4 December 1996

BRUNO PONTECORVO: FROM ROME TO DUBNA[1]

(personal recollections)

Giuseppe Fidecaro
CERN, European Organization for Nuclear Research,
Geneva, Switzerland

Junior physicists in Rome after the war (I was one of the youngest among them) knew of Bruno. He had studied at the Istituto di Fisica dell'Università with Fermi in the early thirties, and had later joined his group, getting known as the youngest of the "I ragazzi di Via Panisperna". We all knew that the Fermi group had dispersed to different places in the world before the war, but I did not know where exactly Bruno was based. Meanwhile the research in physics at the Istituto had moved from nuclear physics to cosmic rays, and continuity with the Fermi school had been assured by Edoardo Amaldi, the only one left from the old Via Panisperna group.

I happened to meet Bruno in Rome only at the end of August 1950, just at the time of a very important turning point of his life. Only much later did I have the chance to evoke with him some of his scientific achievements and their impact on particle physics – he was still speaking the coloured Italian of Tuscany, and from his witty language I gained a perception of his understanding of nature, and also of his love for life and human beings.

A large part of what follows will be recalled by others in this book, but there are facts that I lived or heard from witnesses, and also my own way of reading that part of Bruno's scientific work with which I am more acquainted. Perhaps these notes will help to remind us how rich Bruno's life was.

Bruno had strong ties with Rome before the war. Born in Pisa in 1913, after two years spent at the University of Pisa he moved in 1931 to Rome to finish his studies there, following the advice of his older brother Guido, a biologist. Bruno spent only five years in Rome, but these years were of fundamental importance for his formation as a physicist. Bruno was proud to have *grown up* in Rome. He had great admiration for Fermi [1] and all through his life one of his ambitions was in fact to pursue Fermi's teaching. In Rome

[1] In B. Pontecorvo, Selected Scientific Works, S.M. Bilenky et al. eds. Published by the Italian Phys. Soc., Bologna 1997; the Russian Edition by Nauka Fizmatlit, Moscow 1997.

he worked at the old Istituto in Via Panisperna, in Fermi's group, contributing with Edoardo Amaldi, Oscar D'Agostino, Franco Rasetti, Emilio Segré, Gian Carlo Wick to the series of experiments which led to the discovery of the slowing down of neutrons and to the production of radioactive elements by neutrons.

In the spring of 1936 Bruno was granted a fellowship by the *Ministero dell'Educazione Nazionale* to spend some time abroad. Following Fermi's advice he went to Paris, to the Laboratoire de synthèse atomique at Ivry directed by Frédéric Joliot-Curie. In Paris, at the Institut du Radium directed by Irène Curie, following his idea that electromagnetic transitions between two nuclear isomers should have large internal conversion coefficients, Bruno found a new type of *radioactive* nuclei emitting monochromatic electrons instead of the usual continuous β spectrum [2]. Similar results were obtained independently, though a little later, by Segré and Seaborg. At Ivry, Bruno proved the production of β-stable isomers using 3 MeV X-rays [3], and also produced β-stable isomers in (n,n) reactions using fast neutrons.

Bruno expected to stay in Paris only one year: he remained there four years, until the war events in June 1940 obliged him to escape to the South of France (by bicycle), and then to Portugal and the United States. He reached the U.S. in August 1940. In the U.S. Bruno was introduced by Emilio Segré to Well Surveys, Inc., Tulsa, Oklahoma who engaged him as a research physicist. Thus, Bruno was the first to develop the neutron well logging technique [4], still used today in oil fields. United States Patents were issued to Bruno for his inventions in this field [5, 6, 7].

Bruno received several attractive offers for work in the oil industry. However, his interest in fundamental research in the end prevailed over economic considerations and early in 1943 he accepted an offer to join the recently established Anglo-Canadian nuclear research laboratory in Montreal, whose staff included several distinguished scientists, refugees from various countries in Europe. Bruno got an appointment as a member of the United Kingdom staff, like other American-recruited staff. Thus Bruno returned to nuclear physics, and it was at Chalk River that he started his new and extraordinary scientific adventure in the field of particle physics, which he pursued all along his life.

Studies for the design of a heavy water natural uranium reactor were started at the Montreal laboratory, located in the campus of the Université de Montréal, under the original Director Hans von Halban, Austrian. At that time the Americans were not considering building such a reactor. The decision to build a large heavy water natural uranium reactor, the NRX, was only taken in 1944 when, at the end of April, John Cockcroft was brought from England to replace von Halban, and cooperation with the United States was restored after initial difficulties. A site for the reactor was chosen a few km north of the Chalk River village, about 150 km northwest of Ottawa on the Ontario side of the Ottawa river. A new settlement, called Deep River from the name of the nearby Ottawa river, was created about 15 km away to lodge the staff. Bruno moved from the Montreal Lab to Chalk River in December 1945, and lived with his family at Deep River. In 1946 *a lively and enjoyable tennis season was opened, the first . . . at Deep River. Pontecorvo . . . was by common consent the star player* [8]. The local tennis club still awards a cup each year for the winner of the men's singles championship and Bruno was in fact the first winner of this cup with his name suitably inscribed [9]. Others will certainly recall in this book the Bruno's keen interest in tennis and other sports.

Bruno was responsible for several physics aspects of the reactor. *He devoted almost all his effort to NRX design problems during the period 1943-5, writing some 25 reactor related reports, and, starting in mid-1944, working closely with the design engineers on the shielding. During 1945-6 he worked on (i) a large BF_3 neutron counter (for detecting fuel failures by the delayed-neutron emission from fission products released into the cooling water) and (ii) with Brian Flowers, and with Dave Kirkwood, on the development of sensitive neutron monitors for the initial start-up of NRX (from "zero" flux). Because of this latter responsibility Bruno and Dave were two of only four physicists allowed in the NRX Control Room at the July 1947 start up, for which the "Operations" Division was responsible* [10]. B.W. Sargent, Head of the Nuclear Physics Branch, was the third of the four physicists chosen to join the operators on the night of 21-22 July 1947: the chain reaction started at 6:13 am, on 22 July 1947 [8]. The fourth physicist was Donald G. Hurst, who joined the Montreal laboratory in August 1944 and did some work with Bruno on the design of the shielding [10]. The NRX reactor, finished two years after the end of the war, with its 20 MW_t and its neutron flux five times that of any other reactor in existence was the world's best research reactor [8].

Only two papers related to Bruno's work on the design of the NRX reactor are recorded in the scientific literature. A paper with Pierre Auger and A.M.

Munn concerning a measurement of the mean free path of slow neutrons in heavy water [11], and a shielding related paper with A.M. Munn [12], both published in 1947. A third paper was published the same year with J.V. Dunworth on the excitation of Indium 113 by 2 MeV X-rays [13], a follow-up of Bruno's work in Paris [3]. A paper, with Pierre Auger, also concerning shielding, mentioned by Bruno in [1] was not found in the literature. Bruno's other reactor-related papers were archived as internal reports.

