объединенный
институт
ядерных
исследований
дубна

E1,2-82-414

B.Pontecorvo

THE INFANCY AND YOUTH OF NEUTRINO PHYSICS: SOME RECOLLECTIONS

Submitted to the International Colloquium
on Particle Physics (Paris, July 1982)

1982

§ 1. Introduction. - When I started to prepare the present report for the 1982 Paris International Colloquium on the history of elementary particle physics, I was faced right away with the circumstance that the program included talks on both neutrino and weak interaction physics, which are very close subjects indeed. Thus I decided to underline those moments in the field under consideration which are related to the properties of neutrinos as such (their detection methods, penetrating power, number, lepton charges, sources, importance in astrophysics...).I decided also that I should not worry too much about covering arguments treated also in other talks at our Colloquium and that I would write mainly for professional physicists of the young generation who are acquainted very well with the things which are being done now in neutrino physics, but not so well with the very background from which neutrino physics came to be what it is nowadays. Now I am not going to write a small book on neutrinos and I must select a few episodes. I shall talk about events which had a deep influence upon me. They are either extremely significant or not necessarily very important, but familiar to me and perhaps somewhat curious. In a word my talk is quite subjective. All the episodes I have "seen" with either my eyes or the eyes of physicists close to me. I am preparing my talk at first digging out of my memory and only subsequently (and quite seldom!) out of literature, with the aim of checking and precising.

Let nobody express the opinion that such a "strategy" is dictated by my laziness. Of course there is some truth in such an opinion, as the proverb "excusatia non petita, accusatia manifesta" is suggesting, but the full story is: there are some old scientists (apparently I am one of them) who would like very much to let people know what in their life they (think they) have accomplished, but are usually ashamed to act openly according to such a desire. Well, our present colloquium (plus the strategy I have chosen), provides a good (and possibly decent!) chance of satisfying the desire.

I apologize to many physicists, including a few close friends, for not having given them the credit they would deserve in an objective report of the neutrino physics development.

Two words concerning the question about the time at which happened the events I shall cover: according to the desire of the organising committee, I should not touch upon episodes which took place later than the latest fifties.

Even a dry, subjective and incomplete enumeration of events may serve as a quick introduction of the reader to the atmosphere of the past. Well informed people should not read the next four paragraphs, where there is presented such enumeration. Neutrino physics passed through periods, not necessarily implying a strict time sequence, which may be choosen in a more or less arbitrary way as follows.

§ 2. First period (1896-1930): the gestation of neutrino physics. - It includes, as far as experiment is concerned, the discovery of radioactivity (Becquerel 1896), of beta rays (Rutherford 1899), of the continuous beta spectrum (Chadwick 1914), the measurement of the heat released by beta rays (Ellis and Wooster 1927); as far as theoretical work is concerned, the quantum theory of radiation (Dirac 1927), the relativistic equation of spin 1/2 particles (Dirac 1928); as far as new experimental methods are concerned, the invention of counters capable of detecting single charged particles (Geiger, Rutherford and Muller 1908), of the cloud chamber (Wilson 1912), of nuclear photoemulsions (Misovsky 1925). I shall not cover this period in my talk.

§ 3. Second period (1930 - the early fifties): the infancy of neutrino physics. - Among theoretical achievements it includes the invention of the neutrino (Pauli 1930), the theory of atomic nuclei made up of protons and neutrons (Ivanenko; Heisenberg;Majorana 1932),the beta decay theory (Fermi; Perrin 1933), the meson theory of nuclear forces (Yukawa 1935), the first discussion of double beta decay (Geppert-Maier 1935), the Gamov-Teller selection rules in beta decay (Gamov and Teller 1936), the "truly neutral" neutrino (Majorana 1937), the first consideration of neutrinoless double β decay (Furry 1939), the investigation of neutrino emission in thermonuclear reactions in the Sun and other stars (Bethe 1939), the URKA process - the first discussion of the neutrino role in star evolution (Gamov and Schonberg 1941), the "big-bang" theory (Gamov 1946), the introduction of lepton charge (Marx; Zeldovich; Konopinsky and Mahmoud 1953); as far as experiment is concerned, the second period includes the discovery of the positron (Anderson 1932), of the neutron (Chadwick 1932), of artificial radioactivity (Curie and Joliot 1934), of positron emission in beta decay (Curie and Joliot 1934), the first search experiment of nuclear recoils in beta decay (Leipunsky 1935), the observation of orbital electron capture by nuclei (Alvarez 1937), the discovery of muon (Anderson ann Neddermayer 1938), of the neutron radioactivity (Snell; Robson 1948), the first sensitive determination of the (anti) neutrino mass upper limit from the ^{3}H beta decay (Curran et al.; Hanna and Pontecorvo 1949), the observation that antineutrinos are not interacting with ^{37}Cl nuclei in the reaction $\tilde{\nu}_e + {}^{37}\mathrm{Cl} \rightarrow e^- + {}^{37}\mathrm{A}$ (Davis 1956) and, last but not least, the observation of free antineutrinos from a nuclear reactor through the inverse beta process (Reines and Cowan 1956); among the new experimental methods it includes the invention of the diffusion chamber (Langsdorf 1939), of the nuclear reactor (Fermi 1942), of the principle of phase stability in high energy accelerators (Veksler; McMillan 1944), of radiochemical methods, including the ^{37}Cl - ^{37}A method, for detecting neutrino (Pontecorvo 1946), of the scintillation counter (Kallman 1947), of the Cerenkov counter (Jelley 1950), of the bubble chamber (Glaser 1952). A few episodes in this period occupy a central place in my talk.

§ 4. Third period (1941-1959): the youth of neutrino physics. - It is extending from the observation and investigation of neutrino processes other than the beta decay and from the conception of the notion of weak processes to the discovery of P and C violation, the V-A theory and the birth of high energy neutrino physics. It is difficult to mention here all the most significant contributions, and only events connected directly with neutrino properties are being considered. The third period includes a number of cosmic ray experiments, such as the direct proof of the muon decay and the measurement of its mean life (Rasetti; Auger et al. 1941), the discovery that the muon is not an hadron (Conversi, Pancini, Piccioni 1947), the discovery of the pion and the π-μ decay (Lattes, Occhialini and Powell 1947), the observation that the $\mu \rightarrow e + \gamma$ decay does not take place (Hincks and Pontecorvo; Sard and Althaus; Piccioni 1948), that in the muon decay three particles are emitted, the charged one being an electron (Hincks and Pontecorvo; Steinberger; Anderson et al.; Jdanov 1949) and other experimental results such as the observation of artificial pions at the Berkeley phasotron (Gardner and Lattes 1948), the observation of τ and θ modes in the kaon decay (Whitehead et al.; Barkas et al.; Dalitz et al.; Harris et al.; Fitch et al. 1956), the discovery of P and C violation in the ^{60}Co decay (Wu et al. 1957), in the pion and muon decays (Garwin et al. 1957), electron-neutrino angular correlation in beta decay (^{35}A, ^{6}He) finally found in agreement with the V-A theory (Hermannsfelt et al. 1957), the $\pi \rightarrow e + \nu$ process finally observed with a probability in agreement with the V-A theory (Fazzini et al.; Schwartz. Steinberger et al. 1958). the demonstration that neutrinos are left-handed (Goldhaber 1958), the introduction of the spark chamber (Fukuni, Miyamoto 1959) and a proposal which opened a new field in weak interaction physics - the use of high energy neutrino beams from π-μ and other decays (Pontecorvo; Markov; Schwartz 1959); as far as theory is concerned, the period under consideration includes the conception of the deep analogy between the electron and the muon and the notion of weak processes (Pontecorvo 1947; Klein; Puppi 1948), the "two meson" prediction (Marschak and Bethe 1947), the introduction of the ρ parameter for the description of the $\mu \rightarrow e\nu\nu'$ decay (Michel 1950), the discussion of possible parity violation in weak interaction (Lee and Yang 1956), PC invariance (Landau; Lee and Yang 1957), longitudinal neutrinos (Landau; Lee and Yang; Salam; Sakurai 1957), the V-A universal weak interaction (Marshak and Sudarshan; Gell-Mann and Feynman 1958), neutrino oscillations (Pontecorvo 1957), the suggestion that ^{8}B is a source of relatively high energy solar neutrinos (Fowler 1958), the "Kiev symmetry" or the "prequark" lepton-hadron symmetry (Gamba et al.1959), the neutrino emission from hot stars due to the Fermi interaction (Pontecorvo 1959).

