The idea of the neutrino

To avoid anomalies of spin and statistics Pauli suggested in 1930 that a neutral particle of small mass might accompany the electron in nuclear beta decay, calling it (until Chadwick's discovery) the *neutron*.

Laurie M. Brown

During the 1920's physicists came to accept the view that matter is built of only two kinds of elementary particles, electrons and protons, which they often called[1] "negative and positive electrons." A neutral atom of mass number A and atomic number Z was supposed to contain A protons, all in the nucleus, and A negative electrons, $A - Z$ in the nucleus and the rest making up the external electron shells of the atom. Their belief that both protons and negative electrons were to be found in the nucleus arose from the observations that protons could be knocked out of light elements by alpha-particle bombardment, while electrons emerged spontaneously (mostly from very heavy nuclei) in radioactive beta decay. Any other elementary constituent of the atom would have been considered superfluous, and to imagine that another might exist was abhorrent to the prevailing natural philosophy.

Nevertheless, in December 1930 Wolfgang Pauli suggested a new elementary particle that he called a *neutron*, with characteristics partly like that of the nucleon we now call by that name, and partly those of the lepton that we now call *neutrino* (more precisely the electron antineutrino, but this distinction is not needed here). Pauli's neutron–neutrino idea became well-known to physicists even before his first publication of it, which is in the discussion section following Heisenberg's report on nuclear structure at the Seventh Solvay Conference,[2] held in Brussels in October 1933.

Shortly after attending this conference, Enrico Fermi published his theory of beta decay, which assumes that a neutrino always accompanies the beta-decay electron, and that both are created at their moment of emission. Perhaps because of the rapid acceptance of Fermi's theory and the tendency to rethink history "as it should have happened," the true nature of Pauli's proposal has been partly overlooked and its radical character insufficiently emphasized. Contrary to the impression given by most accounts, Pauli's "neutron" has some properties in common with the neutron James Chadwick discovered in 1932 as well as with Fermi's neutrino.

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Flaws in the model

By the end of 1930, when our story begins, quantum mechanics had triumphed not only in atomic, molecular and crystal physics, but also in its treatment of some nuclear processes, such as alpha-particle radioactivity and scattering of alpha particles from nuclei (including the case of helium, in which quantum-mechanical interference effects are so important). However, the situation regarding electrons in the nucleus was felt to be critical. The main difficulties of the electron–proton model of the nucleus were:

- The symmetry character of the nuclear wave function depends upon A, not Z as predicted by the model; when $A - Z$ is odd the spin and statistics of the nucleus are given incorrectly. For example, nitrogen ($Z = 7$, $A = 14$) was known from the molecular band spectrum of N_2 to have spin 1 and Bose–Einstein statistics.
- No potential well is deep enough and narrow enough to confine a particle as light as an electron to a region the size of the nucleus (the argument for this is based on the uncertainty principle *and* relativistic electron theory).
- It is hard to see how to "suppress" the very large (on the nuclear scale) magnetic moments of the electrons in the nucleus, which conflict with data on the hyperfine structure of atomic spectra.
- Although both alpha and gamma decay show the existence of narrow nuclear energy levels, the electrons from a given beta-decay transition emerge with a broad continuous spectrum of energy.

The strong contrast between the successes and the failures of quantum mechanics applied to the nucleus are nowhere more evident than in a book by George Gamow.[3] In it, all the passages concerning electrons in the nucleus are set off in warning symbols (skull and crossbones in the original manuscript).

Some physicists (among them Niels Bohr and Werner Heisenberg[4]) took these difficulties to indicate that a new dynamics, possibly even a new type of space–time description, might be appropriate on the scale of nuclear distances and energies, just as quantum mechanics begins to be important on the atomic scale. These physicists were impressed by the similarity of the nuclear radius to the value e^2/mc^2, the classical electron radius of H. A. Lorentz. At this distance it had been anticipated that electrodynamics would probably fail (and maybe, with it, the special theory of relativity). Bohr was willing to relinquish the conservation of energy, except as a statistical law, in parallel with the second law of thermodynamics. At the same time Heisenberg was considering the introduction of a new fundamental length into the theory. It seemed that anything might be considered acceptable as a way out of the dilemma—or perhaps anything except a new elementary particle.

