusion of two hydrogen isotopes can cyclically be catalyzed when negative muons are stopped in suitable mixtures of such isotopes. This possibility is due both to his contribution to the discovery of the pion [2] (since muon beams are currently generated via pion decay) and to the quoted observation of the non-hadronic behaviour of the muon [1].

In a simplified way, the process can be summarized in the following steps: i) The muon forms at first a *muonic atom*, substituting one electron. ii) The muonic atom may get bound to a second nucleus in a *muonic molecule*. iii) Due to the higher value of the muon mass ($m_\mu \cong 200 \times m_e$, where m_e is the mass of the electron), inside such a system the nuclei are confined within distances about 200 times smaller than the ones of an ordinary (electronic) molecule. iv) The fusion probability is thereby enhanced by an enormous factor, so that the corresponding nuclear synthesis takes place also at ordinary temperature and pressure conditions. v) Once the fusion has occurred, in most of the cases, the catalyst muon is left free, and re-enters the process.

The total energy release associated to one muon is obviously determined by the number N_f of cycles it is allowed to run during its lifetime, and by the energy delivered in each fusion reaction occurring during the cycle itself.

Stopping negative muons in deuterium-tritium mixtures has for a long time been recognized as the most convenient choice for the purposes of getting a large amount of energy via MCF. Among other reasons, this is due to the high formation rate of $dt\mu$ molecular systems, to the fast fusion process which follows and to the energy release per fusion reaction, which is also more favourable, in the d-t case, than for other hydrogen isotope choices [24]. Passing from $N_f \simeq 1$ (for the cases of hydrogen-deuterium mixtures or pure deuterium) to $N_f \geq 100$ for deuterium-tritium mixtures (a well-founded result already in the '80s [25,26]) the energy gain associated to one single muon increases by a factor greater than 200.

In order to understand the physical limits of the possibilities of MCF for technological (energy distribution) purposes, one might recall that the number N_f of cycles undergone by one single muon, following *e.g.* the scheme shown in fig. 1, is given by the relation

$$(5) \quad N_f = \frac{\lambda_c}{\lambda_0 + \omega_s \lambda_c},$$

where λ_c is the cycling rate (defined as the inverse of the time elapsing between two subsequent fusion events); λ_0 is the free muon decay rate [27] ($\lambda_0 \cong 4.55 \times 10^5 \text{s}^{-1}$) and ω_s is the probability that the muon is lost during a catalytic cycle for reasons other than

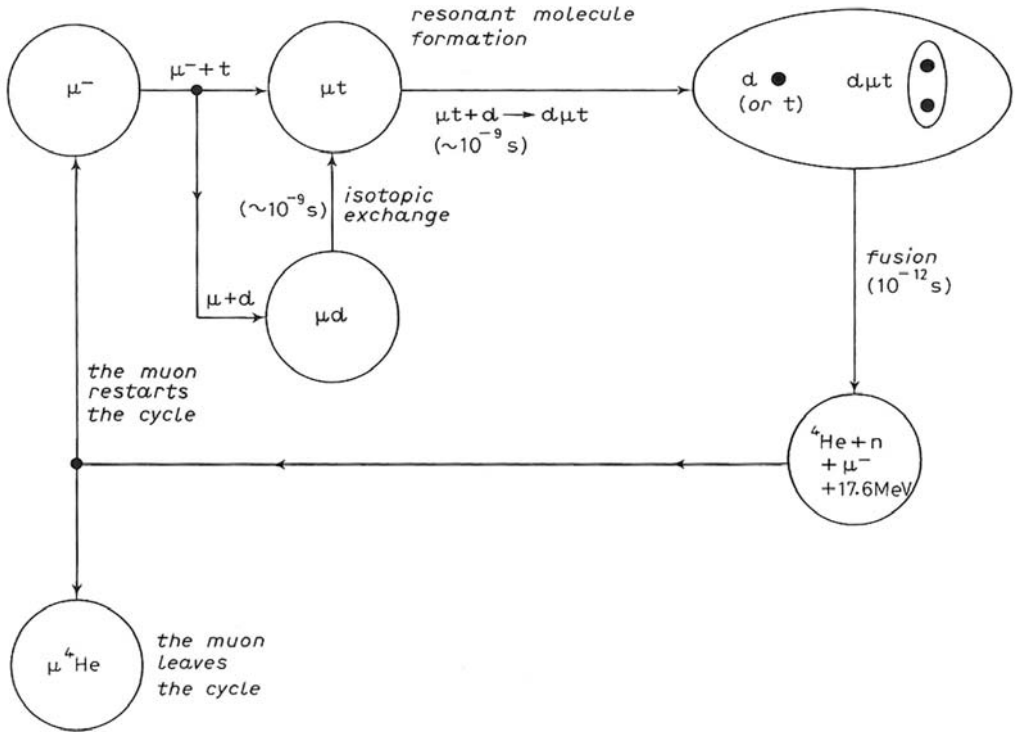


Fig. 1. – Main reactions of the muon-catalytic cycle in an equimolar mixture of liquid deuterium and tritium.

its decay (among which the *sticking* of the muon to the helium nucleus produced in the

$$(6) \quad d + t \longrightarrow {}^4\text{He} + n + \mu^- + 17.6 \text{ MeV}$$

fusion reaction has a particular importance).

It is significant to consider eq. (5) in the limiting cases:

$$(7) \quad N_f(\lambda_0 = 0) = \frac{1}{\omega_s},$$

$$(8) \quad N_f(\lambda_c \rightarrow \infty) = \frac{1}{\omega_s},$$

$$(9) \quad N_f(\omega_s = 0) = \frac{\lambda_c}{\lambda_0}.$$

3'2. MCF energetics towards scientific break-even. – Historically, the main appeal of the MCF discovery was represented by its perspectives for energetics, a field in which it introduced the concept of *muonic confinement* aside of the canonical *magnetic* and *inertial confinement* procedures, by which the hydrogen isotope nuclei are squeezed enough to undergo nuclear synthesis.

With this in mind, aside of the last considerations expressed, one has to include the fact that the effective energy delivery E_d associated to the catalysis cycle of one muon

$$(10) \quad E_d = N_f \times 17.6 \text{ MeV} < \frac{17.6 \text{ MeV}}{\omega_s}$$

compares to the energy cost C_{μ^-} of the muon production, the *scientific* break-even being attained when

$$(11) \quad G = \frac{E_d}{C_{\mu^-}} = 1.$$

On the other hand, eqs. (7) and (8) show that, until $\omega_s \neq 0$, both the muon instability and a high value of the cycling rate are inessential: even for a stable muon, or for an extremely high cycling rate, the number of fusions associated to one muon is limited by ω_s .

In its turn, eq. (9) indicates that, once ω_s has been minimized, the increase of λ_c is a quite significant way to improve N_f . One recognizes then that, for the purposes of energy delivery via MCF, three points are relevant: to increase N_f , to decrease ω_s , and to decrease C_{μ^-} .

A significant experimental effort was developed in the last decades in order to optimize the first two parameters, with particular care in identifying the best temperature and density conditions of the hydrogen isotope mixture.

The nowadays currently reported value $N_f = 150$ [28] in practice confirms the quoted results [25, 26] which relaunched the scientific interest on MCF about twenty years ago. As to the effective values of ω_s , more recently the results of accurate measurements have allowed to attain values around $4-5 \times 10^{-3}$ [29, 30] at target densities up to $1.45 \times \rho_{\text{liq}}$, where ρ_{liq} is the density of liquid hydrogen. As far as C_{μ^-} is concerned, updated estimates [28] quote an optimal value of 5 GeV, in agreement with the results of measurements [31] and calculations [32].

One sees then from eq. (10) that these results correspond to $E_d \lesssim 3-4$ GeV; from the relevant ratio in eq. (11), then, one would infer that, even assuming the 5 GeV optimal number for the muon cost, a factor of about 2 still remains to be gained in E_d (*viz.* N_f) to attain the scientific break-even.

For the purposes of a technological use of MCF, moreover, the effective value G_{eff} of the energy gain associated to the catalytic cycle of one single muon is given by

$$(12) \quad G_{\text{eff}} = G \times \eta_A \times \eta_R \times \eta_{\text{th}},$$

where η_A , η_R and η_{th} are efficiency factors connected respectively to the muon production by the accelerator, to the power requested to recirculate the beam particles which do

not stop within the hydrogen isotope target, and to the conversion of thermal to electric energy. An estimated product $\eta_A \times \eta_R \times \eta_{th}$ of about 10^{-1} takes the effective gain to about

$$(13) \quad G_{\text{eff}} = G \times 10^{-1} \cong 7 \times 10^{-2},$$

which still is far from the technological break-even condition $G_{\text{eff}} = 1$.

3'3. MCF-induced studies of mu-atomic and mu-molecular physics. – Among the significant spin-off of MCF studies, one should not disregard the fact that they stimulated experimental and theoretical research on the processes involving muonic atoms and molecules in a hydrogen isotope mixture, which are essential to establish the optimal conditions for muon cycling in the catalysis process of nuclear fusion. This induced a harvest of systematic studies, concerning:

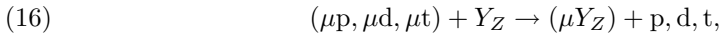
i) *The formation stages of the hydrogen (μp), deuterium (μd) and tritium (μt) muonic atoms, i.e.* the electromagnetic cascade processes through which they de-excite from the outer initial orbits towards the ground states, possibly lingering in intermediate metastable states.

ii) *The elastic scattering processes* of the muonic atoms undergone while they diffuse through the surrounding medium, being neutral systems of unusually small dimensions ($a_\mu = \frac{\hbar^2}{m_\mu e^2} = 2.56 \times 10^{-11}$ cm).

iii) Among the *possible inelastic processes*, the transfer reactions of the muon from one hydrogen isotope atom to a heavier atom, where the muon is more tightly bound, such as *e.g.* the isotopic exchange reactions



or the transfer reactions of the muon from one hydrogen atom to a heavier nucleus, of the type



which occur at such high rates that a small contamination of a heavier hydrogen isotope or other impurities in the target may produce the total disappearance of the lightest (hydrogen or deuterium) muonic atoms in a time which is small with respect to the muon lifetime.

iv) *The formation of muonic molecular ions*, through the main channels



$$(19) \quad (\mu d) + d \rightarrow (d\mu d),$$

and

$$(20) \quad (\mu t) + d \rightarrow (d\mu t),$$

from which the fusion reaction (6) promptly proceeds releasing 14 MeV neutrons.

The features of processes from (14) to (20) and their relative interplay were studied for the last five decades (for the present purposes, relevant results are summarized in recent reviews [28, 29, 33]), and still are investigated for optimizing purposes along the lines which follow:

a) A chief interest remains in identifying the target physical conditions and hydrogen isotope mixture which speed up the mu-molecular ion formation (characterized in its turn by a resonant behaviour in the cases of processes (19) and (20)).

b) Particular attention needs to be reserved to the different channels which may take the muon outside the fusion cycle, as is the case for the muon sticking to the helium nucleus resulting from reaction (6).

c) Other investigation efforts are directed to establish how the initial state in the molecular systems produced in reactions (17)–(20) influences the MCF cycle, with special interest in the rotational states and the ortho-para transition effects.

One should not forget, moreover, that special attention was dedicated to mu-atomic and mu-molecular processes also by research in subnuclear physics, since their understanding (see, *e.g.*, refs. [34–37]) proved to be essential also for the interpretation of muon nuclear capture experiments in gaseous and liquid hydrogen and deuterium [18], *i.e.* quite essential processes for the investigation of low-energy weak interactions.

3.4. A MCF-based intense neutron source. – Until the effective energy gain G_{eff} associated to the catalytic cycle of one muon prevents MCF from being used for industrial energy purposes (see eq. (13)), the process itself may represent interesting possibilities for applications in which the energy cost of producing one negative muon is not essential.

Starting from the early '90s, for instance, materials scientists pointed out that, for the purpose of direct experimental study of the neutron radiation damage on fusion reactor components, an intense neutron source with adequate energy spectrum would be necessary [38]. The fusion process occurring in d-t fusion reactors, on the other hand, is the same occurring within $(d\mu t)$ molecular ions (see reaction (6)), providing 14 MeV neutrons. For the investigation of the radiation damage produced in the first wall and in other structural components of the reactor by the neutrons released, a d-t MCF-based 14 MeV neutron source would then provide an ideal probe.

The possibility was then quantitatively examined, with particular regard to the obtainable neutron flux (which should be as high as $10^{14} \text{ cm}^{-2}\text{s}^{-1}$) and relevant gradient, as well as to the irradiation test volume and surface [39]. The results obtained at the present level of technology point out both the feasibility of the intense neutron source with the necessary characteristics and that it would represent an interesting solution compared to others based on stripping or spallation sources [40].

3'5. Conclusions. – As a conclusion of the present contribution, we should like to outline the following final remarks:

i) Rare pion decay studies going beyond present experimental data represent an interesting field of investigation on the elements of the CKM matrix and of its unitarity, looking for more precise confirmations of the Standard Model of electroweak interactions or hints towards new physics.

ii) Negative muon beams derived from pion decay were extensively used to investigate the perspectives of the muon catalytic process of nuclear synthesis and related phenomena. While the exploitation of MCF for energetic purposes still seems far from present possibilities, the field deserves and undergoes systematical exploration both on the mu-atomic and mu-molecular standpoint and for application purposes.

The scientists who work along these directions share the pride and responsibility of having gathered one of the many fascinating features of Beppo's scientific legacy.

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On the origin of cosmic-ray electrons

GIUSEPPE (PEPPO) GAVAZZI

Dipartimento di Fisica “G. Occhialini”, Università degli studi di Milano-Bicocca, Italy

1. – Introduction

The writer has been the last student who graduated with Beppo few years before his retirement. His thesis: “The energy spectrum of cosmic-ray electrons (CRE)” [1] was inspired by Beppo’s obsession for the origin of CRs. The thesis was focused on the calculation of the diffusion equation that could account for all forms of energy losses suffered by electrons in their interstellar journey, aimed at constraining the primary electron spectrum from the spectrum observed within the solar cavity. The idea was to find observational constraints to models of CR acceleration by supernovae and their remnants. CRE and radio astronomy are adjacent fields of research because CRE lose energy via the synchrotron mechanism on galactic magnetic fields, producing radio waves. So, even when I eventually became a radio astronomer at the Leiden Sterrewacht, working on radio galaxies, I kept wondering about the origin of cosmic rays.

2. – Supernovae as sources of CRE

The power law spectrum and the polarization of the radio signals from extragalactic radio sources are clear signatures of the synchrotron losses of relativistic electrons spiraling onto micro-gauss magnetic fields. The next obvious question is whether CRE, and their heavier and more abundant brothers, the protons, are accelerated by young Pop I stars, such as supernovae (and their remnants) or by old Pop II sources, like old pulsars

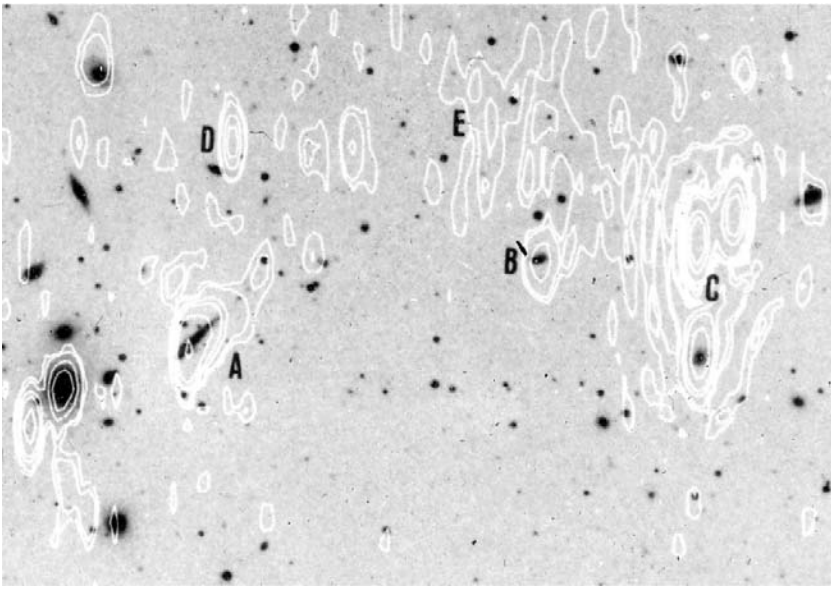


Fig. 1. – Contours of the 1415 Mhz radio emission, as obtained from early Westerbork observations of the cluster A1367, superposed to a negative print of the POSS. UGC-6697 is labeled “A” (adapted from [2]).

in binary systems. With a pioneering work, Lequeux [3] observed a correlation between the size of the radio sources associated with nearby spiral galaxies (including the MW) and the distribution of the HII regions at their interior, where stars are born and die, arguing in favor of the first hypothesis. The issue deserved an independent confirmation, however.

3. – Radio sources in external galaxies

The best place to do radio astronomy in the late '70s was the Netherlands, where the Westerbork synthesis telescope was in operation since 1970. I was sent to Leiden by Beppo and by Cesare Perola to work with Walter Jaffe on cluster radio galaxies. At the same time E. Hummel was doing his PhD thesis at Groningen on the properties of normal, nearby galaxies as sources of continuum radio emission. Unexpectedly he [4] concluded that there was no correlation between the continuum flux density of the extended sources associated to spiral galaxies and their integrated colors. The claim was against Lequeux's interpretation.

