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50 years ago Pauli invented the neutrino

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1. Introduction

In this essay I wish to describe the physical and philosophical ideas that, on the one hand, led Pauli on 4 December 1930 to propose a new neutral particle and, on the other hand, encouraged Bohr in his old idea of only statistical energy conservation. It is my aim to place these opposing views of the two friends into the historical context, but I have no intention to give a complete history of the neutrino.

The earlier history of the neutrino has been written by Pauli himself in a detailed and fascinating essay [1, 2] following a talk he gave in the Zurich Physical Society on 21 January 1957 (see below). Here I wish to limit the description to essentially the same period of time which for me ends with Pauli's death on 15 December 1958.

I am of course aware of the increasing importance neutrinos have played in particle physics since then, in particular the question of their mass, and the cosmological consequence this has. However, the main purpose of this essay is to honour Pauli at this 50th birthday of his particle.

2. The problem of the continuous beta-ray spectrum

The history of the neutrino really begins with Chadwick's discovery in 1914 of the continuous β-spectrum [3]. This work was done in Berlin where Otto Hahn and Lise Meitner had investigated the same problem by photographic plates. It was the first quantitative intensity measurement by counters; the β-ray velocity, of course, was determined by an applied magnetic field. Figure 3 of this paper, which was written in German, shows the continuous spectrum and also some discrete structure superimposed upon it.

This result led to serious difficulties of interpretation. The most natural assumption seemed to be that the β-rays were emitted together with γ-rays not counted in Chadwick's experiment and that the primary energy E_{pr} was shared statistically. The average energy E_{av} of the β-ray electrons then was determined by the form of the spectrum.

This was the point of view of C. D. Ellis which, however, was challenged by Lise Meitner and led to the well-known controversy of 1922 [1]. In fact, to

Meitner the discrete nature of nuclear energy levels known from α- and γ-decays meant that the β-ray electrons were emitted with the discrete primary energy E_{pr}. These primary electrons then would knock out secondary electrons in the counter whose continuous spectrum is then detected. While the discrete structure seen in the β-spectrum seemed natural in Meitner's interpretation Ellis had to assume it to be due to internal conversion of the γ-rays into secondary electrons.

This controversy was decided in 1927 when Ellis and Wooster made a calorimetric measurement of the energy E_{ca}, delivered by the β-rays [1]. According to Ellis' explanation the result should be $E_{ca} = E_{av}$ whereas Meitner's prediction corresponds to $E_{ca} = E_{pr}$. The experimental result was clearly in favour of Ellis. But Meitner, not satisfied with the result, repeated the experiment in 1930 adding, however, an important piece of equipment. It consisted of counters to detect simultaneously Ellis' γ-rays.

The real problem arose from Meitner's result: there were no γ-rays. The alternative that this result forced upon theory was most intriguing: Either energy conservation was only valid in the statistical average or a new very elusive particle had to be invoked. While Bohr was happy to adopt the first point of view, Pauli's deepest convictions did not leave him any choice but to propose a new particle. Indeed, for Pauli symmetries – continuous and discrete (see below) – and the ensuing conservation laws, were the most fundamental ingredients of any theory. It was an almost mystical belief in the harmony of the world, similar to Kepler's [4]. But why could Bohr so happily abandon strict energy conservation? To understand this we have to switch back to the days of the old quantum theory.

3. The controversy between Bohr and Pauli

Already in 1922, Bohr's intense thinking about the interaction between atoms and radiation had led him to abandon strict energy conservation in the realm of atomic processes [5]. This conclusion corresponded to an equally mystical belief of Bohr's, namely that in the microscopic world one had to be preprepared for suprises (see below). It also was in agreement with Bohr's favorite verse by Schiller (according to Pauli see Ref. 4): 'Nur die Fülle führt zur Klarheit Und im Abgrund wohnt die Wahrheit' ('Only fullness leads to clarity and the truth dwells in the abyss').

Bohr's conclusion, though, was rational and compelling; it may also indicate already a preference for probabilistic formulations. As Bohr says at the beginning of his paper [5], the introduction of discontinuities into the laws of Nature brought about by quantum theory represents a decisive rupture with the notion of classical electrodynamics. Since at that time the latter was the only theory available to describe the radiation emitted or absorbed by atoms there resulted an incompatibility between its continuous nature and the discreteness of atomic levels. According to Bohr the only way to overcome this incompatibility was by giving up strict energy and momentum conservation.