Pauli's proposal

It was in this context of ideas that Pauli dared to suggest the existence of a new neutral particle. His proposal, intended to rescue the quantum theory of the nu-

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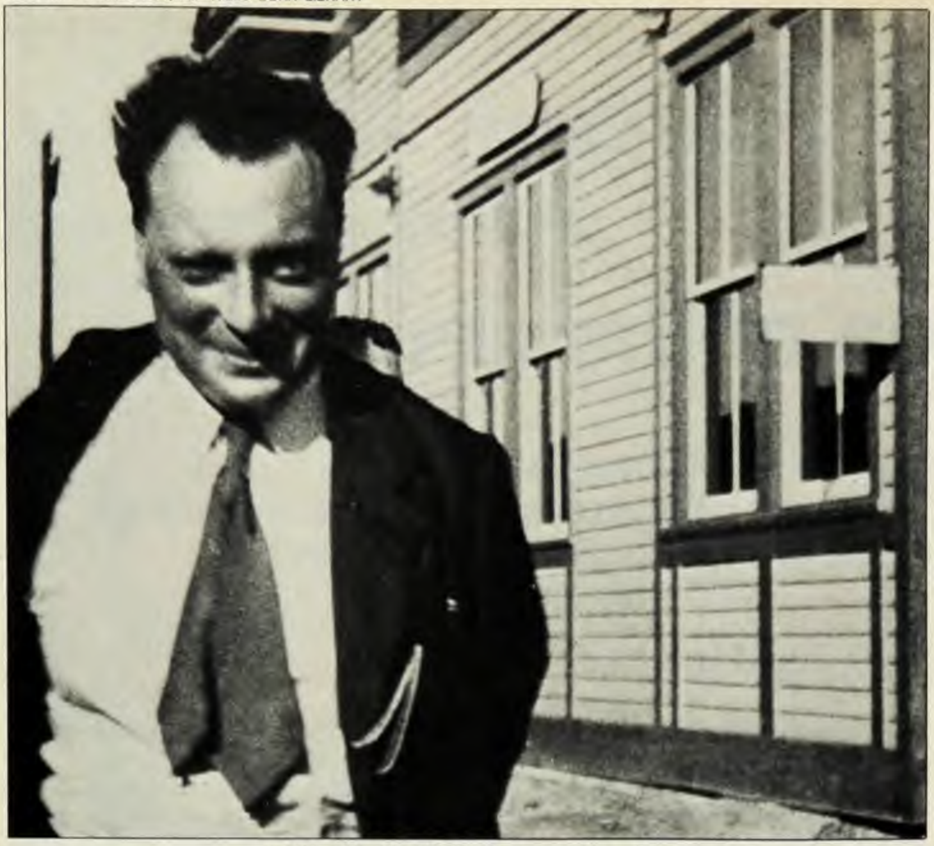

PAULI ON THE WAY TO PASADENA, 1931

cleus from its contradictions, was presented in good humor as a "desperate remedy," although it was a serious one. (The Viennese version would have been, according to the old joke: desperate, but not serious.) During the next three years he lectured on what he called the "neutron" at several physics meetings and he discussed it privately with colleagues.

Pauli's first proposal was put forward only tentatively, as he recalled in a lecture he delivered in Zürich in 1957, after receiving news of the experiments confirming parity violation in beta decay.[5] Invited to a physics meeting in Tübingen, Germany, which he was unable to attend (because of a ball to be held in Zürich, at which he declared he was "indispensable"), he sent a message with a colleague as an "open letter," although it was intended mainly for Hans Geiger and Lise Meitner. An English translation of this letter is given in the Box on page 27. Pauli was anxious for their expert advice as to whether his proposal was compatible with the known facts of beta decay.