My contribution to the issue came in quite serendipitously. Among luminous radio-galaxies, that were the primary targets of my investigation, nearby clusters like Coma, happen to contain spiral galaxies that are faint radio continuum sources. I remember Cesare, with whom I was discussing some work done on the powerful head-tail radio galaxy 3C-264 (see his contribution in this book) readdressing my attention to a less ex-

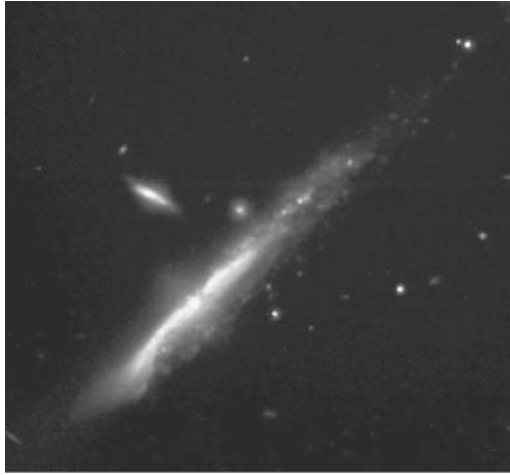


Fig. 2. – A modern composite color picture (here rendered in black and white) of UGC-6697 obtained with the VLT (adapted from [6]).

treme, yet intriguing radio source associated with the rather inconspicuous spiral galaxy, UGC-6697 (see fig. 1) that became, and still remains, my favorite galaxy ever since [5, 6] (see fig. 2). Encouraged by the detection of faint radio signals from spiral galaxies up to the distance of the Coma cluster, Walter and I started surveying a large stretch of the Great-wall, searching for radio emission from more spiral galaxies. We began using the Westerbork interferometer, then in the '80s we switched to the more flexible and sensitive Very Large Array (VLA). We detected several hundred spiral galaxies encompassing two decades in radio luminosity. What makes them faint or bright? In other words why some contain few, other many sources of CRE? Massimo Tarenghi, another fellow of the Milano group, was just back from Kitt-Peak where he spent some years taking plates and spectra of galaxies in clusters with the 4 m Mayall telescope. He pointed out that the brightest radio sources we were discovering were in fact associated with “emission-line galaxies”. This rather vague indication convinced me that further clues on the nature of radio spirals were to be searched with optical tools. Using catalogued data first, then our own measurements taken with various 1-2m-sized telescopes (that in the mean time were increasing their capabilities by factors of 100, owing to the invention of CCD detectors) we started taking broad-band photometry of galaxies. There was a clear effect that flatly contradicted Hummel’s earlier result: incontrovertibly the radio luminosity is proportional to the blueness of galaxies. In other words galaxies that contain a “young” stellar population harbor brighter radio sources [7]. The early work of Kennicutt [8], who pioneered the technique of narrow-band imaging of galaxies, clearly showed that the intensity of the Balmer line H_{α} provides the most accurate estimate of the current massive star formation rate in galaxies, which in turns correlates with the rate of supernovae explosions. Stimulated by his work we did an enormous effort, primarily against the aw-

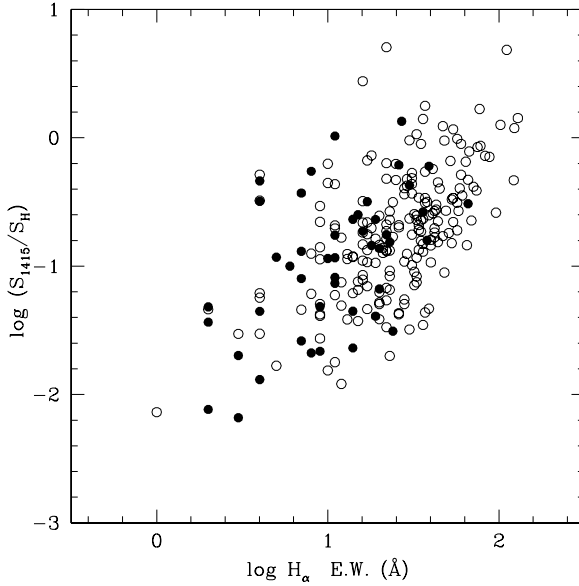


Fig. 3. – The final proof that supernovae are the sources of CRE derives from the correlation between the radio continuum flux at 1415 MHz (normalized to the near-IR flux (H) which is proportional to the galaxy mass) and the equivalent width of the H_α line, a quantity proportional to the current star formation rate. Data include all spiral galaxies in the Virgo and Coma supercluster. Filled dots mark AGNs.

ful winter European weather, trying to get H_α line measurements of our radio-selected spirals. The measurements, finally taken with a simple photometer at the 1.3 m telescope on the roof of the library building at Kitt-Peak, clearly showed a proportionality of the radio continuum emissivity from spiral galaxies with their rate of production of young massive stars, thus of supernovae explosions [9], in agreement with [3]. These results still hold true. If we include in fig. 3 the latest set of H_α measurements that we took on Virgo galaxies in 2005 [10], we see a definite, although not perfect correlation between the radio flux at 1415 MHz, per unit galaxy mass, and the massive star formation rate, represented by the equivalent width of the H_α line. Symbols are coded according to whether there is evidence for a “Liner” in the galaxy nucleus (Liners are low-activity AGNs). AGNs seem not to contribute to the observed correlation, reinforcing Lequeux claim that the bulk of the radio emission does not originate in the nuclei, but is spread over the galaxy disks, as are the HII regions. No one is arguing anymore against supernovae as the primary sources of CRE. Beppo can relax and give up the next cigarette!

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Perspectives of high-energy astrophysics

LAURA MARASCHI

Istituto Nazionale di Astrofisica, Osservatorio Astronomico di Brera, Milano, Italy

ALDO TREVES

Università dell'Insubria, Como, Italy

1. – Introduction

In the first half of the last century Physics underwent the deepest changes since its foundation as a modern discipline. With his contributions to the discovery of positrons and pions, Occhialini, Beppo for us, has been a major actor of that heroic epoch.

In the second half of the century, and particularly in the second decade a similar revolution invested Astronomy, leading to the discovery of the cosmic background radiation at 3 K, of neutron stars in pulsars and X-ray binaries, of black holes in active galactic nuclei and in X-ray binaries. Beppo's inspiration has been directly or indirectly instrumental in this renaissance epoch of Astronomy. In this respect one can quote his influence on Giacconi strongly encouraging him to move to the States (go west young man!), his efforts in starting European gamma-ray Astronomy (COS-B) and finally his influence in the selection of the SAX mission (see the papers of C. Perola and L. Scarsi in this volume).

While physics has passed the epochs of revolutionary changes and is in a phase where basic progress is expected on relatively long time scales, the lull without a foreseeable tempest, Astronomy is undergoing continuous profound revisions, the most recent leading to hypothesize the presence of a new form of “dark energy” on cosmological scales. Anticipation of future evolution is up to personal prejudices, *e.g.* ours, perhaps conservative, being that the very theory of gravitation may need to be rethought.

We have been asked to introduce significant perspectives in high-energy astrophysics. Following the predictments of Beppo, who always emphasized the importance of continuous laborious refinement of experimental techniques especially in quiet phases of scientific development⁽¹⁾, we outline the progress of modern gamma-ray astronomy and the great expectations for the future in this area. Furthermore we choose to devote special emphasis to the evolution of pulsar astrophysics which has witnessed a Renaissance period reaching the level of a role reversal, physics benefitting from astronomical results rather than the more common opposite.

2. – “Modernity” of gamma-ray astronomy

2.1. *From cosmic rays to gamma-ray astronomy.* – The quest for high-energy emission from celestial objects was initially based on the knowledge about cosmic rays. The radio emission from the galactic halo demonstrated the existence of a component of high-energy electrons besides the well-known proton component. Interaction of high-energy protons with the interstellar medium implies production of pions which decay into electrons and gamma-rays. The possibility of measuring these gamma-rays, which would carry information on the spatial distribution of cosmic rays in the Galaxy, meant a fundamental step forward in the cosmic-ray field previously confined to direct measurements of particles impinging on the Earth atmosphere.

As described in detail in this book, Beppo promoted a substantial effort in Europe involving the whole European community of high-energy astrophysics for a big gamma-ray satellite experiment competing and superseding the first NASA gamma-ray satellite SAS-2 (1972-1973). The success of the COS-B satellite operating for almost 7 years (1975-1982) set the stage for “modern” gamma-ray astronomy. It revealed a richness of point sources, some of which without clear correspondence with otherwise known astrophysical sources (unlike the case of the Crab and few other pulsars) and discovered the first extragalactic gamma-ray source, identified with 3C 273, the first quasar discovered in the previous decade.

The '70s saw the fantastic development of X-ray astronomy which somehow overshadowed the importance of high-energy radiation. The richness of sources and of source types accessible with the focusing X-ray telescopes of which the Einstein satellite (1978-1981) carried the first example, was overwhelming. Besides the improved capabilities of spectral diagnostic and variability studies, the first images were obtained, with strong impact especially on the physics and astrophysics of compact objects, neutron stars and black holes. The hypothesis that massive black holes were responsible for the exceptional activity observed in the nuclei of some galaxies found growing support.

However, a paradox was developing: while the X-ray background was interpreted with increased confidence as the cumulative contribution of sources at cosmological dis-

⁽¹⁾ On this regard we remember Beppo in one of his typical paradoxical veins claiming that one of the greatest achievements of the century was the construction of the zip, which could substitute buttons, but only when the technology offered a reliable, standard and cheap product.

tances [1] none of the extragalactic X-ray source classes showed spectra compatible with that of the background. This led to hypothesize a large population of quasars buried in gas and dust and therefore difficult to find from optical X-ray searches.

With the launch of CGRO in 1991 AGN became a main topic in gamma-ray astronomy. “Gamma-ray AGN” are peculiar with respect to classical AGN. Extreme and powerful high-energy phenomena are revealed: streams of plasma emanating from galactic nuclei with bulk velocities extremely close to the velocity of light (Lorentz factors typically 10). Here high-energy particles are not a “minority”, the bulk motion itself is relativistic (*e.g.*, [2]). Of course “relativistic jets” were not revealed by imaging, but inferred from the extremely short variability timescales coupled to the enormous apparent luminosities giving rise to inconsistencies that can be solved by relativistic beaming of the emitted radiation.

The Beppo-SAX mission (1996-2002) conceived in the '80s with the active and essential support of Beppo and severely delayed by the Challenger disaster combined for the first time X-ray and hard X-ray gamma-ray capabilities. Two fundamental discoveries in high-energy astrophysics followed. The first recognition of a highly obscured AGN ensued from the comparison between the X-ray emission above 20 keV and below 10 keV revealing an extremely high column density, directly supporting previous speculations on the origin of the X-ray background. The first localization of a Gamma-Ray Burst was obtained thanks to the rapid orientation of the X-ray telescope towards the region where the gamma-ray event had been jointly discovered by the gamma-ray burst monitor and by the Wide Field Cameras all onboard SAX. The X-ray position allowed the optical identification of the event showing unambiguously its extragalactic nature (see the paper by G. C. Perola, this volume).

Meanwhile several groups had been developing techniques to detect celestial sources of very high energy (VHE) photons with ground-based telescopes observing Cherenkov light cones associated with leptonic showers. This technique allows to reconstruct the arrival direction of the particle initiating the cascade. The telescopes had been pointed at galactic sources as the most promising, leading to the discovery of TeV emission from the Crab Nebula [3]. After the CGRO discovery that many bright blazars were strong gamma-ray emitters, the TeV telescopes pointed at one of the closest blazars Mkn 421 and obtained the first detection at TeV energies of an extragalactic source [4]. This result showed without ambiguity that black holes can accelerate particles in large numbers up to TeV energies and probably well beyond, yielding an important element to understand the origin of cosmic rays.

2.2. Today and the future. – The quest for obscured AGNs at high red-shifts carried out with CHANDRA and NEWTON-XMM coupled with the deepest searches in the optical and infrared domains (VLT, Keck, Spitzer) is one of the dominant themes in X-ray astronomy today. However it is difficult to establish the AGN nature of a celestial source if no signs of activity are detectable. The mid IR can offer clues but definitive progress awaits a substantial increase in sensitivity in hard X-rays where some emission from the

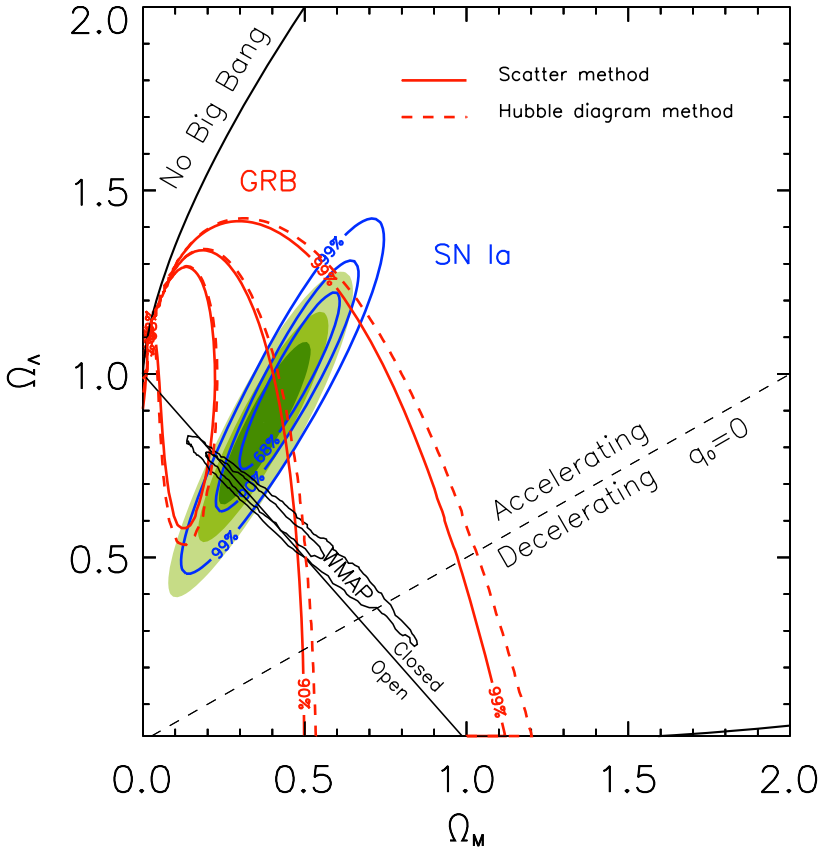


Fig. 1. – Constraints on cosmological parameters, from [5]. Ω_M and Ω_λ represent the fractions of the total density of the Universe in the form of matter and “dark energy”, respectively. Constraints set by the CMB fluctuations measured with the satellite WMAP are shown as irregular contours. Constraints obtained from the red-shift distance relation for Type Ia SuperNovae correspond to the quasi-elliptical contours. Continuous and dashed lines represent the constraints obtained with two methods from 15 GRBs with known red-shift (see [5]).

central engine should leak through. This will come from the realization of focusing optics in hard X-rays/soft gamma-rays, requiring large focal distances that may involve two satellites flying in formation. Projects along these lines are planned for the next decade.

The first discoveries with the SWIFT satellite, a mission inspired by the Beppo-SAX concept, of a few Gamma-Ray Bursts at very high red-shifts (up to 6.295) open up prospects of using Gamma-Ray Bursts as cosmological probes, similarly to powerful QSOs, but with the advantage of a more luminous though transient emission (see fig. 1). While at present the use of GRBs as standard candles is under debate, it is inspiring to

think that they could provide independent constraints on the Cosmological parameters which today seem to point towards a universe completely dominated by dark matter and dark energy, that is mass and energy of which at the moment we have no direct knowledge in other branches of physics.

TeV astronomy with large telescope arrays, of which the HESS and MAGIC projects, both European Collaborations, are presently the most advanced, is becoming a unique window on high-energy processes in the Universe. A census of the sites of production of the highest energy particles injected in the Universe will be possible, thus approaching the problem of the origin of cosmic rays. Significantly the late Livio Scarsi, perhaps the most inspired heir of Beppo's vision, enthusiastically worked to support a project for the detection from space of the most energetic photon cascades in the atmosphere.

On the other hand, with energy thresholds reaching down to the 10–100 GeV range the sensitivity Cherenkov Telescopes will be complementary and competitive with that of the next gamma-ray satellite, GLAST (launch 2007). Already the detection of blazars at intermediate redshifts yields interesting constraints on intergalactic absorption by far-infrared radiation, thus probing a cosmological background not accessible to direct measurements due to the galactic foreground [6].

The forthcoming possibility of studying spectra of individual sources and of the gamma-ray and VHE background itself over an extended energy range, from 1 GeV to 10 TeV, carries unprecedented potential both for understanding the physics of the sources and for probing cosmic backgrounds from the far infrared to the optical range through absorption against bright high-energy sources, a method which overcomes the difficulties of direct measurements.

Features in the VHE background spectrum are expected and could lead to discover signatures of particles thought to be constituents of the presently unknown dark matter, in particular the lightest supersymmetric particle, the neutralino, with fundamental implications for theoretical physics and cosmology.

3. – The renaissance of pulsar physics

3'1. *Pulsars and neutron stars.* – Immediately after their discovery [7], pulsars were recognized as neutron stars, which up to then and for some forty years were only the product of the fantasy of theoretical physicists. Actually neutron stars had already been observed few years before, with the very starting of X-ray astronomy, and the detection of Sco X-1 [8]. However only in the seventies the nature of Sco X-1 was understood.

Neutron stars and pulsars are the Holy Grail of physicists. Their radius is three times the gravitational one, and for millisecond pulsars the rotational energy is a percent of the mass energy. Pulsars are indeed special and general relativistic stars. The very structure, and in particular the mass radius relation depends deeply on the nuclear potential. Large parts of the star are superfluid and superconducting. Yet the external magnetic fields are generally as large as 10^{12} – 10^{13} G, and in some cases (magnetars) they can exceed 10^{15} G, a regime where quantum electrodynamics effects dominate over the classical ones, and the sole magnetic energy is a percent of the mass energy.

Pulsars are fabulous clocks. They can speed down as little as few picoseconds in a year and the slowing down is so regular that one can measure period derivatives, and predict safely the phasing. Because they are such good clocks, pulsars are the only astrophysical objects for which one can measure the energy loss, independently from the observation of the luminosity. For most pulsars the former quantity is just a tiny fraction of the latter. In most cases pulsars, even if losing energy at a substantial rate, are visible only in the radio band, since there the emission is exalted by a coherence condition. It is the absence of spectral information which justifies after some 40 years our ignorance on pulsar electrodynamics, for which a plausible and self-consistent scenario is still missing. Interestingly enough, apart from the radio, young pulsars may show up in gamma rays. This was demonstrated by SAS-2, COS-B, and the Compton Gamma Ray Observatory and represent a strong motivation for future mission like GLAST, which may unveil the mode of energy loss of pulsars.

3'2. *The Hulse-Taylor system 1913+16.* – The perfection of pulsar timing makes them ideal probes of the space-time, and in this regard it is of little importance that the observations are only in the radio band. Many pulsars are in binary systems. In a way the most impressive cases are those where the system is very close, and the pulsar companion is itself a collapsed object, a white dwarf or a neutron star: the case of a black hole, even if eagerly searched for, has not yet been found. The prototype of neutron star/neutron star system 1913+16 was discovered by [9] with the Arecibo radio telescope, which for many years was the only instrument capable of detecting the source. The system exhibit a number of general relativistic effects: the periastron precession is five orders of magnitude larger than that of Mercury. The pulsar timing has enabled to observe also the orbital shrinking of the binary system, in perfect agreement with the predictions of General Relativity (GR) for energy loss through gravitational waves. The pulsar mass has been measured with an unprecedented precision of few parts in a thousand. 1913+16 has become the most successful laboratory of gravitational theories, and has dominated the field for 25 years.