In 1947 Bruno travelled to Europe for the first time after the war. He came to Rome and on 17 December 1947 he gave a seminar [14] *Sulla disintegrazione dei mesoni ed i suoi prodotti* in the new Istituto di Fisica. The headquarters of the Istituto di Fisica were in fact no longer in Via Panisperna. They had been moved to a new large building in the *Città Universitaria*, (too large for the *few* people working there at that time).

As will be discussed later, after the publication of the Conversi, Pancini, Piccioni experiment in the 1st February 1947 issue of the Physical Review [15] Bruno had in fact developed an interest in cosmic-rays, and had started experimental work on the disintegration of the μ-mesons with E.P. Hincks (Ted). This turning-point in Bruno's activity took place around the time of the first operation of the NRX reactor, namely after the major effort on the design and the commissioning of the reactor had been made, and *the space around the reactor was* [getting] *jammed with experiments as physicists, chemists, and metallurgists competed for space and time* [8]. However, research activity at Chalk River started diversifying, with the laboratory getting open to *basic* research even earlier [10]. Bruno's and Ted's first cosmic-ray experiment was finished just before Bruno's departure for Europe (their paper [16] was received by the Physical Review on 9 December 1947).

Shortly after the 1947 Bruno's visit to Rome development work was started by Italo Federico Quercia at the Istituto di Fisica e Centro di Studio per la Fisica Nucleare, on the neutron well logging technique, in collaboration with AGIP, the State Oil Company of Italy [17]. The circuit diagrams of the electronics were supplied by Bruno, together with other relevant information. I had this information from Giovanni Muratori who was responsible for this project on the AGIP side and was well acquainted with Bruno's well logging work. Muratori later joined CERN where he led the mechanical engineering section for electronics experiments for many years.

Bruno remained in Canada until January 1949, when he moved to England and joined the new Atomic Energy Research Establishment (A.E.R.E) at Harwell, Berks, directed by John Cockcroft. At that time Bruno had already acquired British citizenship. Cockcroft had moved to England already in September 1946.

From Harwell Bruno paid a second visit to Italy in 1949, when he attended the Basle-Como International Conference on Nuclear Physics, Quantum Electrodynamics, and Cosmic Rays, jointly organized by the Swiss and the Italian Physical Societies. Enrico Fermi also went to this conference, attended by most Italian physicists (including myself). I found no evidence of a possible visit to the Istituto di Fisica in Rome of Bruno at this time. On the other hand, with so many physicists in Como he could hardly have any reason to travel further South, except to meet relatives.

Bruno's stay at Harwell lasted only a bit more than a year. Early in 1950 he received an offer of a Chair at the University of Liverpool, where a large synchro-cyclotron was under construction. After a short visit to Liverpool Bruno decided to accept this offer and to move to Liverpool in autumn, after a long holiday in Italy.

Bruno and his family left on 25 July 1950 by car for Italy. They spent most of their holidays in the Rome region, but only on 25 August they moved to Rome. On 1 September they left by plane for Stockholm where they arrived before 9 p.m. Next morning they proceeded to Helsinki, and then disappeared into thin air . . . [1].

In Italy, August is the traditional summer holiday time. In particular, even today, work stops in the whole country around 15 August (*ferragosto*), except for the tourist industry. Life slowly starts again the week after, with the arrival of the cooler weather.

In 1950, 15th August happened to fall on a Tuesday. The Pontecorvos *disembarked* in Rome on the Friday following the *ferragosto* week. During his short stay in Rome (certainly during the week beginning on Monday 28 August, but not later that Thursday 31 August) Bruno paid a visit to the Istituto di Fisica.

Several people were still absent. I remember that Mario Ageno took care of him, at least for part of the time. Bruno had celebrated his 37th birthday a few days earlier, before getting to Rome. Mario was two years younger than Bruno. He had moved from Genoa to Rome in 1934 to finish his studies under Fermi just at the time the experiments on the slowing down of the neutrons had been concluded. I vaguely remember that Bruno came to the Istituto in the morning and returned to the Istituto later in the afternoon. Mario had been my tutor and it was quite natural for me to join him and Bruno and to accompany them to town at the end of the visit. It was a beautiful day and we walked together in the typical warm light of a late afternoon in Rome. The conversation developed almost entirely between Bruno and Mario. I was in fact only 24 years old, a young boy in comparison to them, and I felt myself too young to take part in their conversation. For this reason, I am convinced that Bruno paid little or no attention at all to me.

In the vicinity of the *Stazione Termini*, the main railway station of Rome, about ten minutes walk from the University, we separated. Not many days later we learned that Bruno and his family had disappeared, and this is the main reason why I still remember this episode of my life. Probably Mario and I were the last physicists to talk to Bruno in the Western World.

I found no track of Bruno's last visit to the Istituto di Fisica in the short annual report that Edoardo Amaldi published in 1951 in *La Ricerca Scientifica*, as he used to do every year [18]. I tried to reconstruct more exactly the date of Bruno's visit from the story of his last days in Rome [1]: he was busy on 29 and 30 August with his plane reservations and with his British-registered car. I guess that he came to the Istituto on Thursday 31, but I cannot exclude Monday 28. I never had the chance to talk to Bruno of his last days in Rome in more recent times.

Thus Bruno never went to Liverpool. I did go to Liverpool four years later, and spent almost two years working on the large synchro-cyclotron on which Bruno had envisaged working. The Director of the laboratory was still the same Professor Herbert Skinner who had sponsored the offer of a Chair to Bruno. People in Liverpool only once or twice mentioned to me the name of Pontecorvo, without comment.

As is well known, the world got confirmation that Bruno was in the USSR early in 1955, but it was only in 1978 that he was allowed to travel abroad, to Italy, to take part in the celebrations in honour of Edoardo Amaldi on the occasion of his 70th birthday. I saw him on this occasion, and two years later in Erice. In 1975, the Director-General of CERN appointed me Co-Chairman of the joint Scientific Committee set up under the terms of the collaboration agreement between CERN and the State Committee of the USSR for the Utilization of Atomic Energy, a responsibility that I kept until 1986. The purpose of this Committee was to supervise the collaboration of CERN teams and teams from the Western World working at CERN with Scientific Institutes in the USSR. In addition the Director-General gave me the task of coordinating the co-operation with the Joint Institute for Nuclear Research (JINR), Dubna. In that capacity I visited Dubna very many times, but I never met Bruno over there as I had no *official* reason to meet him. Although well known at CERN, Bruno in fact never participated directly in the CERN research programme. Similarly, the JINR management had no *official* reason to invite him to the several meetings that I had with them. When later Bruno started to visit CERN I met him quite often, but I never had reason to go beyond courtesy contacts.

