NEUTRINOS TO 1960 - PERSONAL RECOLLECTIONS

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Résumé : Nous donnons d'abord une description des jalons qui conduisirent à la détection du neutrino libre, de l'idée d'utiliser une explosion nucléaire comme source de neutrinos jusqu'à la réalisation détaillée d'une expérience auprès d'un réacteur nucléaire. Sont discutés également la logique conduisant à la recherche de la détection du neutrino loin de sa source ainsi que des développements ultérieurs dans la physique expérimentale du neutrino.

Abstract.- An account is given of the events which led to the detection of the free neutrino starting from the tentative idea to use a nuclear explosion as the neutrino source to the detailed realization of the experiment at a nuclear reactor. The logical requirement for detection of the neutrino at a point remote from its origin is discussed as are some subsequent developments in experimental neutrino physics.

During an event one has a bias born of the hope for discovery, the passions of the moment. Looking backward, events are softened by the patina accumulated through the intervening years and the inevitable distortion occasioned by faulty recall and the tendency to make it appear more prescient, more important than it actually was.

I have been asked to speak about the history of the neutrino to 1960, giving my recollections of the events which led to the detection of that fascinating particle and some subsequent developments.

Ever since the first explosion of a nuclear bomb in New Mexico in the summer of 1945, I was interested in the physics of, and concerned about the consequences of, the unusual phenomena which characterized such an explosion. In addition to the evident blast, thermal radiation, gamma rays and neutrons, nuclear explosions were believed to produce a relatively short intense pulse of neutrinos. But aside from a passing thought regarding neutrinos my attention was directed to these other manifestations.

In 1951 following the tests at Eniwetok Atoll in the Pacific I decided to search for a physics problem which, unlike the weapons developments, was of a more fundamental nature, one on which I could put my stamp.

Accordingly, I approached my boss, Los Alamos Theoretical Division Leader, J. Carson Mark, and asked him for a leave in residence so that I could ponder. He agreed, presumably as a reward for my contributions to the weapons activity, and I moved to an empty office, sans important calls from Washington, secretaries, meeting, and other associated trappings. I sat in that stark office at age 33, staring at a blank pad for several months searching for a meaningful question worthy of a life's work. It was a very difficult time. The months passed and all I could dredge up out of the subconscious was the possible utility of those bombs neutrinos for direct detection. Fortunately Fermi was a summer visitor at Los Alamos, and it occurred to me that he might have some useful comment to make regarding my nascent idea. As I recall the conversation went as follows:

"I have been thinking about detecting neutrinos, and I think the bomb is the best source."

Fermi thought for a minute and said yes that appeared to be so. Then I said it seemed to me that a detector with sensitive mass of a ton or so would be required. He agreed. I then said that I had no idea how to construct such a detector. He allowed that he did not either and that ended the conversation.

Some months later, while stranded in the Kansas City airport, Clyde Cowan and I found ourselves discussing exciting questions in physics. I suggested that the detection of the neutrino was a supreme challenge and perhaps one could use a bomb as a source. He agreed, and we decided to work together, our joint enthusiasm overcoming our ignorance regarding suitable detection techniques.

I have, on occasion, reflected on my good fortune in having found such a stimulating and capable collaborator as Clyde Cowan. Our modus operandi was very simple--we were open with each other and were perfectly willing to hear out each other's ideas no matter how preliminary or half baked.

Reason for Direct Detection

Before continuing with the narrative it is perhaps appropriate to recall the evidence for the neutrino at the time Clyde and I started on our quest. The Pauli-Fermi neutrino[(1)] was postulated in order to account for an apparent loss of energy--momentum in the process of nuclear beta decay. All the evidence up to that time was obtained "at the scene of the crime" so to speak since the neutrino once produced was not observed to interact further. A moment's reflection reveals the logical difficulty occasioned by such a circular chain of reasoning. In order to demonstrate the neutrino's existence it was necessary to detect an interaction of the neutrino at a location remote from the point of origin.* It must be recognized, however, that independently of the observation

*A 1934 estimate by Bethe and Peierls[(2)] based on Fermi's theory indicated a cross-section for inverse beta decay to be $<10^{-44}$ cm^2/nucleus. Such a tiny interaction would enable a neutrino in the few MeV range to penetrate >1000 light years of liquid hydrogen, a number most discouraging to one seeking to detect a neutrino.

of a "free neutrino" interaction with matter the theory was so attractive in its explanation of beta decay that belief in the neutrino as a "real" entity was general. Despite this widespread belief, the free neutrino's apparent undetectability led[3] it to be described as a "elusive, a poltergeist." S.M. Dancoff in a 1952 article entitled, "Does The Neutrino Really Exist?" went so far as to state that the question of existence is meaningless.

I recall giving a talk on the need for detection of the free neutrino as definitive proof of its existence. Fermi, who happened to be in the audience nodded his head in agreement.[4]

In any event the question of the free neutrino's existence is a deep one in the sense of Niels Bohr's definition, since either a yes or a no would have profound consequences. If the answer is yes, the range of validity of energy-momentum conservation is extended and a new tool for the investigation of fundamental forces becomes available. If the answer is in the negative, one of the deep invariance principles of nature is seen to be flawed.

Detection Technique

According to the Pauli-Fermi theory (1930-1934), the neutrino should be able to invert the process of beta decay as shown in Equation (1):

$$\nu + A^{Z} \rightleftarrows \begin{array}{l} A^{Z-1} + e^{+} \\ \text{or} \quad A^{Z+1} + e^{-} \end{array} \qquad (1)$$

It was not known in 1952 whether the neutrinos emitted in e^+ or e^- decay were identical (Majorana) or differed (Dirac). We chose to focus on the reaction:

$$\nu + p \rightarrow n + e^{+} \qquad (2)$$

because of its simplicity and the possibility that the scintillation of organic liquids newly discovered by Kallmann et al[5] might be employed on the large ($\sim 1\text{m}^3$) scale appropriate to our needs. The initial idea was to view a large pot of liquid scintillator with photomultiplier tubes located on its boundary. The neutrinos would then produce positrons which would ionize causing light flashes which could be sensed by the photomultipliers and converted to electrical pulses for display and analysis.

I recall a conversation with H.A. Bethe in which he asked how we proposed to distinguish a neutrino event from other bomb associated signals. I described how, in addition to the use of bulk shielding which would screen out gamma rays and neutrons, we could use the delayed coincidence between the product positron and neutron to identify the neutrino interaction. It was not until many months later that Cowan and I recognized this signature would drastically reduce backgrounds so that we were able to use a steady fission reactor as a source instead of a bomb.

I have wondered since why it took so long for us to come to this now obvious conclusion and how it escaped others despite what amounted to a description of its essence as we talked to those around us. But of one thing I am certain, the open, free communication of our ideas was most stimulating to us and played a significant role in our eventual success. It must be admitted that we were not inhibited in our communication by the concern that someone would scoop us. Neutrino detection was not a popular activity in 1952.