3'3. *The Double Pulsar 0737+30.* – The recent renaissance of pulsar physics is in large part related to the pulsar program planned at Parkes, through new deep pulsar surveys using a multi-beam receiver, focused at the search of millisecond pulsars, covering also galactic regions previously unexplored. The team is basically a collaboration of British, Italian, and Australian scientist. The British Italian connection reminds of the ones in the thirties and forties, where Beppo was a protagonist. From the Italian side the initial experience on pulsars derived through the participation in the COS-B mission, and one can trace again in that, through a number of scientific generations, the leading touch of Beppo.

The most sensational result of the Parkes collaboration is the discovery of a double pulsar system 0737-30 [10, 11], which is a magnificent general relativity laboratory, in many aspects much superior to 1937+16, since the system is much closer, and especially because both pulsars are directly measurable. All classical tests of GR can be performed:

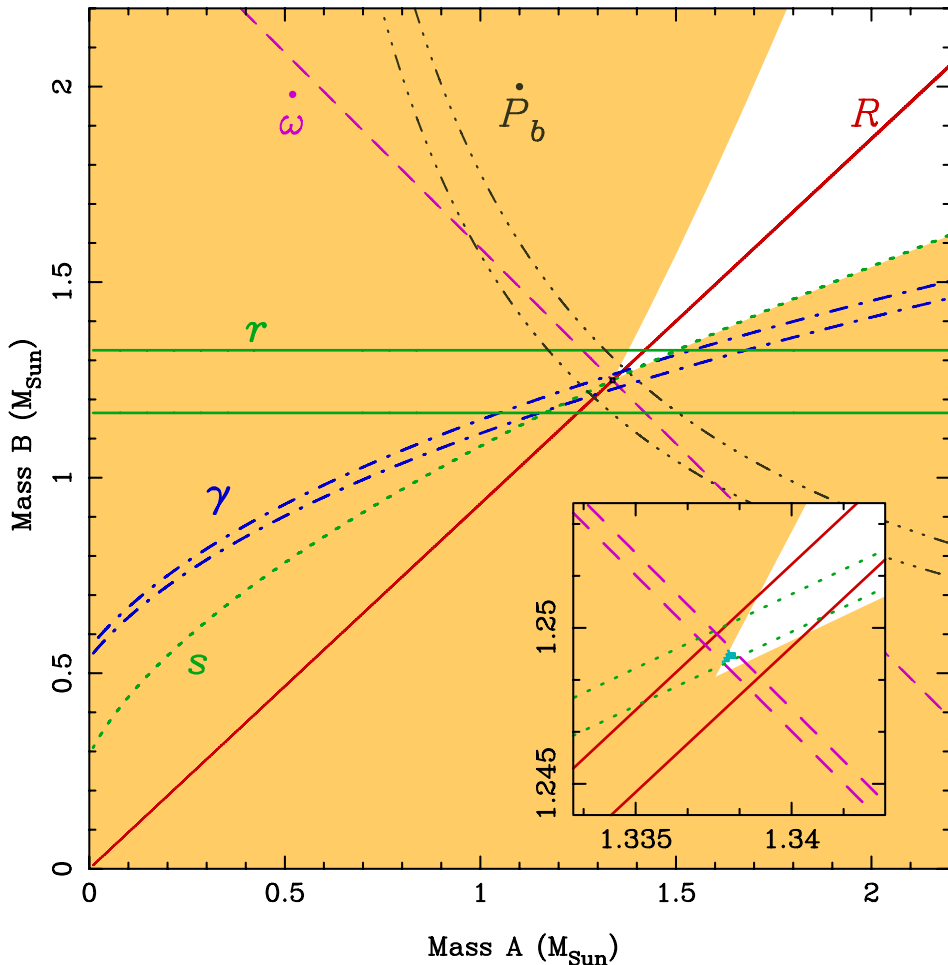


Fig. 2. – The masses of PSR 0737-30 A, B. The superposition of the strips and of the cusp represents the allowed region. For details see M. Kramer *et al.*, *Science* 2006, in press (astro-ph/0609417) from which the figure is taken.

the periastron precession was observed in days (17 deg/years). GR effects are so large, that one should take into account second-order terms in the post Keplerian expansion. A direct measurement of neutron star moment of inertia is close. As an example, we report in fig. 2 the region of masses permitted for the two neutron stars in the system. GR has been proved with unprecedented precision, and the stage is approaching for the search of deviations from GR.

There are various other fields where the Parkes collaboration has produced magnificent results, *e.g.* the probing through pulsar timing of the gravitational field inside globular clusters (*e.g.* [12]) or the discovery of transient radio pulsars [13]. The activity on pulsars appears now to have new impetus all around the world.

3'4. The future: Pulsars as gravitational wave detectors? – It is hard to foresee what the results of these renovated efforts will be. However we would like to point to one project which, though theoretically considered since the discovery of pulsars, may only now arrive at some achievements: the use of an array of very stable pulsars, distributed through the Galaxy, for detection of gravitational waves, which would disturb the pulsar timing (*e.g.* [14]). May this relatively inexpensive technique compete with over-ambitious space missions? It is natural to remind of the physics of cosmic rays and elementary particles, through photographic emulsions, which were the only affordable technique in the hardships of post war years. They yielded the fundamental result of the discovery of pions and their decay into muons, immortally related to the name of Beppo.

4. – Conclusions

From the point of view of fundamental physical problems, the two issues considered in this paper are intimately related. Gamma-ray astronomy reaches black-hole physics and cosmology, including the possible detection of signature of the constituents of Dark Matter (neutralino annihilation) or of relics of the Bing Bang (cosmic strings, monopoles).

Gamma-ray bursts are undoubtedly the result of a collapse of a massive star into a black hole either in a single event or through the coalescence of an extremely close binary such as those first discovered as pulsar binaries. Their study will allow to get more insight into the physical phenomena acting in regions characterized by strong gravitational fields, a realm which thus far is essentially unexplored.

* * *

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PERSONAL REMINISCENCES

Personal remembrances

BERTRAM STILLER

Washington, D.C., USA

Shortly after their arrival in Brussels, Belgium, to take up professorships at the Université Libre, the Occhialinis, Beppo and Connie, organized the first International Conference on Cosmic Ray Research with Nuclear Emulsions, fig. 1. It was my good fortune to receive an invitation to this conference.

During the conference, I was invited to their home for dinner and, because my hosts had planned to see a John Ford movie that night, they asked me to baby-sit their infant daughter, Connie Pooh, today known as Etra. Although somewhat surprised, I nevertheless agreed, since I had already experienced the infancy of my two sons. The evening went well, and upon their return, they asked me to spend the night since it was 1951 and taxis were still difficult to find. I readily agreed before I realized that there was only one bedroom, and that I would bed down on the living room floor. In the morning, I was awakened by a familiar chattering which turned out to be coming from a group of amused young women, who were the Occhialinis' scanning team. There was no room at the lab, as yet, and so microscopes had been set up in their living room. We had used them the previous evening, but they failed to mention that the scanning team would be in residence in the morning.

With this unexpected and startling experience began a friendship between us that lasted for well over 50 years. Although we shared a strong interest in cosmic rays, elementary particles, space physics, and in the use of nuclear emulsions as particle detectors, we never had the opportunity to work or publish together.

In July, 1957, while we were both attending the First International Symposium on Nuclear Emulsions in Strasbourg, France, Beppo invited me to visit his cosmic-ray research group at the University of Milan. Later that month, I went to Milan and spent a few days visiting his laboratory. Beppo asked one of his microscopists, who knew some English, to act as kind of a mentor during my visit, since I could not speak Italian. This



Fig. 1. – Beppo and Connie, Université Libre, Brussels, Belgium, 1951.

was Mila Puppo, who was such a wonderful mentor that she became my wife in 1960, and this event has made the years since then as happy and joyful as one could possibly expect. This accidental and totally unexpected occurrence was something that the four of us would marvel at, on many occasions.

During 1959-1961, while Beppo, Connie and Connie Pooh were at MIT, at the invitation of Bruno Rossi, I spent a number of months in the Physics Department of the University of Milan, on Via Saldini, working with Beppo's graduate students. Beppo, after having visited our group at the Naval Research Laboratory in Washington, D.C., asked me to introduce his students to some of our ideas in automating track measurements and emulsion processing equipment that we had developed at NRL. I was keen to do this because it would give me an opportunity to become better acquainted with Mila. When we decided to marry in 1960, the Occhialinis were kind enough to offer us their apartment on Viale Argonne, during their stay in the States, which offer we were happy to accept because of the high cost of rental housing in Milan. Shortly after moving in, we learned that Freddy and Elizabeth Herz were also joining Beppo's group, and so we invited them to stay with us. Thus the Occhialini apartment became host to a "*mélange-à-quatre*" of newlyweds.

Since Mila, my wife, had all her family in Italy, it became customary for us to spend our summers in Italy. Thus it was, that we began to spend part of each summer visiting with Beppo and Connie. During the week that we generally spent with them, we managed to cover not only the latest turns in Italian and U.S. politics but also comings and goings of their many friends in science. In particular, the physicists F. Houtermans and B. Pontecorvo were closely followed, not only because of their research, but also because of the unusual political positions that they had both chosen for themselves. Of course, Beppo's experiences with two Nobel Prize winners, P. Blackett and C. Powell, were also topics that we returned to with fair regularity.

The research using a counter-controlled cloud chamber embedded in a magnetic field—a system which Beppo and Blackett developed together—led to their confirming the existence of the positron, a positively charged electron, and to their discovery that electron-positron pairs were produced by gamma rays. In 1948, Blackett received the Nobel Prize and the award was, in part, for the research done with Beppo. Blackett acknowledged this debt by opening several paragraphs in his Nobel Address with the phrase, “Occhialini and I did...”. Beppo always spoke of Blackett with great warmth and admiration.

When Beppo joined Powell's group at the University of Bristol, he realized almost immediately that the nuclear emulsions being used to study cosmic rays needed to be increased in sensitivity, so that they would be able to register tracks made by high-energy charged particles. He induced C. Waller, a chemist with Ilford Ltd, to attack this problem, and after some intense efforts, a much increased level of sensitivity was achieved. Beppo then sent stacks of these sensitive emulsions to be exposed on mountains tops, at the Pic du Midi and at the Andes. These exposures then led to the discovery of the π - μ decay which, as is well known, was a fundamental discovery for high-energy particle physics. Powell was awarded the Nobel Prize in 1950 for the development of sensitive nuclear emulsions and their use in cosmic-ray physics. Powell failed to mention Beppo in his Nobel Address. If ever there as a poor award of a Nobel Prize, this is the prime example. Of course, Beppo remained disappointed about this for the rest of his days.

In 1968, Beppo and Connie acquired a summer cottage in the town of Cinquale on the Italian Riviera, some kilometers north of Viareggio. It was an A-frame house, of small dimensions, sitting on a flat plot of ground.

Here, they began their first ventures into gardening and landscaping. There was much discussion about where to plant trees, fencing, shrubbery and so on, in addition to the modifications to the cottage that Beppo planned. Mila, having spent the war years in a small village, Ponte Organasco, in the Apennines, was much more acquainted with gardens than I, and so she became the advisor to Connie. My role, because of the well-known *fai da te* knowledge of Americans, developed into discussions and disagreements with Beppo, about modifications that he wanted to make to the house. Beppo had grown enamored with power tools that he found in the U.S. and so one of his dreams was to parallel-wire the cottage for 110 volts a.c. so that he could operate U.S. power tools. This we accomplished without too much difficulty.

The Superstrada A-12 had only been built from Genoa to La Spezia at the time that Connie and Beppo were summering in Cinquale. When the extension to Pisa was



Fig. 2. – Beppo, San Pellegrino.

designed, the plan put the highway almost into their backyard. This was the end of summers on the Italian Riviera. It was a real disappointment for them because of all the effort they had put into the cottage. There was another possibility; a mountain cottage that Beppo had inherited from his father. Unfortunately, Connie was not a mountain person and so this had no great appeal for her. On the other hand, Beppo, having spent some years in the Andes during the war, was quite keen on rehabilitating this long-abandoned house, fig. 2.

It was located about a kilometer from the village of San Pellegrino, in the middle of the Alpi Apuane, at an altitude of about 1500 meters. Everything such as food, structural materials, tools, etc., had to be carried from the town along a rough path through fields, brush and woods to reach the house. After much hard work, Beppo decided that a wagon would be an ideal alternative to carting everything on one's back. Pulling the cart over the path turned out to be almost as tiring as carting things on one's back. At any rate, here we began an even larger set of renovations than on the cottage in Cinquale. After all, that had been a livable home to start with. Here there was no heat and even in summer, at that altitude, nights are not very warm. But here, Beppo invited in friends from Milan, since I generally spent only a week or two with him, and there was an ongoing series

In 1979, the Wolf Foundation of Israel awarded Beppo the Wolf Prize. This award, which has also been given to a number of Physics Nobelists, came as recognition of the importance to physics of the research that Beppo had done, not only with the two Nobelists I mentioned earlier, but also in recognition of the discoveries that he and his group had made since the time of his return to Italy. It was the first and only occasion on which I received mail from Beppo. A postcard with two words; "mission accomplished". The monetary part of this award made it possible for Beppo and Connie to seek a home suitable for their retirement. They found a grand old Tuscan farmhouse, named Cannarecchi, near the village of Marcialla, in Tuscany, about midway between Florence and Siena. They gave up their apartment in Milan and took up residence at Cannarecchi. This led to some changes in their lifestyles. Connie learned to drive because they were no longer within walking distance of shops. Beppo purchased a wonderful small motorbike. And using the two vehicles, they began to deal with a new life as displaced urbanites. Of course, the new house needed more than the usual renovations. It was sufficiently large and old, consisting of two stories plus a partial basement, and a large barn, that there were many projects to plan. Connie now pitched into these projects in much greater measure than she had before. We updated our transport to a splendid 1975 Alfa Romeo Giulia Super Berlina and so we were able to run down to Marcialla more easily than before. In fact, shortly after they settled into Cannarecchi, Beppo was invited back to Israel, as were other recent Wolf Prize winners, for the presentation to the next winner. Mila and I went down to house-sit while they went to Israel, and we had a great time looking after the house, their cat and dog, and Connie's garden. Although I never drove my Alfa down from San Pellegrino to Castelnuovo, the road between Marcialla and Cannarecchi has a fine, steeply descending S curve which was always a high speed challenge.

As the summers ticked by, Cannarecchi was slowly fitted with some modern conveniences but most of the interior was left untouched. A beautiful front door was fitted. A pond appeared one year, which Beppo said was required in the event of fire. In truth, the total volume of water it held would hardly have dealt with a serious house or brush fire. Of course, the inevitable 110 V a.c. was installed, along with a fine workshop area, which was usually too cluttered to be able to find workspace. Best of all perhaps, was the apartment that Etra, now an architect, designed and had built into the former pig sty, which was part of the ground level of the house. Summers being what they are in Tuscany, this became the most desirable sleeping room in the summertime. Connie's vegetable garden was always in need of some additional spading, and there was never a dearth of physical activity when we visited them. An unfortunately cold winter led to the death of many of their trees, and this became a reforestation project with the aid of the Italian Forestry Service. Cannarecchi became a permanent home for Beppo and Connie, and there was a steady stream of company coming and going. Reaching there without a car was done with some difficulty. A bus route existed from Florence but it only went through Tavarnelle, on its way to Siena. This meant that someone had to meet visitors who came on the bus, since taxis were not easily found. Beppo's bike was not up to carrying passengers, and so Connie and her VW Beetle served as a "taxi". Tavarnelle had many shops so there would always be something to buy, making such taxi service useful for the household.

After Beppo passed away in 1993, we felt even closer to Connie and tried to visit more than once each summer. Mila's parents began to need our help, and so I would visit Connie by myself, taking the train and bus instead of driving alone. These visits involved helping her with her bee hives, her wine cellar, vegetable garden, and her distillation of lavender flowers to make lavender essence. Also, it was time for nostalgia, after the many years that had been shared. But inevitably, Italian and U.S. politics kept us up far into the night. We made our last visit in the summer of 2000, when it became clear to me that Mila's deteriorating health would prevent us from visiting Cannerecci again. It took Etra and myself a lot of scolding of Connie to obtain a computer and learn to use it. Thereafter, we kept in touch by means of ubiquitous e-mail messages until Connie entered a retirement home on the Italian Riviera. And now there are just Connie Pooh and me, to reminisce about Beppo and Connie, and that night long ago in Brussels.

Working with Beppo: Personal recollections

RICCARDO LEVI-SETTI

The Enrico Fermi Institute and Department of Physics, The University of Chicago, USA

My association with Beppo lasted about six years, from the Summer of 1950 to the Spring of 1956. It was an intensive, rigorous training in many areas of scientific, historical, literary, inventive, political, and, yes, even artistic human endeavour.

My first encounter with Beppo's personality dealt with our interpersonal communication. Beppo's thinking was extremely fast, and often overlapping thoughts were expressed in incomplete sentences. It took long hours spent in the darkroom with Beppo, while monitoring the processing of thick nuclear emulsions when I first joined him in Brussels, for me to begin to follow his rapidly changing lines of thought so that I could answer in sync. This phase was eventually overcome, and in the process, I was exposed to fascinating Beppo's recollections, ranging from his adventures in the Amazon and Mato Grosso in Brazil, to his work with Patrick M. S. Blackett at Cambridge and Cecil F. Powell at Bristol. I soon came to the realization that some of the scientific attributes of these mentors may have rubbed off on Beppo, and, indirectly, I benefited from this exposure myself. Here I was, confronted with the "deus ex machina" who was largely responsible for the discoveries which brought the Nobel prize to his two tutors, telling me what he learned from his association with them, without expressing any bitterness from having been shunned from sharing the deserved reward. I was deeply moved.

Beppo's inventiveness transpired immediately in the training I was fortunate to have been exposed to. There was a three-dimensional way of thinking about designing laboratory equipment, from the plumbing needed in the specialized vats needed for processing photoemulsions, to the delicate devices to be constructed for improving the precision of microscope measurements which I was assigned to perform. I remember clearly how several of his inventions were immediately incorporated into custom-built microscopes with the aid of Ing. Cantù of the Koristka microscope factory in Milan.

The rigours of the scientific method were imprinted by Beppo in my mind and that of my coworkers every time something new was found in our lab, and our results had to be conveyed to the scientific community in a convincing manner. This process usually took place at night, and I mean sometimes through the entire night, after we presented Beppo with a timid draft of one of our papers. There we were exposed to Beppo's dialectic abilities which generally threw to shreds our data and presentations, and often sent us back to the lab. At times, even if Beppo was (secretly) in agreement with our logic, he enjoyed taking an adverse, opposite viewpoint, and we had to struggle to defend our conclusions. And then, there was the process of finding an appropriate title for our papers, which was to be suggestive of a discovery, but had to preserve an escape route if we were carried away by our excitement. A delicate balance was to be attained, and that is why the titles of our papers contained subtle nuances manifested through the use of the adjectives "possible" or "probable". As a result, our papers carried a great deal of Beppo's guidance, and it was his generosity toward us that prevented him from adding his name among the authors. The same can be said of Connie (Occhialini-Dilworth), who not only helped us with measurements and critical insights, but who was always in charge of making our English intelligible.

