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FERMI'S β-DECAY THEORY

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I

Throughout his lifetime Enrico Fermi (1901–1954) had considered his 1934 β-decay theory as his most important contribution to theoretical physics. E. Segrè (1905–1989) had vividly written about an episode at the inception of that paper:[1]

> *Fermi gave the first account of this theory to several of his Roman friends while we were spending the Christmas vacation of 1933 in the Alps. It was in the evening after a full day of skiing; we were all sitting on one bed in a hotel room, and I could hardly keep still in that position, bruised as I was after several falls on icy snow. Fermi was fully aware of the importance of his accomplishment and said that he would be remembered for this paper, his best so far. He sent a letter to Nature advancing his theory, but the editor refused it because he thought it contained speculations that were too remote from physical reality; and instead the paper ("Tentative Theory of Beta Rays"*[a] [*FP 76*]) *was published in Italian and in the Zeitschrift für Physik.*[b]

In 2001 there was a centennial celebration of Fermi's 100th birthday. I contributed a paper to that celebration. One passage of my paper read:[3]

> *One day in the 1970's, I had the following conversation with Eugene Wigner in the cafeteria of Rockefeller University*:

[a]Original title: "Tentativo di una teoria dei raggi β."
[b]Fermi also published in 1933 a short paper in Italy. See Ref. 2.

Y: *What do you think was Fermi's most important contribution to theoretical physics?*

W: *β-decay theory.*

Y: *How could that be? It is being replaced by more fundamental ideas. Of course it was a very important contribution which had sustained the whole field for some forty years: Fermi had characteristically swept what was unknowable at that time under the rug, and focused on what can be calculated. It was beautiful and agreed with experiment. But it was not permanent. In contrast the Fermi distribution is permanent.*

W: *No, no, you do not understand the impact it produced at the time. Von Neumann and I had been thinking about β-decay for a long time, as did everybody else. We simply did not know how to create an electron in a nucleus.*

Y: *Fermi knew how to do that by using a second quantized ψ?*

W: *Yes.*

Y: *But it was you and Jordan who had first invented the second quantized ψ.*

W: *Yes, yes. But we never dreamed that it could be used in real physics.*

What this passage reflected was not just the very different evaluations, by Wigner and me, of Fermi's β-decay theory, but in fact the very different evaluations of this theory by Fermi himself and his generation of physicists and by my generation. Recently I looked into the old literature and was able to understand better the reason for this difference.

II

In 1932 there were two discoveries that greatly shocked the world of physicists:

On February 17th, J. Chadwick (1891–1974) sent a short article to *Nature*[4] with the title "Possible Existence of a Neutron." It had great immediate impact: One realized that nuclei were made of protons and neutrons rather than protons and electrons, thus understanding the many regularities of the composition of light nuclei. But at the same time it created a new difficult puzzle: Since there were no electrons inside a nucleus, from where came the electron emitted in β-decay?

Seven months later on September 1st, C. D. Anderson (1905–1991) sent a short paper[5] to *Science* entitled "The Apparent Existence of Easily Deflectable Positives." Before this paper physicists had been dumbfounded for some five years by Dirac's famous equation of the electron: It was pure *magic*, but also afflicted[c] with a strange *sickness*. Should one believe it or not? Anderson's paper gave a definite resounding answer to this question: *Yes. One must believe it.* Furthermore Dirac's "hole theory," unbelievable as it was, was *nevertheless also correct.*

[c]Page 1056 of D. F. Moyer, *Am. J. Phys.* **49**, 1055 (1981) (Ref. 6).

After Anderson's paper, the remaining puzzling question was about β-decay. β-decay was first discovered near the end of 19th century. Through many efforts, finally in 1927, C. D. Ellis (1895–1980) and his collaborators confirmed that the electrons emitted in β-decay did not have a fixed energy. Instead they had a continuous spectrum. Niels Bohr (1885–1962) thought that this proved that in β-decay energy was not always conserved. But W. Pauli (1900–1958) proposed that energy was still conserved, but during β-decay, in addition to the electron, there was also emitted from the nucleus another particle which had no electric charge. (This particle was later named the neutrino.) But since at that time there were only three known particles: proton, electron and photon, Pauli did not dare to formally publish this bold proposal. For example, on December 1st, 1931 Pauli visited Princeton. A few days later[d] he had dinner in a Chinese restaurant in New York City with I. I. Rabi (1898–1988). Pauli told Rabi: "I think I am cleverer than Dirac, I shall not publish it." The "it" here referred to his neutrino hypothesis. Obviously Pauli thought Dirac was unwise to have published that year in the Proceedings of Royal Society an article about an undiscovered magnetic monopole.