Notice that the average number N of authors in a typical experimental investigation is still < 5. In the subsequent, fourth period, which might be called the period of mature neutrino physics, $N > 10$! A number of episodes of the third period occupy a central place in my talk.

§ 5. Fourth period (1960-): the maturity of neutrino physics. - It is extending from the discovery of two types of neutrinos to the discovery of neutral currents, of tau leptons, the weak decays of charmed particles, etc., the theory of electro-weak interactions and ... GUT.

I shall not touch upon this period, because it is starting in the sixties. Notice that the periods considered sooner do differ from the "period of maturity" by an additional circumstance: a given result or experiment being planned etc. nowadays is associated usually with a given facility (let us say CERN-Gargamelle, Fermilab-HPWF, Serpukhov -SKAT ...) rather than with the surname(s) of the author(s).

A comparison of the various periods indicates an amazingly fast growth of neutrino physics which, together with its far reaching ramifications in the field of astrophysics and cosmology, is today a definitely quantitative science, healthy and powerful, and yet leaving lots of room for qualitative surprises.

§ 6. Pauli: a giant. - It is difficult to find a case where the word "intuition" characterises a human achievement better than in the case of the neutrino invention by Pauli.

First, 50 years ago there were known only two "elementary" particles, the electron and the proton, and the very idea that for the understanding of things the existence of a new particle becomes imperative was in itself a revolutionary conception. What a difference from the present day situation, when at the slightest provocation lots of people are ready to invent any number of particles!

Second, the invented particle, the neutrino, should have quite exotic properties, especially an enormous penetrating power. True, Pauli at the beginning did not recognize fully such unescapable implications of his idea and modestly conceeded that the neutrino may have a penetrating power about equal or ten times larger than a quantum. Incidentally, a dimensional thermodynamical argument, showing that neutrinos of energy ~ 1 MeV or wave length λ must have an astronomically large mean free path, let's say equal to a thickness of water milliard of times greater than the Earth-Sun distance, was first given by Bethe and Peierls /1/ who considered the two inverse processes (I am using modern notations): $Z \rightarrow (Z+1)+e^-+\tilde{\nu}_e$ (this is a beta process taking place with a characteristic time T) and the inverse reaction $\tilde{\nu}_e+(Z+1) \rightarrow Z+e^+$, characterized at the mentioned neutrino energy by a cross section :

$$\sigma \leqslant \lambda^2 \cdot \frac{1}{T} \cdot \frac{\lambda}{c}$$

The argument, which today is self-evident (almost all good arguments look obvious "a posteriori") made a deep impression upon me. I did not forget it many years later, when I suggested how free neutrino experiments might be performed with the help of reactors /2/.

Third, the neutrino, because of its fantastic penetrating power appeared first as a particle which, as it were, cannot be revealed in the free state, and on the existence of which you can judge on the basis of the laws of energy and moment conservations, by detecting the nuclear recoils in beta decay, that is with the help of a method which today is quite currently used in searches for neutral particles - the so-called "missing mass" method. Experiments of this type were suggested by Pauli and the first of these was performed in Cambridge by Leipunsky /3/. Here I would like to underline that 50 years ago there was known only one process involving the neutrino, the beta decay of heavy nuclei, which is a 3-particle process in the final state. Extremely important experiments of Ellis and others showed that the average energy (measured in a calorimeter) of the beta rays is equal to the average energy of the beta spectrum, measured in a magnetic spectrometer. This clue, together with the notion that there is a maximum energy of β rays, was certainly not missed by Pauli. All the other processes in which, as we know now, neutrino take part, were not known at the time. Among these several two-particle decays from charged particles stopping in a track detector ($\pi^+ \rightarrow \mu^+ + \nu_\mu$; $\mu^- + {}^3He \rightarrow {}^3H + \nu_\mu$ ...) leave behind beautiful signatures, since the emitted charged particle has always the same momentum, of course equal to that of the invisible neutrino. Examples of these processes are well known today. If in the time previous to the Pauli hypothesis such a two-particle events had been discovered, there would not have been the need of Pauli genious to invent the neutrino. However, I would like to mention here that, at the time, Bohr thought that the continuous beta spectrum might arise from energy non-conservation in individual processes, so that, strictly speaking, in order to solve the dilemma neutrino versus energy non-conservation, one may not be allowed in principle to make use of conservation laws.

Some more words on the Pauli invention, about which he wrote himself a few tens of years after his famous proposal, which, incidentally, was never published in a scientific periodical. Maybe not all of you know that the first idea on the existence of the neutrino appeared in a letter /4/ to a group of specialists in radioactivity, who were to meet in Tubingen, the letter starting with these words: "Dear radioactive ladies and gentlemen". At this meeting Pauli was not present because he was expecting much more from a ball which he wished to attend in Zurich, the night of December 6, 1930. But in that letter there were not only jokes. There are two ideas that only a man of great intuition could have. These ideas I will formulate in the today and the Pauli terminology.

1) In the nuclei there must exist electrically neutral particles, neutrons (Pauli also called them neutrons) having spin 1/2.

2) In the beta decay together with the electron there must be emitted a neutral particle, the neutrino (Pauli called it neutron), so that the total energy of the electron, neutrino and recoil nucleus is definite, as it should be.

Thus Pauli "invented" two particles at the same time and both were very necessary (keep in mind, among other things *) the so-called nitrogen catastrophe, that is the proof, given in the classical spectroscopic investigations of Rasetti, that nuclei ${}^{14}N$ obey the Bose statistics, so that they can hardly consist of protons and electrons only). Pauli for a time thought he had invented only one particle, and not two, because mistakenly he considered them to be identical. Soon, however, he understood his error, namely, in the first official publication /5/ about the neutrino (so it was called by Fermi) at the 1933 Solvay Congress. The subsequent colossal step was done by Fermi.