The period in which I worked with Beppo marked the transition between individual experimentation and collaborative work with a number of colleagues, often residing at different institutions. This transition required at first an education with which we were not familiar. I learned my lesson when I tried to present a new finding of mine, and I was reminded that this finding was not entirely mine but belonged as well to distant researchers with whom I was tied by a collaboration agreement. I soon benefited by this ethic when the reverse situation occurred, and step-by-step we all became involved in collaborating successfully with 36 co-authors from six laboratories. This was the beginning of the communal efforts which became the norm in high-energy physics research, to reach enormous proportions at the modern TeV-region particle accelerators.

Besides having developed and perfected the technology of processing thick nuclear photographic emulsions, Beppo excelled in ordinary black and white photography, both from the technical to the artistic viewpoint. Still vivid in my mind is a day spent in Verona with Beppo, taking pictures of the 24 romanian bas-reliefs on the doors of the San Zeno cathedral at various times of the day, to let the shadows from the sun vary the moods of our photographs. And then, the action continued in the darkroom, where the principles of temperature development to which we were accustomed in dealing with nuclear emulsions were transferred to the printing of photographs. Thus, I learned how to modulate light and shadows while printing, to correct unwanted exposure gradients, by locally warming the paper in the developer bath, and many other tricks of the trade which could make photography akin to painting. I definitely owe it to Beppo to have inspired in me a passion for photography at both levels mentioned above, so that my career after Beppo always pivoted around photographic methods of physical research. After nuclear emulsions, I dealt with bubble chamber pictures of elementary particle phenomena, and, in a major switch of interests, with the development of high resolution scanning ion microscopy and imaging secondary ion mass spectrometry, and its applications to

materials science and biology. Three-dimensional thinking, exercised with Beppo, was of great help in the design and construction of my ion microprobe. The use of this novel instrument compelled me to deal with another switch, from film photography to digitally-acquired images and computer-based image processing and printing. Even with this new tool, some of the canons of artistic photography inherited from Beppo, such as the importance of composition, have become an instinctive guidance. This can be detected now in my analytical maps of chromosomes (my presently sponsored biological research), and in my pictures of trilobites (my palaeontological avocation).

In conclusion, working with Beppo, although not always easy, left a profound imprint on my scientific personality, and I have no doubt that this has been the case with all my early collaborators who had the fortune of sharing my experience.

G. P. S. Occhialini: One of my masters

SERGIO P. RATTI

Università di Pavia, Italy

I think I had four “masters” in my life, over and above my parents: Giovanni Polvani, Beppo Occhialini, Marcello Conversi and Martin M. Block: four persons so different, for their character, culture, personality; mostly for their approach to younger physicists and students.

Giovanni Polvani was my “master of life”, he hired me as “assistente incaricato” no more than 1 month after my graduation, November 14th, 1957; he hired me in a permanent position as assistant professor on February 2nd, 1959. When I got married he said to my wife: “young lady remember: 80% of your husband’s career depends upon your patience, understanding and cooperation”. She did that for over half a century now. Incidentally, Beppo wanted to call up a meeting the day of my wedding and insisted that I should postpone the ceremony not to interfere with the scientific program. I did not do that and he came anyhow to the wedding party telling his proverbial jokes. Polvani was the “Professor” with a capital P. He talked very rarely to me; in my career I visited his apartment only once; I always addressed him as a third person as usual in the Italian language when formally talking to a person. I was assisting him during the exams. For at least 6 years, over and above my teaching assignments, I held classes in his place while he was elected Rector of the University and later President of the National Research Council —CNR (he used to say: “my son, don’t touch thermodynamics, just tell me when you get to the point”. Close to the point —thermodynamics having been postponed to acoustics and waves— he would say: “my son, don’t touch the II Principle. I want to cover that, it is too important”. And only too close to the end of the term he would apologize for not having time to go personally into the classroom and give me permission to cover II and III Principles. For the benefit of the reader, the II Principle sounded as “the trace left by Joule’s eddy experiment is indelible!” proving a very personal view of the subject). On the other hand I had maximum freedom to organize my own research. When he died, I felt the loss of my second father.

Marcello Conversi —while at the University of Pisa— starting around 1955-56, was the co-leader, together with Carlo Succi the senior assistant professor to Polvani, of the experiment in which Carlo Rubbia and I did our thesis work in a small mountain laboratory in Val Formazza, northern Italy. When Carlo got the Nobel Prize in 1984, Marcello, in an article written on one of the major Italian magazines, was so kind to quote my name as one of his students. Many years later, Marcello, then at the University of Rome, joined an experiment I proposed at the Triga Mark II reactor of the University of Pavia —where I was appointed as full professor in 1972— to search for neutron-antineutron oscillations, predicted by one version of $SU(n)$ symmetries and strongly supported by Bob Marshak, a friend of mine, past President of the American Physical Society, President of the City University of New York, the inventor of the $V - A$ theory of weak interactions. With him, intense was the discussion of several physics issues, ranging from diffraction dissociation, resonance classification, Regge trajectory role in hadron physics, to cosmic ray physics and future development of accelerator technology. He asked me once to move to the University of Rome, in an ambience where Enrico Persico, Giorgio Salvini, Giorgio Parisi, Nicola Cabibbo, Carlo Bernardini, Giorgio Careri, among other, where the leading scientists. He wanted me to go there one day to discuss seriously the possibility. I took the plane, went there and at the Alitalia Air Terminal (at that time into the same building of Stazione Termini), tried to get a taxi. It was a nightmare: people fighting to jump into the cars, shouting aloud. I was so discouraged that, as soon as I saw Marcello I told him I would rather continue to live into the provincial, parochial, . . . sleepy Pavia. We remained friends, I visited him rather regularly till he passed away many years ago now.

Martin Block was my master independent of the physics projects in which we were and are involved. I spent the academic year 1961-62 with him at Northwestern on sabbatical thanks to Beppo (I'll go back to it later) working on hyperfragment physics detected in a liquid helium bubble chamber that I still consider superb: it was a little larger than a children shoe box $8\text{ cm} \times 10\text{ cm} \times 20\text{ cm}$. During my sabbatical I wrote as many as 7 papers in a year, including one of the first measurements of the Λ^0 -hyperon lifetime; the measurement of the Λ^0 - K^- relative parity; the (still unique) measurement of the s -wave contribution to the totally neutral decay $\Lambda^0 \rightarrow n \pi^0$; the measurement of the Michel ρ parameter for the β -decay of negative muons, still the only experimental proof that the $V - A$ weak interaction basic parameters ($\rho = 3/4$ is the predicted value, $\rho = 0.751 \pm 0.035$ the measured value) must be the same for particle and antiparticle. During that year we almost destroyed a desk by kicking it with our feet in excited discussions on how to invert error matrices into statistical weighting factors in a certain computer program. His character is well known for not being among the mildest in the world; however, after 45 years we are still very good friends. He continuously invites me to visit him in Aspen, Colorado, where he now lives, but I could not (yet) find the time to do so, as I got plenty of things to do here in Italy.

Beppo: G. P. S. Occhialini was quite different. I never published a paper with him, I never worked in any of his experiments. Nonetheless he has been one of my “masters” (on an occasion I’ll mention later, Bruno Pontecorvo said: “I suggest to all of you to collaborate with Beppo Occhialini, he won’t get a Nobel Prize, but you would have a good chance...”). Is it a sign of destiny?). When I was a student, I never had the privilege to listen to a lecture delivered by him in person. Alberto Bonetti and Livio Scarsi did that for him. However I learnt later that he was not “absent”; he used to meet his assistant professors in his house till very late at night to discuss all details of the lecture, to find out the best way to propose a new subject. I remember when I took the exam “Fisica Superiore” (actually, since 1991 till the new reform I held that chair in Pavia): I studied that exam together with Carlo Rubbia mostly in the laboratory up in the mountain. As “poor laureandi” we used to spend 25 days at the mountain laboratory and one week home; 25 days up, one week down. All that for about one and a half year. We were asking and answering each other the funniest questions and make calculations, discussing whether or not a π was proper in a formula or rather a 4π or even an 8π (due to the 2 spin projections of an electron or the 2 polarization states of electromagnetic waves). I felt very confident when I got to the table in a classroom located in the basement of the Physics Institute in via Saldini, Milan.

There were Beppo, Livio Scarsi and Alberto Bonetti; the exam lasted —as usual— about an hour and had nothing to do with the program written in the book and covered in all the 40 lectures of the course. At the end, after a brief private consultation, they called me back in the classroom and said: “Ok, you got an A (30/30)”. My face did not look “that” happy and they did ask me why. In Italy there is the possibility of getting an A⁺ (laudem-lode) and I thought that, after all they did not ask any “normal” questions and that I could have forgotten all the program and they would not know. Therefore I spoke out, gently but very plainly, my opinion. Beppo said: “Ok, let me then ask you another question in the program”. At that point Alberto and Livio both said with almost an unique voice: “No Beppo, the student is right, we discussed physics with him and asked questions proper for an exam to appoint an assistant professor. It was not a normal exam. He is close to being a professional young researcher” and they added a “lode” to my vote. I felt very proud in that particular instant; furthermore I had the impression that from that moment Beppo looked at me smiling with more affection.

He was the first “professor” who allowed me to address him using the confidential Italian second person. He was on my thesis examining committee; my thesis advisor was Giovanni Polvani with a substantial help from Carlo Succi (and a minor contribution by Riccardo Giacconi at the beginning, very few months before he left to the United States).

The arrival of Occhialini in Milan encouraged the formation of a group to build Wilson chambers to be triggered —since that was the technique with which he did in England the search that did not gain him the Nobel Prize— and exposed to cosmic rays. Actually in the late fifties Carlo Succi and his group built perhaps the largest Wilson chamber (about 1 cubic meter sensitive volume) ever used in a cosmic-ray experiment.

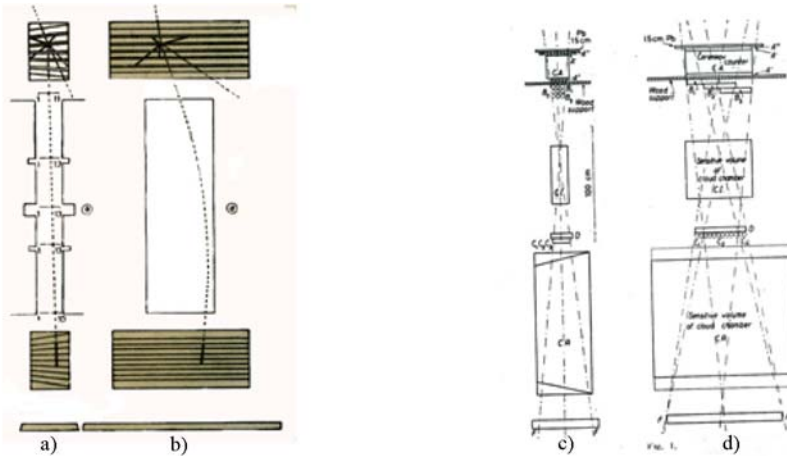


Fig. 1. – a,b) the apparatus of the Russian experiment, measuring the mass of particles produced into the upper chamber and stopped into the bottom plate-chamber measuring the mass by means of momentum and range; c,d) the apparatus of the Milan-Pisa experiment measuring the mass of a particle by measuring the relativistic velocity through the ionization left in the upper CI chamber and the light produced into the Cherenkov counter, plus the range in the bottom CR chamber. At the very bottom an anticoincidence counter rejects the through tracks.

In present terms my dissertation would have been preparing an apparatus to search for the τ lepton, then known “only” as heavy muon, based on the use of 2 triggered Wilson chambers. At that time a Russian group (or rather Georgian?) led by Alikhanian (A. Alikhanian, A. Alihanov, H. Weissenberg, V. Morozov, G. Muschriswili, A. Dadaian, N. Shostakovich, A. Akaian and many others) in cosmic-ray experiments, exploited from 1948 till 1957, detected a number of non interacting particles, similar to the muons, generated in a upper Wilson chamber, stopping in a set of carbon plates 1cm thick of a second Wilson chamber separated by a horizontal magnetic field. The mass was the result of the contemporary measurement of momentum and range. Figure 1a,b shows their setup. Those experiments claimed the detection of particles having a mass $m = 550 m_e$, m_e being the electron mass, *i.e.* about $280 \text{ MeV}/c^2$. The title of my thesis was “*Design of an experiment to search for a particle having mass 550 electronic masses*”; unfortunately it is now known that the mass of the τ -lepton is around $1.8 \text{ GeV}/c^2$. By measuring the relativistic velocity β (from ionization with a “top” helium Wilson chamber and/or with a water Cherenkov counter —fig. 1c,d) and range in a “bottom” cloud chamber, equipped with a set of carbon plates 0.3cm thick, we did not detect any of such particles but rather exactly the predicted number of nuclear interactions ending into nuclear stars delivering fragments which would not cross more than 3 plates, *i.e.* fragments which would be “not” visible in the gas gaps outside 1 cm thick carbon plates.

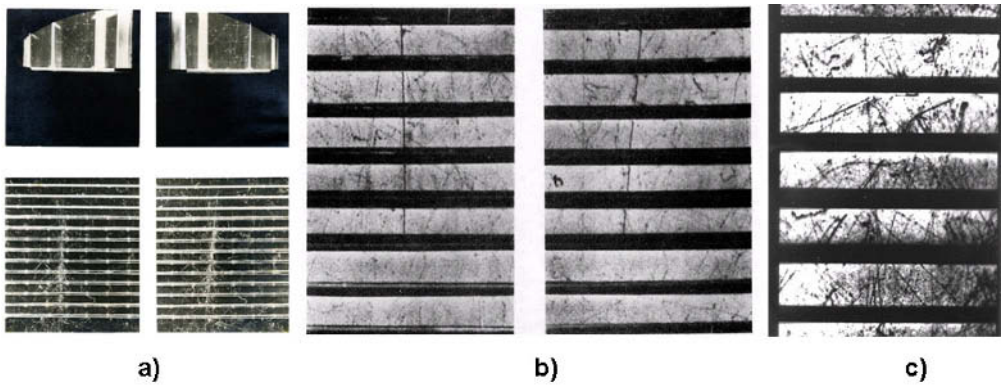


Fig. 2. – a) Polaroid picture showing a particle crossing the ionization chamber and producing a high-energy interaction in the 5th plate 0.3 cm thick, whose secondary particles are absorbed by the remaining plates, thus not triggering the veto counter; b) stereo-pictures of a stopping muon; c) an enlarged view of a neutral interaction in the second visible gas gap, with a produced pion plus secondary fragments stopped within 1 cm of carbon (3 consecutive plates).

Figure 2a shows a Polaroid picture taken to check the performance of the 2 chambers containing an incoming cosmic ray in the ionization chamber (where the electric field separates positive and negative ions condensed into 2 separate parallel tracks); fig. 2b shows a stopping muon in plate 7, while fig. 2c shows a neutron star in the second gas gap, producing fragments all stopped within 3 plates, *i.e.* stopped by less than 1 cm of carbon.

Figure 2a shows a Polaroid picture taken to check the performance of the 2 chambers containing an incoming cosmic ray in the ionization chamber (where the electric field separates positive and negative ions condensed into 2 separate parallel tracks, while fig. 2b shows a neutron star in the second gas gap, producing fragments all stopped within 3 plates, *i.e.* stopped by less than 1 cm of carbon.

I was told Occhialini appreciated my thesis work. I think he had a role in having assigned to my dissertation the “Giovanni Gentile jr.” Prize for the best thesis 1957.

I still have the picture (fig. 3a) of the major professors in the examining committee — Giovanni Polvani, Piero Caldirola, Giuseppe Occhialini, Carlo Salvetti, Guido Tagliaferri, Antonio Lovati— taken the day of my thesis defense. Among the “laureandi”, that day were Guido Vegni and Elisabetta Abate (who married a German physicist and lives now in Bochum), a skinny and short student whose name is Skof (known by us all confidentially as Skoffino —I do not remember his name) who was later appointed associate professor at the University of Bari, plus other students whose names do not come to my mind after half a century (fig. 3b).

In 1952, the Istituto Nazionale di Fisica Nucleare (INFN) was founded in Italy as a branch of Comitato Nazionale per l'Energia Nucleare (CNEN). The first director was



Fig. 3. – a) From left: Beppo Occhialini, Giovanni Polvani, Carlo Salvetti, Piero Caldirola (in the background, from left: Guido Tagliaferri, Antonio Lovati and Elisabetta Abate); b) the students getting the title: from left: a skinny Guido Vegni, not identified, E. Abate; not identified, Skoff, myself, not identified.

Piero Caldirola and the second Beppo Occhialini. His direction was not free from some “extravaganza”. To move from one town to another, from Milan to the mountain Lab, or to CERN that was being built at fast speed, all of us had to fill in a form: where to go, when to go, how to go. Among the possible transportation means the form contemplated: train, airplane, mule, sleigh, roller skates, camel (just in case an experiment would happen to be done in the dunes of the desert). Milan was the only INFN Section having listed all those transportation possibilities. I found it no more than “peculiar”. Some year after graduation Beppo decided to move to cosmic and space physics with Livio Scarsi and Alberto Bonetti who, a bit later, got a full professorship in Florence. Guido Vegni—together with Mariella Di Corato and Adele Sichirillo—were fully immersed in experiments with nuclear emulsions: the G-stack collaboration was—perhaps—the very first worldwide collaboration set up in high-energy physics, even after CERN entered into full operation. Ettore Fiorini and myself—both very young indeed—wanted to move to bubble chamber physics. We were introduced to the CERN community and joined the analysis of the heavy liquid bubble chamber BP3 built by André Lagarrigue at the Ecole Polytechnique.

The adventure to CERN was the result of several discussions with Beppo. He used to invite us in his apartment for after dinner meetings. In his family there was a strict hierarchy established all along: the little dog Dynah a dachshund, was the boss; then came Connie Pooh, then Connie and—he used to say—after the janitor, there was Beppo, ready to say always “yes” (but I do not believe a word of all that!). My personal believe is that the real boss was Connie Dilworth, authoritarian enough to manage the family with Beppo delegating to her all possible family affairs, but Connie Pooh whom he loved immensely, I believe. For the little girl it must have not been soo easy to deal with such an extraordinary father, living thirty years ahead of everybody around.