The idea that such a sensitive device could be operated in the close proximity (within a hundred meters) of the most violent explosion produced by man was somewhat bizarre, but we had worked with bombs and felt we could design an appropriate system. In our proposal a detector would be suspended in a vertical vacuum tank in the near vicinity of a nuclear explosion and allowed to fall freely for a few seconds until the shock wave had passed. It would then gather data until the fireball carrying the fission fragment neutrino source ascended skyward. We anticipated a signal consisting of a few counts assuming the predicted ($\sim 10^{-43}$ cm^2/proton) cross-section, but background estimates suggested that our sensitivity could not be guaranteed for cross-sections $<10^{-39}$ cm^2/proton, four orders of magnitude short! It is a tribute to the wisdom of Los Alamos Director, Norris Bradbury, that he approved the attempt on the grounds that it would be ~ 1000 times as sensitive as the then existing limits[4].

The plan is shown schematically in Fig. 1. As already mentioned, we did not in fact use the bomb but designed the detector around the delayed coincidence. A letter to Fermi describing our new reactor based approach evoked the reply shown in Fig. 2.

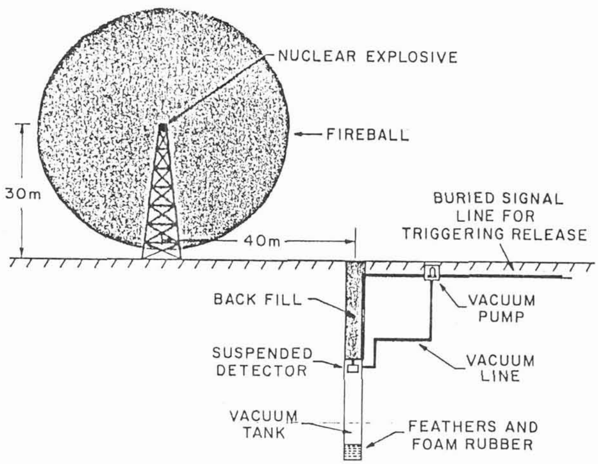


Fig. 1. Scheme for detecting neutrinos from a nuclear explosion.

Reflecting on the trail that took us from bomb to reactor, it is evident that it was our persistence which led us from a virtually impossible experiment to one that showed considerable promise. The stage had been set for the detection of neutrinos by the discovery of fission and organic scintillators--the most important barrier was the generally held belief that the neutrino was undetectable.

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
INSTITUTE FOR NUCLEAR STUDIES

October 8, 1952

Dr. Fred Reines
Los Alamos Scientific Laboratory
P.O. Box 1663
Los Alamos, New Mexico

Dear Fred:

Thank you for your letter of October 4th by Clyde Cowan and yourself. I was very much interested in your new plan for the detection of the neutrino. Certainly your new method should be much simpler to carry out and have the great advantage that the measurement can be repeated any number of times. I shall be very interested in seeing how your 10 cubic foot scintillation counter is going to work, but I do not know of any reason why it should not.

Good luck.

Sincerely yours,

Enrico

Enrico Fermi

EF:vr

Fig. 2. Letter from Fermi on hearing about our plan to observe the neutrino.

Hanford

Our first attempt was made at one of the Hanford Engineering Works reactors built during the Second World War to produce plutonium for the atomic bomb. The detector employed is shown in Fig. 3.

Those days at Hanford were both stimulating and exhausting. For a few months we stacked and restacked several hundred tons of lead and boron-paraffin shielding. We worked around the clock as we struggled with dirty scintillator piped, white reflecting paint that fell from the walls under the action of toluene based scintillator and cadmium propionate neutron capturer, etc, etc.

But, despite our efforts, background rates due to cosmic rays and electrical noise during reactor off periods frustrated our attempts to achieve the required sensitivity. Fig. 4 shows the system when we were within a factor ~ 75 of our goal.

We finally traced the reactor off electrical noise to an elevator which ran up and down the reactor during the restacking of fuel rods. In desperation we took turns riding the elevator and notifying the other when to turn off our detector so as to avoid

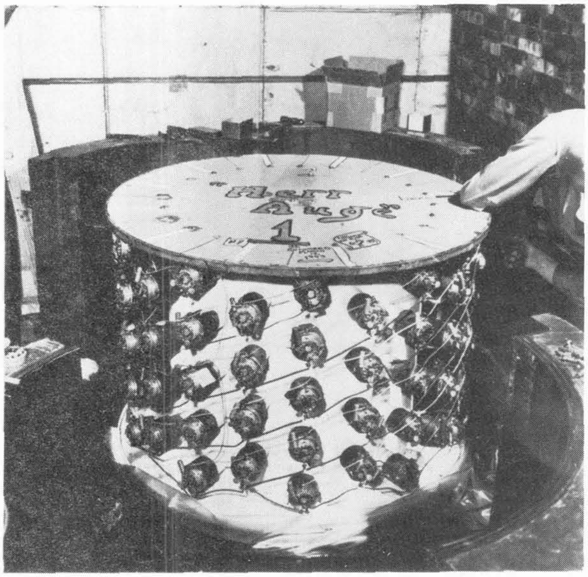


Fig. 3. First large (0.3 m^3) liquid scintillation detector in shield. The liquid was viewed by 90 2-inch photomultiplier tubes.

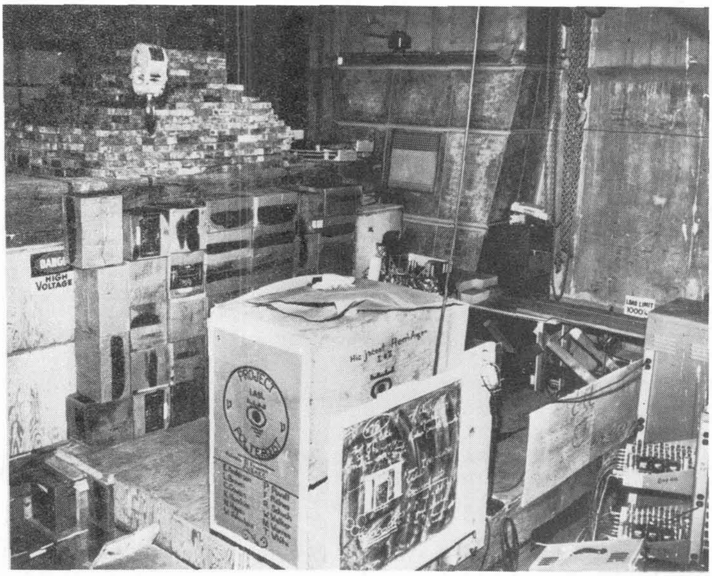


Fig. 4. Shield configuration. The note on the blackboard indicates that we were within a factor of 75 of the required sensitivity. The members of the group for the Hanford Phase of the search are listed on the "Project Poltergeist" sign.

Table 1. Listing of data from the Hanford experiment.