Our friendship started on the occasion of one of my visits to Dubna, made after I left the joint Scientific Committee. I had in fact several friends among the colleagues of the Institutes in the USSR that I used to visit. I still had good relations with many of them, on a personal level. I do not remember exactly why I met Bruno in the Hall of the Dubna Hotel where I was staying. On this occasion I gave him a reprint of my talk [19] at the International Conference on *The restructuring of physical sciences in Europe and the United States, 1945-1960* held in Rome in September 1988.

Next morning Bruno returned to the Hotel. He looked very happy and somewhat excited. He had deeply appreciated my acknowledgement [19] of his 1947 Chalk River paper [20]: "In 1947, shortly after the discovery of the Conversi, Pancini, Piccioni effect, Pontecorvo was the first to notice *that the probability* ($\sim 10^6$ *sec.*$^{-1}$) *of capture of a bound negative meson is of the order of the probability of ordinary K-capture processes, when allowance is made for the difference in the disintegration energy and the difference in the volumes of the K-shell and of the meson orbit* ... Thus he called attention to the possible equality of the coupling constants of electrons and *muons* to nucleons, and essentially laid down the first two sides of the *Puppi triangle*. It should be noted that if Fermi, Teller and Weisskopf where the first to point out the existence of a 10^{10}-10^{12} disagreement of the time of capture of mesons in carbon with previous estimates and to recognize the need for a very drastic change in the forms of meson interaction, Pontecorvo, in his paper published six months later, went into more detail in the analysis of the consequences of the Rome experiment and discussed features of weak interaction with deep insight. Pontecorvo's idea in the following years developed through the work of other authors ... into the more general idea of a *Universal Fermi Interaction* ..."

I was glad to see his warm reaction, and it was my turn to get somewhat excited: I knew that his enthusiasm was in fact also an acknowledgement of the CERN experiment [21] which about ten years later proved the validity of his *intuition*, as he called it in more recent times [1], and of my work in particular. We recalled facts and anecdotes relating to his paper and the CERN experiment, and then Bruno took me to see the horses of his son Tito. We spent the whole morning together. After this friendly encounter we had frequent occasions to meet and talk, at Dubna, CERN, and in Rome.

Bruno and I had good reasons to be happy. The validity of the *Universal Fermi Interaction*, cornerstone of the weak interactions, is taken for granted today, and the fact that it was questioned for years in the past is generally ignored, buried as it is under the dust of time in chapters of the history of particle physics. Bruno is much better known today for neutrino physics. The fact that one of his major achievements had been recalled after more than forty years of almost complete silence was source of great satisfaction for him.

I too was happy. The disintegration of a π-meson into an electron and a neutrino had been searched for years without success. Its absence was creating serious difficulties to the theory of weak interactions. It was seen for the first time in 1958, just in the first experiment done at CERN with an accelerator [21]. This experiment, though immediately recognized as the experiment which gave experimental grounds to the *Universal Fermi Interaction* by establishing the electron-muon symmetry, also lay semi-forgotten in text-books and in history books. I could not be happier to see it acknowledged by somebody like Bruno for what it was, namely as an experimental result in the interplay of theory and experiments and not as simple confirmation in the shadow of evolving theories.

In his seminar *Autobiographic Notes* at CERN on 14 September 1989 Bruno recalled the great influence of the Conversi-Pancini-Piccioni experiment in Rome [15] on his life as a scientist. Until 1947, cosmic-ray physics was a quite remote field for Bruno, though he had acquired some knowledge from his friends: G. Bernardini and G. Occhialini in Florence, P. Ehrenfest Jr. in Paris, F. Rasetti and P. Auger in Montreal [22]. There is no better way to describe how Bruno entered the cosmic-ray field pursuing it in the accelerator era than by copying what Bruno himself wrote with great sense of humour [22]: *As soon as I read the Conversi et al. paper and the considerations of Fermi et al. ...related to it, I became fascinated by the particle which we call now the muon. That was indeed an intriguing particle, "ordered" by Yukawa, discovered by Anderson, and found by Conversi et al. to* *be ill-behaved to the point that it had nothing to do with the Yukawa particle ! I found myself caught in an antidogmatic wind and started to put lots of questions, such as: why the spin of the muon should be integer ? who said that the muon must decay into an electron and a neutrino and not in an electron and two neutrinos, or into an electron and a photon ? is the charged particle emitted in the muon decay an electron ? are particles other than electrons and neutrinos emitted in the muon decay ? in what form there is released the nuclear muon capture energy ?*

The first answers to these questions were given by Bruno only a few months later, in his first letter to the Physical Review [20] after the Conversi, Pancini, Piccioni experiment [15]. Bruno pointed out that as a consequence of the Rome experiment [15] *the usual interpretation of the β-process as a "two-step" process ("probable production of virtual meson and subsequent β-decay of the meson) completely loses its validity, since it would predict too long β-lifetimes: the meson is no longer the particle responsible for nuclear β-processes, which are to be described according to the original Fermi picture (without mesons). Consequently there is no need to assume that charged mesons have integral spin, as the Yukawa explanation of β-processes required. Once we believe that the ordinary β-process is not connected in any way with the meson, it is difficult to see strong reasons for the usual assumption that the meson decays with emission of a β-particle and a neutrino. We shall consider then the hypothesis that the meson has spin $\frac{1}{2}\hbar$ and that its instability is not a β-process, in the sense that it does not involve the emission of one neutrino. The meson decay must then be described in a different way: it might consist of the emission of an electron and a photon or of an electron and 2 neutrinos [23] or some other process.* Bruno concluded his paper by announcing that an experiment was being attempted to find out whether the decay of a *meson* was accompanied by the emission of a 50 MeV photon. This was the first of a series of experiments on muon decay.

Bruno, together with Ted Hincks, built an elegant apparatus which, using a limited number of counters and sophisticated electronics, allowed them to prove that neither a photon was emitted in the decay of cosmic-rays *mesons* at sea level [16], nor a neutral meson decaying into two photons [24]. These results were later described in more detail in a paper published in Canada [25]. Similar results were obtained by other authors independently (R.D. Sard and Althaus [26], O. Piccioni [27]) slightly later.

Bruno and his Chalk River colleague and friend Ted then rearranged the counters and the electronics of their setup and went on with their experiment to study the absorption of the decay products of the mesons. They obtained information on the spectrum of the charged particle emitted in the decay, ruling out the possibility of a two-body disintegration into an electron and a neutral meson, and showing that a maximum energy of about 50 MeV for the decay electrons would be consistent with their results [28]. From the observed bremsstrahlung component they concluded that the charged particle was indeed an electron [29, 30]. The complete results of this investigation were published later, together with a general discussion which included the results of selected experiments and theoretical considerations and added *to the body of evidence in support of the "electron + 2 neutrinos" process, requiring a spin* $\frac{1}{2}\hbar$ *for the* μ*-meson* [31].