Beppo's character was not that easy; he liked a lot to discuss by paradoxical arguments; as of a sudden changing subject for a long while, then going back to the original point without an apparent reason. On the contrary he was miles ahead of all the others; jumping from an issue to another was a kind of joke for him, jokes built up with words. As Emilio Segré said once: "he is a living example that appearance is often misleading: he seems to be a little crazy but is full of good sense: he seems to be naïve in physics, while he knows a lot; he speaks 4 or 5 different languages but it is very difficult to understand him in any of them". His office looked very similar to being a real mess; appearing totally disorganized he would be able to put together a worldwide collaboration covering 3 to 4 continents. He was so full of ideas that not all of them were within intellectual and technical reach.

During several house meetings we investigated the possibility of using our cloud chamber at CERN. Honestly bubble chambers could collect data at a much faster rate; they could be filled with hydrogen or deuterium, both very neat and nice nuclei indeed; the dead time of the cloud chamber was too long and it could not recover adequate sensitivity to meet the fast rate of the accelerator beam. One night, Beppo came up with one of his "paradoxical" proposals: "why don't we build a dozen cloud chambers or so" he said, "organize them in such a way that they can be exposed, one after the other, matching the pace of the accelerator beam, for instance moving them on a round, circular rail, say some 15 m in diameter, thus letting enough time for the Wilson chambers to recover their sensitivity?". The idea was not unreasonable at all. We took it seriously but soon we had to realize that —over and above the "bureaucratic burden" of going through the CERN establishment— we had neither the manpower nor the financial resources to build a dozen $2\text{ m} \times 2\text{ m} \times 0.8\text{ m}$ cloud chambers in our department. Thus the idea was abandoned. In parallel to the CERN adventure, in 1960, Beppo made a phone call to Gianni Puppi at the University of Bologna asking for help to introduce the group (Ettore Fiorini, and me) into the bubble chamber community of the USA. At that time Martin Block was visiting the Universities of Trieste and Bologna on sabbatical from Duke University; he had built a tiny neat helium bubble chamber; Puppi was too a famous professor and we could never address him directly! Beppo made arrangement so that Ettore and I could go to Bologna and work for a side analysis on pictures of the only liquid-helium bubble chamber that ever produced prominent physics, doing scanning and measurements from 8 pm till 8 am every night; the Bologna member of that mini-group was Giorgio Giacomelli who just went back to Bologna after receiving a PhD in physics at the University of Rochester, N.Y. The chamber was exposed to the Berkeley Bevatron to a stopping negative K-meson beam, to take advantage of the *s*-wave states in the K^- capture by the very symmetrical helium nucleus with the scope of studying nuclear hyperfragments generated in K^- - ^4He well-defined states. The pictures detected quite a lot of stopping muons as unavoidable background. During that analysis Ettore Fiorini moved for a sabbatical to Duke University where Martin was full professor. I remember shipping there an aluminum briefcase full of punched cards to join the measurements done at Duke. Working overnight was not that exciting, nonetheless I found that arrangement very charming because of the nice and cheap restaurants easy to find in

Bologna. Meanwhile, Martin moved from Duke to Northwestern University, in Evanston Illinois and the research was concluded while I was there spending my sabbatical year 1961-62. I am still proud to have written in a scientific paper that we corrected for all systematic errors. Indeed: "...to demonstrate explicitly our *a priori* knowledge in this experiment, concerning our systematic errors..." we quoted the value with the statistical error only. Of the activities at NWU, I already wrote few lines at the beginning of this contribution. Coming back from Northwestern (or maybe, was it a little before my sabbatical in the USA?), I found that Beppo appointed Alfred Freddy Herz, an Australian physicist as our group leader in place of Connie Dilworth who got tired to "grow up a bunch of kids" who wanted to get involved in research that was not inspiring her (when I became member of the CERN Track Chamber Committee, Charlie Peyrou was so kind to let me read the letter through which I was introduced to him by Connie. It was a very "peculiar" letter indeed. I need not to recall the details but at the very end Charlie and I ended the episode with a good laugh). Freddy was a guy of the "iron sergeant" style. We managed to come along and we wrote the paper containing the discovery of the A_1 -particle produced in diffraction dissociation off complex nuclei. I presented the paper to the Rochester conference in Berkeley in 1966. In his review talk Arthur Rosenfeld stated: "the A_2 is a genuine resonance, the A_1 is a European effect". Nonetheless, we discovered the A_1 , finally acknowledged by the Data Particle Group.

Beppo came back to Italy from Bruxelles in 1949, after the well-known peregrinations in South America and Europe (someone else has written about this point in this book). In 1968 the Institute of Physics celebrated the twenties anniversary of his going back to our country (and his sixtieth birthday) with a very spontaneous and touching ceremony. It is interesting to go through the signatures of the participant that I reproduce in fig. 4, 5, 6. Some 130 persons participated into the event, from all over Europe and the Americas. If you read carefully the names you may find the signature of 3 Nobel Prize Laureates (Cecil F. Powell, P. M. S. Blackett and Emilio Segré) but also the signature of two young men who much later would go and shake hand with the King of Sweden, *i.e.* Riccardo Giacconi (in the middle of the sixth row, first page: fig. 4) and Carlo Rubbia (in the middle of the seventh row, second page: fig. 5), not to mention Bruno Pontecorvo (first signature in the fifth row, first page: fig. 4) who didn't obtain a Nobel Prize but would have deserved one as much as Beppo Occhialini. Polvani wanted to be among the last to sign (right signature, second line from the bottom: fig. 6).

Concerning Bruno Pontecorvo signature there is kind of a mystery. At that time, the time of the iron curtain, the visas to leave the USSR were very difficult to obtain, even for a member of the Russian Academy of Sciences like Bruno, most of all for those considered as "refugees" who wanted to go back to their original countries. In the first page of signatures, Bruno Pontecorvo clearly appears on the fifth line, first column, but in the written contribution to the Symposium, Bruno writes that he regrets not having been able to attend in person. It must be that he gave permission to insert a label with his signature on it into the list. I cannot see any alternative explanation for the puzzle.

E. Janyes Giovanni La Pietra E. Mastrapa
 G. Mendelsohn Giuseppe Bellini Giuliano Toraldo di Francia
 G. Baldini Alberto Roncati Ricci
 Renato Chelloni Giuseppe Mambroffo Jr. Lavin.
 Bruno Lombardo Peter Forster Gianni M. D.
 Rote Offito Riccardo Giacconi R. Armenteros
 Emilio Segre Giorgio Spinolo Luis Galgani
 G.F. Handlil Carlo Franzmetz E. Panizza
 M. P. Panelli A. Luginique Julio Kell
 Grande Umaneri Ernesto Marchini P.M.S. Blissett
 Giuseppe Cocconi Gianni (DA) Donato Patti
 C. Manna Giovanni Maria Prosperi C.F.P. Moll
 Riccardo Leisetti ~~Alberto Schott~~ Dono Patti
 Gabriele Villa I. Janyes Enrico J. Tassi
 Ives Goldschmidt - clermont Antonino Bullia Tommaso Provanza

Fig. 4. - First page of the signatures.

H. Minicard Lilla Maldo Coli Aldo Vries
 Stefano Halden H. H. H. E. M. G. Latta
 B. P. P. A. F. F. Mariella di Grato
 Lilla H. H. P. H. H. Michele Rottier
 Peter Anthony W. H. H. Emilio Bellotti
 Dario Haccagni J. H. H. Nive Bellini Testi
 Nive Dallapal Freddy Herz P. H. H.
 Malone Lombardi H. H. H. Onella Rott
 F. G. Frank A. H. H. A. H. H.
 Pier Giorgio Sone O. H. H. Carl Maria Gault
 Guido Vagni Jean H. H. E. P. H.
 J. H. H. L. H. H. F. H. H.

Fig. 6. – Third page of the signatures.

At that symposium I presented a paper that I found appropriate to the man, by the title: “Some speculations in the framework of Regge Poles”, an issue that at that time was quite popular. The opening words are: “Having in mind the extremely dialectical games that Beppo plays, from time to time, with his disciples and collaborators, I am tempted to propose a numerical game. More correctly I should say a speculation on experimental numbers. I learnt from him that experimentalists provide numbers and that theorist propose interpretations, but that both should think about the measured numbers which basically constitute the real facts. Since all in the audience are friend of him, all know how he is able to play with words; perfectly capable to reach the conclusion he likes by using your words”.

I presented an analysis of a set of 2-body and 3-body reaction produced in $\pi^{\pm}p$ and pp collision at lab. momentum between 4 GeV/c and 12 GeV/c, analyzed in terms of Regge pole exchange to prove the approximate validity of amplitude factorization: a questionable but valid interpretation of the data.



Fig. 7. – The title of an article written to celebrate the 60th birthday and the 20th year since the return of Beppo to Italy.

At that time I was collaborating to a daily newspaper: “Il Giorno”, published in Milan, trying to spread scientific knowledge to ordinary people. On that occasion I wrote an article that appeared in the issue of October 15th by the title “*G. Occhialini contestatore dell’universo*” (“G. Occhialini protester of the Universe”, fig. 7).

Indeed 1968 was the “big year” of the student protest, the occupation of the University buildings and it was a pretty hard time for all faculty members. Beppo was very uneasy with all that; he basically understood what the students wanted, their issues, their instances, but did not approve the method, the violence, the arrogant approach.

However he could not find the right way to address the issue. The permanent assembly meetings of over 400 shouting students squeezed like sardines into the large auditorium were not for him the place to stand at the podium and talk; it was not the kind of situation he would have liked to approach. During more than one period of occupation he first locked himself into his own office and finally took off the wall the plate with his name written on it. At the very end of the symposium to celebrate Beppo's coming back to Italy a couple dozen students asked to be admitted to the symposium and questions to the conveners. They wanted to know about the role of Science into the Human Society; when, how and where the scientists fought their fights to impose the right use of science and technology. There was then a natural appendix to the meeting that lasted about 2 hours. Beppo did not talk but answers came from several foreign participants. Among the contributors to the symposium there were jewish like Emilio Segré who left the Country to the USA, due to the prosecution of the fascist regime, spanish physicists like Raphael Armenteros whose family had to expatriate from Spain due to the dictatorship of the Caudillio general Franco, Erwin Fenyves, a Hungarian physicist still under a soviet domination. Thus the answers came, either in English or in French or in a shaky Italian, but with a series of clean and concise argumentations, keen to capture the real message of a renovated spirit, the embryo of a society that might have been a new one. They *explained* why certain "difficult and painful" decisions were taken to avoid the happening of catastrophic events: how and why movements encouraging free thinking spread around, all over the World; how and why scientist can never avoid assuming personal responsibilities. Most of these people, although scientists, did not dare to speak out aloud against the "nazi" domination, against violence, against maccarthism, fundamentalism and blind capitalism.

There were, in summary, several eminent personalities who had protested twenty, thirty or even forty years before, who had been deeply anti-establishment although senior members of the "academy", who acted with strict coherence during all their lives, who in that moment were facing, in a new form, a living evidence of the logical continuation of their own juvenile protest. Beppo listened to all these talks with apparent satisfaction and all that turned out to be an event within the event.

Three years after the 1968 symposium I was appointed full professor at the University of Pavia, where I still teach and work so that I lost "direct" and constant contact with Beppo Occhialini, meeting him from time to time either at the Annual Conference of the Italian Physical Society, or at Conferences or at the Accademia dei Lincei Meetings, where he was a distinguished effective member and I was nothing but a simple invited auditor.

My research field moved from bubble chambers to faster detectors. Searched—unsuccessfully at the Triga Mark II reactor of the University of Pavia—for neutron-antineutron oscillations, experiment by which I recovered the collaboration with the group lead by Marcello Conversi and in which I used flash chambers and Resistive Plate Chamber RPC for the first time. Thereafter, I moved to systematic search for charmed particles, both mesons and baryons, discovering a charmed baryon that would be called "primordial neutron", a $[css]$ state (Ω_c^0) containing none of the ordinary quarks that has

the shortest lifetime ever directly measured, *i.e.* $\tau = [72 \pm 11(\text{stat}) \pm 8(\text{syst})] \times 10^{-15}$ s. Got interested in fractal geometry and complexity; got interested in the application of the RPC detectors to the Positron Emitting Thomography (PET). In brief, I got along on my own for about 35 years.

I was shocked when I learnt —it was in Rome at a workshop of the Accademia dei Lincei, in the late nineties— that Beppo gave up smoking; I could not imagine him without a cigarette in his hand, without a cloud of smoke around his body, without the smell of burnt tobacco around him. Beppo, always smoking a cigarette, using its last quarter inch to light the next one.

My scientific career was deeply affected by the influence of professor G. P. S. Occhialini, although I am not sure I know what the initials P. S. stand for. Somebody told me they are his second and third names Paolo and Sebastiano. But I am not sure.

When he died I was in the USA and could not attend his funeral. Since when I left Milan, I miss him a lot: the man who always stimulated my curiosity, and let my passion for physics grow to the limit of my intellectual capability. When I close my eyes, I still see him, hear his voice and his provocative words, arguing about physics. But this is probably a sign of my age, which is growing fast. I feel I was unable to fully collect his scientific inheritance.

G. P. S. Occhialini vu par un de ses amis

JACQUES LABEYRIE

Gif sur Yvette, France

Giuseppe Occhialini, “Beppo” pour ses amis, naquit le 5 septembre 1907 à Fossombrone, dans les Marches (Italie). Etant jeune, il rêvait de s’adonner à la poésie ou à la peinture, mais son père, professeur de physique, le persuada de se diriger vers cette science. Il commença ses études à Pise, puis à l’Université de Florence, où il fut nommé assistant de recherches en 1930. Il eut aussi une jeunesse sportive: il avait constitué avec quelques amis un groupe de spéléologie. Dans la Montagne de Marbre, au-dessus de Carrare, ils découvrirent un très grand gouffre, le Corquia, et l’explorèrent jusqu’à plus de 500 mètres de profondeur (le record du monde à l’époque). Il disait souvent: “la spéléologie, c’est l’exploration du pauvre”. Voulait-il dire par là qu’il faisait partie des pauvres?

C’est à Florence qu’il se lia de grande amitié avec un autre jeune physicien qui devait lui-aussi devenir célèbre: Bruno Rossi. Celui-ci avait déjà commencé à étudier les “rayons cosmiques”, et dès lors, ce sujet gouverna leur activité de chercheurs. De ces rayons cosmiques, on savait seulement qu’ils étaient très pénétrants, et donc porteurs d’une énergie immense. C’est sans doute les propriétés inconnues de leur énergie immense qui attirèrent Beppo. Il faut se rappeler qu’à cette époque on ne connaissait, en fait de particules nucléaires (on ne disait pas encore “élémentaires”) que les “rayons” alpha, bêta, et gamma des radio-éléments naturels. On commençait tout juste à essayer de construire les premiers accélérateurs de protons, et Chadwick n’avait pas encore découvert le neutron.

En 1931, Beppo reçut une bourse d’étude de son gouvernement pour séjourner un an au fameux laboratoire Cavendish, à Cambridge. Il y resta, en fait, plus de deux ans auprès de Patrick Blackett, un physicien de la jeune science nucléaire, et qui étudiait celle-ci au moyen de la Chambre de Wilson. Beppo lui apporta de Toscane la pratique des compteurs de Geiger, et des circuits électroniques associés à ceux-ci. Je crois que c’est Beppo qui eut l’idée de marier les deux techniques, pour déclencher la Chambre de Wilson par “coïncidence” au moment du passage d’une particule cosmique, au lieu de

la déclencher au hasard, comme cela se faisait jusque là. On gagnait ainsi énormément en efficacité pour détecter ces particules et leurs éventuelles réactions; cette astuce les amena presque aussitôt à la découverte des “cascades” et “gerbes” produites par l'action des rayons cosmiques sur la matière. Surtout, dès 1932, elle leur permit d'observer pour la première fois la création d'antimatière, sous la forme d'une paire d'électrons, négatif et positif. Blackett et Occhialini confirmaient ainsi les photos que venait d'obtenir en Californie Carl Anderson, également à la Chambre de Wilson, et qui montraient des traces de particules positives dues à l'action des rayons cosmiques (celui-ci les baptisa “positrons”); un peu plus tard, il découvrit des trajectoires d'une sorte d'électron lourd, le méson μ . Anderson reçut le Prix Nobel en 1936 “pour la découverte de l'électron positif”. Blackett reçut le Prix Nobel en 1948 “pour le développement de la chambre à brouillard de Wilson”; quant à Occhialini, il fut oublié. Mais il resta quand même en très bons termes avec Blackett.

En 1937 Beppo fut invité comme Professeur à l'Université de São Paulo. Il y était encore, en 1942, lorsque le Brésil se rangea aux côtés des Alliés. Devenu ainsi malgré lui un “ennemi”, il fut obligé de quitter l'Université et alla vivre chichement non loin de là, comme guide pour de rares touristes, dans la forêt de montagne d'Itatiaia, dans la région de São Paulo.

En 1944, c'est le retour en Grande-Bretagne, et Blackett l'aide alors à trouver une place au laboratoire Wills, à Bristol. Sous la direction de Cecil Powell une petite équipe y étudiait les réactions de physique nucléaire au moyen des émulsions photographiques. Beppo, toujours attiré par l'étude des rayons cosmiques, fut séduit par l'apparente simplicité de ces détecteurs. De plus, leur substance active, l'émulsion de gélatine et de bromure d'argent, avait l'avantage d'une densité plus de mille fois supérieure à celle des gaz de la Chambre de Wilson. Pour accroître le volume utile des plaques, et donc leurs chances d'interaction avec les rayons cosmiques, il travailla avec le directeur de la firme Ilford, Waller, à réaliser des émulsions qui étaient beaucoup plus épaisses que celles que l'on fabriquait jusque-là: cent cinquante microns contre une vingtaine de microns. En outre on avait accru leur concentration en bromure d'argent; on augmentait ainsi leur sensibilité, ce qui permettait de voir les traces dues aux particules très rapides, donc très peu ionisantes, et qui étaient invisibles jusque là. Beppo fit de plus un travail considérable pour transformer ces émulsions épaisses, molles et déformables, en un véritable appareil de mesure, permettant non seulement de détecter toutes les particules ionisantes, mais aussi d'identifier leur masse, leur charge et leur énergie.

Il se trouve qu'après son retour du Brésil, Beppo était tombé sous le charme de Max Cosyns, physicien à l'Université Libre de Bruxelles et collègue d'Auguste Piccard avec lequel il avait réalisé les premiers ballons stratosphériques, puis le Bathyscaphe. Cet amoureux de la verticale s'était aussi spécialisé dans la recherche et l'exploration des grands gouffres pyrénéens. Pendant les vacances de 1946, Beppo l'avait accompagné, emmenant à tout hasard dans ses poches quelques exemplaires de ses nouvelles émulsions.