Run	Pile status	Length of run (sec)	Counts per minute* Net delayed pair time	Accidental background rate
1	On	4000	2.56	0.84
2	On	2000	2.46	3.54
3.	On	4000	2.58	3.11
4	Off	3000	2.20	0.45
5	Off	2000	2.02	0.15
6	Off	1000	2.19	0.13

*Delayed coincidence rates: *reactor on* (10,000 seconds), 2.55 ± 0.15 count/min.; reactor off (6,000 seconds), 2.14 ± 0.13 count/min. Reactor-associated delayed coincidence rates, 0.41 ± 0.20 count/min.

the noise. This exercise, which went on for hours, resulted in the reactor off data which showed a hint of a signal (6) (table 1).

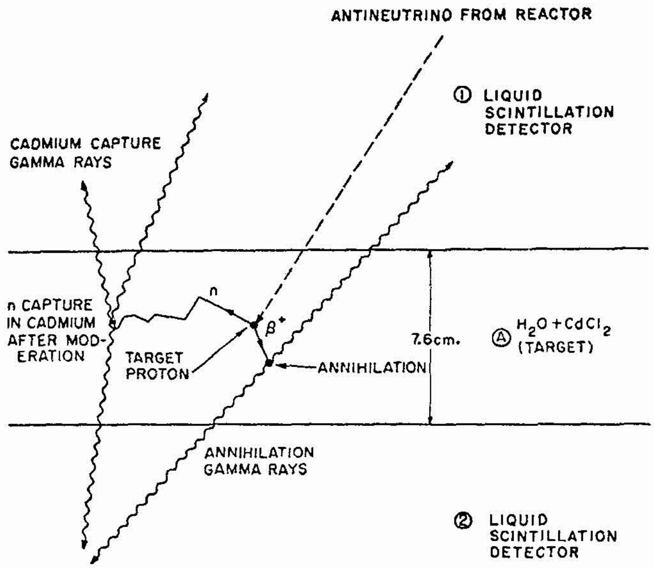
Savannah River

Encouraged by these results we considered how it might be possible to build a detector which would be even more discriminating in its rejection of background. We were guided by the fact that neutrons and positrons were highly distinctive particles and that we could make better use of their characteristics.

Fig. 5 is a schematic of the detector designed to this end. The neutrino is shown entering and interacting in the water target. The product e^+ deposits its energy in the water and annihilates giving two oppositely directed 0.5 MeV gamma rays which are detected by the scintillation counters A & B. The neutron slows down in the water and is captured several microseconds later by Cd which it contains. The neutron capture gammas are also detected in coincidence in counter A & B. This pair of prompt coincidences in delayed association with the e^+ pulses provided a most distinctive signature for the neutrino reaction.

These ideas were translated into hardware and associated electronics with the help of various support groups at Los Alamos. Then, in the Fall of 1955 at the suggestion and with the moral support of John A. Wheeler, the detector was taken to a new, powerful (700MW at that time), compact heavy water moderated reactor at the Savannah River Plant.* (Figs. 6).

*It is interesting that as with Hanford the Savannah River reactors were built to provide materials for weapons, this time for the hydrogen bomb.



Scheme of antineutrino detection experiment.

Fig. 5. Schematic of neutrino experiment at Savannah River.

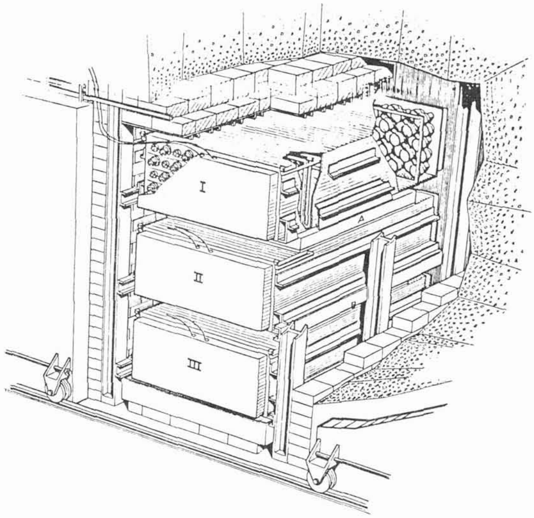


Fig. 6a. Sketch of detectors inside their lead shield. The tanks marked 1, 2, and 3 contained 1400 liters of triethylbenzene (TEB) liquid scintillator solution, which was viewed in each tank by 110 5-inch photomultiplier tubes. The tubes were immersed in pure nonscintillating TEB to make light collection more uniform. Tanks A and B were polystyrene and contained 200 liters of water, which provided the target protons and contained as much as 40 kilograms of dissolved $CdCl_2$ to capture the product neutrons. The assembled detector weighed about 10 tons, exclusive of shielding.

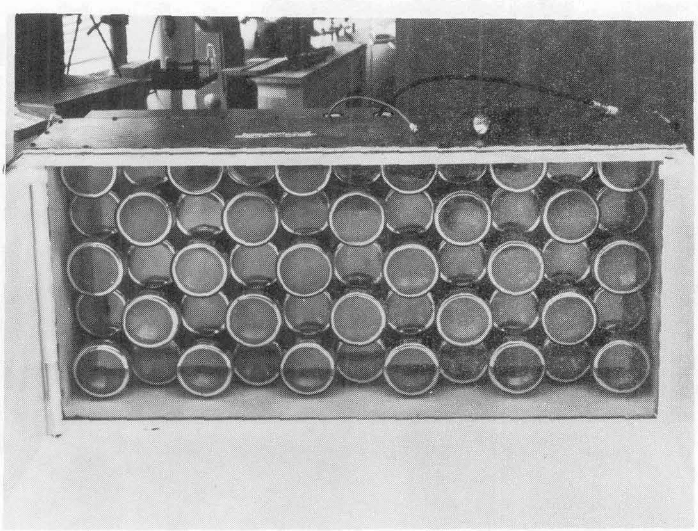


Fig. 6b. Inside end view of detector tank showing 55, 5" photomultiplier tubes.