It must be remembered that nearly at the same time Jack Steinberger had started a similar investigation, to measure the range of the decay electrons, using a Geiger counter technique [32]. Steinberger's final results showed that the decay electron spectrum did not extend beyond 55 MeV [33], in agreement with the results of Hincks and Pontecorvo. However, Hincks and Pontecorvo were the only group to give evidence that the charged particle indeed was an electron.

Bruno's and Ted's cosmic-ray work, though initiated by considerations of *basic* research, had some fall-out on the Chalk River reactor work, thus confirming the enlightened decision of C.J. Mackenzie, President of the National Research Council, to keep the laboratory open to fundamental research: *The cosmic-ray expertise, maintained by Ted Hincks after Bruno left, turned out to be a valued asset a few years later when it was used to measure spallation-neutron production – a requirement of the future planning program for power reactor systems.* [10]. Hanna and Hincks in fact did cosmic-ray work at Echo Lake, Colorado, 3260 m for this purpose [34], and this is what Bothwell wrote several years later in his book: *Meanwhile* [1948] *Bruno Pontecorvo and Ted Hincks of the nuclear physics division made a name for themselves through the study of cosmic rays. (Cosmic rays also produce neutrons and cause fission.)* [8].

Nevertheless, Bruno felt himself guilty [22] for the effort that he was putting in his cosmic-rays work at Chalk River. He wrote: *We were working in a reactor laboratory and because of that we developed a sort of feeling of guilt in doing cosmic ray research. True, our head B.W. Sargent (the physicist who discovered the rules relating beta decay probabilities to the energies* *of the electrons emitted) was looking with sympathy to our work. Nevertheless I cannot forget that Ted and I were reluctant to spend Laboratory money and how happy we were when Ted invented a "threshold amplifier", which saved a lot of counters, permitting to increase essentially the efficiency of detecting photons in coincidence with electrons from the hypothetical $\mu \to e\gamma$ decay !* ... [22]. May I say that Bruno's feeling only was a proof of his sense of responsibility and loyalty towards the laboratory.

Coming back to neutrino physics, Bruno is better known today for his work in this field. However, here again most people acquainted with his more recent work are unaware that it was Bruno who opened the way in 1945 to the long series of neutrino experiments in the world. He was in fact the first to propose a method to detect free neutrinos in his P.D-141 Chalk River report [35], followed 18 months later by a second report, the P.D.205 [36], at a time the neutrino was generally considered an undetectable particle and it was common belief that it did not make sense to start looking for it.

Experiments on the recoil of nuclei in beta disintegrations had already been performed with a view to confirming the neutrino hypothesis. Bruno pointed out [36, 37] that these experiments, based on energy-momentum conservation, could only either disprove the neutrino hypothesis or increase "indirect" evidence for its existence. "Direct" proof of the existence of the neutrino could only come from the detection of processes produced by free neutrinos, like inverse β processes. For this purpose Bruno invented the Chlorine-Argon radiochemical method, based on the separation of the unstable Argon-37 produced in the reaction $\nu + {}^{37}\mathrm{Cl} \to \mathrm{e}^- + {}^{37}\mathrm{A}$, and the successive detection of the 2.8 keV Auger electron emitted with a 34 days half-life after the K-capture reaction [36].

Bruno's first idea in 1945 [35] had been to use the reaction $\bar{\nu} + {}^{35}\mathrm{Cl} \to \mathrm{e}^+ + {}^{35}\mathrm{S}$. Sulphur-35 nuclei could be identified by their 87.1 day half-life decay into Chlorine-35 with emission of 120 keV maximum energy negative electrons. When Bruno wrote his paper the Seaborg *Table of isotopes* [38] gave no information on the decay of Argon-37 except for its half-life (34 days), and presumably Bruno was not even aware of the new results on this decay [39], published only a few months before P.D.-141 was issued [10]

It should be noted that in those days the problem whether $\nu \neq \bar{\nu}$ was still open. In the case that ν and $\bar{\nu}$ were different the Chlorine-Sulphur method could only be used to detect reactor neutrinos, while Chlorine-Argon would be approprite for solar neutrinos. There is no doubt that Bruno was aware of this. He considered the solar neutrino flux too low for an experiment in P.D.-141 [35], but gave more serious consideration to solar neutrino experiments in P.D.-205 [36] (railway car tanks filled with CCl_4 in a tunnel in the Canadian Rockies were also considered [10]).

Tests for a neutrino detector were started with Argon-37 prepared at the Chalk River heavy-water reactor using an (n, γ) reaction. Bruno's group included G.C. Hanna and D.H.W. Kirkwood. In those days scintillation counters did not yet exist and proportional counters were only used to detect strongly ionizing particles such as low energy protons and alphas. It was currently believed in fact that for a proper behaviour the gas amplification factor had to be kept small, let us say $\lesssim$100.

It was by chance that Bruno and his collaborators discovered, independently of Curran et al. [40], the high gain regime (up to 10^6), while looking at Argon-37 pulses in a Geiger counter whose high voltage was set below the avalanche regime [22]. They realized that large-gas-amplification proportional counters could be advantageous for the detection of the 2.8 keV Auger electrons with low background, and developed a complete technique for precise measurements of energy, which in fact proved essential for the detection of neutrinos using radiochemical methods.

With a proportional counter and a 10^4 multiplication factor Bruno and co-workers could measure the energy of the 2.8 keV Auger electrons [41]. Further work with a 2×10^5 gain was done to investigate in detail the nuclear capture of L electrons in ^{37}Cl ($\sim$10 ion pairs, $\sim$280 eV) [42], previously observed by Bruno and co-workers for the first time [41]. The same technique was used to study the β spectrum of tritium, which was found compatible with a neutrino mass $\lesssim$500 eV [43], a significant result in those days. The results were compared with those obtained by Curran et al. [40] using the large-gas-amplification proportional counters technique developed independently by them. The essentials of the investigation of the L-capture and of the spectrum of ^{3}H were described in a paper [44]. The apparatus simply consisted of ... a proportional counter, a linear amplifier, and a 30-channel pulse height analyser.

A more detailed discussion of the Chlorine-Argon method was done in 1949 by L. Alvarez, substantially confirming the choice made by Bruno [45]. Alvarez's paper is often quoted together with Bruno's paper, and the two papers are quoted in chronological order. However, sometime Alvarez is credited with having invented the Chlorine-Argon method "independently" from Bruno. As remarked also by S.T. Petkov in his article on Bruno's contributions to weak interaction and neutrino physics [46], the reference to P.D.-205 and to conversations with Bruno in [45] do not seem to justify this credit.