§ 7. Fermi: one more giant. - Fermi got acquainted with Pauli hypothesis in Rome at an International Conference of Nuclear Physics (1931), where the β decay problem was discussed. There Bohr talked in favour of energy non-conservation. Fermi was quite impressed by the Pauli particle, which he started to call "neutrino". Fermi evidently was already thinking deeply about the problem at the time of the Solvay Congress; his famous paper "A Tentative Theory of beta Decay" /6/ appeared only 2 months after the end of such Congress (1933). This is a quantitative theory, which had a great influence on the development of physics. Without any doubt the idea on the existence of the neutrino would have remained a vague notion without Fermi's contribution. This theory amazingly resisted almost without change until the Glashow-Weinberg-Salam synthesis and underwent only relatively small, although quite important and numerous additions. I feel

---

*) Details on the theoretical thinking (Rutherford, Pauli and especially Majorana) about the neutron before its experimental discovery by Chadwick are most interesting, but I have not the possibility to discuss them here. I shall mention only that after having read the famous Curie-Joliot paper about the projection of protons by the radiation from a Po + Be sources, Majorana noticed that obviously there was evidence in favour of "neutral protons" (that is: neutrons).

quite confident that, had been Fermi alive, he would have made himself at least most of the additions, under the pressure of new experimental facts, about some of which I will talk later.

I would like now to say some curious facts about the appearing of the theory, facts, which I have seen with my eyes, since in that period I was working in Rome.

1) The Journal "Nature" refused the paper of Fermi, because it appeared too abstract to be of interest for the readers. I am sure the editor has regretted such episode for all his life.

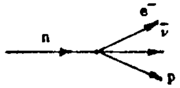
2) The second curious thing has to do with the difficulties Fermi encountered. Such difficulties were not mathematical, but physical. The necessary mathematics, the secondary quantisation, he learned quickly, but the most serious difficulty was to recognize the fact that the electron and the neutrino are created when a neutron transforms into a proton. Of course, this is a thing that every student knows today: elementary particle interactions are explained by the exchange of elementary particles. This is quantum field theory and is an unescapable consequence of the quantum theory and of the theory of relativity. Particles are created and destroyed. This was the difficult point for Fermi. Pauli, in spite of its pioneer work in quantum electrodynamics, did not formulate clearly this point in the beta decay case. If you read the famous Fermi article on β decay, you see how he worked making an analogy with the Dirac quantum theory of radiation (photons are created and destroyed!) and how by analogy he selected the V variant of the β decay.

I still remember his words: when the excited Na atom emits the 5890 A line, the photon is not sitting in the atom (it is created); similarly the electron and the neutrino are created when a neutron is changing into a proton.

Here I should say that at about the same time and independently of Fermi, Perrin /7/ solved the same conceptual difficulties which I have just mentioned. Perrin also made conclusions about the neutrino mass identical to those of Fermi and very modern indeed, in the sense that Perrin and Fermi talked both of the neutrino mass question (a paramount question today!) in an absolutely undogmatic way and pointed out that the neutrino mass, if finite, could be determined by measuring beta decay spectra near the end point. In the most favored case (^3H beta decay) such experiments were initiated in the forties /8,9/. The results of this type of measurements in the eighties are expected with great excitement by the entire community of physicists, following a most interesting recent paper by V.Lyubimov, Tretjakov which claimed a definite finite value of the neutrino mass. Let us come back to the beta decay theory.

At a variance with the electromagnetic interaction (through the exchange of a photon) Fermi assumed that the two currents, the heavy particle (n,p) and the light particle (e,ν) currents have a contact interaction

$$k\bar{\psi}_p\gamma_\mu\psi_n\bar{\psi}_e\gamma_\mu\psi_\nu$$



where k is a constant of the order of 10^{-49} erg cm^3 (today we all know that $k = G/\sqrt{2}$, where $G = 10^{-5}/M_p^2$ is the Fermi constant, $h = c = 1$), $\bar{\Psi}_p$, Ψ_n are the creation operator of the proton and the destruction operator of the neutron, etc. Fermi assumed that weak currents, as we call them now, are four-vectors, as in electrodynamics. At the beginning, Fermi felt that the nucleon weak current $\bar{\Psi}_p\gamma_\mu\Psi_n$ is analogous to the electromagnetic current $\bar{\Psi}_p\gamma_\mu\Psi_p$ and that the lepton weak current $\bar{\Psi}_e\gamma_\mu\Psi_\nu$ is analogous to the electro-magnetic field. However, in his formulation "the heavy particle", (in Fermi's words) and "the light particle" currents, as a matter of fact, are on identical foot. Thus Fermi created its perfect building starting from a few experimental results on the beta decay of heavy nuclei, especially RaE and from an analogy with Dirac theory of radiation.

I would like to underline here that our knowledge since that time has increased tremendously; however (almost) all the new things fit wonderfully into the Fermi picture.

§ 8. Majorana. - In 1937 Majorana raised a most important problem in neutrino physics and, in general, in elementary particle physics: the problem about the true neutrality of electrically neutral fermions. The question at issue is that of the Majorana neutrino (and neutron!).

I feel now that a few introductory words are in place about a third giant - Ettore Majorana, whose personality should be of great interest not only to physicists but also to writers.

When I joined as a third year student the Physical Institute of the Royal University of Rome (1931) Majorana, at the time 25 years old, was already quite famous within the community of a few italian physicists and foreign scientists who were spending some time in Rome to work under Fermi. The fame reflected first of all the deep respect and admiration for him of Fermi, of whom I remember exactly these words: "once a physical question has been posed, no man in the world is capable of answering it better and faster than Majorana". According to the joking lexicon used in the Rome Laboratory, the physicists, pretending to be associated within a religious order, nicknamed the infallible Fermi as the Pope and the intimidating Majorana as the Great Inquisitor. At seminars he was usually silent but occasionally made sarcastic and paradoxal comments, always to the point. Majorana was permanently unhappy with himself (and not only with himself!). He was a pessimist, but had a very acute sense of humour. It is difficult to imagine persons as different in character as Fermi and Majorana. Whereas Fermi was a very simple man (with a small reservation: he was a genious!) who considered ordinary common sense to be a very positive human quality (which he was certainly well provided with!), Majorana was conditioned by complicated and absolutely non trivial living rules. Starting from 1934 he met with other physicists and frequented the Laboratory more and more seldom. In 1938 he literally disappeared. Probably he committed suicide, but there is no absolute certainty about this point. He was quite rich and I cannot avoid thinking that his life might not have finished so tragically, should he have been obliged to work for a living. Thus the scientific activity of Majorana lasted less than ten years (1928-1937). For this reason, and also because he did not like to publish the results of all the investigations he had made, Majorana's contribution to science is much less than it could have been. The publication of the famous paper /10/ relevant to neutrino physics, for example, was prompted by a fortunate circumstance. In 1937 Majorana decided to take part in a competition for an university chair. He just wrote the paper at issue in order to increase his chance to get the chair! Had it not been for such an occasion, the paper probably would never have appeared in print.