Hélas! cet été-là, il pleuvait sans cesse, ce qui confinait nos explorateurs dans une petite auberge du Pays Basque. Ils profitèrent de ce contretemps pour se faire emmener dans la Jeep de l'aubergiste à l'observatoire du Pic du Midi, à 2883 mètres d'altitude.

C'était la place idéale pour y déposer durant quelques semaines un "stack" (un paquet) de ces premières émulsions épaisses dans l'espoir d'y capturer la trace bien visible de quelque interaction cosmique inconnue, d'une "étoile" comme Beppo devait les appeler plus tard.

De retour à Bristol il développa les plaques, et les analysa au microscope à immersion, aidé de Cesar Lattès et de Hugh Muirhead, et des "scanneuses" du laboratoire: dans l'une de ces plaques on voyait pour la première fois la trace d'une trajectoire de particule très rapide, dont la masse apparaissait inférieure à celle du proton, mais supérieure à celle du méson μ , et d'où partait, une longue trajectoire, celle d'un méson μ , se terminant par sa désintégration en électron et neutrino celui-ci étant invisible. C'est ainsi, dans l'été 1946, grâce à l'utilisation de ces nouvelles émulsions épaisses et denses, que fut découvert le méson π , ou "pion". C'était la particule, très recherchée, de l'interaction "forte", qui liait entre eux les nucléons et qui avait été prédite par Yukawa dès 1935, mais que personne n'avait pu encore observer.

Powell eut le Prix Nobel, en 1950, "pour le développement de la méthode photographique pour étudier les processus nucléaires". Une fois de plus, Occhialini fut oublié (et même deux fois: pour la découverte du méson π et pour la transformation des émulsions en un instrument de détection de très grande qualité). Entre temps, dès 1948, il avait quitté le laboratoire de Bristol, et rejoint celui de Bruxelles, auprès de Cosyns, où il exerça bientôt ses talents d'animateur scientifique incomparable. En été, avec quelques collègues ils continuaient leurs explorations spéléologiques. C'est ainsi que le dernier jour des vacances de 1950, Beppo fut parmi les trois spéléos qui découvrirent, l'entrée du formidable réseau souterrain de la Pierre Saint-Martin. L'exploration de celui-ci, deux ans plus tard, fut un des grands moments de sa vie (voir fig. 1).

En 1950 il retourna en Italie, où il reprit la vie universitaire et fut nommé professeur à Gènes, puis à Milan, en 1952. En 1960 il y eut un court intermède d'un an à Boston, où il alla comme professeur invité auprès de son ami Bruno Rossi.

En Europe cependant, comme aux Etats-Unis, la recherche sur les particules élémentaires avait bien changé: c'en était fini des expériences astucieuses dans des laboratoires universitaires, à la façon qu'aimait Beppo et qui était aussi celle de Blackett, de Chadwick ou des Joliot à Paris. La pluie de rayons cosmiques, seule source jusque-là de ces particules élémentaires, était devenue bien trop faible au goût des physiciens et des administratifs de la Science. On vit alors, dès la fin de la Seconde Guerre mondiale, des accélérateurs géants se construire ici et là, fournissant sous des énergies de plus en plus élevées des flux de protons de plus en plus grands, et qui étaient servis par des équipes de plus en plus nombreuses: la "Physique des Particules" était née. Mais Beppo, homme aux goûts simples, n'aime pas trop cela, et pour continuer la recherche sur ses chers rayons cosmiques il va s'évader dans un domaine qui vient de naître: l'espace extra-terrestre. Dès 1960 il va élargir sa petite équipe de Milan par une alliance avec la jeune équipe d'astrophysique de Saclay. Ensemble, elles développent l'utilisation d'un nouveau détecteur observable à distance, la Chambre à étincelles; embarquée sur les ballons stratosphériques du CNES, celle-ci va tout d'abord confirmer dans la haute atmosphère la présence en grand nombre d'électrons cosmiques primaires de très haute énergie (déjà aperçus en petit nombre deux ans auparavant par une équipe américaine avec une Cham-

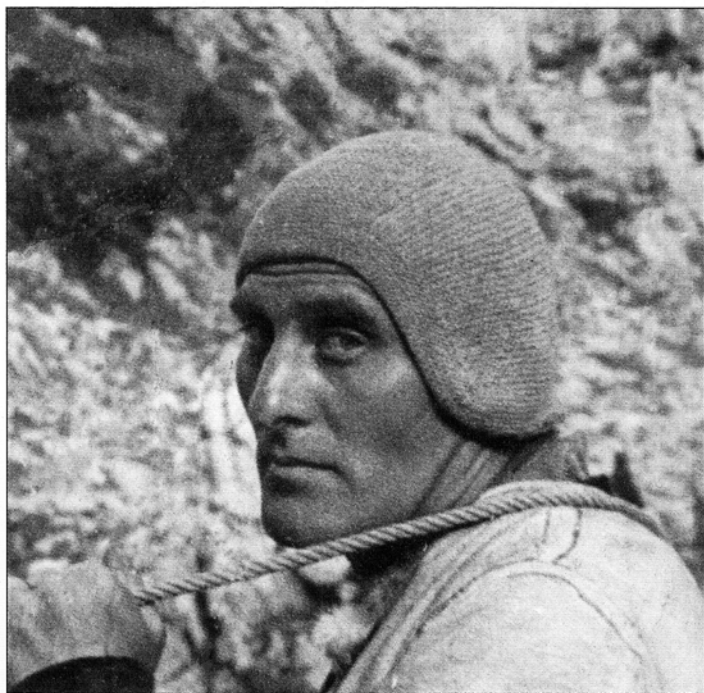


Fig. 1. – Août 1950. Beppo en train d'assurer un de ses camarades lors de la remontée du gouffre d'Escurets dans les Pyrénées.

bre de Wilson embarquée). Ensuite c'est la découverte de la première source galactique de rayons gamma (le pulsar du Crabe). Entre temps, Beppo organise un monde nouveau: celui des expériences sur les satellites scientifiques européens. Il est l'un des fondateurs et animateurs du "COS-Group" de l'ESRO (le groupe cosmique de l'Organisation Européenne de Recherche Spatiale, devenu ensuite l'ESA, pendant européen de la NASA), et étend la coopération des cosmiciens à quatre autres groupes, anglais, danois, hollandais et allemand.

Jusqu'en 1982, aidée par leurs moyens nationaux respectifs, en plus de l'ESRO, de la NASA et le l'INTERCOSMOS soviétique, cette association amicale des six laboratoires va réaliser une douzaine de grosses expériences spatiales pour découvrir et étudier les rayonnements à très haute énergie qui arrivent sur notre planète.

Je crois que c'est dans cette période là de sa vie que Beppo reçut le Prix Wolf qui lui fit certainement un grand plaisir.

Ces travaux demandaient une longue patience. Ainsi il fallut plus de dix ans pour achever le plus significatif d'entre eux, l'obtention de la première carte de notre Galaxie par son rayonnement gamma de très haute énergie (entre 70 MeV et 5 GeV) à partir du satellite COS-B. Mais on retombait ainsi dans une nouvelle sorte de "Big Science"

et Beppo la quitta vers 1982 pour intégrer le CFR (Centre des Faibles Radioactivités) à Gif sur Yvette, et y participer avec un très grand talent à une nouvelle méthode de datation des roches par les traces de fission spontanée: il était enfin revenu à ce travail de laboratoire qu'il aimait tant. Beppo mourut à Paris le 30 Décembre 1993, entouré de l'affection de quelques amis.

Un mot encore, on n'a pas parlé d'une de ses qualités les plus frappantes qui était la modestie.

Early cosmic-ray experiments on ESRO satellites — Some memories of via Celoria

JOHN BLAND

Professor Emeritus, University of Calgary, Canada

1. – Introduction

When asked to contribute to this publication, I wondered whether my memory would serve me well enough to describe my “Beppo” experiences. On reflection, I decided that only people over 60 might find it interesting and that some of them have possibly forgotten as much as me.

In 1963 my wife Beatriz, then expecting our first child, and I arrived in Milan. Previously I had spent some time working in Chacaltaya, Bolivia with Ismael Escobar, after completing my PhD at Imperial College under Harry Elliot. I do not recall applying for the position in Milan. I imagine that Beppo wrote a letter of invitation and worried about paying me only after my arrival. Such was the way that academics migrated in those days.

2. – The first months

It was late summer when we arrived and Beppo and Connie had set off to the summer residence in Tuscany. This annual migration involved the transport of books, papers and other paraphernalia and the Occhialini family. I remember that Nella (Beppo’s indispensable secretary), Livio Scarsi and Emanuele Quercigh, who owned cars, were kept busy.

Beatriz and I were taken in charge by Nella. She found us a place to stay and drove us to meet Beppo and Connie.

After the summer holidays, I moved into the office I shared with Livio Scarsi. I soon got to know most people in the extended GO research domain. In those days, there were offices and labs, in the main Istituto building as well as the large pavilion in the grounds. It was there that I met an American “foreign legionnaire”, John Kydd. Apart from Nuclear Physics, John’s main activity was the restoration of a vintage MG sports car. I suspected that he had moved to Italy because of the skilled motor mechanics available at quite cheap rates (at least in 1963).

By Christmas, it was agreed that I should stay in Milan for the foreseeable future. A source of funds was required and the necessary steps were taken to appoint me as a “Professore Incaricato” of the University. For the first time in Italy, I needed to overcome the bureaucratic hurdles such as obtaining a certificate that I had been tested for V.D! By that time my spoken Italian was just intelligible although grammatically poor. Fortunately my lectures on cosmic rays were given to well-educated students so that I could always substitute an English expression. One of the students, Constantinos Paizis had just arrived from Greece about that time.

3. – Finding a satellite project

Beppo and Connie were both famous for their investigations of nuclear reactions using cloud and spark chambers. A fruitful collaboration with Laberyie’s group at Saclay resulted in the construction of a spark chamber instrument for the measurement of cosmic-ray electrons and positrons. This instrument was flown aboard high-altitude balloons. The recovery of the payload was essential to recover the photographic record of spark chamber events. These events were searched for evidence of electron showers developing through the lead plates in the chamber. It was anticipated that a measure of the East-West effect caused by the geomagnetic field would furnish a measure of the electron/positron ratio [1,2]. By 1964 both Beppo and Connie were deeply involved with the European Space Research Organization (ESRO). ESRO (the forerunner of ESA) had been set up using CERN as a model. It was hoped that Europe’s first space activities would be harmonious pure-science collaboration. However, the difficulties of reconciling national interests, financial constraints, as well as the ambitions of diverse scientific communities resulted in vigorous and sometimes acrimonious debate (ESRO history is well documented and available on-line, [3]). Beppo took a leading role as chairman of the COS committee which selected experiments, broadly defined as “cosmic ray”, for the first satellites. Such a procedure meant that the designs of the satellites and the choice of experiments had to proceed by successive approximations. This gave rise to innumerable meetings with ESRO in Paris and later at ESTEC at Noordwijk.

In Milan there were no formal meetings about cosmic-ray experiments. Plans were usually made with Beppo holding court in his large office. Usually he walked around smoking cigarettes, pausing from time to time to sign papers brought in by Nella, answering the telephone or dictating memos. Occasionally the interspersed scientific discussion would be summarised by Beppo with the preface “in sommal”. Fortunately Connie would be able to come up with the detailed plans after the groundwork had been laid during

these brain-storming sessions. I particularly remember how adept she was in plotting data with paper and pencil in the days before PCs.

4. – S-79

A proposal for a satellite experiment to measure cosmic-ray electrons (designated later as S-79) was submitted by the Milan-Saclay collaboration in 1964. Inclusion in the payload of HEOS-A was not approved until the autumn of 1965. Since HEOS, as the acronym implies, was a highly-eccentric orbit satellite, tracking and telemetry requirements required much planning and negotiation. It was agreed that Milan would take responsibility for the construction of the detector package and Saclay would build the electronic package. Both groups had a hand in the overall design and testing.

In order to discriminate between electrons and fast protons, S-79 included a gas Cherenkov detector. I helped to design this detector and initially proposed to use the gas sulphur hexafluoride which would have provided a suitable refractive index at moderate pressure. The photomultiplier had to be kept out of the acceptance angle of the instrument. It was therefore necessary to design suitable optics to bring the Cherenkov light to a focus at the photo-cathode. Beppo enlisted the assistance of Prof. Scandone at the firm of Officine Galileo in Florence. I recall my visits to the factory and working on the design with Ing. Pietro Marcio. On one occasion it was decided that ray-tracing should be carried out by the “computing centre” while we went to lunch. I wondered what sort of computer was being used and was surprised when we went to pick up the results, to see beyond the doorway, a group of elderly gentlemen armed with tables of trig functions and very large slide-rules.

As the S-79 project evolved it was necessary to have frequent meetings with our French partners. I particularly enjoyed several trips to the CEN. at Saclay and working with Yves Koechlin, Alain Raviart, Lydie Koch among others.

I recall a meeting in the Occhialini apartment which frequently served as a branch office, during which Beppo expressed his doubts about using sulphur hexafluoride as a Cherenkov radiator because of its possible scintillation properties. Consequently the detector was instead filled with nitrogen at a relative large pressure of 15 atmospheres. A prototype of the pressure vessel was deliberately inflated to many times the working pressure to see if it would explode. Pietro Marcio and myself went to the testing site of the Breda factory to observe the procedure from the safety of a concrete bunker. There was an anti-climax as the device simply hissed as it burst at the seams.

The mechanical design of the electron telescope was entrusted to Renato Ballerini. Not only was he an excellent draftsman but he also solved many practical problems such as the bubble-free potting of photomultipliers and the use of magnesium alloy (including the electroplating procedure for anodizing). Beppo had recruited a talented and dedicated group of instrument makers. I remember wondering what language was spoken in the machine shop until I found that the majority of the staff came from Bergamo. Ballerini had a mischievous sense of humour. On one occasion during a field trip to Frascati, he pretended to be carefully reading the municipal notice-board just in case Beppo had put

up new orders! Beppo rarely sent out internal memos but occasionally if, for example, he found books on the wrong shelf in the departmental library, he would pin up a new directive *urbi et orbi*.

After the final payload of HEOS was approved, the contract to build the spacecraft was awarded to the Junkers company in Munich. The inclusion of a flux-gate magnetometer to measure fields $\sim 10^{-5}\text{G}$, meant that our experiment had to have a minimum of ferromagnetic material and current loops. I spent some time winding coils to compensate for the geomagnetic field and playing with a phase-lock amplifier connected to a magnetometer in order to measure the stray field of the prototype detector.

The results from S-79 were bedevilled by instability of the high voltage supply, an effect well known to those of us who have participated in stratospheric balloon experiments. Some data on the production of high-energy electrons during solar flares were extracted [4] (in large part due to Connie's perseverance, I suspect).

5. – Gruppo Spazio activities

During this period, I was ably assisted by young co-workers who were studying for the *Laurea*. Thanks are due to Bruna Lietti, Giovanni Bignami, Renzo Besozzi and Anna M. della Ventura for making my job of supervision such a pleasant one. They all made important contributions to the investigations required to prepare a satellite experiment.

It would be a mistake to imagine that the Gruppo Spazio was entirely dedicated to satellite experiments. A lot of work was still in progress with photographic emulsions applied to research of elementary particles.

Meanwhile Gianni Degli Antoni and Giuliano Boella were both pioneers in what was then referred to as *Cybernetics*. One of the very first programmable desk-top computers was the Olivetti P101. Programs were stored on large magnetic cards. A program in those days, would perhaps provide the sine or cosine of an angle! More complicated programs would have to be submitted to the University's IBM 7900 using a stack of punched cards.

Another line of their research involved the fabrication of germanium surface-barrier detectors which required some fairly complicated operations in a clean room.

Emanuele Quercigh started his pioneering work on spark chambers at via Celoria before joining the Track Chamber division at CERN.

Somehow Beppo managed to nurture these and many other scientific ventures. He provided space, funding and encouragement but rarely added his own name to the published results. I was not involved in the discussions that occurred between Beppo and the higher levels of government regarding the Italian Space Program. In those days, it seemed that almost all the government ministers came from an academic background. Nevertheless I suspect that Beppo had to struggle for resources against the competing industrial, military and political interests.

6. – The first ESRO stabilized satellites

The story of the satellites TD-1 and TD-2 are recorded in the history of ESRO [3]. The provision of a stabilized platform was essential for astronomical observations. Optical measurements could be carried out from near-earth orbits which were also convenient for measurements of the ionosphere from above. Beppo and Connie perceived an opportunity to forego measurements of cosmic-ray particles and to investigate instead whether the sun produced gamma rays during solar flares. Thus the experiment S-88 was proposed and accepted for inclusion in the payload of TD-1. I helped to design this experiment. The need for extreme directionality so that only solar events would be recorded, was the first of several problems to be tackled. Fortunately I came across a brilliant design by Ng [5] for a directional Cherenkov detector. The device consisted of a large plano-convex plastic lens. The conical pattern of Cherenkov light from fast electrons moving within a small angle of the axis of the lens, would be totally-internally reflected on the convex surface and be brought to a focus around the edge of the lens. The peripheral light would be detected by three photomultipliers mounted on the annulus. A three-fold coincidence circuit for the photomultiplier signals would define the directionality of the instrument. Of course, gamma rays had to be converted into electrons by pair production or Compton scatter. Obviously there was a trade off between conversion efficiency and scatter. Tests of the directional Cherenkov were carried out at the electron accelerator at Frascati in early 1968 and provided encouraging results.

S-88 would be an in-house development and thus the electronic package would have to be built by a local contractor. I remember that there were two contenders for the contract. One was the firm of Laben which was well-known at that time for the manufacture of excellent pulse-height analyzers. The other firm was FIAR which at that time was a pioneer in the field of reliability as applied to electronic circuitry. I remember that the representative of FIAR would seek appointments with Beppo in order to explain in some detail the advantages of modern project management and how computer programs would identify critical dead-lines etc. I cannot imagine that these ideas made much of an impression on Beppo but persistence paid off and eventually FIAR was chosen as the contractor.

We now know that the sun is not a significant source of gamma rays except for some line emissions from nuclear reactions at energies well below those for which S-88 was designed. For that reason as well as technical problems due to the failure of the TD-1 tape-recorders, S-88 was not a successful mission. It was, however, the forerunner of the spectacularly successful X-ray and gamma ray experiments such as COS-B which are milestones in the Beppo legacy.

My own involvement in S-88 ceased in the summer of 1968 when I left Milan to take up a job with the University of Calgary. Oberto Citterio ably took over many of the responsibilities for the experiment from me at that time.