The idea of a neutrino detector at Chalk River was abandoned in 1949, when Bruno moved to England. There was nobody left to do the experiment: Dave Kirkwood had departed in August 1948 and Hanna got involved in other experiments [10]. However, Bruno did not abandon the idea. In Bristol he calculated cosmic ray background with the help of Camerini for various Cl-A experiments that he was planning to do [22].

The Chlorine-Argon method was resumed by R. Davis, Jr. a few years later at Brookhaven in an attempt to detect reactor neutrinos [47]. The negative result obtained in 1956 was considered as a first evidence that neutrinos and antineutrinos were not identical [48]. A new experiment was then started by Davis and collaborators to detect solar neutrinos, but it took several years before they could overcome the background. The Geiger counters were in the first place replaced with proportional counters and the pulse height measured, but further reduction of the background was necessary [49]. In 1968 Bruno met Davis at the first neutrino conference in Moscow and expressed the opinion that *measuring the form of the counter pulse, in addition to the amplitude, should result in a considerable decrease of the effective background in his solar experiment* [22]. This suggestion proved correct as Bruno found out from Davis at the ν'72 conference in Hungary [50]. Thus the experiment, *grace to the heroic effort of R. Davis, terminated brilliantly, but many many years after its conception* [22]. Additional references can be found in [46].

Bruno continued to work on large-gas-amplification proportional counters after he moved to Harwell [51], as can also be seen from the work of his collaborators [52, 53, 54]. Bruno dicussed the Chlorine-Argon method, including solar neutrinos, with Fermi in Chicago, probably in 1948 and later at the Basle-Como conference in 1949. Fermi was not enthusiastic at all about the neutrino application of the Chlorine-Argon method, but liked very much the proportional counters. *Don Quixote was not the hero of Fermi* - Bruno recalled [22] - leaving to somebody else, perhaps Segré, the task of expressing a judgement on Fermi ! However, the detection of the inverse β process was recalled by Fermi in his 1950 lectures at Chicago as the most conclusive proof of the existence of the neutrino [55], and when Reines and Cowan informed him in 1952 that they had a suitable detector to see neutrinos from a fission reactor (not from a nuclear explosion as they had originally in mind), Fermi answered with a very encouraging letter ... [56].

On the contrary, Pauli very much liked the idea of the Chlorine-Argon experiment. Bruno met him in Zürich and had lunch with him and Peter Preiswerk in 1947-8 on the occasion of his first trip to Europe after the war. Pauli – Bruno recalled – *remarked that it was not clear whether the "reactor neutrinos" should definitely be effective in producing the reaction, but he thought that they probably would (as you see, this is the Majorana point of view)* [22].

Bruno returned to neutrino physics early in 1959. An 800 MeV meson factory with a huge current was being designed at the Laboratory of Nuclear Problems of the JINR in 1958 and Bruno started considering the experimental programme for it (an accelerator which was never built) [22]. Under these circumstances it occurred to him that neutrino experiments were perfectly feasible at particle accelerators, in particular to find whether ν_e and ν_μ (or $\bar{\nu_e}$ and $\bar{\nu_\mu}$) were identical. The idea was to dump an intense proton beam into a thick absorber and produce an (isotropic) source of $\bar{\nu}_\mu$, ν_e and ν_μ from π-μ-e decay, with a negligible background of $\bar{\nu_e}$. A Reines and Cowan detector (insensitive to ν_e and ν_μ), whose operation was known, placed at an angle $\gtrsim 90°$ (to reduce possible background from neutrons) should give no counts if $\bar{\nu_e} \neq \bar{\nu_\mu}$ as the reaction $\bar{\nu_\mu} + p \to e^+ + n$ would be forbidden and the corresponding reaction $\bar{\nu_\mu} + p \to \mu^+ + n$ could not take place from π^+-μ^+ decay for energetic reasons.

Bruno described this experiment in more detail in his famous paper *Electron and Muon Neutrinos* [57]. This paper was the first one to discuss in detail the problem of the identity of ν_μ and ν_e, (or $\bar{\nu_\mu}$ and $\bar{\nu_e}$). In particular Bruno made a long list of 21 reactions produced by neutrinos or antineutrinos, clearly marking the reactions forbidden if $\nu_e \neq \nu_\mu$. The notations introduced by Bruno in this paper are the same notations used today. It is interesting to note that two neutrinos appear in the weak interactions Lagrangian written by Bruno in this paper: an electron neutrino and a muon neutrino. Similarly, a few months earlier Oneda and Pati [58] inserted in the Lagrangian two different particles, a neutrino and an hypothetical particle ω.

In both cases there was no fundamental reason to make the two particles identical, and in fact Bruno wrote [22]: *I have to come back a long way (1947-1950). Several groups, among which J. Steinberger, E. Hincks and I, and others were investigating the (cosmic) muon decay. The result of the* *investigations was that the decaying muon emits 3 particles: one electron (this we found by measuring the electron bremsstrahlung) and two neutral particles, which were called by various people in different ways: two neutrinos, neutrino and neutretto, ν and ν', etc. I am saying this to make clear that for people working on muons in the old times, the question about different types of neutrinos has always been present. True, later on many theoreticians forgot all about it, and some of them "invented" again the two neutrinos (for example M. Markov), but for people like Bernardini, Steinberger, Hincks and me ... the two neutrino question was never forgotten. Of course, the question became much more precise in my mind, in the sense that possible "partners" arose: maybe ν_e is always the partner of the electron, ν_μ of the muon ... How to perform the decisive experiment I was able to formulate [57] clearly enough (the use of muon neutrino beams). At that time the idea of the experiment was not obvious, although the statement may be strange today: one must search for electrons and muons produced in matter by muon neutrinos; if $\nu_\mu \neq \nu_e$ one should find that $N_e \ll N_\mu$, N_e and N_μ being the number of electrons and muons produced correspondingly.*

In his paper Bruno gave reasons why the hypothesis of distinct electron and muon neutrinos was attractive, in particular he recalled that reactions like the $\mu \to e+\gamma$ did not occur (a subject already investigated by Bruno and Ted Hincks [16]), though a branching ratio of 10^{-4} had been predicted [59], and concluded that *the hypothesis of two different types of neutrinos, unable to annihilate each other, is attractive from the point of view of symmetry and systematics of particles and also could help us understand the difference in the nature of the muon and electron.*

Bruno's paper *Electron and muon neutrinos* [57] was submitted to the JETP editor on 9 July 1959, namely just a few days before the opening of the Ninth International Annual Conference on High Energy Physics, held at Kiev on 15-25 July. Therefore, at the time of writing his paper he did not know the latest results on the $\mu \to e + \gamma$ decay. These results were summarized by the *rapporteur* A.I. Alikhanov in the Weak Interaction Plenary Session. Alikhanov [60] recalled that in the case of existence of an intermediate vector boson the branching ratio $R(\mu \to e+\gamma)/R(\mu \to e+\nu+\bar{\nu})$ was 10^{-4} [59]. In the case of non-existence of an intermediate vector boson the $\mu \to e+\gamma$ process could still exist but the prediction of its branching ratio would not be definite. Alikhanov did not make any reference to the problem of the two neutrinos.