One year later a young fellow who had come to Copenhagen from Harvard, Slater, also tried his luck with the problem. In order to 'provide for only statistical conservation of energy and momentum' he proposed 'a virtual field of radiation originating from oscillators having the frequencies of possible quantum transitions' [6]. His original idea had some resemblance with de Broglie's 'onde guide', namely 'a field to guide discrete quanta' [6] but Bohr's assistant Kramers somehow talked him out of this [6].

The sum of all these efforts was the famous and controversal paper by Bohr, Kramers and Slater of 1924 [7]. Obviously, Pauli rejected it unconditionally. But he also received important help from other sides, namely from Einstein with whom he had discussed the paper in Innsbruck and, somewhat later from Geiger and Bothe and from Compton whose experiments gave clear evidence for the exactness of the conservation laws.

The nicest way to see Pauli react to the Bohr-Kramers-Slater paper is through his own letters which are contained in the first volume of correspondence published recently [8]. His letter to Bohr, dated Hamburg 2 October 1924, is probably among the most beautiful Pauli has written, both because of its warmth and because of its perfect German. As all his letters to Bohr of this time it starts with 'Sehr verehrter, lieber Herr Professor' ('Very honored, dear Mr. Professor'). It is interesting to compare this with the 'Lieber Herr Professor' that Pauli used to address his teacher Sommerfeld for whom he had enormous respect. The relation with Bohr was deeper, it was a friendship with fatherly features on Bohr's side.

But this friendship did not prevent Pauli to tell Bohr, in very retained language though, his disapproval which he first formulates in Einstein's arguments before giving, in true modesty, his own opinion. And as always at that time the letter closes by 'Ihr stets dankbarer (yours ever thankful) Pauli'.

The letter written to Kramers the following year, dated Hamburg 27 July 1925, has quite a different pitch; it is more like the Pauli that every physicist believes to know, witty and sarcastic. Surprisingly, he says that Bohr would soon have abandoned the view of Bohr–Kramers–Slater even if the experiments mentioned above would not have been done. For, he says, this view moves in an entirely wrong direction; and he underlines: 'Nicht der Energiebegriff ist zu modifizieren, sondern der Bewegungs- und der Kraftbegriff' ('Not the notion of energy has to be modified but rather the notion of motion and of force') – a fact soon realized in the new quantum mechanics. Mockingly he calls the Bohr–Kramers–Slater endeavour 'Kopenhagener Putsch' ('Copenhagen upheaval').

4. From Rutherford's to Pauli's neutron

In 1920 Rutherford proposed a neutral nucleus composed of a proton p and an electron e and called it 'neutron' [1]. Experimentally this particle was not found, however. It was the special case $(1, 0)$ of the nuclear model of that time, namely $(A, Z) = Ap + (A - Z)e$, A being the atomic number and Z the electric charge. This model, according to which the nucleus (A, Z) consists of $2A - Z$ Fermi-particles, ran into difficulties with the analysis of the band spectra of molecules according to the new quantum mechanics. Indeed, Kronig in 1928 and Heitler and Herzberg in 1929 noted an anomaly in the spectroscopic results for the isotope ^{14}N of Nitrogen $(A = 14, Z = 7)$ [1] as compared with the so-called 'Wechselsatz', a name apparently due to Pauli [9].

The 'change' in this 'theorem' referred to the spin-statistics properties of nuclei with even an odd number of particles, $2A - Z$: An even number required integer spin and Bose statistics, an odd number half-integer spin and Fermi statistics. According to the above nuclear model, ^{14}N should have 21 particles so that the Wechselsatz predicted a half-integer spin and Fermi statistics. The result from the band spectra, however, was spin one and Bose statistics.

The consequences drawn from this result was to question the validity of the nuclear model rather than that of the Wechselsatz. This may appear surprising since the latter made explicit use of the connection between spin and statistics which in full generality was proven by Pauli only in 1940 [10]. But, of course, it was only through quantum field theory that the problem of the spin-statistics theorem could be fully appreciated. Anyhow, in the late twenties Pauli was keenly aware of the importance of the Wechselsatz, as is clear from the analysis of the hyperfine structure of $^6Li^+$ done with his collaborator Güttinger [9].

It is therefore not suprising that his proposal of a new very penetrating neutral particle to resolve the dilemma of the β-decay spectrum took the Wechselsatz explicitly into account. Indeed, Pauli required this new particle to have spin $\frac{1}{2}$ and to obey Fermi statistics and called it neutron. This name was available since Rutherford's neutron had not been found.