In the 1957 lecture Pauli also tells how he became convinced of a crisis associated with beta decay. During the decade that followed the discovery by Chadwick in 1914 of beta rays with a continuous energy spectrum, it became established that *these* were the true "disintegration electrons," rather than those making up discrete electron line spectra, which were later shown to arise from such causes as photoelectric effects of nuclear gamma rays, internal conversion and Auger processes. Because a continuous spectrum seemed to disagree with the presence of discrete quantum states of the nucleus (as indicated by alpha and gamma emission), some workers, including Meitner, thought that the beta rays were radiating some of their energy as they emerged through the strong electric field of the nucleus.[5,6,7]

This led C. D. Ellis and William Wooster at the Cavendish Laboratory in Cambridge, England, who did not believe in the radiation theory, to perform a calorimetric experiment with radium E (bismuth) as a source. Their result, later confirmed in an improved experiment by Meitner and W. Orthmann,[8] was that the energy per beta decay absorbed in a thick-walled calorimeter was equal to the mean of the electron energy spectrum, and not to its maximum (endpoint). Furthermore, Meitner showed that no gamma rays were involved. According to Pauli (in 1957), this allowed but two possible theoretical interpretations:

- The conservation of energy is valid only statistically for the interaction that gives rise to beta radioactivity.
- The energy theorem holds strictly in each individual primary process, but at the same time there is emitted with the electron another very penetrating radiation, consisting of new neutral particles.

To the above, Pauli adds, "The first possibility was advocated by Bohr, the second by me." [5]

But although the conservation of energy, and possibly other conservation laws in beta decay were very much in Pauli's mind at this time, this was not his *only* reason for proposing the neutrino. He makes this point (already obvious from his Tübingen letter) quite explicit in his 1957 Zürich lecture. After pointing out one of the major difficulties with the nuclear model containing only protons and electrons (the symmetry argument mentioned above), Pauli says:

"I tried to connect this problem of the spin and statistics of the nucleus with the other of the continuous beta spectrum, without giving up the energy theorem, through the idea of a new neutral particle."

Neutrinos—ejected or created?

It is often overlooked in discussing the history of the neutrino idea that Pauli suggested his particle as a constituent of the nucleus, with a small but not zero mass, together with the protons and the electrons. (Chien-Shiung Wu, for example, emphasizes the non-conservation of statistics that would occur in beta decay without the neutrino.[6,7,9] However, Pauli refers rather to the spin and statistics of *stable* nuclei such as lithium 6 and nitrogen 14.) This point is of some significance; had Pauli proposed in 1930 that neutrinos were created (like photons) in transitions between nuclear states, and that they were otherwise not present in the nucleus, he would have anticipated by three years an important feature of Fermi's theory of beta decay. Pauli did not claim to have had this idea when he wrote the Tübingen letter, but he did say (in his Zürich lecture) that by the time he was ready to speak openly of his new particle, at a meeting of The American Physical Society in Pasadena, held in June of 1931, he *no longer* considered his neutrons to be nuclear constituents. It is for this reason, he says, that he no longer referred to them as "neutrons"; indeed, that he made use of no special name for them. However, there is evidence, as we shall see, that Pauli's recollections are incorrect; that at Pasadena the particles *were* called neutrons and *were* regarded as constituents of the nucleus.

I have not been able to obtain a copy of Pauli's Pasadena talk or scientific notes on it; he said later that he was unsure of the matter and thus did not allow his lecture to be printed. The press, however, took notice. For example, a short note in *Time,* 29 June 1931, headed "Neutrons?", says that Pauli wants to add a fourth to the "three unresolvable basic units of the universe" (proton, electron and photon); adding, "He calls it the *neutron.*"

Upon examining the program of the Pasadena Meeting, I discovered that Samuel Goudsmit spoke at the same session as Pauli (and even upon the same announced subject—hyperfine structure). I wrote to Goudsmit and received a most interesting reply, from which I should like to quote:

"Pauli accompanied my former wife and me on the train trip across the US. I forgot whether we started in Ann Arbor or arranged to meet in Chicago. We talked little physics, more about physicists. Pauli's main topic at the time was that he could imitate P. S. Epstein and he insisted that I take pictures of him while doing that. We spent a couple of days in San Francisco, where we almost lost him in Chinatown. He'd suddenly rush ahead and around a corner while we were window shopping ... He may have talked about the "neutron" on that trip, but I am not at all certain ..."