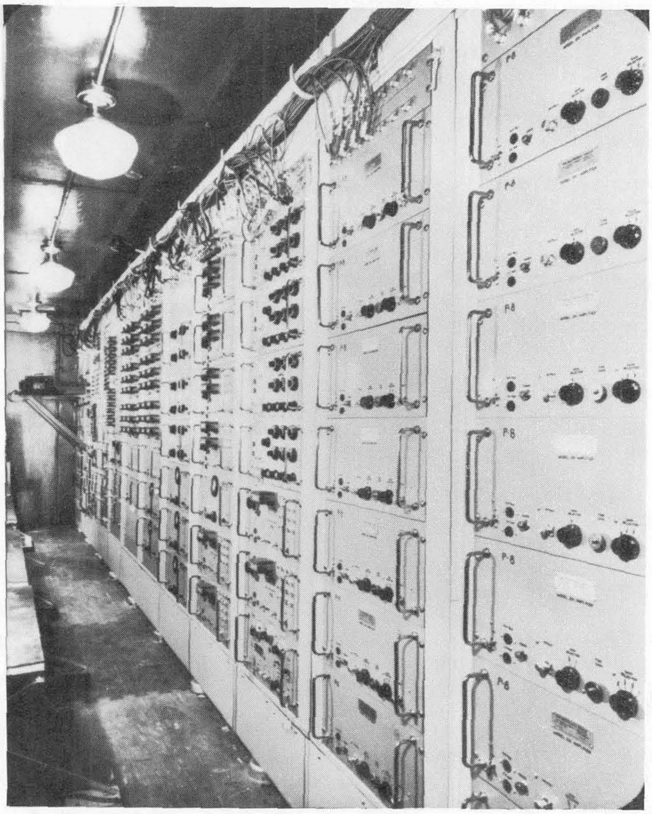


Fig. 6c. Electronics used for selection and recording of neutrino events. Modern electronics would occupy less than one tenth the space.

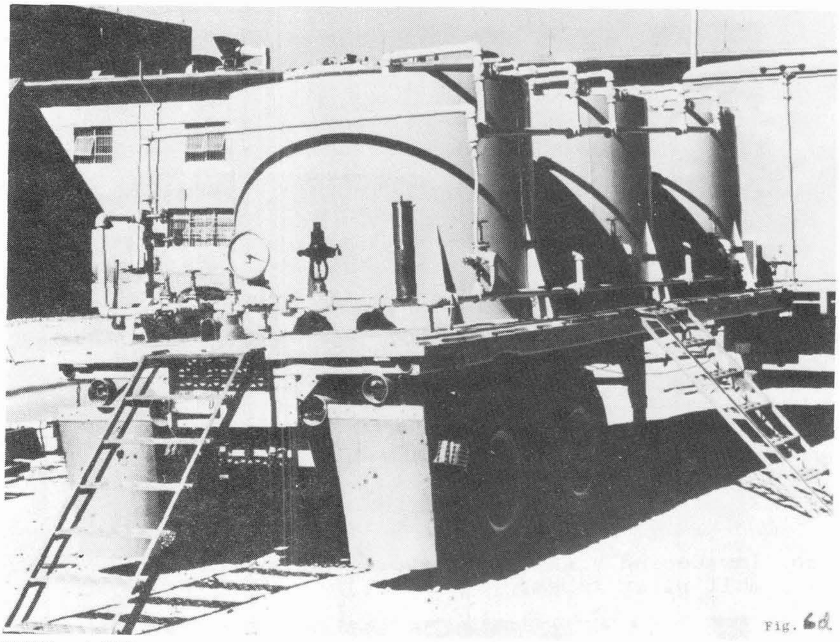


Fig. 6d. The scintillator was stored and shipped in three 1200 gallon steel tanks mounted on a flat bed trailer. The tanks were wrapped with electrical heating strips and covered with fiber glass insulation to keep the scintillator in solution on the trip from Los Alamos to Savannah River.

The Savannah River reactor was well suited for neutrino studies because of the small size and the availability of a well shielded location 11 meters from the reactor center and some 12 meters underground in a massive building. The high $\bar{\nu}_e$ flux, $1.2 \times 10^{13}/\text{cm}^2\text{sec}$, and reduced cosmic ray background were essential to the success of the experiment which even under those favorable conditions involved a running time of 100 days over the period of approximately one year.

Observation of The Neutrino

At the Savannah River we carried out a series of measurements to show that:

a) The reactor-associated delayed coincidence signal was consistent with theoretical expectation.

b) The first pulse of the delayed-coincidence signal was due to positron annihilation.

c) The second pulse of the delayed coincidence signal was due to neutron capture.

d) The signal was a function of the number of target protons.

e) Radiation other than neutrinos was ruled out as the cause of the signal by means of an absorption experiment.

Our standard of proof was that every test must yield the anticipated result for us to conclude that we were observing the Pauli-Fermi neutrino. An unanticipated result would imply either experimental error or the need to modify our view of the neutrino.

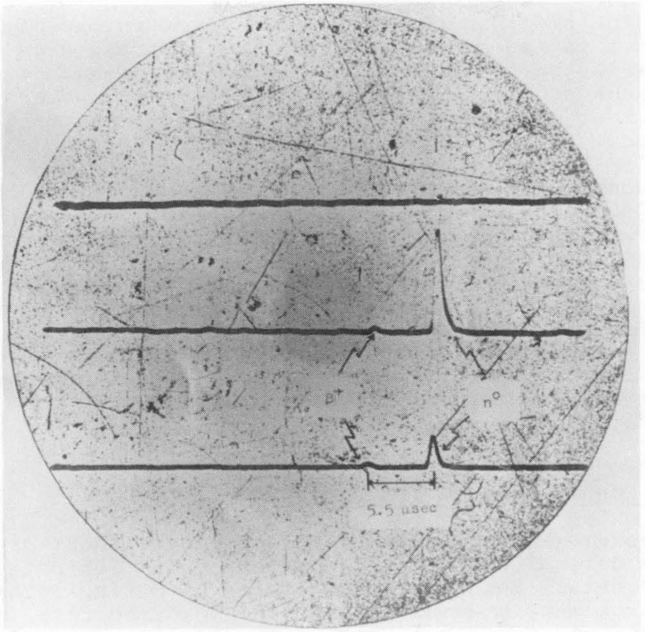


Fig. 6e. A characteristic record. Each of the three oscilloscope traces corresponds to a detector tank. The event recorded occurred in the bottom triad. First seen in coincidence are the positron annihilation gamma-ray pulses in each tank followed in 5.5 μsec by the larger "neutron" pulses. A second oscilloscope with higher amplification was operated in parallel to enable measurement of the positron pulses. The positron is denoted by β^+ and the neutron by n^o.

Signal Rate

A reactor-associated correlated signal rate of 3.0 ± 0.2 events per hour was observed. This represented a very favorable set of signal to background ratios: signal to total accidental background of 4/1, signal to correlated (as in neutron capture) reactor-independent background 5/1, and signal to reactor-associated accidental background > 25/1. Determining the positron and the neutron detection efficiencies with sources and using the crudely known $\bar{\nu}_e$ flux, we found the cross-section for fission $\bar{\nu}_e$ on protons to be

$$\bar{\sigma}\ \mathrm{exp} = (12^{+7}_{-4}) \times 10^{-44}\mathrm{cm}^2$$

compared to the expected*

$$\bar{\sigma}\ \mathrm{th} = (5 \pm 1) \times 10^{-44}\mathrm{cm}^2.$$

First and Second Pulses

The first pulse of the delayed coincidence pair was shown to be due to a positron by varying the thickness of a lead sheet interposed between the water target and one of the liquid scintillators,

*This was the pre-parity prediction. In retrospect it appears that the $\bar{\nu}_e$ spectrum and hence σ_{th} was not this well known.

so reducing the positron detection efficiency in one of the detector triads but not in the others. The signal diminished as expected in the leaded triad but remained unchanged in the triad without lead. A further check provided by the spectrum of first pulses showed better agreement with that from a positron test source than with the background.