This proposal is contained in an 'open letter to the group of radioactives at the regional meeting in Tübingen' dated Zürich, 4 December 1930, and written from his office at Gloriastrasse [11]. In this letter, Pauli asks the addressees to examine the question of the experimental detection of such a neutron and gives more details about its probable characteristics. But he also says that he does not yet dare to publish the idea. Indeed, only the two old particles, proton and electron, were accepted at that time. The letter closes by excusing himself not to be able to attend the meeting because of a dance he had to attend in Zürich.

Among the physicists present at the Tübingen meeting, who reacted quite positively to the proposal, was Lise Meitner whose copy of the letter was used by Pauli in his essay on the history of the neutrino [1]. Thus Pauli himself considered this letter as the birth certificate for his particle. In fact, it is the first explicit, if not official, statement of the idea, not counting some earlier written remarks, and considering that a second letter on the same idea to Oskar Klein was dated 12 December 1930 [12].

The first official, if not written, mention of Pauli's neutron was made in a talk he gave at the meeting of the American Physical Society held in Pasadena from 15 to 22 June 1931. In October of the same year Pauli attended the nuclear physics conference in Rome [12] where he told Fermi about his idea. Fermi responded very positively.

5. Neutron, neutrino and beta-decay theory

In 1932 Chadwick who had initiated the problems with the continuous β-spectrum concluded from his experiments that Rutherford's neutron, that is, the nucleus with $A = 1$ and $Z = 0$, must exist [13]. And based on the existence of the neutron n, Heisenberg [14] analyzed the structure of nuclei in terms of the new model $(A, Z) = Zp + (A - Z)n$ which immediately resolved the discrepancies between the Wechselsatz and the spectroscopic results for ^{14}N and ^6Li.

Since the discovery of the real neutron led to confusion with Pauli's particle, Fermi baptized the latter 'neutrino', the Italian diminutive of neutron. Pauli used this name in the first written account of his proposal in the proceedings of the Solvay Conference held in Brussels in October 1933 [1]. In this report on the continuous β-spectrum, written in fluent French, Pauli first discussed the arguments against Bohr's proposal of violated energy and momentum conservation and then describes his neutrino hypothesis.

Bohr, on the other hand, kept to his point of view expressed explicitly in his Faraday lecture of 1932 where he makes the prophetic remark that 'in atomic theory, notwithstanding all the recent progress, we must still be prepared for new surprises' [1]. But although, according to Pauli, Bohr publicly gave in only in 1936 [15], a letter from Bohr to Pauli dated 15 March 1934 shows that he actually took the neutrino quite seriously [12].

On the other hand, already in 1933, Fermi published the first account of his theory of β-decay [16] and explained the spectrum, concluding that the neutrino mass should be close to zero, a conclusion also reached independently by F. Perrin in Paris [1].

It may perhaps seem surprising that Pauli had not developed the β-decay theory himself. He was, indeed, particularly well prepared for this problem, both through his pioneering work on quantum field theory and also through his papers on the mathematical properties of the Dirac matrices [4]. As so often in his career he may have been dissuaded by his ever alert criticism which, perhaps, already let him suspect the difficulties inherent in this theory. These difficulties were, in fact, pointed out later in a note Pauli presented to the USSR Academy of Sciences in 1938, in which he showed that Fermi's theory led to an infinite self-energy thus preventing the application of perturbation theory in higher than first order [4].

6. The detection of the free antineutrino

The following two decades saw a steady increase in the credibility of the neutrino. In 1952 Langer and Moffat gave a quantitative upper limit on the neutrino mass by analyzing the upper end of the β-spectrum, and Rodback and Allen succeeded in verifying momentum conservation in β-decay by doing recoil measurements [1]. But, in fact, this time between the mid-thirties and the mid-fifties is the least evenful period in the history of the neutrino.

For Pauli this period ended with the telegram he received on 15 June 1956 from F. Reines and C. Cowan announcing the definite detection of (anti-) neutrinos, $\bar{\nu}$, emerging from a nuclear reactor by observing the reaction $\bar{\nu}+p \rightarrow \bar{e}+n$ [1]. Pauli answered the same day with a Chinese proverb: 'Thanks for message. Everything comes to him who knows how to wait. Pauli' [4].

This experiment had first been done in 1953 but only three years later Cowan and Reines considered the evidence sufficient for a definite publication [17]. In order to detect the exceedingly small cross-section of $6 \times 10^{-44}\ \mathrm{cm}^2$ a new scale of high energy experimentation had been necessary, making use of 3 liquid scintillation tanks of 1520 liters capacity each and requiring 1371 hours of total running time.

For Pauli this triumph was the end of all uncertainties about his particle but not the end of the surprises it had in store for him.