Goudsmit does not now recall exactly what Pauli said at Pasadena, except that he mentioned the "neutron"; however, he sent me a copy of his report at the Rome Congress on what Pauli had said four months earlier in Pasadena. To continue, then, with Goudsmit's letter:

"Fermi was arranging what was probably the first nuclear physics meeting. It was held in Rome in October 1931 ... It was the best organized meeting I ever attended, because there was very much time available for informal discussions and get-togethers ... Fermi had arranged marvelous leisurely sightseeing trips for the group. There were about 40 guests and 10 Italians.

"Fermi ordered the then 'young' participants, namely [Nevill] Mott, [Bruno] Rossi, [George] Gamow (who could not leave Russia but sent a manuscript) and myself, to prepare summary papers for discussion ... As you know, I don't use and don't keep notes. But I have a clear picture of Pauli lecturing [at Pasadena] and his mention of the 'neutron' ... Pauli was supposed to attend the Rome meeting, but he arrived a day or so late. In fact, he entered the lecture hall the very moment that I mentioned his name! Like magic! I remarked about it and got a big laugh from the audience."

Goudsmit's Rome report

At Fermi's request, then, Goudsmit reported at the Rome Conference on Pauli's talk in Pasadena. Here is what he said:[10]

"At a meeting in Pasadena in June 1931, Pauli expressed the idea that there might exist a third type of elementary particles besides protons and electrons, namely 'neutrons.' These neutrons should have an angular momentum $\frac{1}{2}\, h/2\pi$ and also a magnetic moment, but no charge. They are kept in the nucleus by magnetic forces and are emitted together with beta-rays in radioactive disintegration. This, according to Pauli, might remove present difficulties in nuclear structure and at the same time in the explanation of the beta-ray spectrum, in which it seems that the law of conservation of energy is not fulfilled. If one would find experimentally that there is also no conservation of momentum, it would make it very probable that another particle is emitted at the same time with the beta-particle. The mass of these neutrons has to be very much smaller than that of the proton, otherwise one would have detected the change in atomic weight after beta-emission."

GOUDSMIT (MIDDLE) AND FERMI (RIGHT) WITH UNIDENTIFIED MAN

Goudsmit added that Pauli believed "neutrons may throw some light on the nature of cosmic rays."

It does appear clear from this passage (to which Pauli evidently made no objection at the time) that at Pasadena the neutron was intended to be a particle that could be bound in the nucleus by magnetic forces. In his letter to me Goudsmit also said, "It was Maurice Goldhaber who some time ago pointed out that I was the first to put Pauli's idea on paper and in print."

After leaving Pasadena Pauli remained in the United States until the fall, when he went to Rome. He gave a seminar at the Summer Session of the University of Michigan at Ann Arbor (probably at one of their Symposia on Theoretical Physics, where Fermi had, the previous summer, given his famous lectures on the quantum theory of radiation). At the seminar Pauli spoke, according to the Berkeley theorists J. F. Carlson and J. Robert Oppenheimer,[11] about "the elements of the theory of the neutron, its functions and its properties."

Tracks in the cloud chamber

Carlson and Oppenheimer wondered whether Pauli's "neutrons" could be used to solve yet another puzzle: the appearance of certain lightly ionizing cloud-chamber tracks from cosmic rays that had been reported.