Altogether, Alikhanov listed seven experiments, five electronic (four performed in 1959, one in 1955), and two freon bubble chamber experiments (performed in 1959). Out of the five electronic experiments three, done in 1959, gave for the branching ratio $R\,(\mu \to e+\gamma)\,/R\,(\mu \to e+\nu+\bar{\nu})$ upper limits of $\lesssim 10^{-6}$. The other four experiments gave upper limits about one order of magnitude smaller. Forgetting the oldest experiment, it appears that at the time of writing his paper Bruno only knew that one experiment had given for the branching ratio a limit of 10^{-6}. Thus, only at the conference did he learn that the two-neutrino problem was getting hot.

May I mention that one of the three $\mu \to e+\gamma$ experiments performed in 1959 giving a branching ratio smaller than $\sim 10^{-6}$ was a CERN experiment in which I was involved [61]. In his Kiev report Alikhanov pointed out that the two bubble chamber experiments, though not rich in statistics had the advantage of *clearness*, with reference – I assume – to the fact that these experiments could detect the internal bremsstrahlung process $\mu \to +\nu+\bar{\nu}+\gamma$ and thus more safely separate $\mu \to e+\gamma$ decays from this type of background. The CERN experiment had been precisely designed with the idea of not rejecting γ background events electronically. Thus it was possible to record 25 bremsstrahlung events and show that the mean life of these events was equal to the mean life of the muon. The latter determination could only be done with an electronics device. However, this part of the experiment was not communicated to the rapporteur and therefore was not be presented at the conference. This is the experiment which introduced me to the neutrino chapter of Bruno's scientific work.

In the course of the discussion that followed the Alikhanov presentation Bruno raised the problem of the two neutrinos and described his proposed meson factory experiment along the lines already sketched in his paper. Then he reported considerations due to R. Ryndin and himself. Without entering into details, Bruno pointed out the possibility of producing intermediate vector bosons in interactions of very high energy neutrinos in the nuclear field. He clearly meant to take advantage of the large increase in the cross section with energy obtainable with high energy accelerators, not with meson factories. Shortly afterwards M. Schwartz proposed, independently from Bruno, using high-energy neutrinos from π-decay to investigate weak interactions [62].

The problem of the two neutrinos was solved at Brookhaven by G. Danby et al. [63]. Meanwhile it had become clear [64], as mentioned by these authors, that any general mechanism preserving the unitarity of the Fermi theory would lead to a $\mu \rightarrow e + \gamma$ branching ratio not too different from that evaluated by Feinberg [59]. Under these circumstances the hypothesis that the two neutrinos could be distinct was gaining interest. Dubna could not compete with the Brookhaven AGS in this case !

I should like to stop here, but not without mentioning that an attempt was nevertheless made at Dubna to search for anomalous scattering of muon neutrinos by nucleons (in case it existed) using neutrinos and antineutrinos produced from the decay in flight of $\pi^{\pm}$ generated by the 10 GeV proton beam of the Dubna synchrotron, and a Geiger counter detector [65]. An upper limit for the cross section was given.

Bruno's interest focussed later on the problem of neutrino oscillations, a hot problem in neutrino physics today, also raised by Bruno [66, 67] and later by him generalized from ($\nu \leftrightarrow \bar{\nu}$) to ($\nu_e \leftrightarrow \nu_\mu$) oscillations [68]. This chapter will not be touched on here. I should only like to recall, as this too is not widely known, that Bruno also suggested, independently of A. Pais, the associated production of kaons and hyperons [69] (another example of Bruno's fertile mind), which in turn led to strangeness conservation in strong interactions.

Bruno died on 24 September 1993 in Dubna. I happened to be in Protvino at that time and had in mind to pay a visit to Dubna, to Bruno in particular. I was shocked when I learned that he was no more with us. Protvino friends kindly invited me to join a little delegation going to Dubna to Bruno's funeral. I am grateful to them. I had the sad privilege, together with Guido Piragino, to give Bruno the last greetings from the country which had given him birth and had raised him as a physicist.

Acknowledgements

Mr. Geoff C. Hanna, a former colleague of Bruno at the Chalk River laboratory, contributed additional information on the Canadian period, some of which came from Dr. Donald G. Hurst. Dr. Malcolm Harvey, Chalk River, and Mrs Joan Melvin from Deep River sent me valuable documentation and information. Professor Douglas Stairs, McGill University, an old friend of mine, enthusiastically put me in touch with the people above. Dr. Giovanni Muratori, CERN, gave me detailed information on Bruno's neutron well logging work. To all, I should like to express my gratitude.

References

[1] M. Mafai: *Il Lungo Freddo*. A. Mondadori, Milano, 1992.

[2] B. Pontecorvo: *Recent Experimental Results in Nuclear Isomerism*. Nature, **144**, (July 29, 1939) 212.

[3] B. Pontecorvo and A. Lazard: *Nuclear Isomerism Produced by a Continuous Spectrum of X-Rays*. Comptes Rendus, **208** (Jan. 9, 1939) 99.

[4] B. Pontecorvo: *Neutron Well Logging, a new geological method based on nuclear physics*. The Oil and Gas Journ., Vol. 40, No. 18, 32-3 (Sept. 11, 1941). See also, B. Pontecorvo: *Radioactivity Analysis of Oil Well Samples*. Geophysics VII, (1942) 90-94.

[5] 2,349,753 - Issued May 23, 1944. B. Pontecorvo: *Method and Apparatus for Geophysical Exploration*.

[6] 2,353,619 - Issued July 11, 1944. B. Pontecorvo and G. Swift: *Geophysical Prospecting*.

[7] 2,398,324 - Issued April 9, 1946. B. Pontecorvo: *Well Surveying*.

[8] R. Bothwell: *Nucleus, the history of Atomic Energy of Canada Limited*, University of Toronto Press, 1988. Robert Bothwell tells in this book the story of the birth of the Anglo-Canadian Projet, its transfer to the National Research Council of Canada after the end of the war in 1945, and its transformation into Atomic Energy of Canada Limited (AECL) in the early fifties.

[9] Malcolm Harvey: *private communication*, 24 June 1996.

[10] G.C. Hanna: *private communication*, 24 October 1996.