Incidentally Majorana was a close friend of E.Amaldi, to whom we owe the publication of Majorana's collected papers, a most interesting book /11/ on his life and work, and a number of articles in which he (E.A.) has been fighting successfully against deformations of the great figures of Majorana and Fermi. Now I am coming back to physics.

In the late fifties and in the sixties the opinion was frequently expressed that neutrinos a'la Majorana, although beautiful and interesting objects, are not realised in nature. It is certainly not possible to agree today with such an opinion. On the contrary, the question raised by Majorana has become more and more important and nowaday is, in fact, the central problem in neutrino physics.

The paper /10/ is the last original one written by Majorana. I wish to cover only the main physical and qualitative aspects of the paper which has anticipated the times by some forthy years and I shall not touch upon its very important formal aspects. Maybe the best to do is to translate in english the summary, the introduction and a few more phrases of the paper, which as far as I know, was written only in italian.

Symmetrical theory of the electron and the positron

E.Majorana, Nuovo Cimento, 5, 171-184, 1937

Summary. - The possibility is demonstrated of reaching a full formal symmetrization of the quantum theory of the electron and the positron using a new quantization process. This is modifying somewhat the meaning of the Dirac equations in the sense that there are no more reasons either to talk about negative energy states or to presume the existence of "antiparticles" corresponding to negative energy "holes" for new types of particles, especially neutral ones.

The interpretation of the so called "negative energy states" proposed by Dirac (P.A.Dirac, Proc.Camb.Phil.Soc. 30, 150, 1924. See also W.Heisenberg, Z.Physik 90, 209 (1934)), as it is well known, leads to a description essentially symmetrical of electrons and positrons. The essential symmetry of the formalism is precisely due to the circumstance that the theory yields results indeed symmetrical as far as the convergence difficulties can be avoided. However the artificial ways which have been suggested in order to give the theory a symmetrical form in agreement with its content are not entirely satisfactory either because the starting approach is always asymmetrical or because the symmetrization is obtained later through methods which should be avoided (such as the cancellation of infinite constants). Thus we have tried a new way which leads more directly to the desired aim.

As far as electrons and positrons are concerned, we should expect from the theory only a formal improvement; however in our opinion it is important (for possible extensions of the theory) that the very notion of negative energy states disappears. As a matter of fact we shall see that it is perfectly possible to construct in a very natural way a theory of neutral particles without negative states.

From the first paragraph I wish to quote the following words: ... "It (that is the new proposed method of quantization B.P.) is of importance especially for Fermi fields, whereas for the electromagnetic field simplicity suggests that nothing must be added to old methods. Incidentally we shall not face the systematic study of the logical possibilities offered by our new point of view and limit ourselves to the description of the process of quantization which, as it seems, is of importance for actual applications; it appears to be a generalization of the Jordan-Wigner Method (P.Jordan and E.Wigner Z.Physik 47, 631 (1928)) and allows not only to give a symmetrical form to the electron-positron theory, but also to construct an essentially new theory for particles without electrical charge (neutrons and hypothetical neutrinos). Although it is perhaps not possible now to ask to the experiment a choice between the new theory and that in which the Dirac equations are simply extended to neutral particles, one should keep in mind that the new theory is introducing in this unexplored field a smaller number of hypothetical entities."...

From the second paragraph: "... The advantage of this method (that is the theory of Majorana. B.P.) over the elementary interpretation of Dirac equations, as we shall see better below, is that there is no more any reason to assume the existence of antineutrons or antineutrinos. Of the last ones actually the use is made of in the theory of the beta decay with emission of positrons (see G.C. Wick, Rend.Accad.Lincei 21, 170, 1935), but such theory, obviously, can be modified in such a way that the emission of a positron as well as an electron is always accompanied by the emission of a neutrino..."

For the benefit of the young reader who from the biginning of his activity has been used to hear not only about electric charges but also about other types of "charge" (baryon, lepton...) I would like to underline that in 1937 only the notion of electric charge was known . Now Majorana first invented explicitly truly neutral fermions or Majorana particles, that is fermions which are identical to their own antiparticles. Majorana particles are called by him "two-component" (one particle with two spin orientations), the Dirac particles being four-component ones (particle and antiparticle, each with two spin orientations). Majorana considered "material" particles (with finite rest mass). Second Majorana, putting the question about an electrically neutral fermion being described either by his theory or by the Dirac theory, implicitely introduced the notion of charges other than electrical.Majorana particles are fermions which have neither electrical nor any other charges. Electrically neutral fermions which are not Majorana particles are described by the Dirac theory, are not truly neutral and have a (non electric) charge. Notice that explicitly the notion of baryon and lepton charges were introduced only in 1949 /12/ and 1953! /13/.

From one phrase of Majorana I quoted above it is seen that he had in mind definitely the question as to whether the Majorana versus Dirac nature of a fermion can be established by modern (1937!) experiments. Concerning this question, I shall consider first the case of neutrinos, ignoring now two very important circumstances that Majorana then could not have in mind: a) the neutrino longitudinal polarization /14/ connected with parity non conservation (1957) and b) the possibility of small violations of (lepton) charge conservation and of the related possible existence (1958) of non-stationary neutrino states (oscillations) /15/ (in modern terminology, weak interaction eigenstates are not necessarily mass eigenstates). As it can be guessed from one of the above quotations, Majorana probably thought about experiments which in principle might answer the following question: are neutral leptons emitted, say, together with negative beta rays, capable of being absorbed by nuclei with the emission, again, of negative electrons? I think that probably he did not mention explicitely such a possibility because at the time detecting neutrinos was unfortunately and wrongly considered neither a serious proposal nor even a decent argument of conversation (the expected cross section being ridiculously small!).

I personally was faced with the Majorana neutrino - Dirac neutrino dilemma more than once and each time for long periods. The first time when I proposed and developed /2/ the Cl-A method of detecting neutrinos, the second time when I invented possible neutrino oscillations /15/ (about these episodes I shall talk below in other paragraphs) and again in the sixties, seventies and the eighties in connection with the theory of oscillations and double beta decay. Racah almost immediately /16/ reacted to the Majorana paper and was the first to write clearly about the idea mentioned above on the different inverse beta decay behaviour of Dirac and Majorana neutrinos.

Because uranium reactors and methods of detecting neutrinos had not yet come into being at the time of the Racah paper, this had no direct influence upon the development of experiments with free neutrinos. However it should be mentioned that the theoretical interpretation of the "negative" result in the successful reactor Cl-A-experiment of Davis /17/ rested at the time on an idea first expressed by Racah. At first view the result of Davis, that antineutrinos from reactors are not able to be absorbed with the emission of negative electrons, can be interpreted (and so it was) as a demonstration of the Dirac nature of neutrinos, if you wish, as a demonstration of the existence of a (non electrical) neutrino charge. However, as it is known now, this interpretation is premature, because of the important circumstances a) and b) mentioned above. Two words about this at the end of this paragraph.