The complex problem of the energy loss of relativistic charged particles was crucial to the interpretation of the various components of the cosmic rays observed in the atmosphere, and had attracted the attention of many theorists. Carlson and Oppenheimer were unable to account for cloud-chamber tracks that appeared thinner than those of an "ordinary radioactive" beta particle. Their calculations of energy loss (which agreed in a general way with independent calculations by Heisenberg and Hans Bethe, and with an older classical estimate by Bohr) showed that charged particles should have a relativistic increase of ionization with energy. The particles leaving light tracks were very penetrating (and thus probably relativistic) and it was concluded that they could not be electrons or protons. (These quarklike tracks have not, to my knowledge, been explained. Perhaps they were examples of old and "faded" tracks, which often plagued cloud chambers of the untriggered variety.)

Carlson and Oppenheimer decided therefore to make a theoretical investigation, as they said,[11] of the "ionizing power of the neutrons which were suggested by Pauli to salvage the theory of the nucleus. These neutrons, it will be remembered, are particles of finite proper mass, carrying no charge, but having a small magnetic moment ..."

Could thin tracks, like those in the cosmic rays, be seen from beta decays?

"If they were found, we should be cer-

The participants in the Seventh Solvay Conference, where Pauli presented his neutrino idea, included, in the **first row,** E. Schrödinger, I. Joliot, N. Bohr, A. Joffé, M. Curie, O. W. Richardson, P. Langevin. Lord Rutherford, T. De Donder, M. de Broglie, L. de Broglie, L. Meitner, J. Chadwick, and in the **second row,** E. Henriot, F. Perrin, F. Joliot, W. Heisenberg, H. A. Kramers, E. Stahel, E. Fermi, E. T. S. Walton, P. A. M. Dirac, P. Debye, N. F. Mott, B. Cabrera, G. Gamow, W. Bothe, P. M. S. Blackett, M. S. Rosenblum, J. Errera, E. Bauer, W. Pauli, J. E. Verschaffelt, M. Cosyns (in back), E. Herzen, J. D. Cockcroft, C. D. Ellis, R. Peierls, A. Piccard, E. O. Lawrence, L. Rosenfeld. The photograph is by Benjamin Couprie.

tain that the neutrons not only played a part in the building of nuclei, but that they also formed the cosmic rays."

The calculations of Carlson and Oppenheimer were published[12] almost a year later, in September 1932; by that time they no longer believed that "neutrons" might leave observable cloud-chamber tracks. In addition, the situation in nuclear physics had changed profoundly, as it also was about to in cosmic-ray physics: Chadwick's study of "the penetrating radiation produced in the artificial disintegration of beryllium" had revealed the existence of the neutron, announced the previous February; Anderson's discovery of the positron in cosmic rays was announced in August.[13] Certainly one could no longer speak of the proton as synonymous with positive electricity, and one might suppose that now a new particle like Pauli's would be acceptable; but this was not the case:

For one thing, the positron was thought to be only the *absence* of a negative electron of negative energy, a *hole* in the vacuum. For another, the neutron of Chadwick, the *heavy* neutron, was generally regarded as a composite object (it was not thought to be unstable when free), a kind of tightly bound hydrogen atom or neutral nucleus made of a proton and an electron, like other nuclei. It was perhaps thought to be elementary only by Ettore Majorana in Rome who (according to Emilio Segrè) called it the *neutral proton*.

For our purposes, the Carlson–Oppenheimer article is significant in what it tells us about the view held by Pauli, in that summer of 1931, about his neutral particle, which, following the Berkeley authors, we will now call the *magnetic neutron* to distinguish it from Chadwick's neutron and Fermi's neutrino. Carlson and Oppenheimer state that the neutral particle of spin ½, satisfying the exclusion principle, was introduced by Pauli not only to resolve the difficulties in nuclear theory, but "on the further ground that such a particle could be described by a wave function which satisfies all the requirements of quantum mechanics and relativity . . . The experimental evidence on the penetrating beryllium radiation suggests that neutrons of nearly protonic mass do exist; and since our calculations may be carried through without specifying the mass or magnetic moment of the neutron, we shall consider the most general particle which satisfies the wave equation proposed by Pauli. It is important to observe that there may very well be other types of neutral particles, which are not elementary, and to which our calculations do not apply . . ."