The second pulse was shown to be due to a neutron by varying the cadmium concentration in the target water. As expected for neutrinos, removal of the cadmium totally removed the correlated count rate, giving a rate above accidentals of 0.2 ± 0.7/hour. The distribution of time intervals between the first and second pulse spectrum agreed with that expected for neutron capture gamma rays. A false pulse sequence in which neutrons also produced the first pulse was ruled out by use of a neutron source which showed that fast neutrons cause primarily an increase in accidental rather than correlated rates, a fact incompatible with the observed reactor-associated rates noted above.

Signal as a Function of Target Protons

The number of target protons was changed without drastically altering the detection efficiency of the system for both background and for $\bar{\nu}_e$ events. This was accomplished by mixing light and heavy water in approximately equal parts. The measured rate for the diluted target was 0.4 ± 0.1 of that for 100% H_2O, a number to be compared with the expected value of 0.5.

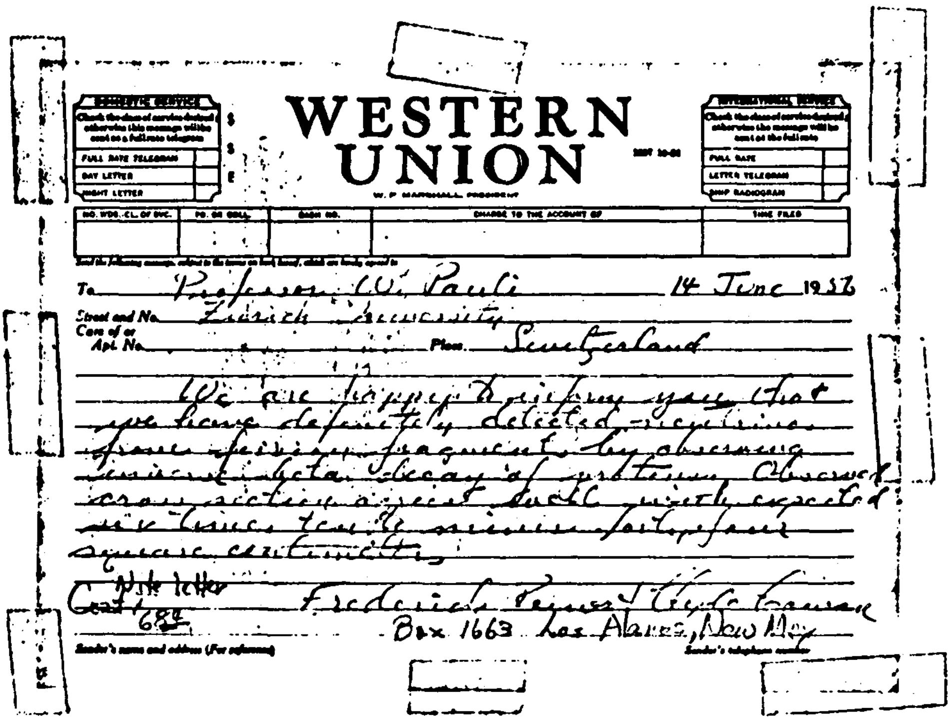
Absorption Test

The only known particles, other than $\bar{\nu}_e$ produced by the fission process were discriminated against by means of a gamma-ray and neutron shield. When a bulk shield measured to attenuate gamma rays and neutrons by at least an order of magnitude was added, the signal was observed to remain constant; that is the reactor-associated signal was 1.74 ± 0.12/hour with, and 1.69 ± 0.17/hour without the shield.

The tests were completed and we were convinced [7]. It was a glorious feeling to have participated so intimately in learning a new thing, and in June of 1956, we sent a telegram to Pauli notifying him of our results. (Fig. 7).

The quest was completed, the challenge met. There was, however, something missing--independent verification by other workers. As it turned out we were, in fact, correct but it took some eight years for this check to occur and that as a by-product of neutrino experiments at accelerators [8] (1964). I suspect that the unseemly delay was due to the fact that our result was not unexpected but it may also have had to do with the initially highly classified nature of the neutrino source.

Some twenty years later stimulated by the possibility of neutrino oscillations other groups also observed $\bar{\nu}_e$ + p at reactors [9].



WESTERN UNION

To Professor W. Pauli 14 June 1956

Street and No. Zurich University

Place Switzerland

We are happy to inform you that we have definitely detected neutrinos from fission fragments by observing inverse beta decay of protons. Observed cross section agrees well with expected six times ten to minus forty-four square centimeters.

Night letter

Frederick Reines Clyde Cowan

Box 1663 Los Alamos, New Mex

Fig. 7. Telegram to Pauli informing him of our results. The text read: "We are happy to inform you that we have definitely detected neutrinos from fission fragments by observing inverse beta decay of protons. Observed cross section agrees well with expected six times ten to minus forty-four square centimeters."

An Encore?

Having detected the neutrino the question arose, what next? What, as Luis Alvarez wrote me at the time, did we propose to do as an encore? A survey of the old notebooks indicated a variety of possibilities ranging from a study of the neutrino itself to its use as a tool in probing the weak interaction. Also, it was natural to ask whether our new experimental methods could help attack other problems.

One question I found particularly fascinating was: Did the neutrino possess a direct elastic scattering interaction with electrons

$$\bar{\nu}_e + e^- \overset{?}{\rightarrow} \bar{\nu}_e + e^- \qquad (3)$$

e.g. via a magnetic moment? This question had great appeal for a variety of reasons which were not entirely sensible. First, there was no theoretical guide to suggest that such a reaction between two of nature's "simplest" particles occurred and second, reminiscent of the earlier conversation with Fermi regarding bomb neutrinos, I had no idea how to construct a suitable detector. Despite these excellent reasons for choosing a more sensible problem I decided to work on it. It may be that with Cowan's departure from Los Alamos (1957) I wanted to prove I could do something even more speculative than our successful joint venture.

The essence of the detection problem was to distinguish an electron produced by the imagined elastic scattering process from an electron produced by gamma rays or beta emitters. This sorting out of such a non-descript process occupied me, and a succession of colleagues, for some twenty years[10]. The key to the solution was the recognition (1959) that if one chose a low Z medium most of the gamma ray background would arise from compton recoil electrons, whereas a $\bar{\nu}_e$ scattering would occur only once. It was therefore possible, in principle, to construct a detector in which spatial anticoincidences of the sequential compton electrons would be discriminated against, so reducing this source of background. Fig. 8 illustrates the principle of the method. While this idea was being translated to experimental reality and then eventual detection[11]* (1976), various theoretical developments took place in weak interaction physics. As the theorists labored they made predictions ranging from vague qualitative guesses about magnetic moments (1934) to statements that the interaction was zero[12] (1957), that it was given by (V-A) (1958)[13] and that it is undefined. The situation had finally settled down by 1976 to a prediction with the advent of the Weinberg, Salam, Glashow theory.