Let us come back to the Majorana idea. In 1938 a paper of Furry appeared /18/, which looks to me as a typical "incubation" paper. It was stimulated by the Majorana and Racah thinking, and does not contain very important new results. However it is describing in detail the line of thought of Racah about possible nuclear reactions induced by Majorana and Dirac neutrinos, is quite pessimist about the possibility of solving the dilemma Dirac neutrino-Majorana neutrino experimentally and is obviously the fore-runner of the following, most clever and important paper of Furry /19/, where the neutrinoless double β decay is first considered. In neutrinoless double β decay the neutral lepton virtually emitted together with a negative electron by a neutron, must be absorbed by a second neutron with the emission of a second negative electron. The "Racah chain" is present here but the idea of the experiment is new and very subtile in this case. The search for neutrinoless double beta decay nowadays even more than in the past is a very important tool and may answer the question related to the neutrino (Majorana or Dirac) nature. Neutrinoless double beta decay has not yet been observed: brave important experiments have been performed and are performed now in order to search for it. An observation of neutrinoless double β decay would definitely imply a Majorana nature of the neutrinos described by stationary states. A negative result in the search for neutrinoless double β decay is not easy to interpret because of the circumstances a) and b) mentioned above in this paragraph. Here may be it is worth to underline that negative results in experiments of the Cl-A type in a reactor and especially in the search for neutrinoless double β decay have already shown that the helicity of neutrinos (playing the role of lepton charge) is almost perfect, <u>if</u> not absolutely perfect *). Were it not for such helicity the probability of double β decay would be larger by..., but in italian there is such a proverb: "<u>if</u> my grandmother had wheels, she would be a car". Let us return to Majorana and consider also the case of neutrons.

It is amazing how much is implied, explicitly, or implicitly, in his famous paper. I have already stressed that there one can either see or see between the lines electrically neutral fermions both without any charge and with some charge (lepton, baryon ...). True, implicitly all charges are supposed to be structly conserved, but this is not stated in words. Now we know that among bosons there may be "hybrid particles", that is bosons having a charge which is not strictly conserved /20/ and oscillating between two different states like neutral kaons. If there exist such electrically neutral hybrids among fermions /15/, we would expect that they are not described by stationary states; that they oscillate one into another and that they are superpositions of particles with definite, different masses, which are described by stationary states and are truly neutral (or Majorana) fermions. Now let me joke for a minute and you will see where I am driving to: the Majorana neutrons and neutrinos described in the 1937 paper prophetically anticipate the modern fresh GUT wind, with neutrino finite masses, neutrino and neutron oscillations nucleon decay and all that!

---

*) For the sake of clearity I would like to underline here that the "phenomenological" neutrino and antineutrino beams, the very words and notations ν, $\tilde{\nu}$ with which every experimentalist is used to deal, are bound to remain in physics for a long time even if the Majorana point of view is the correct one.

§ 9. <u>The ^{37}Cl-^{37}A method</u>.- I would like now to give a subjective account of a few pages in the development of neutrino physics, in which in some ways I was involved. In 1946 neutrinos were generally considered undetectable particles. Many respectable physicists were of the opinion that the very question about detecting free neutrinos was nonsense (not only because of temporary difficulties), just as nonsense is the question as to whether the pression in a vessel is or is not, say, less than 10^{-50} atmospheres. I remembered well the Bethe-Peierls /1/ argument and it occurred to me at the time that the appearance of powerful nuclear reactors made free neutrino detecting a perfectly decent occupation. I was living in Canada then and was well acquainted with reactor physics. The NRX Canadian reactor, in the design of which I was taking part, was not working yet, but it was clear to me that under the very compact shield, where the cosmic ray soft component was considerably weakened, one might dispose of a neutrino flux $\sim 10^{12}$ cm^{-2}sec^{-1}. At the time, scintillators, which were so successfully used many years later by Reines and Cowan /21/ to detect free reactor antineutrinos, had not yet been invented. Well, it occurred to me that the problem could be solved by radiochemical methods, that is, by concentrating chemically the isotope resulting from the inverse beta process from a very large mass of matter irradiated by neutrinos /2/. A careful inspection of the famous Seaborg table of artificial radioisotopes indicated a few possible target candidates, by far the best of which was a chlorine compound, the reaction at issue being:

$$\text{neutrino} + {}^{37}\text{Cl} \rightarrow {}^{37}\text{A} + e^- , \qquad (1)$$

where ^{37}A decays by K-capture with the liberation of 2.8 keV energy in the form of X-rays and Auger electrons. I wrote here "neutrino" and not $\tilde{\nu}_e$ because at the time the question as to whether $\nu \neq \tilde{\nu}$ was not clear *). Now there are lots of practical reasons why ^{37}Cl is so good and I shall not list them here. One of them, however, was not known to me "a priori" and was discovered by chance. In order to experiment on the future neutrino detector, at Chalk River we were preparing conventionally in a reactor ^{37}A, and putting it inside a detector, which, according to our intentions, was supposed to be and in fact was, a Geiger-Muller counter. Well, once, looking at an oscilloscope connected to the counter, we saw plenty of pulses from ^{37}A about equal in amplitude at voltages on the counter much lower than the Geiger threshold, and discovered /22/ (independently of Curran et al. in Glasgow) the high gas gain (up to 10^6) proportional regime. Now this was very important, of course, from the point of view of detecting neutrinos, since it permits to decrease the effective background of the counter. At the time there was a sort of dogma about proportional counters, i.e., that they cannot work at multiplication factors larger than ~ 100, which is true of course, if you have a large input ionization (alpha particles, etc,), but is absurd if you

---

*) The question is still unclear now (1982), but at a different level! Today the "phenomenological" answer, is, of course that $\nu \neq \tilde{\nu}$, in the sense that the neutral lepton emitted in β^--decay together with the electron has an helicity different from that of the neutral lepton emitted together with the positron in β^+ decay. However, as explained in the preceeding paragraph, such an answer does not settle one of the main questions in today neutrino physics: have neutrinos a Majorana mass? in other words, are particles described by mass eigenstates Majorana particles?

you have an input ionization of a few ion pairs.

In my 1946 paper /2/ I already considered as a source of neutral leptons not only a powerful reactor, but also a concentrate of radioelement(s) extracted from a reactor and ... the Sun.

I discussed the ^{37}Cl-^{37}A method with Fermi in Chicago (1948?) and later at the Basel-Como conference in 1949 (including solar possibilities). Fermi was not at all enthusiast about neutrino applications of the method, but liked very much our proportional counters, with the help of which together with Hanna we first observed L-capture (in ^{37}A, $\sim$ 250 eV, $\sim$10 ion pairs) /23/ and measured the ^{3}H spectrum going quite down at the time with the upper limit of the neutrino mass /8/. In retrospect I understand very well Fermi's reaction. As I think that Segre said, Don Quixote was not the hero of Fermi. He could not have sympathy for an experiment which, true, grace to the heroic efforts of R.Davis /17/, terminated very brilliantly, but many many years after its conception.