Thus we find, surprisingly, that there were thought to be also purely *theoretical* grounds for considering a neutral particle with a magnetic moment; it is one of the few simple types of elementary particles that are allowed by relativistic quantum theory. In the wake of Chadwick's neutron discovery, Carlson and Oppenheimer in 1932 redefined Pauli's particle to be one whose wave function obeys a certain relativistic wave equation. We should not, however, assume that the Berkeley theorists were soft on new particles. On the contrary, the final paragraph of their lengthy article reads, "We believe that these computations show that there is no experimental evidence for the existence of a particle like the magnetic neutron."

Pauli's wave equation for the neutral particle, given at Ann Arbor, is a variant of the linear Dirac equation for the electron, containing an additional term (*Zusatzglied*) called the "Pauli anomalous magnetic moment" term.[14] This equation describes a spin-½ particle that may be either charged or neutral; the extra term makes a contribution to the charge-current four-vector, which need not vanish for a neutral particle.

Fermi is positive

Carlson and Oppenheimer derived a general formula for the collision cross section of magnetic neutrons and examined the result for small velocities. (They were well aware of the perils involved in pushing this highly singular interaction to excessive energies.) For the collision of a neutron against a particle of equal mass, they found a large probability, nearly independent of velocity and proportional to the square of the magnetic moment. The average energy loss per collision was relatively large, and they deduced that such a particle "will never produce ion traces in a cloud chamber, since it tends to lose an appreciable fraction of its energy, and suffer an appreciable deflection at every impact." For targets much lighter or heavier than the neutron, smaller energy losses occur; cloud-chamber tracks might result in this case, but the collision probabilities are small unless the magnetic moment of the neutron is assumed to be improbably large. The concluded (correctly) that there is no evidence for magnetic neutrons. (The heavy neutron, with a magnetic moment only one thousandth of a Bohr magneton, leaves no tracks.) At the Seventh Solvay Conference in 1933, Pauli no longer felt the magnetic neutron to be "well-founded."

Let us return now to the Rome Congress of 1931, which Pauli considered important in the development of the neutrino concept, for there he had the opportunity to discuss it with Bohr and especially with Fermi, with whom he had a number of private conversations. While Fermi's attitude toward the neutrino was very positive, Bohr was totally opposed to it, preferring to think that within nuclear distances the conservation laws were breaking down.[15]

"From the empirical point of view," said Pauli, "it appeared to me decisive whether the beta spectrum of the electrons showed a sharp upper limit" or, instead, an infinitely falling statistical distribution. Pauli felt that if the limit were sharp, then *his* idea was correct, and Bohr's was wrong.

In mid-1933, Ellis and Mott suggested that the beta-ray spectrum has indeed a sharp upper limit, corresponding to a unique energy difference between parent and daughter nucleus.[16] Furthermore, they added,

> "According to our assumption the β-particle may be expelled with *less* energy than the difference of the energies . . . of the two nuclei, but not with *more* energy. We do not wish in this paper to dwell on what happens to the excess energy in those disintegrations in which the electron is emitted with less than the maximum energy. We may, however, point out that if the energy merely disappears, implying a breakdown of the principle of energy conservation, then in a β-ray decay energy is not even statistically conserved. Our hypothesis is, of course, also consistent with the suggestion of Pauli that the excess energy is carried off by particles of great penetrating power such as neutrons of electronic mass."

The question of the upper limit of the beta spectrum, although not easily resolved, is of some importance, for the shape of the upper end of the spectrum is sensitive to the neutrino mass. This was discussed again by Ellis at an international conference in London, held in the fall of 1934, where he referred to accurate magnetic spectrograph measurements of W. J. Henderson that strongly suggested a neutrino of zero mass.[17] Fermi's theory of beta decay had already been published,[18] and Ellis assumed it in his analysis, but an energy-nonconserving theory, that of Guido Beck and Kurt Sitte, shared equal time with Fermi's at the conference.