[11] P. Auger, A.M. Mumm, B. Pontecorvo: *The Transport Mean Free Path of Thermal Neutrons in Heavy Water*. Canad. J. Res. A, **25** (1947) 143.

[12] A.M. Munn and B. Pontecorvo: *Spatial distribution of neutrons in hydrogenous media containing bismuth, lead, and Iron*, Canad. J. Res. A, **25** (1947) 157.

[13] J.V. Dunworth and B. Pontecorvo: *Excitation of Indium 113 by X-rays*, Proc. Cambridge Phil. Soc, bf 43 (1947) 123.

[14] E. Amaldi: *Centro di Studio per la Fisica Nucleare e delle Particelle Elementari. Attività svolta durante l'anno 1947.* La Ricerca Sc., Anno 17°, 394 (1947).

[15] M. Conversi, E. Pancini, O. Piccioni: *On the Disintegration of Negative Mesons*. Phys. Rev. **71** (1947) 209.

[16] E.P. Hincks and B. Pontecorvo: *Search for Gamma-Radiation in the 2.2-Microsecond Meson Decay Process*. Phys. Rev. **73** (1948) 257.

[17] E. Amaldi: *Centro di Studio per la Fisica Nucleare e delle Particelle Elementari. Attività svolta durante l'anno 1949.* La Ricerca Sc., Anno 20°, 927 (1950).

[18] E. Amaldi: *Centro di Studio per la Fisica Nucleare e delle Particelle Elementari. Attività svolta durante l'anno 1950.* La Ricerca Sc., Anno 21°, 1149 (1951).

[19] G. Fidecaro: *From Cosmic Rays to Physics with accelerators*. Proc. Int. Conf. on the Restructuring of Physical Sciences in Europe and the United States 1945-1960, eds. M. De Maria, M. Grilli, F. Sebastiani, World Scientific, p. 654.

[20] B. Pontecorvo: *Nuclear Capture of Mesons and the Meson Decay*. Phys. Rev. **72** (1947) 246.

[21] T. Fazzini, G. Fidecaro, A.W. Merrison, H. Paul, A.V. Tollestrup: *Electron Decay of the Pion*. Phys. Rev. Lett. **1** (1958) 247.

[22] B. Pontecorvo: *The Infancy and Youth of Neutrino Physics: Some Recollections*. Proc. Int. Colloquium on the History of Particle Physics: some discoveries, concepts, institutions from the thirties to fifties, 21-23 July 1982, Paris, in Journal de Physique, Colloque C-8, supplément au n° 12, Tome 43, Décembre 1982, Les Ulis, France, p. 221. – Following a request from the editors of *Usp. Fiz. Nauk* Bruno was asked to prepare an article*) on the basis of previous reports, including the paper quoted here. I have chosen to use extracts from the original text i.e. *The Infancy*

. . . , which avoids translation into Russian and re-translation back into English. – *) B.M. Pontecorvo, Usp. Fiz. Nauk **141** (December 1983) 675 [*Pages in the development of neutrino physics*, Sov. Phys. Usp. **26** (12), (December 1983) 1087].

[23] L.W. Nordheim: *On the Nature of the Meson Decay.* Phys. Rev. **59** (1941) 554.

[24] E.P. Hincks and B. Pontecorvo: *On the Stability of the Neutral Meson.* Phys. Rev. **73** (1948) 1122.

[25] E.P. Hincks and B. Pontecorvo: *On the Absence of photons among the decay products of the 2.2 microsecond meson.* Canad J. Res. A **28** (1950) 29.

[26] R.D. Sard and E.J. Althus: *A Search for Delayed Photons from Stopped Sea Level Cosmic-Ray Mesons.* Phys. Rev. **74** (1948) 1364.

[27] O. Piccioni: *Search for Photons from Meson-Capture.* Phys. Rev. **74** (1948) 1754.

[28] E.P. Hincks and B. Pontecorvo: *The Absorption of Charged Particles from the 2.2-μsec. Meson Decay.* Phys. Rev. **74** (1948) 697.

[29] E.P. Hincks and B. Pontecorvo: *On the Decay Products of the μ-Meson.* Phys. Rev. **75** (1949) 341(A).

[30] E.P. Hincks and B. Pontecorvo: *The Penetration of μ-Meson Decay Electrons and Their Bremsstrahlung Radiation.* Phys. Rev. **75** (1949) 698.

[31] E.P. Hincks and B. Pontecorvo: *On the Disintegration Products of the 2.2-μSec. Meson.* Phys. Rev. **77** (1950) 102.

[32] J. Steinberger: *On the Range of the Electrons in Meson Decay.* Phys. Rev. **74** (1948) 500.

[33] J. Steinberger: *On the Range of the Electrons in Meson Decay.* Phys. Rev. **75** (1949) 1136.

[34] M. Bercovitch, H. Carmichael, G.C. Hanna, E.P. Hincks: *Yield of neutrons per interaction in U, Pb, W, and Sn by protons of six energies between 250 and 900 MeV selected from cosmic radiation*, Phys. Rev. **119** (1960) 412.

[35] B. Pontecorvo: *On a method for detecting free neutrinos*, Report P.D.-141, Chalk River Laboratory, 21 May 1945.

[36] B. Pontecorvo: *Inverse β process*, a lecture given on 4 September 1946 to a Nuclear Physics Conference in Montreal. Issued as Report P.D.-205, National Research Council of Canada, Division of Atomic Energy, Chalk River, Ontario, 20 November 1946. – The Conference, 2-6 September 1946, was organized by Dr. C.J. Mackenzie, President of the National Research Council, primarily to inform the University community of the research facilities that were becoming available at Chalk River, and to encourage their use, particularly by graduate students for their thesis work. No Proceedings were published, but some of the lectures were written up as Chalk River Reports, e.g. P.D.-205. The Conference was held at the McGill University, in the Macdonald Physics Building well known for the work of Ernest Rutherford before he moved to England in 1907.

[37] B. Pontecorvo: *The Neutrino and the Recoil of Nuclei in Beta Disintegrations*. Rep. on Progr. in Phys., Volume XI (1946-47) p. 32-42.

[38] G.T. Seaborg: *Table of isotopes*, Rev. Mod. Phys. **16** (1944) 1.

[39] P.K. Weimer, J.D. Kurbatov, M.L. Pool: *K-electron capture in radioactive argon a^{37}*, Phys. Rev. **66** (1944) 209.

[40] S.C. Curran, J. Angus, A.L. Cockcroft: Nature **162** (1948) 302; Phil. Mag. **40** (1949) 53. See also Phys. Rev. **76** (1949) 853.

[41] D.H.W. Kirkwood, B. Pontecorvo, G.C. Hanna: *Fluctuations of Ionization and Low Energy Beta-Spectra*. Phys. Rev. **74** (1948) 497.