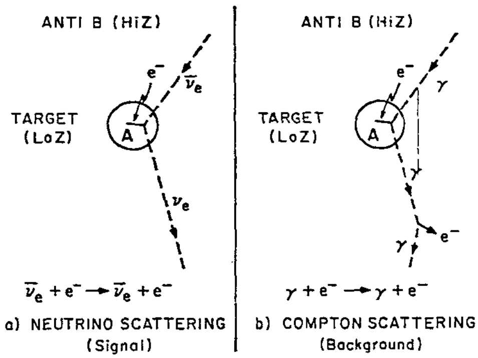


Fig. 8. Schematic of $\bar{\nu}_e$, e^- detector and discrimination idea.

a.) $\bar{\nu}_e$ collision occurs only in detector A.

b.) γ ray collides in A producing a compton recoil electron and then passes into B where it produces a second compton recoil. Such an event is rejected. A is small compared with a compton mean free path.

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*The cross-section was measured to be $\sim 10^{-45} cm^2/e^-$, one hundred times smaller than for the much more distinctive inverse beta reaction.

Once again, as in the case of the inverse beta decay process even prior to experimental verification of the elastic scattering reaction, theorists, in particular astrophysicists, assumed its existence and used it in building stellar models.

I find it interesting to contemplate the possible consequences of a closer coupling between theory and experiment in this case. If I had required a theory in the first place I would not have started to consider the scattering experiment when I did. If I had followed the theorists peregrinations I would have sacrificed the steadfastness of purpose which eventually led me and my colleagues, Sobel and Gurr, to the solution. This is not to say that experimentalists should proceed independently of theory but it does suggest that the coupling should not be too tight.

More on Reactor Neutrinos

It is remarkable how, despite the small number of neutrino reactions which can be studied at fission reactors, the field can be so rich in fundamental consequences. Aside from the detection of the free neutrino we were aware of the predictions of Lee and Yang[14] regarding a parity factor of two associated with the cross-section and made a first crude attempt to measure it. A precise comparison with prediction was not possible at the time (1956) because of the poorly known $\bar{\nu}_e$ spectrum from fission, but it was a start. In due course free neutrino experiments have achieved sufficient accuracy to reveal the factor of two discrepancy with the old parity conserving theory.

But whether the puzzle so posed would have led to the discovery of parity non-conservation in the absence of a theoretical framework is a subject for debate.

Neutrino Stability, $\bar{\nu}_e + d$

When we first turned on our detector at Savannah River in the Fall of 1955 no signals were observed. As we checked our apparatus a desparate thought crossed our minds: the neutrino might be emitted from fission but did not survive the 11 meter journey from the reactor to our detector. Perhaps the neutrino was unstable! A moment of excitement ensued until we made some adjustments in our apparatus and neutrino-like signals began to appear. The consequence of these errors resulted in a notebook entry which suggested making a check of the inverse square law dependence of the neutrino signals on the distance from reactor to detector. But in any event we had no theoretical basis for questioning the stability of the neutrino. On the other hand, we realized once again that experiment was the final arbiter in these matters.*

Many years later, stimulated by theoretical interest in neutrino stability, Sobel, Gurr and I used the experimental numbers taken in the 1950's to establish limits on the radiative mode of neutrino decay[15].

Currently, (1982), studies are underway at reactors[9] to test evidence of neutrino instability of the sort proposed by Pontecorvo and by Nakagawa et al.[16] and called neutrino oscillations. If such oscillations in fact occur it will imply that the neutrino has a rest mass and that the various lepton families are related.

Aside from the reaction of $\bar{\nu}_e$ with protons and electrons, only one other reaction appeared accessible to study at fission reactors

$$\bar{\nu}_e + d \rightarrow n + n + e^+ \qquad (5).$$

We had used deuterons in our detection experiment but our initial interest in pursuing it for its own sake was dampened by the feeling that its observation would add nothing fundamental to neutrino physics. After all the deuteron was a well understood nucleus. Nevertheless, I was once again intrigued by the difficulty of detection, arising in this case from the paucity of reactor $\bar{\nu}_e$ flux above the 4.1 MeV threshold and started to consider how it might be observed. Nothing was initially further from our minds than the counterpart of photo disintegration of the deuteron

$$\gamma + D \rightarrow n + p \qquad (6)$$

i.e. the so called neutral current interaction

$$\bar{\nu}_e + d \rightarrow n + p + \bar{\nu}_e \qquad (7)$$

but once started, the search led some years later to the detection of both branches (charged[17] 1969, neutral[18] 1979).

The physics accessible to the reactor $\bar{\nu}_e$ is thus seen to be surprisingly rich despite the limited number of reactions available for study.

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*In a conversation with O. Piccioni (early in 1955) and later (June 1955) with P. Morrison prior to our Savannah River experiment, they suggested neutrino instability as a possible explanation should the neutrino search yield a negative result. I found the idea of neutrino instability to be a "repulsive" thought but nevertheless proceeded to imagine what sorts of decay products there might be if the neutrino was, in fact, unstable. e.g.

$$\nu \rightarrow \nu_1 + \nu_2 \qquad (4)$$

I noted: "...now if have boson of mass 0 and spin $\frac{1}{2}$ it is difficult to understand its difference from a photon" and recognized that such a photon might be readily detectable, so labelling a decay. Looking backward these considerations were at best poorly developed and premature, but they alerted us to an interesting possibility.

Are ν_e and ν_μ identical?

Having detected a neutrino associated with nuclear beta decay decay we puzzled as to whether the neutral particle from (π,μ) decay, was the same as the neutrino from nuclear beta decay. We wrote in a 1956 article in Nature:[19]

"The question arises as to the identity of these neutrino-like particles with the neutrino of nucleon decay. It is to be noted that in nuclear beta decay the initial and final nuclei both quite obviously interact strongly with nuclei. This is not the case in (π,μ) decay, where the emission of a "neutrino" converts the interaction from strong to weak. Furthermore, despite the apparent equality of the nuclear beta-decay matrix elements with (μ,e) decay, both the initial and final products of the latter interact weakly with nuclei."

However dubious this argument, it had the virtue that it led us to ask a fruitful question.

I recall asking (1956) two distinguished theorists as to the evidence for the identity of the two neutrinos and was told in rather unceremonious terms that there was no good reason to assume them to be different.

A more useful reply would have been to allow the possibility that since one could not rule out their lack of identity, an experimental test was desirable, especially so in view of the recent detection of $\bar{\nu}_e$. Today's particle physicists have a much more adventurous attitude in such matters.

In any event, Cowan and I proposed to go to an accelerator and test the identity. The reaction we got from Los Alamos was difficult to understand. The advice went as follows: "You two fellows have had enough fun. Why don't you go back to work."