Now I am coming back to the question as to whether reactor antineutrinos may induce the reaction (1). Well, passing through Zurich sometimes between 1947 and 1948 I had lunch with Preiswerk and Pauli. I told Pauli about my plans with the ^{37}Cl-^{37}A method; he liked very much the general idea and remarked that it was not clear whether "reactor neutrinos" should definitely be effective in producing the reaction (1), but he thought that they probably would (as you see, this is the Majorana point of view). Until 1950 I continued to think about the problem and to test low background proportional counters in that connection and in connection with solar problems. For example I remember that Camerini, who at the time was working in Bristol and was a great specialist in cosmic ray stars, helped me to calculate the cosmic ray background in various Cl-A experiments which I was planning to do. Anyway the effective background of my counters was sufficiently low to detect solar neutrinos through ^{37}A decay,as now it may be seen from recent successful solar experiments of R.Davis. Since 1950 I stopped experimenting on the problem because I happened to work in an accelerator Laboratory (and not in a reactor Laboratory) and also as there was no site deep underground enough in the USSR for a solar experiment (however at the Elbrus neutrino observatory such a site will be soon available). However, I kept thinking about counters (... and the Sun) and when I had the privilege to meet R.Davis at the first Neutrino Conference in Moscow (1968), I 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 bavkground in its solar experiment. As I found out later from him at the ν'72 conference in Hungary it works really that way.

As far as the interpretation of solar neutrino experiments is concerned, I extensively investigated the importance of possible neutrino oscillations in solar neutrino astronomy even before the first results of R.Davis had been obtained, that is before the so-called "solar neutrino puzzle" came into being. I would like to talk about this but it is a story too recent to be told at our Colloquium.

§ 10. The muon properties and the notion of weak interactions. - Many physicists do not know that, after the discovery of radioactivity, it took a period of about fifty years for the notion of weak interaction to be conceived and universally recognized. About a short phase of this period, which is related to the development of our knowledge of muon properties,below there are given some recollections, beginning with the famous experiment of Conversi, Pancini and Piccioni /24/.

About this experiment I heard while working in Canada. Until 1947 cosmic ray physics for me was a quite remote field some knowledge of

which I had acquired from my friends in Florence (Bernardini and Occhialini), in Paris (P.Ehrenfest Jr., a very promising experimentalist, working in the cosmic ray Auger team, who prematurely lost his life in a mountain accident),in Montreal (Rasetti, one of my teachers, who in Quebec first measured directly the mean life of the "mesotron", and Auger, who did the same measurement together with Maze, and under whom I was working in Canada during the war).

Now as soon as I read the Conversi et al. paper and the considerations of Fermi et al. /25/ related to it, I became fascinated by the particle that we call now the muon. That was indeed an intriging 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 I 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 charge 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?

To the questions which were related to the muon capture I replied /26/ almost immediately and, as it turned out, correctly, moving from the remark that the rate of (nuclear) electron capture and that of muon capture are quite close (when proper account is taken of the different electron and muon orbit volumes). The answers were: 1) the muon capture must be a process practically identical to the beta process proceeding according to the reactions*) $\mu^- + p =$ neutrino + n ; 2) in the muon capture most of the released energy is "invisible", because it is carryed away in the form of neutrinos, a conjecture which was supported by experiments and agrees with 1); 3) the muon spin must be 1/2.

A very difficult point to explain for me was: how the muons are copiously produced in cosmic rays? I felt sure that the muon is a fermion. A fermion cannot be produced singly. Muon-neutrino pairs cannot be produced copiously because of my main conclusion, that the muon-neutrino coupling to nuclei is weak. I had to invoke a muon pair theory of nuclear forces by Marshak, which I really did not understand, and missed the point, that is the real muon source. Such a source should have been a muon "pregnant" object, in the vivid and proper expression of Weiskopf /27/, who missed the point too for some reason. The source is, of course, the pion. The right answer was to be given soon by Marshak and Bethe /28/ in their remarkable paper "On the two meson hypothesis", published at about the some time as the epoch-making discovery of the pion and of the π-μ decay by Lattes, Occhialini and Powell /29/.

That the muon and electron nuclear capture processes are very closely related, i.e. that they are both "weak processes", was absolutely clear to me at the time /26/ and then to a few other physicists /30/. Such electron-muon symmetry was the first hint of an universal weak interaction (but how far away still from the 1958 form of such interaction, the V-A theory of Marshak-Sudarshan and Feynman-Gell-Mann /31/ implemented later with the Cabibbo hadron mixing!).

As far as the questions related to the muon decay are concerned, they could be answered only by performing experiments to the point. I became actively interested in cosmic ray physics, quickly read and digested a very good concise booklet on cosmic rays edited by Heisenberg /32/, a sort of vade-mecum for beginners. Together with T.Hincks, a wonderful physicist gifted of an acute sense of humour,

---

*) It took 15 years before the reactions $\mu^- + p \rightarrow n + \nu_\mu$, $\mu^- + {}^3He \rightarrow {}^3H + \nu_\mu$ were directly observed in the experiments of R.Hildebrand and in our own experiments (together with Sulyaev et al.).

we started a very friendly, unforgettable and stimulating experimental collaboration. We prepared in a short time an experimental set up, which, for the time, was relatively complicated. Prompt and delayed concidence techniques were used and of course the particle detectors were Geiger counters. 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 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 $\mu \rightarrow e\gamma$ decay! Incidentally the money spent for all our cosmic muon research in Canada was infinitesimal in comparison with that which is spent today in a typical high energy experiment running for only a few hours.

We found out 1) that the decay $\mu \rightarrow e+\gamma$ does not take place (searching for electron photon delayed coincidences) /33/; 2) that in the muon decay 3 particles are emitted; $\mu \rightarrow e + \nu + \nu'$ (measuring the electron spectrum by the absorption method) /34/; 3) that the charged particle emitted in the muon decay is indeed an electron (measuring the intensity of its bremstrahlung radiation) /34/. The first result was obtained /35/ also independently by other groups and so was the second one /36/. The third result has been obtained by our group only. It is the one which took up most of our effort and ingenuity, and yet, it is probably the less significant from today point of view: what else could be the muon decay charged particle if not an electron? But one should have in mind the severe antidogmatism which was well in place at the time. Incidentally the pragmatic atmosphere we were breathing can be recognized also in the title of one of our papers:"On the stability of the neutral meson". In the investigation /37/ we demonstrated that in the muon decay either a neutral meson,hypothesised at the time, is not emitted or, if it is emitted, it does not decay into two photons with a mean life $\lesssim 10^{-10}$sec.

In concluding this limited and subjective recollections of some of the early muon investigations, I must mention here a theoretical investigation which was and is still today of great importance: the introduction of the Michel parameter ρ in the muon decay /38/, more generally, the description by Michel of processes in which two real neutral leptons are participating.

Well, we have seen that the observation of neutrinoless double β decay (a process in which two virtual neutrinos participate) would show that the neutrino has a Majorana mass. Now the Michel ideology, in a successful experiment with real neutrinos, might permit to conclude that two neutral leptons are of the Dirac type. This has been underlined by S.P.Rosen for the case of neutrino-electron scattering (so recently, that I do not feel entitled to quote him according to the rules of our Colloquium).