7. Parity violation and Pauli's acceptance of Bohr's attitude

In their Nobel Prize-winning paper of 1956 T.D. Lee and C. N. Yang [18] concluded that the evidence for parity conservation in β- and μ-decay was insufficient. Pauli, of course, had received a preprint of it. But, firmly convinced of the validity of conservation laws, he handed the paper to me who then was his assistant.

This conviction not only applied to continuous conservation laws but to discrete laws as well. In fact, in a letter of 14 July 1933 to Heisenberg he writes [12] 'Fast noch wichtiger als die Erhaltungssätze von Energie und Impuls bei Kernprozessen sind mir aber die Erhaltungssätze aller diskret quantisierten Grössen...' ('Almost more important than the conservation laws of energy and momentum, however, are for me the conservation laws of all quantities with discrete quantization...').

Seen against this background it will become clear why the Monday, 21 January 1957 was a day of truth for Pauli. He himself has described this day and the reflexions it brought to his mind in the letter to Weisskopf mentioned before [15]. The opening of this letter reads: 'Now the first shock is over and I begin to collect myself again (['zusammenklauben'] as one says in Munich)'.

In the evening of the 21st he had to give the talk about the history of the neutrino in the Zurich Physical Society mentioned in the Introduction [1]. The afternoon mail had brought three preprints sent by Telegdi from Chicago with his own experimental results, as well as those of Mrs. Wu and of Lederman in New York. All three papers confirmed maximum parity violation both in β- and in μ-decay. Pauli had already knowledge of these results through a letter by Villars from Geneva which had arrived in the morning mail and contained the report from the New York Times [15].

The same morning mail had also brought two preprints on the two-component theory of the neutrino [15]. Pauli had mentioned the possibility, noticed first by Weyl, of Lorentz-invariant two-component wave equations in his Handbuch article of 1933 on the general principles of wave mechanics [19]. However, in this article the following sentence, put in italics, appears: 'Indessen sind diese Wellengleichungen... nicht invariant gegenüber Spiegelungen (Vertauschung von links und rechts), und infolgedessen sind sie auf die physikalische Wirklichkeit nicht anwendbar' ('However, these wave equations... are not invariant against reflexions (exchange of left and right), and therefore, they are not applicable to physical reality'). This is in complete agreement with the statement he had made at about the same time in the letter to Heisenberg mentioned earlier.

It is interesting that in the revised edition of 1958 [20], for which I had done the proof reading as assistant, the remark about the non-applicability of two-component equations to physical reality is replaced by a footnote saying 'Neuerdings wurden diese Gleichungen auf das Neutrino angewendet, um nichtspiegelinvariante schwache Wechselwirkungen darzustellen'. ('Recently these equations were applied to the Neutrino in order to describe non-reflexion-invariant weak interactions'.)

Pauli's talk in the evening of the 21 January 1957 was the most brilliant I had heard him give. When he spoke about Bohr as his main opponent in regard to the neutrino he mentioned the remark from Bohr's Faraday lecture quoted earlier, namely that one must be prepared for surprises, particularly in β-decay, adding 'that at the end of my talk I would come back to the surprises Professor Bohr had foreseen here' [15]. So the physics community of Zurich became firsthand witness of the shock of parity violation.

The depth of the shock for Pauli can be seen from the philosophical turn in the letter of Pauli to Weisskopf: 'How can the strength of an interaction produce or create symmetry groups, invariances or conservation laws? This question prompted me to my premature and wrong prognosis' [15]. But a truely disturbing reflexion comes after the mentioned remarks on Bohr: 'He was not right with the energy law (only in Nature, 1936, did he give in!) but, who knows, will there be any stop after this new principle? According to it, perhaps the beta interactions are still 'too strong' to violate also the energy law; but what if there are still weaker interactions for which also the energy law does no longer hold (as Bohr wanted it originally)?' [15]. To read this from the man whose unconditional belief in conservation laws made the neutrino idea possible in the first place, leaves one perplexed.

Of course, Pauli's disturbing question is much attenuated by the observation made by himself right afterwards, namely that an external field of cosmic extension could do the trick of violating any conservation law 'for us men' while leaving complete symmetry 'for God Himself' [15]. The idea of an external field in relation with parity violation was indeed discussed at that time by Stueckelberg in Geneva and others and had made its way into the Zurich Physics Colloquium.

Thus, psychologically, the idea of an external field had a conciliatory effect on Pauli, and he was now also prepared to understand and even to adopt Bohr's philosophical attitude of openness towards the surprises hidden in the subatomic world.

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