* * *

GIORGIO SIRONI contacted me and asked me to provide this account of my experiences. He and GIULIANO BOELLA have made several corrections and helped me remember

some details. I am sure I have forgotten many former colleagues who made important contributions, and I hope they will forgive me.

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Beppo Memoire. Space in late '60s Milan

MARTIN J. L. TURNER

Department of Physics and Astronomy, University of Leicester, UK

Italy, mainly pushed by Beppo and Connie, was leading the way in space astronomy in the 1960s —although I suppose nobody would have thought of calling it that, at the time, “instruments” were called “experiments”. I gravitated to Milan because of this; in the UK, Ariel V was still a good many years off. Milan was a real powerhouse of satellite experiments, with instruments being prepared for both cosmic and solar gamma ray observations and also cosmic-ray studies. Coming from a cosmic-ray background and from huge ground-based instruments, the miniaturisation and the mass discipline were very new to me, and I learned a great deal from scientists and engineers at the institute.

In parallel with the concentration on engineering —space hardware was being made in the workshops— there was a lively enthusiasm for the astronomy. Laura Maraschi and Aldo Treves were amongst those in constant debate with Connie on the science, creating an atmosphere of intellectual excitement, and being at the cutting edge. Beppo —when he was not on the telephone to Rome or ESRO, keeping the show on the road— was a benign but incisive presence, always ready to probe one’s motivation and scientific justification.

This was the time of student “occupations” and for a good while, students occupied the institute, protesting about issues that seemed important at the time, no doubt, but which I cannot even remember now. With the building inaccessible, Beppo and Connie opened their own flats to the staff, so we could continue to meet and discuss our work. Connie had a flat in via Aselli, which was her own “Theory Department”, and when this was not sufficient, the Occhialinis opened their apartment in via Amadeo. Beppo always seemed to be doing several things at once, usually combining long phone conversations to ministers and officials, with local discussions with us. His habit of walking about while telephoning was facilitated by a very long lead on the instrument and he would intermittently vanish from the room, followed by the wire, to reappear half an hour later,

still in conversation. After a while the students were evicted from the department, and we entered the building each day past a scary looking armed “carabiniere” until some agreement was reached with the students, and life returned to normal.

At the time we were in via Celoria as part of the Università degli Studi, but change was on the way, and the new “Istituto di Fisica Cosmica e Tecnologie Relative” was formed. This sounds well in Italian, but confused foreigners. I remember Connie being very amused to receive a letter addressed to “The Institute of Cosmetic Physics and Relativistic Technology”. I do not know about cosmetic physics, but would dearly have loved to work in an institute of relativistic technology. Beppo focused all our efforts on the research, and military service for the research students, which was compulsory, did little to interrupt matters. Beppo arranged for them to be appointed to the Air Force Meteorology Office in Milan and when their daily military duties were over they could come to via Celoria and continue their research. All except Nanni, of course, who got himself a commission in a crack alpine regiment and disappeared off to the Alps for long periods, much to Beppo’s disgust.

It seemed possible to work on anything and everything. I began working on determination of the incident direction of gamma rays from spark chamber pictures; but Tony Dean and I managed to write a theoretical paper on production of pulsed gamma rays in pulsars as well. I also got involved with a team of people including Nanni, Piero Musso, Gianna Cioni and Anna Della Ventura, on the automatic, *i.e.* computer, recognition of gamma ray interactions in spark chambers, amongst the much more common interactions of cosmic rays. At the time, vast amounts of time were spent identifying those rare gamma ray events from thousands of individual pictures, using the human eye and brain; we hoped to automate the process using the greatly increasing capabilities of computers. Beppo supported us in our desire to present this work in a computer science conference in Geneva, where Nanni introduced me to real Swiss Fondu, and the serious art of drinking Poire Williams.

It seems unbelievable now, but we had two gamma ray observatories, TD1A in orbit and COS-B on the stocks, there were solar observatories being prepared for launch, and I remember that someone, possibly Nanni, was working on a lunar occultation satellite, which of course later became EXOSAT. This was all because of Beppo and Connie. They not only made Italy the leading European nation in space observatories at the time, but also put a huge amount of work into the growing ESRO programme, and developing collaborations across Europe and with the US. There were so many distinguished visitors, I remember only a few, including Tommy Gold, and a young Hale Bradt, who had just discovered pulsed X-rays from the Crab Pulsar; we also had the Apollo astronauts. The presence of Italian scientists in a host of major laboratories around the world, which was such a feature of the 1970s, was in many ways the result of the strong links Beppo and Connie set up, from their base in Milan.

For me those few years working in Milan were a magical time. I came, very inexperienced, and a novice in the world of space science; I left with many friendships, which have stood the test of time, two children born in Milan, and an experience of working in an international environment which opened many future opportunities.

Beppo and the road to INTEGRAL

ANTHONY JOHN DEAN

School of Physics and Astronomy, University of Southampton Highfield, UK

1. – Luck of the draw

It was the spring of 1968 and I was to be married in the September of that year. It was the early days of space research, I had just got my PhD working on the NASA-OGOV mission and I wanted to gain some experience abroad. I wrote an unsolicited letter to a number of non-UK universities working in the field of gamma-ray astronomy asking for a job. Beppo's proactive response came by return of mail, inviting me to visit the Milan group and offered to pay my expenses. I thought "Wow!" Here is someone who does not waste time. Even as an undergraduate physics student I had always been fascinated by Occhialini's work on the positron and the pi-meson, little thinking that he would play such a big part in my life. I duly came for the "interview", with some trepidation, which was rapidly dispelled. I was expecting a formal across the table interview, this was not to be so, Beppo was too smart for that sort of approach. First he suggested that I talk to all the Milanese scientists in order for me to be familiar with the activities with the group. I spent a large part of the day doing just that, it was very interesting but did have its amusing side. I went from one group member to another, with my non-existent Italian, and in those days their limited English we soon ran out of conversation. It was to be my introduction to Italian espresso coffee, and the way Italians drink it. Not knowing what else to say or being able to say it, each person took me to the bar (already a bit of a cultural and agreeable shock for an Englishman to find a bar within a university physics department) for a coffee, which they drank in one gulp without sitting down, returning straight back to the laboratories and, for me, onto the next in line. After about twenty to thirty coffees (I was too polite to refuse!) I was eventually returned to Beppo's office, where he chatted to me. The caffeine must have played a part and made

me look energetic enough for Beppo's taste since he offered me a job on the spot. And so on caffeine high began my long association with Beppo, Italian science, a five year honeymoon in Italy, and my love affair with Italy.

I was soon to discover that Beppo was a great Anglophile, and always liked to have a Brit in his group. I succeeded John Bland and overlapped with Martin Turner, with whom I shared an office whilst in Milan and who went on to do great things with a whole string of X-ray missions right up to the present day XMM-Newton and XEUS. Beppo must have shaped all our minds. One of the key lessons I learnt from him was not to be afraid of uninhibited lateral thinking in the pursuit of exploring new ideas, concepts and problem solving.

2. – My early days at via Celoria

I duly arrived at via Celoria in October 1968. My wife of two weeks and I had driven from the UK in a left-hand drive VW Beetle I had bought cheaply before leaving. Our sole possessions were the car and two suitcases. Finding an apartment took us some time and Constantino Paizis kindly housed us during this search. Eventually we found one in the Ortica area and moved in. Beppo and Connie were fantastic at this stage, they searched around their cantina and heaven knows where else to find chairs, beds and a beautiful old carved wooden chest, all of which saved us a minor fortune. With a trip to Standa and Upim we bought some basic kitchen items and thus had a home.

Before leaving the UK I had been developing unusual Cherenkov devices for use with spark chamber gamma-ray telescopes. John Bland had just left for Calgary; he had started the TD1-S88 experiment, and this involved an extremely sophisticated focussing Cherenkov system. Beppo did the logical thing and made me part of the S88 team, led by Oberto Citterio and which included the young Nanni Bignami as a research student. Within a few days of my start at Milan we had to go to the synchrotron in Frascati to set up a gamma-ray beam for the calibration of S88. I thus had to leave my young bride to herself in Milan, whilst I learnt the finer merits of particle accelerators and Frascati wine. Gabriele Villa (also then a research student) found her a TV so that she could learn Italian. Beppo had told us that watching films was an excellent way of picking up a new language. But he did urge caution; he told us that when he first went to England the first film he saw was a gangster movie. He knew no English but a phrase that stuck in his mind was “do you want a smack in the mouth”, he had thought this was some sort of a greeting, with near disastrous consequences when he first tried it out! I experienced a similar episode whilst on the trip to Frascati. We had planned to meet for breakfast one morning, and I was about 5 minutes late and a knock came at the door. It was Nino Delleria, I gesticulated to say I was coming and was greeted with the word “basta”, mistaking this for something else in English I thought “wow, these Italians are rather pernickety about timeliness” until I looked the word up in my Collins pocket dictionary to find out he was basically saying “no problem”. It was only on the return journey to Milan that I realised why it was necessary to take such an enormous Fiat van to Frascati with the diminutive S88 looking lost in one corner on the way down. On the outward

journey this had perplexed me somewhat. As we ground our way back to Milan S88 was hidden from view behind the numerous damigiane all full of Frascati wine, the result of most of our Milan colleagues having placed their orders beforehand.

We had great fun and adventures working on S88; it was an immense learning experience in the widest sense. I was fascinated to see how Beppo could handle big high-powered meetings to get what he wanted, and amused and delighted by his wickedly humorous comments and asides afterwards.

3. – “Hic Sunt Leones”

As S88 entered into the more routine phase, we started thinking of new projects. At this time Livio Scarsi was busy setting up a new research group in Palermo. So in order to provide him with a helping hand, and given that Sicily is an excellent venue for stratospheric balloon launches Beppo suggested that we should start a collaborative balloon-borne project between Milan and Palermo. He asked me to review some possibilities. At that time it should be remembered that we were in the Laboratorio di Fisica Cosmica, and the study of extraterrestrial photons was only one amongst several of the available cosmic particle channels actively followed at via Celoria. After many long discussions and working sessions in Beppo’s and Connie’s apartment, we decided to think seriously about astronomy in the MeV range. Beppo asked me to write a science case for him to consider. I duly wrote the document explaining the need to extend X-ray measurements to the non-thermal domain, talked about the importance of studying nuclear lines the electron-positron signature and so on. Towards the end I inserted a figure that showed the existence of X-ray spectra up to some keV, the 100 MeV range measurements as well as the projected missions in these two energy bands, there was nothing in the middle. Back came the figure with “Hic Sunt Leones” written in the gap. Beppo had given his approval.

Incidentally I soon learnt that it was a rather important to make the decision of fixing a working session with Beppo at his apartment in the morning or the afternoon. The afternoon sessions invariably ended in the early hours of the following morning and meant a return home rather the worse for wear once Beppo had produced a whisky bottle, which usually happened.

Little did we appreciate the problems that entering this difficult energy band would entail; the background noise problem and the imaging problems being the most noticeable. We are still working on their resolution today! We did know that if you surrounded a gamma-ray detector with lead in order to “shield” it, the background simply increases at altitude; likewise we also knew neutrons would also be a major problem. Thus our conceptual design had a lead shield which also defined an aperture of about 10 degrees to provide the directionality. Around this we put a sandwich shield system made up of layers of lead interspersed with thin organic scintillators. The idea being, since the increase in counts on the inside of a passive system is derived from electromagnetic cascades initiated by higher-energy photons, then the sandwich system would pick up the showers first and veto them. For the neutrons we played with a variety of systems, perhaps the most exotic being a high percentage boron Cherenkov device. This material had

a colossal fraction of boron in it and was very transparent. It was made in house by Aldo Iguini, who loved chemical work. The idea was to remove thermal neutrons and select only the high β electrons. Eventually the drive for some reasonable spectral resolution won and we finished with a commercial (NE311) born-loaded liquid scintillator as the central gamma-ray detector. Solar neutron and gamma-ray astronomy were two of the main balloon activities at via Celoria at that time; both systems had their background noise counts dominated by the other group's desired particles. We used to joke that the best thing to do would be to concentrate on developing our own instruments and swap them over when it came to making observations.

There was a short time period available for the development of the new γ -ray instrument. Due to the prevailing stratospheric wind directions there was only a narrow window of a few weeks in the mid summer period for balloon launches from Sicily, and we had to have the payload ready, or wait another year. We had a real adventure with the construction of this first MeV gamma-ray telescope. We required several large cylinders of plastic scintillator to construct the sandwich system. Eventually it became apparent that they would not be ready in time due to delays in delivery of the cylindrical blanks and some other problems, so we adopted plan B (or more correctly unplanned B). We had discovered (Aldo again) that we could dope clear silicon rubber compounds with commercial scintillation liquids to form a gelatinous material which was very transparent to the scintillation light nicely coupled to a high scintillation yield. Through a superhuman effort by Andrea Bussini and the Palermo workshop in the few weeks before launch we developed this material, built some thin Perspex walled and hollow cylindrical containers to hold the scintillation gel and had them ready working and tested for launch from Trapani-Birgi on July 23rd 1971. See fig. 1. The endeavour had its rewards, during the flight we made some of the first sky observations in this region of the spectrum and were able to publish positive flux measurements.

4. – Big is beautiful — γ -10 and Dotty

The success of our first foray into the low-energy gamma-ray domain was pleasing, but at the same time we realised that more sensitivity was required if we were to maximise the science output. With our lack of a thorough understanding of the background problem and hence better ideas of how to reduce it, our only means of improving the sensitivity was to increase the detection area. We decided to design a payload that exploited the carrying capacity of balloons to the limit. This resulted in a payload of about 2 tonnes and a sensitive area raised from the $\sim 100 \text{ cm}^2$ of the first prototype to around 500 cm^2 . We called this telescope γ -10, the detector and shielding system worked on the same principles as the earlier Milan-Palermo device.

The dramatically increased payload weight caused a great deal of head scratching when it came to the mechanical engineering design. The massive central shield had to be supported through the delicate sandwich scintillators and of course driven in elevation. (An alt-azimuth steering system was used). Moreover this arrangement had to be non-magnetic, very strong, extremely lightweight, and with the holes through the shield kept

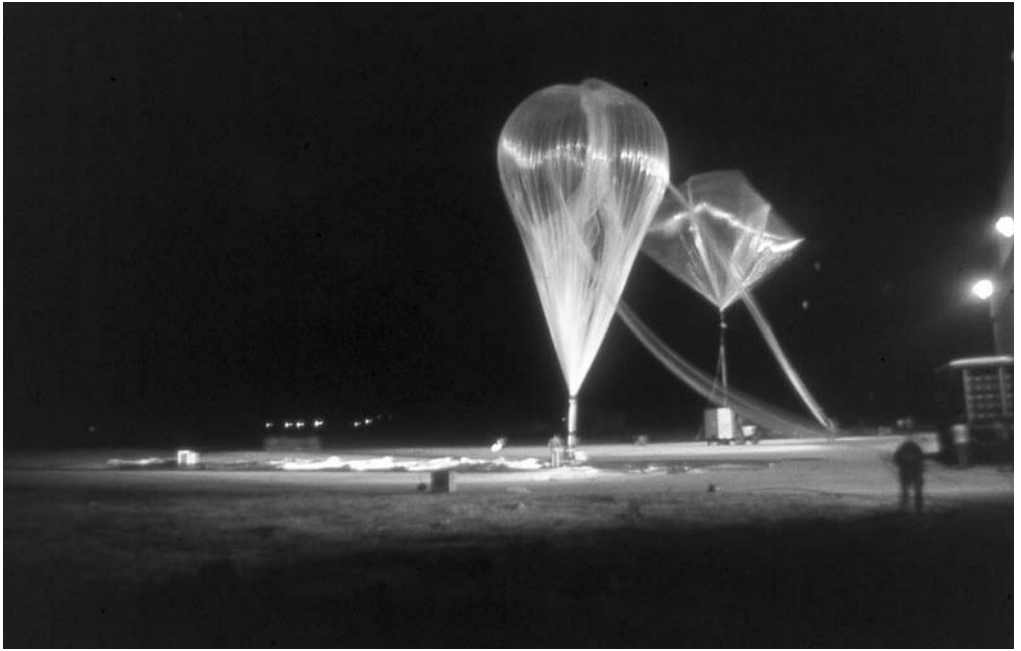


Fig. 1. – Ready for launch in 1971. This picture illustrates the CNES double balloon technique that was employed for launches from Trapani.

to a minimum. Andrea Bussini came up with a brilliant design, which in fact provided some of the shielding to boot, thus making its effective mass very low! Laboratory calibration also presented some problems, we had made the central detector big enough to stop 20 MeV gammas, and I remember using big tanks of cleaning fluid with neutron sources to get discrete calibration lines above 10 MeV.

We were also very aware that astronomical sources generally had steeply decreasing photon fluxes for emissions above 10 keV. This meant that it would be considerably easier to make flux measurements at lower photon energies. Beppo gave the go-ahead for us to make a large telescope for operation in the energy band immediately below γ -10, *i.e.* 20–200 keV. We called this one Dotty, since it had a fine collimator as was designed to provide good (for the standards of the day) point source location. Dotty was made with as large an area as was technically possible. Due to the limited budget, it had to be inexpensive; this was all part of the package. The idea for the large-area low-cost detectors came from something we had noticed when calibrating our hard (20–200 keV) X-ray piggy-back device for γ -10. This was designed around a 5 inch diameter thin NaI(Tl) crystal in direct optical contact with a 5 inch photomultiplier tube. We found that the amplitude of the pulses varied considerably over the surface area of the NaI(Tl)/photocathode combination. This effect considerably impaired the spectral resolution of the device, as the magnitude of the positionally dependent systematic variances were similar to those

derived from the statistical errors caused by the finite number of photoelectrons. The systematic variances could be reduced by moving the PMT further away from the crystal and thus distributing the scintillation light over the entire photocathode for each X-ray event. Furthermore we also realised that we could field detectors that were much bigger than the area of the expensive photocathode for very little loss in spectral resolution. The light was collected from within a large diffusion chamber. The other major cost component was the large area of expensive NaI(Tl) detectors required. However Nuclear Enterprises of Edinburgh sold off cuts of NaI(Tl) very cheaply. These were the unwanted bits of NaI(Tl) that came out the boule between the commercial 3 inch cylinders. The idea was to cleave these off cuts along the crystal planes, mosaic them and encapsulate them in-house at via Celoria. This economic, but effective approach very much appealed to Beppo, and he gave the go-ahead.