This response troubled Clyde and he left Los Alamos. I left two years later.

Looking back we had much to be thankful for. We had indeed been in the right place at the right time. The unlikely trail from bombs to detection of the free neutrino could, in my view, only have happened at Los Alamos.

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After describing the absorption experiments for beta decay products starting with Ellis and Wooster (1927), Chadwick and Lea, Nahmias (1935) and others, Crane states:

> "The use of the large neutrino flux from a chain reacting pile to test for the inverse beta decay process has been a subject of conversation among physicists since the advent of the chain reacting pile, and it would be surprising if experiments of this sort were not going forth in one or more of the government laboratories."

Perhaps surprisingly, Cowan and I were little influenced by Crane's observation and the suggestions for neutrino detection by Pontecorvo and Alvarez. Pontecorvo, B. National Research Council Canada Rept. P.D. 205 (Nov. 20, 1946) (unpublished); Alvarez, L.W. University of California Radiation Lab Rept., UCRL-328 (1949) (unpublished). A series of ingenious arguments regarding the absorption of solar neutrinos by the sun and earth led Crane to exclude all cross-sections for inverse beta reactions and ionization processes, which were $>10^{-36}$ (or possibly $>10^{-37}$) with the exception of the region 10^{-31} to 10^{-32} cm^2. The reader is referred to Crane's article for further details.

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COMMENTS AFTER THE REINES TALK

J. TIOMNO.- I found Professor Reines' talk most interesting but I like to comment on a small point.

I agree that Occam's razor worked since the beginning to suppress the hypothesis that $\nu_\mu \neq \nu_e$, with which most people worked. I remember that after I presented the paper with Wheeler at the American Physical Society (universal couplings) most of the criticism was that μ_o (of the doublet μ, μ_o) equal ν (of the doublet e, ν) was simpler, thus $\mu_o \neq \nu$ unnecessary. My argument was that, althought we also considered $\mu_o = \nu$, the doublet scheme was also simple and more symmetric. It leads to particle conservation in the doublet and, I said, this might result in an experimental fact. I like to mention also that our results were first communicated by Wheeler at the 1948 Washington meeting of the American Association for Advancement of Science as reported in American Scientist.

G. PUPPI.- Following the intervention of Professor Tiomno, I like to confirm that at the time when the universal Fermi interaction was proposed, the belief was, at least in my mind, that the neutral counterpart of the muon (the μ_o) and the neutral counterpart of the electron (the ν) were two different particles.

One of the reasons was that at that time the mass of the neutretto (μ_o) was believed to be different from zero. A second reason was the idea that as a neutron transforms in a proton with the emission of a pair (ν, e),in a similar way a meson transforms in a neutretto (μ_o) again with the emission of a pair (ν, e).

L. MICHEL.- I also wish to confirm what Tiomno and Puppi said about the μ_o, the associated neutral particle to the $\mu^{\pm}$. In the beginning, from the first observations of $\pi \to \mu$ decay in Bristol, the μ_o mass was believed large ($100 m_e$?). It decreased and could be zero. In my paper on μ decay, I asked the question of identity. I pointed out that a value of the ρ parameter larger than 3/4 would imply that the two emitted neutral particles are distinguishable from the point of view of Pauli's principle.

I also raised the question : Are the neutrinos Majorana neutrinos ? There had been many discussions on this question about double β decay. Would you like to comment on this problem ?

F. REINES.- It was thought in 1957 that the positive result of the $\bar{\nu}_e + p \to n + e^+$ experiment coupled with the negative result of the search for the reaction $\bar{\nu}_e + {}^{37}Cl \to {}^{37}Ar + e^-$ could be used to infer that the neutrino is a Dirac particle. But at the time little attention was being given to the helicity of the production process as the selector of the two possible helicities of the neutrino. It is now recognized that the observation of double beta decay in which no neutrinos are emitted would test the identity of ν_e and $\bar{\nu}_e$ a la Majorana. These difficult experiments are in progress or planned at several laboratories. Perhaps in the next few years we will be able to say.

L. MICHEL.- I would like to say that already in 1950 experiments on double β decay were started in order to test if neutrinos were of the Majorana type (no distinction between particle and anti particle). But you have also to know that many papers appeared which were completely wrong about Majorana theory. The Fermi paper and also the Majorana paper were written in double quantization. Most of the subsequent papers were written with the old Dirac theory with negative energy particles ; in this case you cannot have Majorana neutrinos contrarely to the belief of too many authors.

F. REINES.- The search for double beta decay has a long history stimulated by the theoretical work of Maria Mayer in the Thirties. I recall a talk at Los Alamos by Fermi in which he reported the first experimental attempts by E. Fireman. These first and subsequent experiments were fought with difficulty because of the long lifetimes and consequent low rates and high backgrounds, but progress is being made.

A. WATTENBERG.- Implied in much of the discussion is that the muon and electron are different. The experiment of Hincks and Pontecorvo (1948) showed that they must be different to some extent. The experiment was that in muon capture, the emitted electron is not accompanied by a γ-ray to three or four per cent. It was not till about six years later that we knew that the μ and e are different to 10^{-4}.

C.N. YANG.- There are a number of people here who were active already in the early 1930's. I wonder whether they could discuss that were the questions about β decay that were bothersome around that time, and what were the impacts produced respectively by the Pauli suggestion and the Fermi paper.

E.P. WIGNER.- I can only tell about my own reaction which was very strong and relieving. It was connected with a very special observation, that of the spectrum of the nitrogen molecule. I thought before that the nitrogen atom's nucleus consists of 14 protons and 7 electrons and that it obeys Fermi statistics -it consists of 21 Fermions. But the molecular spectrum, its rotational part, showed that it obeys Bose statistics thus confirming the existence of another particle, which we thought to be Pauli's neutrino. And indeed one can think of the neutron to consist of a proton, an electron and a neutrino.

E. AMALDI.- Yang asked which were the difficulties met, at the end of the 20's, by the model of the nucleus then commonly accepted, i.e. a system composed of A protons and (A-Z) electrons. They were many and most of them originated from the application of the "then new" quantum mechanics to the interpretation of nuclear phenomena.

The first difficulty was connected with the problem of confining an electron inside a nucleus. If one imposes its wave length to be not greater than the linear dimensions of a nucleus its kinetic energy turns out to be of at least 25 MeV i.e. about ten times greater than the energy observed for the beta-ray electrons.

The second difficulty was due to the nuclear spin. This was expected to be an integer or a half-integer (in $\hbar$ units) according to whether the number of constituents of the nucleus (each of spin 1/2) was even or odd. A few clear cases of violation of this rule were found from the experimental determination of the multiplicity of the hyperfine structure, just began in those years. For example ${}_3Li^6$ was found to have spin I = 1 while the number of its constituents was 9 (6 protons + 3 electrons).