With the advent of the first relativistic accelerators, pions and muons were produced artificially. In the fifties their propertues started to be studied in conditions uncomparaibly more favourable than before, but now I am not going to tell this story, which culminated with the great theoretical /14/ and experimental /39/ discoveries of the neutrino helicity.

§11.High energy neutrino physics.-My story here is again very personal. Of course the story would sound quite different if it were told by either Markov or Schwartz.I am going to tell you how I came to propose experiments with high energy neutrinos from meson factories and from very high energy accelerators.At the Laboratory of Nuclear Problems of the JINR in 1958 a proton relativistic cyclotron was being designed with a beam energy 800 MeV and a beam current $\sim 500\ \mu A$. By the way, this accelerator eventually was not built. Anyway at the beginning of 1959 I started to think about the experimental research program for such an accelerator. First, it occurred to me that neutrino investigations at accelerator facilities are perfectly feasible and that a healthy and relatively cheap neutrino program could be accomplished by dumping the proton beam in a large Fe block, fulfilling at the same time the function of neutrino source and shield. I would say that the ideology of the LAMPF accelerator neutrino experiments which have been initiated recently is very similar to that of various experiments planned 20 years before for an accelerator which was not built. About one of them, which was intended to clear up the question as to whether $\nu_e \neq \nu_\mu$ I would like to say a few words.

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 bremstrahlung) 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 /40/ clearly enough (the use of muon neutrino beams). At the 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 numbers of electrons and muons produced correspondingly.

In 1959 another problem was of great importance; is the four-fermion interaction a contact interaction or is it due to the exchange of an intermediated boson? This question is still valid today, but now we have the Glashow, Salam, Weinberg theory, which predicts masses of intermediated mesons at about 100 GeV, whereas in 1959 the intermediated boson (without serious reasons) was supposed to have a mass of a few GeV. Obviously the intermediated boson could not be produced at meson factories and at the 1959 Kiev international conference, Ryndin and I proposed, second, to look for the boson making use of neutrino beams from very high energy accelerators /41/. The theoretical idea in the proposal was that in the cross section for the production by neutrinos of the intermediate boson at sufficiently high energies there will appear G instead of G^2. As you know, the question about intermediate bosons is not going to be solved anymore in neutrino experiments (as it seems). The question about two types of neutrinos has been solved at Brookhaven in a beautiful experiment (1962) by Lederman, Schwartz, Steinberger et al.

§ 12. Conclusions. - Below there are listed some of the main problems, of today neutrino physics. The questions are, of course, connected one to another.

1) Are neutrino masses finite?
2) Are all the neutral leptons much lighter than electrons?

3) If the neutrino masses are finite have they all Majorana masses (in which case there are no lepton charges)? or have they all Dirac masses (in which case there exist strictly conserved lepton charges)? May be some neutrinos have Majorana masses, other neutrinos have Dirac masses?

4) Does neutrinoless double decay take place?

5) Do neutrino oscillations take place?

6) How many neutrino types there are?

All these questions have been put since a long time, many of them by Pauli, Fermi, Perrin and Majorana; yet it does not seem probably that there will be definite answers to such questions in the immediate future, although neutrino physics is nowaday a big enterprise indeed.

I am grateful to S.Bilenky for useful discussions.

References

Notice that, in the spirit of the Colloquium, no papers published in the sixties or later are quoted here, except publications connected with history of physics.

1. BETHE H. and PEIERLS R., Nature 133 (1934) 689.
2. PONTECORVO B., National Res.Council Canada, Rep. (1946) PD 205; Helv.Phys.Acta.Suppl. 3 (1950) 97.
3. LEIPUNSKY A., Proc.Camb.Phil.Soc. 32 (1936) 301.
4. PAULI W., Letter (December 4, 1930) to a meeting of physicists, among which H.Geiger and L.Meitner, taking place in Tubingen. The letter was conserved by Meitner and its content was discussed since 1930. One can read the letter, for example, in L.BROWN, Physics Today, Sept. 1978, p. 23.
5. PAULI W., "Septieme Conseil de Physique Solvay 1933", Gauthier-Villars, Paris 1934, p. 324.
6. FERMI E. Ricerca Scientifica 2, No 12 (1933); Z.Physik 88 (1934) 161.
7. PERRIN F., C.R. 197 (1933) 1625.
8. HANNA G. and PONTECORVO B., Phys.Rev., 75 (1949) 983.
9. CURRAN S. et al., Phil.Mag. 40 (1949) 53.
10. MAJORANA E. Nuovo Cimento 5 (1937) 171.
11. AMALDI E. La vita e l'opera di Ettore Majorana, Roma, Accademia Nazionale dei Lincei, 1966.
12. WIGNER E. Proc.Am.Phil.Soc. 93 (1949) 521,
    see also STUCKELBERG E., Helv.Phys.Acta. 11 (1939) 225;
    WEYL H., Z.Phys. 56 (1929) 330.
13. MARX G., Acta Phys.Hung. 3 (1953) 56;
    ZEL'DOVICH Ya. DAN SSSR 86 (1952) 505;
    KONOPINSKY E. and MAHMOUD H., Phys.Rev. 92 (1953) 1045.
14. LANDAU L. Nucl.Phys. 3 (1957) 127.
    LEE T., YANG C., Phys.Rev. 105 (1957) 1671; JETP, 32, 407, 1957.
    SALAM A., Nuovo Cimento 5 (1957) 299.
15. PONTECORVO B., JETP 33 (1957) 549; 34 (1958) 247.
16. RACAH G., Nuovo Cimento 14 (1937) 322.
17. DAVIS R., Phys.Rev. 97 (1955) 766.
18. FURRY W., Phys.Rev. 54 (1938) 56.
19. FURRY W., Phys.Rev. 56 (1939) 1184 (Neutrinoless double β decay). The double β decay with the emission of two neutral leptons was considered by GOEPPERT-MAYER M. Phys.Rev. 48 (1935) 512. Early detailed theoretical investigations of neutrinoless double beta decay are presented in ZEL'DOVICH Ya. et al., Usp.Fiz.Nauk 54 (1954) 361; PRIMAKOFF H. and ROSEN S., Rep.Prog.Phys. 12 (1959) 121, where one can find also previous references to experimental work.
20. GELL-MANN M. and PAIS A., Phys.Rev. 100 (1959) 1487.
21. COWAN C., REINES F., HARRISON F., KRUSE H., McGUIRE A., Science 124 (1956) 103.
22. KIRKWOOD D., PONTECORVO B. and HANNA G., Phys.Rev. 74 (1948) 497;
    HANNA G., KIRKWOOD D. and PONTECORVO B., Phys.Rev. 75 (1949) 985;
    PONTECORVO B., Helv.Phys.Acta. ref. /2/.
23. PONTECORVO B., KIRKWOOD D. and HANNA G., Phys.Rev. 75 (1949) 982.
24. CONVERSI M., PANCINI E., PICCIONI O., Phys.Rev. 71 (1947) 209.
25. FERMI E., TELLER E., WEISKOPF V., Phys.Rev. 71 (1947) 314.
26. PONTECORVO B., Phys.Rev. 72 (1947) 246.
27. WEISKOPF V., Phys.Rev. 72 (1947) 510.
28. MARSHAK R., BETHE H., Phys.Rev. 72 (1947) 506.
29. LATTES G., OCCHIALINI G., POWELL C., Nature 160 (1947) 453.
30. KLEIN O., Nature 161 (1948) 897;
    PUPPI G., Nuovo Cimento 5 (1948) 587.