Fermi spoke at the London Conference, but his subject was the neutron-activation work of the Rome experimental nuclear physics group. He also had attended the Seventh Solvay Conference, held in October, 1933, where he heard Pauli present his first suggestion *for publication* of the existence of a neutrino. The complete Solvay remarks of Pauli are given in English translation in the Box on page 28; we leave it to the reader to decide whether Pauli still thought that the neutrino or the

Pauli proposes a particle

The letter in which Pauli proposed the neutrino, translated from the German of reference 5, reads as follows:

Zürich, 4 December 1930
Gloriastr.

Physical Institute of the
Federal Institute of Technology (ETH)
Zürich

Dear radioactive ladies and gentlemen,

As the bearer of these lines, to whom I ask you to listen graciously, will explain more exactly, considering the "false" statistics of N-14 and Li-6 nuclei, as well as the continuous β-spectrum, I have hit upon a desperate remedy to save the "exchange theorem" * of statistics and the energy theorem. Namely [there is] the possibility that there could exist in the nuclei electrically neutral particles that I wish to call neutrons, which have spin ½ and obey the exclusion principle, and additionally differ from light quanta in that they do not travel with the velocity of light: The mass of the neutron must be of the same order of magnitude as the electron mass and, in any case, not larger than 0.01 proton mass.—The continuous β-spectrum would then become understandable by the assumption that in β decay a neutron is emitted together with the electron, in such a way that the sum of the energies of neutron and electron is constant.

Now the next question is what forces act upon the neutrons. The most likely model for the neutron seems to me to be, on wave mechanical grounds (more details are known by the bearer of these lines), that the neutron at rest is a magnetic dipole of a certain moment μ. Experiment probably requires that the ionizing effect of such a neutron should not be larger than that of a γ ray, and thus μ should probably not be larger than $e.10^{-13}$ cm.

But I don't feel secure enough to publish anything about this idea, so I first turn confidently to you, dear radioactives, with the question as to the situation concerning experimental proof of such a neutron, if it has something like about 10 times the penetrating capacity of a γ ray.

I admit that my remedy may appear to have a small *a priori* probability because neutrons, if they exist, would probably have long ago been seen. However, only those who wager can win, and the seriousness of the situation of the continuous β-spectrum can be made clear by the saying of my honored predecessor in office, Mr. Debye, who told me a short while ago in Brussels, "One does best not to think about that at all, like the new taxes." Thus one should earnestly discuss every way of salvation.—So, dear radioactives, put it to the test and set it right.—Unfortunately I cannot personally appear in Tübingen, since I am indispensable here on account of a ball taking place in Zürich in the night from 6 to 7 of December.—With many greetings to you, also to Mr. Back, your devoted servant,

W. Pauli

* In the 1957 lecture, Pauli explains, "This reads: exclusion principle (Fermi statistics) and half-integer spin for an odd number of particles; Bose statistics and integer spin for an even number of particles."

CHADWICK

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electron were constituents of the nucleus. (That a massless neutrino could be *created* at the moment of its emission with the electron was clearly proposed that year[19] by Francis Perrin, who also attended the Seventh Solvay Conference.) There was, in any case, no doubt that a light or massless neutral particle of spin ½ has to be emitted with the beta-decay electron in order to save the conservation laws, and *that* is surely the idea of neutrino!

Fermi's theory of beta decay is in many ways still the standard theory. Called by Victor Weisskopf "the first example of modern field theory,"[20] it eventually caused Bohr to withdraw[21] his doubts concerning "the strict validity of the conservation laws." A radical generalization of quantum theory was *not* required, though new particles and new interactions were. Within a few months of Fermi's theory, positron beta decay was seen (the first example of artificial radioactivity); and beta decay was to be the prototype of a larger class of weak interactions.