[42] B. Pontecorvo, D.H.W. Kirkwood, G.C. Hanna: *Nuclear Capture of L_I Electrons*. Phys. Rev. **75** (1949) 982.

[43] G.C. Hanna and B. Pontecorvo: *The β-Spectrum of H^3*. Phys. Rev. **75** (1949) 983.

[44] G.C. Hanna, D.H.W. Kirkwood, B. Pontecorvo: *High Multiplication Proportional Counters for Energy Measurements*. Phys. Rev. **75** (1949) 985.

[45] L. Alvarez, University of California (at Berkeley), Report No. UCRL-328, April 18, 1949, Berkeley.

[46] S.T. Petcov: *On B. Pontecorvo Contributions to Weak Interaction and Neutrino Physics*. Proc. 6th Int. Workshop on Neutrino Telescopes, February 22-24, 1994, Venice,

[47] R. Davis: *Attempt to Detect the Antineutrinos from a Nuclear Reactor by the $Cl^{37}\,(\bar{\nu}, e^-)\,A^{37}$ Reaction*. Phys. Rev. **97** (1955) 766.

[48] R. Davis, Jr. *An attempt to detect the neutrinos from a nuclear reactor by the $Cl^{37}\,(\bar{\nu}, \bar{e})\,A^{37}$ reaction*, Bull. Am. Phys. Soc. II **1** (1956) 219.

[49] R. Davis, Jr.: *Solar neutrinos. II. Experimental*, Phys. Rev. **12** (1964) 303.

[50] R. Davis Jr., J.C. Evans, V. Radeka and L.C. Rogers: *Report on the Brookhaven Solar Neutrino Experiment*. Proc. of the *"Neutrino '72"* Int. Conference, Balatonfured, Hungary, June 1972, eds A. Frenkel and G. Marx, Budapest, 1972, p. 5.

[51] B. Pontecorvo: *Recent Developments in Proportional Counter Technique*. Int. Kongress über Kernphysik und Quantenelektrodynamik, Basel 5-9 Sept 1949, Helv. Phys. Acta Vol. XXIII, Suppl. III, Basel 1950, p. 97-118.

[52] P. Rothwell and D. West: *The Calibration of Proportional Counters with X-rays Produced by Radioactive Sources*. Proc. Phys. Soc. A, **63** (1950) 541.

[53] P. Rothwell and D. West: *Proportional Counters in a Magnetic Field; an Investigation of the Nuclear Isomerism in ^{80}Br*. Proc. Phys. Soc. A, **63** (1950) 539.

[54] P. Rothwell: *Fluctuation in the Energy Loss of Fast Electrons in a Proportional Counter*. Proc. Phys. Soc. B, **64** (1951) 911.

[55] E. Fermi: *Nuclear Physics*, J. Orear, A.H. Rosenfeld, R.A. Schulters eds, Univ. of Chicago Press, 1950.

[56] F. Reines: *50 years of neutrino physics*, Proc. of Neutrino'80, E. Fiorini ed., Int. Conf. on neutrino physics and astrophysics, Erice, June 23-28 1980, Plenum, New York 1982, p. 11. – In his ref. [3] Reines, after recalling the Chlorine-Argon radiochemical method suggested by "B. Pontecorvo and L.W. Alvarez" using a fission reactor, points out that the reactor result would have been negative anyhow, though the neutrino exists. I should like to recall that Bruno considered in his proposal both, solar and reactor neutrinos, that he had a specific interest in solar neutrinos, and that, in any case, each suggestion had its own merit as shown later by Davis, first with his reactor experiment, and then by detecting solar neutrinos.

[57] B.M. Pontecorvo, Zh. Exp. Teor. Fiz **37** (1959) 1236 [B. Pontecorvo: *Electron and Muon Neutrinos*, Sov. Phys JETP **10** (1960) 1751].

[58] S. Oneda and J.C. Pati: *V-A Four-Fermion Interaction and the Intermediate Charged Vector Meson*. Phys. Rev. Lett. **2** (1959) 125.

[59] G. Feinberg: *Decays of the μ Meson in the Intermediate-Meson Theory*. Phys. Rev. **110** (1958) 1482.

[60] A.I. Alikhanov: *Weak Interactions, Experiment*. Proc. Ninth Int. Conf. on High Energy Physics, Plenary Sessions VI-IX, p. 163, Kiev 15-25 July 1959.

[61] J. Ashkin, T. Fazzini, G. Fidecaro, N.H. Lipman, A.W. Merrison and H. Paul: *Search for the Decay $\mu \to e + \gamma$ and Observation of the Decay $\mu \to e + \nu + \bar{\nu} + \gamma$*. Nuovo Cim., Series X, **14** (1959) 1266.

[62] M. Schwartz: *Feasibility of Using High-energy neutrinos to study the weak interactions*. Phys. Rev. Lett. **4** (1960) 306.

[63] G. Danby, J.-M. Gaillard, K. Goulianos, L.M. Lederman, N. Mistry, M. Schwartz, and J. Steinberger: *Observation of High-energy Neutrino*

Reactions and the Existence of Two Kinds of Neutrinos. Phys. Rev. Lett. **9** (1962) 36.

[64] T.D. Lee: *Intermediate Boson Hypothesis of Weak Interactions*. Proc. of the 1960 Annual Int. Conf. on High Energy Physics at Rochester, The University of Rochester, Rochester, N.Y., Aug 25-Sept 1, 1960, eds E.C.G. Sudarshan, J.H. Tinlot, A.C. Melissinos, Univ. of Rochester, 1960, p. 567 .

[65] I.M. Vasilevsky, V.I. Veksler, V.V. Vishniakov, B. Pontecorvo and A.A. Tyapkin: *Search for Anomalous Scattering of Muon Neutrinos by Nucleons*. Phys. Lett. **1** (1962) 345.

[66] B.M. Pontecorvo, Zh. Eksp. Teor. Fiz. **33** (1957) 549 [B. Pontecorvo: *Mesonium and antimesonium*, Sov. Phys. JETP **6** (1958) 429].

[67] B.M. Pontecorvo, Zh. Eksp. Teor. Fiz. **34** (1958) 247 [B. Pontecorvo: *Inverse beta processes and nonconservation of lepton charge*, Sov. Phys. JETP **7** (1958) 172].

[68] B. Pontecorvo: *Neutrino experiments and the question of leptonic-charge conservation*, Old and new problems in elementary particles, a volume dedicated to Gilberto Bernardini in his sixtieth birthday, G. Puppi ed., Academic Press, New York and London 1968, p. 251.

[69] B.M. Pontecorvo, Zh. Exp. Teor. Fiz. **29** (1955) 140 [B.M. Pontecorvo: *The Processes of Production of Heavy Mesons and V_1^0-Particles*, Sov. Phys. JETP **2** (1956) 135].