31. MARSHAK R., SUDARSHAN E., Phys.Rev. 109 (1958) 1860;
FEYNMAN R., GELL-MANN M., Phys.Rev. 109 (1958) 193.
32. "Cosmic radiation" edited by HEISENBERG W. (Dover Publications, New York 1946).
33. HINCKS E., PONTECORVO B., Phys.Rev. 73 (1948) 257;
Can.J.Res. 28A (1950) 29.
34. HINCKS E., PONTECORVO B., Phys.Rev. 75 (1949) 698;
77 (1950) 102.
35. SARD R., ALTHAUS E., Phys.Rev. 75 (1948) 1251;
PICCIONI O., Phys.Rev. 74 (1948) 1754.
36. STEINBERGER J., Phys.Rev. 75 (1949) 1136;
LEIGHTON R., ANDERSON C., SERIFF A., Phys.Rev. 75 (1949) 1432.
37. HINCKS E., PONTECORVO B., Phys.Rev. 73 (1948) 1122.
38. MICHEL L., Proc.Roy.Soc.London. Ser. A63 (1950) 514.
39. GARWIN R., LEDERMAN L., WEINRICH M., Phys.Rev. 105 (1957) 1415.
GOLDHABER M., GRODZINS L., SUNYER A., Phys.Rev. 109 (1958) 1015.
WU C., AMBLER E. et al., Phys.Rev. 105 (1957) 1413.
40. PONTECORVO B., JETP 37 (1959) 1751.
41. PONTECORVO B., RYNDIN R., Proc.Kiev Int.High Energy Phys.Conf. (1959) p. 233.

Received by Publishing Department
on May 31 1982.

WILL YOU FILL BLANK SPACES IN YOUR LIBRARY?

You can receive by post the books listed below. Prices - in US $, including the packing and registered postage

D13-11807	Proceedings of the III International Meeting on Proportional and Drift Chambers. Dubna, 1978.	14.00
	Proceedings of the VI All-Union Conference on Charged Particle Accelerators. Dubna, 1978. 2 volumes.	25.00
D1,2-12450	Proceedings of the XII International School on High Energy Physics for Young Scientists. Bulgaria, Primorsko, 1978.	18.00
D-12965	The Proceedings of the International School on the Problems of Charged Particle Accelerators for Young Scientists. Minsk, 1979.	8.00
D11-80-13	The Proceedings of the International Conference on Systems and Techniques of Analytical Computing and Their Applications in Theoretical Physics. Dubna, 1979.	8.00
D4-80-271	The Proceedings of the International Symposium on Few Particle Problems in Nuclear Physics. Dubna, 1979.	8.50
D4-80-385	The Proceedings of the International School on Nuclear Structure. Alushta, 1980.	10.00
	Proceedings of the VII All-Union Conference on Charged Particle Accelerators. Dubna, 1980. 2 volumes.	25.00
D4-80-572	N.N.Kolesnikov et al. "The Energies and Half-Lives for the α- and β-Decays of Transfermium Elements"	10.00
D2-81-543	Proceedings of the VI International Conference on the Problems of Quantum Field Theory. Alushta, 1981	9.50
D10,11-81-622	Proceedings of the International Meeting on Problems of Mathematical Simulation in Nuclear Physics Researches. Dubna, 1980	9.00
D1,2-81-728	Proceedings of the VI International Seminar on High Energy Physics Problems. Dubna, 1981.	9.50
D17-81-758	Proceedings of the II International Symposium on Selected Problems in Statistical Mechanics. Dubna, 1981.	15.50
D1,2-82-27	Proceedings of the International Symposium on Polarization Phenomena in High Energy Physics. Dubna, 1981.	9.00

Orders for the above-mentioned books can be sent at the address:
Publishing Department, JINR
Head Post Office, P.O.Box 79 101000 Moscow, USSR

SUBJECT CATEGORIES
OF THE JINR PUBLICATIONS

Index	Subject
1.	High energy experimental physics
2.	High energy theoretical physics
3.	Low energy experimental physics
4.	Low energy theoretical physics
5.	Mathematics
6.	Nuclear spectroscopy and radiochemistry
7.	Heavy ion physics
8.	Cryogenics
9.	Accelerators
10.	Automatization of data processing
11.	Computing mathematics and technique
12.	Chemistry
13.	Experimental techniques and methods
14.	Solid state physics. Liquids
15.	Experimental physics of nuclear reactions at low energies
16.	Health physics. Shieldings
17.	Theory of condenced matter
18.	Applied researches
19.	Biophysics

Понтекорво Б. E3-82-414

Детство и молодость физики нейтрино: некоторые воспоминания

Доклад весьма субъективен по характеру и никоим образом не является полным. Он не представляет собой главу из истории физики частиц. Это собрание нескольких коротких рассказов, имеющих отношение к физике нейтрино. Первые два из них, о Паули и Ферми, касаются тем, освещенных целым рядом физиков, включая автора, в связи с отмечавшимся недавно пятидесятилетним юбилеем нейтрино. Следующий за ними рассказ о Майоране был освещен значительно менее подробно, во всяком случае на английском языке. За ним следует несколько воспоминаний, очень личного характера, имеющих отношение к экспериментальной и теоретической работе самого автора, связанной с предложением и развитием Cl-A метода детектирования нейтрино, с установлением понятия слабого процесса, а также с предложением нового типа исследований слабых взаимодействий - экспериментов с нейтрино высоких энергий.

Работа выполнена в Лаборатории ядерных проблем ОИЯИ.

Препринт Объединенного института ядерных исследований. Дубна 1982

Pontecorvo B. E3-82-414

The Infancy and Youth of Neutrino Physics: Some Recollections

The talk is quite subjective in character, and is in no way complete. It is not a chapter of history of particle physics. It is a collection of a few short stories related to neutrino physics. Two of these, about Pauli and Fermi, touch on subjects already covered by a number of physicists, including the author, in connection with the recent neutrino's fiftieth birthday. A story about Majorana's work on Majorana's fermions, which is following, has been covered much less extensively, at least in English. There follow a few recollections, very personal indeed, related to the experimental and theoretical work of the author in proposing and developing the Cl-A method of neutrino detection, in establishing the notion of weak processes and in proposing a new type of weak interaction investigations - high energy neutrino experiments.

The investigation has been performed at the Laboratory of Nuclear Problems, JINR.

Preprint of the Joint Institute for Nuclear Research. Dubna 1982