The neutrino can be regarded as one of the first (if not the first) of the new particles that made the new physics of the 1930's, even though it took two more decades to observe the first neutrino-capture event. The weak interactions have been notorious for their capacity to flout the expectations of physicists with regard to symmetries and conservation laws. Although Bohr was too willing, in his 1931 Faraday Lecture,[15] "to renounce the very idea of energy balance," the conclusion of that lecture is probably still appropriate today: "... notwithstanding all the recent progress, we must still be prepared for new surprises."

* * *

This work was supported in part by a grant from the National Science Foundation. I would like to express my sincere appreciation to Arthur L. Norberg of The Bancroft Library, University of California, Berkeley, and to Judith Goodstein of the Robert A. Millikan Memorial Library of the California Institute of Technology. I am much obliged to Samuel Goudsmit for his letter and for his kind permission to quote from it.

Pauli becomes bolder

The discussion comments in which Pauli presented the idea of the neutrino at the Seventh Solvay Conference, ref. 2. The text is based on the translation from the French original by Chien-Shiung Wu, ref. 9, with corrections by Laurie Brown noted in brackets.

The difficulty coming from the existence of the continuous spectrum of the β-rays consists, as one knows, in that the mean lifetimes of nuclei emitting these rays, as that of the resulting radioactive bodies, possess well-determined values. One concludes necessarily from this that the state as well as the energy and the mass, of the nucleus which remains after the expulsion of the β particle, are also well-determined. I will not persist in efforts by which one could try to escape from this conclusion for I believe, in agreement with Bohr, that one always stumbles upon insurmountable difficulties in explaining the experimental facts.

In this connection, two interpretations of the experiment present themselves. The interpretation supported by Bohr admits that the laws of conservation of energy and momentum do not hold when one deals with a nuclear process where light particles play an essential part. This hypothesis does not seem to me either satisfying or even plausible. In the first place the electric charge is conserved in the process, and I don't see why conservation of charge would be more fundamental than conservation of energy and momentum. Moreover, it is precisely the energy relations which govern several characteristic properties of beta spectra (existence of an upper limit and relation with gamma spectra, Heisenberg stability criterion). If the conservation laws were not valid, one would have to conclude from these relations that a beta disintegration occurs always with a loss of energy and never a gain; this conclusion implies an irreversibility of these processes with respect to time, which doesn't seem to me at all acceptable.

In June 1931, during a conference in Pasadena, I proposed the following interpretation: the conservation laws hold, the emission of beta particles occurring together with the emission of a very penetrating radiation of neutral particles, which has not been observed yet. The sum of the energies of the beta particle and the neutral particle (or the neutral particles, since one doesn't know whether there be one or many) emitted by the nucleus in one process, will be equal to the energy which corresponds to the upper limit of the beta spectrum. It is obvious that we assume not only energy conservation but also the conservation of linear momentum, of angular momentum and of the characteristics of the statistics in all elementary processes.

With regard to the properties of these neutral particles, we first learn from atomic weights [*of radioactive elements*] that their mass cannot be much larger than that of the electron. In order to distinguish them from the heavy neutrons, E. Fermi proposed the name "neutrino." It is possible that the neutrino proper mass be equal to zero, so that it would have to propagate with the velocity of light, like photons. Nevertheless, their penetrating power would be far greater than that of photons with the same energy. It seems to me admissible that neutrinos possess a spin ½ and that they obey Fermi statistics, in spite of the fact that experiments do not provide us with any direct proof of this hypothesis. We don't know anything about the interaction of neutrinos with other material particles and with photons: the hypothesis that they possess a magnetic moment, as I had proposed once (Dirac's theory induces us to predict the possibility of neutral magnetic particles) doesn't seem to me at all well founded.

In this connection, the experimental study of the momentum difference [read *balance*] in beta disintegrations constitutes an extremely important problem; one can predict that the difficulties will be quite insurmountable [read *very great*] because of the smallness of the energy of the recoil nucleus.

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