The first to raise the alarm about the difficulties met in the case of N^{14} was R. de L. Kronig /1/ who pointed out that on the basis of the band spectra of N_2 molecule, the spin of N^{14} had to be 1, while this nucleus was supposed to consist of 21 particles (A = 14 protons and (A-Z) = 7 electrons).

A third difficulty was connected with the nuclear magnetic moments which were found from (the separation of) the hyperfine structure to be all of the order of magnitude of a nuclear magneton

$$\mu_N = \frac{\hbar e}{2m_p c}$$

i.e. $m_p/m_e \sim 1800$ times smaller than a Bohr magneton

$$\mu_B = \frac{\hbar e}{m_e c}$$

How can the (A-Z) magnetic moments of the nuclear electrons compensate each other or disappear when their number is odd ?

The forth difficulty, and by far the most dramatic one, was met with which of the two quantum statistics is obeyed by nuclei. In 1928-29 from the study of the Raman effect of the rotational spectra of biatomic molecules with equal nuclei (H^1-H^1 ; N^{14}-N^{14} ; O^{16}-O^{16}) Franco Rasetti /2/ proved that N^{14} has spin I = 1 and obeys Bose-Einstein statistics and not Fermi-Dirac statistics.

The importance of this experimental result from the point of view of the nuclear structure was understood and underlined at the same time by various authors : Wigner, whose paper was published in Hungarian and therefore did not receive the attention it deserved /3/, Heitler and Herzberg /4/ and, some time later, Ehrenfest and Oppenheimer /5/.

All these authors pointed out that a particle compound of n smaller particles all obeying Fermi-Dirac statistics, should obey Fermi-Dirac statistics for n odd and Bose Einstein statistics for n even. Since N^{14} was supposed to consist of 21 particles obeying Fermi-Dirac statistics it had to obey itself the same statistical law. This conclusion and its comparison with the experimental results of Rasetti, gave rise to long discussions among the physicists all over the world. The attitude, of course, was different for different people. The more popular view was expressed by stating that "when inside a nucleus, the electrons loose some of the properties which they have outside" /6/.

An extreme position was that of N. Bohr, who pointed out on various occasions"... the failure of the fundamental quantum mechanical rules of statistics when applied to nuclei..." and that "... according to experimental evidence, the statistics of an ensemble of identical nuclei is determined solely by the number of protons ... while the intranuclear electrons show in this respect a remarkable passivity ..." /7/.

With the discovery of the neutron by Chadwick, in 1932, all these difficulties, in particular Rasetti's results, became clear. The nucleus of N^{14} is composed by 7 protons and 7 neutrons and therefore it should obey Bose-Einstein statistics, provided the neutron itself obeys the same statistical law.

Another story, also belonging to the prehistory of the main subject of his conference, is that of the continuous spectrum of the beta-ray emitted by many natural radioactive nuclei.

It was discovered in 1914 by Chadwick who analyzed the electron spectrum emitted in the beta-decay of RaB with a magnetic spectrometer, combined with one or the other of two different detection techniques : counting of electrons and ionization measurements /8/.

He also found that the "electron lines" discovered in 1910 and studied in some detail by Hahn, Meitner and coworkers, that were so prominent in the photographs of the Berlin group /9/, only formed a small fraction of the total beta emission. The main portion was a continuous spectrum of β-rays which could be identified as the disintegration electrons and on this was superimposed the line spectrum.

The place of origin of the observed electrons, however, became a matter of debate. Rutherford, for example, in 1914 expressed the view that the fundamental phenomenon was the emission from the nucleus of a disintegration electron of a well defined energy and that by collisions with the outer electrons it lost varying amounts of energy, so that, as a statistical effect, a continuous spectrum was formed /10/.

A remarkable step forward appears in a 1922 paper by Ellis /11/ who clearly separate the origin of the electrons belonging to the continuous spectrum found by Chadwick from that of those forming the lines. These were attributed by Ellis to the conversion of monoenergetic gamma rays emitted by the nuclueus.

It seemed incredible, however, that the transition between two well-defined nuclei should lead to the emission of electrons with a continuous energy distribution.

Contrary to Ellis, Meitner insisted that nuclei possess discrete energy levels as shown from the α-particle and γ-ray spectra /12/.

The polemic between the Berlin and Cambridge groups definitely ended with an experiment of a new type made by Ellis and Wooster /13/. It was already well established that in the β-decay there is <u>one</u> electron emitted by the nucleus for each disintegration. Therefore in order to decide whether the spectrum of the electrons emitted by a nucleus was continuous or consists of a homogeneous group which becomes continuous through secondary processes, it is enough to perform by means of a calorimeter, an absolute determination of the total energy carried by the electrons emitted in a well defined and known number of disintegrations. The ratio of the total energy of these electrons to their number represents the "mean energy" $\langle E \rangle$ of the β-ray spectrum of nuclear origin. In the case of subsequent processes the quantity $\langle E \rangle$ should be equal to the upper limit of the β-spectrum, in the case of a direct emission of a continuous spectrum, to its mean energy. The measured quantity of heat per disintegration expressed in eV, was found to amount to

$$\langle E \rangle = 344 \pm 10\% \text{ keV}$$

and corresponded very well to the mean energy of the continuous β-spectrum. Its upper limit is about 1 MeV, which was absolutely excluded by the experimental results. Ellis pointed out that his experiment still left open the possibility that the energy balance could be re-established by a continuous spectrum of γ-rays, which, being not absorbed in the calorimeter, could escape observation.

Meitner still was not convinced by this experiment and decided to repeat it in an improved version. A special differential calorimeter was constructed by Orthmann, a pupil of Nernst, by means of which a higher precision's measurement was performed. The result /14/

$$\langle E \rangle = 337 \pm 6\% \text{ keV}$$

was in excellent agreement with the result of Ellis and Wooster given above.

Furthermore Meitner showed, by means of experiments with Geiger counters, that the γ-ray continuous spectrum, mentioned by Ellis only because of his extreme caution, does not exist.

After these experimental results had definitively proved that the spectrum of the electrons emitted in β-decay is continuous, only two theoretical possibilities were open for its interpretation :

(1) The principle of energy conservation is valid only statistically in the processes of β-decay ;

(2) The conservation of energy is strictly valid in all single β-decay processes, but simultaneously with the electron, also another radiation is emitted which escapes observation. This means that the latter should consist of new neutral particles.

In order to avoid confusion I recall that these alternative explanations were considered and discussed <u>before</u> the discovery of the neutron by Chadwick (February 1932).

The first of these two points of view was supported by Niels Bohr who in many occasions in those years, said the energy princĭple is not valid ; it does not apply to systems as small as nuclei /15/.

The other point of view, i.e. that in the beta decay the emission of the electron is accompanied by the emission of another radiation which escapes observation, was developed by Pauli. In December 1930, i.e. about 14 monts before the discovery of the neutron by Chadwick, he sent an open letter to the participants in a meeting of physicists in Tübingen, in which in particular Geiger and Meitner were present. This letter contains the first formulation of the neutrino hypothesis /16/.

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