We started construction of both γ -10 and Dotty in 1972 aiming at a September 1973 turnaround flight from Palestine Texas. The payloads were too heavy for a CNES-Sicily balloon flight at that time. This was quite an effort, since effectively the same team of people were having to design, build, test and calibrate two separate balloon payloads on precisely the same timescale. There was a rationalisation of effort where possible, *e.g.* the steering platform and associated electronic system were basically duplicated for the two instruments. Again as usual the time schedule was tight, and we could afford little delay. In those days the national customs and excise people could cause big delays and big problems, and the Milan customs office were past masters at creating delays in the delivery of foreign goods, especially hi-tech materials. Our first delivery of NaI(Tl) off cuts was delayed for several months in the Milan office. Nuclear Enterprises had packed them in polythene bags, assuming the shipment would be some days at most. Sodium iodide is an extremely hygroscopic material so that when we eventually received the crystals they were useless, little more than a mushy liquid in the bags. The manufacture of the scintillation detectors now became a real problem and the delay was jeopardising the project. What to do? The same would only happen again. Then we had a bright idea, which in the present age would probably have serious consequences; we managed to persuade Nuclear Enterprises to put some radioactive stickers on the shipment box with my laboratory telephone number and the via Celoria address printed immediately underneath. The result was astounding. Nuclear Enterprises telephoned me to let me know that the scintillation material had been shipped, within about two days I had a telephone call from the Linate customs informing me that a package had arrived from Scotland and would I like to send someone out to pick it up. I guessed that it had probably arrived that morning and we had it in the laboratory by lunchtime!

Unfortunately the flight campaign was a disaster. We prepared, tested and calibrated the payloads in time for the turnaround period and launched γ -10 first. Unfortunately whilst passing through the minimum temperature point whilst climbing to altitude the balloon burst. γ -10 should have returned to Earth on its parachute, but unfortunately it appears that the parachute did not unfurl immediately and, when it did, the resultant jerk broke the universal joint in the suspension chain associated with the steering platform, which was very cold and was not at its full tensile strength. The net result was a free fall

from about 20 km. We eventually found the payload, or what was left of it, burrowed about two metres into the ground. Curiously the only part that was salvable was the small ($\sim 150 \text{ cm}^2$) mosaiced sodium iodide scintillation detector of the X-ray monitor we had included in the payload. Normally these crystals are extremely fragile, but our mosaic/encapsulation technique appeared to be extremely robust. The final NSBF (National Scientific Balloon Facility) report on the flight made interesting reading; their description of the condition of the recovered payload was quoted as being 'poor', I hate to imagine what a payload described as "bad" would look like!

Dotty was launched, but the NSBF telemetry system did not function properly! γ -10 and Dotty eventually resurfaced as MISO and MIFRASO.

5. – Milan-Southampton Collaborative Balloon Projects — MISO and MIFRASO

In October 1973 I moved from Milan to a lectureship position in the Department of Physics at the University of Southampton, turning up two weeks late because of the balloon campaign. I was very happy working with my Milanese colleagues and we all realised that we had an excellent working relationship due to the complementary nature of our individual expertise and the great friendship that had built up between us around our project work. Despite the disasters with γ -10 and Dotty we were determined to carry on with the work. Beppo was extremely supportive of an international collaboration between Milan and Southampton. I managed to get funding in the UK for a new collaborative version of γ -10, which we called MISO (Milano-SOuthampton) and we were back in business. The Bologna group led by Guido do Cocco joined the collaboration during this period.

Again in the quest for maximum sensitive area we decided to push balloon technology to the limits. The conceptual design was basically the same as γ -10 with a sensitive area of 560 cm^2 . The only real difference was that the central detector arrangement consisted of two scintillation elements, one placed above the other within the semi active shield system. The top unit (S1) was a boron loaded liquid scintillator, and the lower unit (S2) a massive (27 cm diameter, 10 cm thick) Sodium Iodide crystal, which cost a small fortune. These were placed inside a plastic scintillator veto system and also separated by a thin plastic veto scintillator. There were three modes of event selection S1, S2 and S1 + S2. It was hoped that the latter "Compton" mode would provide a clean γ -ray event selection, whilst because of the close proximity of S1 and S2 there would not be too much loss in detection efficiency. The collaboration worked very well and extremely efficiently. Milan was responsible for the flight electronics, the overall mechanical design, and the liquid scintillator system whilst Southampton was responsible for the steering platform, the sodium iodide detector, and the semi-active shield. Integration and testing took place at Southampton, and we calibrated using the Van de Graaff at Harwell to produce a controlled series of γ -ray lines and neutron fluxes. We also had a small ($\sim 100 \text{ cm}^2$) co-aligned X-ray monitor on board.

We shipped to NSBF in Palestine Texas for the Spring 1977 turnaround. The campaign started with a disaster. We had air freighted the central robust gondola that

incorporated the main telescope in a custom- built thick plywood container. The platform and the rest of the support equipment was likewise air freighted in a series of similar boxes and trunks. Four of us, Andrea Bussini, Gabriele Villa, Ron Baker and me were already out in Texas as an advance party waiting for the gear to arrive at NSBF. We received a telephone call from our shipping agent informing us that there was some slight damage to one of our boxes, but would it be OK to ship it to Palestine anyway? We thought that we had better drive up to the airport to have a look, just as well. It was a friday afternoon and the fork lift truck driver had clearly started to celebrate the weekend a bit early. Despite MISO having been shipped on top of a pallet, somehow he had managed to drive the forks of the fork lift truck through the sides of the container. Bits of glass from photomultiplier tubes (PMT) and Perspex light guides were rattling around in the bottom of the box! So much for some slight damage to the box! We secured what we could and decided to ship it to NSBF for a detailed inspection. Over the weekend this revealed that by some miracle, although they had managed to destroy most of the PMT, and damage some of the light guides, the forks had missed the plastic scintillators of the semi active shield. Had they damaged this part it would have signified the end of the campaign, since about six months of purchase lead time and mechanical workshop effort would be needed to rebuild these units.

Two factors saved the campaign. At this time we were already building MIFRASO back in Southampton and for economy of scale we had decided to use the same PMT for this instrument as the ones on the MISO shield. We had also decided to use the same potting moulds so that the MIFRASO units were identical. A research student arrived with the replacement photomultiplier units within the week. The other factor was another superhuman mechanical effort by Andrea Bussini, Ron Spicer and Enrico Mattaini, who within a few days (remember the turnaround date is fixed) had rebuilt and polished the Perspex light guides, devised and constructed imaginative mechanical jigs to enable the light guides, scintillators and PMT to be optically joined together without dismantling MISO. Fantastic work! By working around the clock we had MISO ready and fully tested for a turnaround flight on the 22nd of May.

Another “amusing” incident associated with this first MISO campaign, and one which reflects on the Texan attitude, concerns our fine collimator. MISO had been originally designed with a 14° aperture (this *was* the 1970s after all). But at Harwell we had experimented with finer collimation, not really expecting it to work very well at higher (*i.e.* 10 + MeV) energies. To our great surprise the angular response was very good, almost perfectly linear with θ . We decided to incorporate a $\sim 3^\circ$ collimator inside the aperture. However, since the Harwell calibration and collimator tests had taken place shortly before shipment to the USA, there was not enough time to build the fine collimator for dispatch with the rest of the equipment. It was built in Southampton while we were already out in Texas. One of the Southampton crew (Lofty) brought it out as accompanied baggage. Now the collimator was made from lead and was very heavy, so when Lofty arrived at DFW he suggested to the airport authorities that it may not be wise to load it onto the carousel. The combination of Texan masculine pride and the implicit American faith in machines resulted in a look that dismissed him forthwith.

We had to wait nearly two hours longer than we had anticipated before we were able to pick up *Lofty* while they repaired the carousel and conveyed the passengers' baggage, including our collimator (brought up with a fork lift truck this time), to their extremely exasperated owners.

The ensuing balloon flight was fantastic we had nearly 30 hours at altitude during which time we observed several sources including the Crab Nebula, the Seyfert Galaxy NGC4151 and the COS-B source CG135+1. MISO worked perfectly and we published a number of pioneering papers in the field of low-energy gamma-ray astronomy. MISO was flown again in October 1978, September 1979 and May 1980. All flights were successful, MISO was always very well prepared and an extremely reliable instrument. I can only remember one minor failure, that of a temperature sensor on one of the flights; extraordinary. A number of slight modifications to the telescope were made over this period, which led to reductions in the background noise rates by about a factor of three. Our observational technique was to point MISO one collimator FWHM ahead of the target source and maintain this position in earth coordinates with the Alt-Azimuth stabilisation system. The rotation of the Earth was allowed to take the source through the collimator until the axis of the telescope was one FWHM behind the source. In this way we had equal time periods for the measurement of the background and the source plus background. The fixed earth coordinates effectively removed systematic changes in the background noise due to variances with attitude in the earth's atmosphere. The only remaining significant variance in background noise was now related to any altitude changes during the observation period. Many such observations were made to compile the total source exposure. We looked for a triangular profile in the MISO counting rates that correlated with the off-axis angle from the target source. This approach provided a systematic feature to confirm the extraterrestrial origin of the excess events, and the apex of the triangle confirmed the direction of the source to a few arc minutes. During this period we studied a considerable number (for that era) of extragalactic and galactic sources many of them were observed more than one campaign, *e.g.* NGC4151 (1977, 1979, 1980), MCG 8-11-11 (1979), NGC1275 (1979), Mrk 501 (1979), Crab (1977, 1979), Cyg X-1 (1979, 1980), CG 121+4 (1980), CG 135+1 (1978), and CG 195+4 (1978).

In parallel with the MISO flight campaigns we developed the MIFRASO large-area hard X-ray (15 – 300 keV) telescope. This instrument was a direct descendent of *Dotty* and started as a Milan-Southampton collaboration with basically the same personnel as the MISO team. There were some important changes however. The large-area (3200 cm²) of sodium iodide was achieved using the diffusive light collection technique, but there was an important difference with respect to *Dotty*, we had a separate thick NaI crystal underneath to provide active shielding. This design feature necessitated the scintillation light to be collected through the same window that the X-rays entered. After many modifications we eventually used very thin glass windows. Eight 20 × 20 cm² units constituted the telescope. The other important change was the addition of 1800 cm² of xenon gas proportional counters, which were provided by the Frascati group, led by Pietro Ubertini —hence the name MIFRASO. The rest of the system was basically the same as for the MISO telescope.

MIFRASO was a very reliable and successful project and a considerable number of successful flights were achieved from Texas and Sicily over several years following its first flight from Palestine Texas in June 1982. The active shield technique worked extremely well producing a factor of twenty reduction in the background noise in comparison to an unshielded device.

The MISO and MIFRASO days were extremely happy ones. We had a tremendous team spirit and worked extraordinarily well together —it was a thoroughly professional outfit. Our Anglo-Italian camaraderie was fabulous, not only were we producing good science, but it was tremendous fun doing it. We worked extremely hard, but with a smile. Practical jokes were commonplace, as were badminton contests, football matches, Bar-B-Qs, swimming and pool. There was not exactly much to do in a small town such as Palestine, and after about five to six weeks there we became exceedingly good swimmers and pool players, about the only thing to do in the evenings.

6. – Aftermath — INTEGRAL

In many ways the ESA space astronomy mission INTEGRAL is a complex European marriage of the Milan-Frascati-Bologna/Southampton collaboration with German-MPE and French know-how in the field of gamma-ray spectroscopy driven by their interests in nuclear astrophysics. There are two key instruments on board INTEGRAL, one the IBIS imager that is a direct descendent, both technically and team wise from the Anglo-Italian MISO/MIFRASO projects, and the other the SPI spectrometer. Beppo's legacy thus has been indirectly a fundamental and essential ingredient in the making of this project. Without his influence none of the above would have happened, and no INTEGRAL. Today I am still working with my Italian colleagues from yesteryear, and we are still producing superb science in the gamma-ray domain. Currently we have more than 400 gamma-ray sources in the IBIS catalogue and with many more to come. I currently have two Italian research students in the laboratory at Southampton. I wonder what they will go on to achieve?

APPENDICES

APPENDIX A

List of scientific publications of G. Occhialini

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APPENDIX B

Scientific and Editorial Boards

E. BELLOTTI

Dipartimento di Fisica “G. Occhialini”
Università degli Studi di Milano-Bicocca
Piazza della Scienza 3
20186 Milano
Italy
enrico.bellotti@lngs.infn.it

E. FIORINI

Dipartimento di Fisica “G. Occhialini”
Università degli Studi di Milano-Bicocca
Piazza della Scienza 3
20186 Milano
Italy
ettore.fiorini@mib.infn.it

A. PULLIA

Dipartimento di Fisica “G. Occhialini”
Università degli Studi di Milano-Bicocca
Piazza della Scienza 3
20186 Milano
Italy
Antonino.Pullia@mib.infn.it

P. REDONDI

Dipartimento di Psicologia
Università degli Studi di Milano-Bicocca
Piazza dell’Ateneo Nuovo 1
20186 Milano
Italy
pietro.redondi@unimib.it

G. SIRONI

Dipartimento di Fisica “G. Occhialini”
Università degli Studi di Milano-Bicocca
Piazza della Scienza 3
20186 Milano
Italy
giorgio.sironi@mib.infn.it

P. TUCCI

Istituto di Fisica Generale Applicata
Università degli Studi di Milano
Via Brera 28
20121 Milano
Italy
pasquale.tucci@unimi.it

G. VEGNI

Dipartimento di Fisica
Università degli Studi di Milano
Via Celoria 16
20133 Milano
Italy
guido.vegni@mi.infn.it

A. VITALE

Dipartimento di Fisica
Università di Bologna
Via Irnerio 46
40126 Bologna
Italy
Antonio.Vitale@bo.infn.it

List of Authors

B. AGRINIER
111 rue grande
77123 Noisy sur Ecole
France
b.agrinier@wanadoo.fr

C. J. BLAND
6220 Dalton Drive, Calgary
Alberta T3A 1E3
Canada
cjbland@shaw.ca

A. BERTIN
Dipartimento di Fisica
Università di Bologna
Via Irnerio 46
40126 Bologna
Italy
Bertin@bo.infn.it

G. BOELLA
Dipartimento di Fisica “G. Occhialini”
Università degli Studi di Milano-Bicocca
Piazza della Scienza 3
20186 Milano
Italy
giuliano.boella@mib.infn.it

A. BONETTI
Via Roma 205/2
16030 Pieve Ligure (GE)
Italy
albertob1920@libero.it

M. C. BUSTAMANTE
Equipe Rehseis (UMR 7596) CNRS à Paris
7 Denis-Diderot
Centre Jubelot 9
Place Jussieu
75251 Paris Cedex 05
France
mcbusta@Paris7.Jussieu.fr

A. DEAN
School of Physics and Astronomy
University of Southampton
Highfield, Southampton SO17 1BJ
UK
ajd@astro.soton.ac.uk

P. FACCIOLI
INFN, c/o Dipartimento di Fisica
Università di Bologna
Via Irnerio 46
40126 Bologna
faccioli@bo.infn.it

L. GARIBOLDI
Istituto di Fisica Generale Applicata
Università degli Studi di Milano
Via Brera 28
20121 Milano
Italy
leonardo.gariboldi@unimi.it

G. GAVAZZI
Dipartimento di Fisica “G. Occhialini”
Università degli Studi di Milano-Bicocca
Piazza della Scienza 3
20186 Milano
Italy
giuseppe.gavazzi@mib.infn.it

J. LABEYRIE
67 chemin de la Févrie
91190 Gif sur Yvette
France

L. KOCH MIRAMOND
Service d'Astrophysique, CEA Saclay
91191 Gif sur Yvette Cedex
France
lkoch@cea.fr

R. LEVI SETTI
The Enrico Fermi Institute
and Department of Physics
The University of Chicago
5640 S. Ellis Avenue
Chicago IL 60637
USA
riccardo@uchicago.edu

W. O. LOCK
2, Chemin Taverney
1218 Grand Saconnex
Genève
Switzerland
owen.lock.gva@bluewin.ch

L. MARASCHI
INAF-Osservatorio Astronomico di Brera
Via Brera 28
20121 Milano
Italy
maraschi@brera.mi.astro.it

M. MAZZONI
Dipartimento di Astronomia e Scienze
dello Spazio
Università di Firenze
Largo Enrico Fermi 2
50125 Firenze
mazzoni@arcetri.astro.it

E. OCCHIALINI
Via Giusti 11
20154 Milano
Italy
ilaud@etra.fastwebnet.it

J. PAUL
UMR 7164, Astroparticule et Cosmologie
Service d'Astrophysique, CEA-Saclay
91191 Gif-sur-Yvette Cedex
France
jpaul@cea.fr

D. H. PERKINS
Department of Particle Physics
Denys Wilkinson Building
University of Oxford
Oxford OX1 3RH
UK
d.perkins1@physics.ox.ac.uk

G. C. PEROLA
Dipartimento di Fisica "Edoardo Amaldi"
Università di Roma III
Via della Vasca Navale 84
00146 Roma
Italy
perola@fis.uniroma3.it

E. QUERCIGH
PH Division, CH-1211 Genève 23
Switzerland
quercigh@mail.cern.ch

S. RATTI
Dipartimento di Fisica Nucleare e Teorica
Università di Pavia
Via Bassi 6
27100 Pavia
Italy
Sergio.Ratti@pv.infn.it

ANA M. RIBEIRO DE ANDRADE
Coordenação de História da Ciência
Museu de Astronomia e Ciências Afins
MAST/MCT
Rua General Bruce 586
Rio de Janeiro
20921-030 Brazil
anaribeiro@mast.br

L. SCARSI
INAF-IASF Palermo
Accademia Nazionale dei Lincei
(Livio Scarsi passed away on March 2006)

G. SIRONI
Dipartimento di Fisica “G. Occhialini”
Università degli Studi di Milano-Bicocca
Piazza della Scienza 3
20186 Milano
Italy
giorgio.sironi@mib.infn.it

B. STILLER
307 Yoakum Parkway, Apt 607
Alexandria VA 22304
USA
bstiller@mailaps.org

A. TREVES
Dipartimento di Fisica e Matematica
Università dell’Insubria
Via Valleggio 11
22100 Como
Italy
treves@mib.infn.it

P. TUCCI
Istituto di Fisica Generale Applicata
Università degli Studi di Milano
Via Brera 28
20121 Milano
Italy
pasquale.tucci@unimi.it

M. J. L. TURNER
Department of Physics and Astronomy
University of Leicester
University Road
Leicester LE1 7RH
UK
turner@mac.com

G. VEGNI
Dipartimento di Fisica
Università degli Studi di Milano
Via Celoria 16
20133 Milano
Italy
guido.vegni@mi.infn.it

G. VILLA
INAF-IASF
Via Bassini 15
20133 Milano
Italy
gabriele.villa@rm.iasf.cnr.it

A. VITALE
Dipartimento di Fisica
Università di Bologna
Via Irnerio 46
40126 Bologna
Italy
Antonio.Vitale@bo.infn.it

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