Two years later, in October 1933 at the Solvay Congress in Brussels, Pauli was no longer so careful. He said at the conference:[7]

> *In June 1931, on the occasion of a conference at Pasadena, I proposed the following interpretation: The laws of conservation remain valid/recognized, the emission of the particles β being accompanied by a very penetrating radiation of neutral particles, which has not been observed until now. The total energy of particle β and of the neutral particle (or the neutral particles, since we do not know whether there is only one or there are a lot) emitted by the nucleus during a single process, would be equal to the energy which corresponds to the upper boundary of spectrum β. Needless to say we not only keep the conservation of energy, but also of momentum, of angular momentum and of the nature of the statistics in all the basic processes.*
>
> *Regarding the properties of these neutral particles, the atomic weights of the radioactive elements tell us first of all that their mass cannot exceed much that of the electron. In order to distinguish them from the heavy neutrons, Mr. Fermi proposed the name "neutrino." It's possible that the neutrinos' own mass might be equal to zero, so that they would have to propagate with the speed of light, like the photons. However, their penetrating power would exceed by far that of photons of the same energy. It seems to me acceptable that the neutrinos have spin $\frac{1}{2}$ and satisfy Fermi's statistics, although experiments do not give us any direct proof of this hypothesis. We know nothing about the interaction of the neutrinos with other material particles and with the photons: the hypothesis that they possess a magnetic moment, like I have proposed before (Dirac's theory predicted the*

[d]Page 1060 of D. F. Moyer, *Am. J. Phys.* **49**, 1055 (1981) (Ref. 6).

possibility of the existence of magnetic neutral particles), does not seem to me justifiable at all.

To clarify matters, the experimental study of the outgoing momenta in β-decay constitutes a problem of the most importance; we may predict that difficulties will be very great because of the small value of the energy of the recoil nucleus.

This paragraph was important: Every point in it turned out to be *correct*. Why did Pauli become so bold this time? We do not know the answer. But perhaps the discovery of the positron a year earlier, which had confirmed the unbelievable hole theory of Dirac, might have emboldened Pauli: After all, the hole theory was exceedingly bold and *it was based on Pauli's own Exclusion Principle*

III

The Solvay Congress in October 1933 was attended by many people: Bohr, Rutherford, Chadwick, Pauli, Heisenberg, Dirac and Fermi were all there. The comments by Pauli quoted above attracted much attention. But these comments did not answer a puzzling question: Nuclei are very small. To confine an electron inside a nucleus would cause its momentum to become very large by the uncertainty principle. It would then have a very large energy. How could that be?

Two months later, Fermi solved this puzzling problem. His key new ideas were:

(i) To add a term in the Second Quantized Hamiltonian

$$\Psi_e^\dagger(\cdots)\Psi_\nu\,. \tag{1}$$

(ii) Ψ_e, Ψ_ν are both anticommuting operators.

Second Quantized Hamiltonian is what is now called field theory. It was developed in the years 1925–1928 by Dirac, Jordan, Klein, Pauli and Heisenberg. Anticommuting operators were the inventions of Jordan and Wigner in 1928. Fermi was in fact at first not familiar with these operators.[8]

If so, why was it Fermi and not the architects of field theory who wrote this paper on β-decay that shocked the world of physicists? I think there are three reasons:

(a) In 1933 physicists thought matter was composed of protons, neutrons and electrons. In a chemical reaction or in a radiative decay such as in α or γ emission, protons, neutrons and electrons were all conserved. The only particle which was not always conserved was the photon. Thus it was difficult to understand the nonconservation of electrons in β-decay.
(b) From the pure theoretical point of view, field theory was built upon a *theorem* which stated that imposing the Bose–Einstein condition on a multiparticle Schrödinger equation is equivalent to a second quantized equation for field

Ψ_i which satisfies the commutation rule:

$$\Psi_i \Psi_j^\dagger - \Psi_j^\dagger \Psi_i = \delta_{ij} \,. \tag{2}$$

This fundamental theorem and its generalization to the Fermi–Dirac case required rather *complicated proofs*. These proofs were all confined to Hamiltonians for which the *electrons were conserved*.[9–11] It was thus generally believed that second quantization theory cannot be used for any problem with particle nonconservation. Fermi's bold proposal to add the term $\Psi_e^\dagger(\cdots)\Psi_\nu$ therefore shocked Wigner and von Neumann.

(c) Perhaps the most important reason was Fermi's unique approach to physics which was different from that of the architects of field theory: To him physics was not just formalism. Pure formalism to him was only useful if it materialized into explicit applications. A good example was his 1930–1932 paper on quantum electrodynamics,[12] in which he solved a fundamental formalistic problem in quantum electrodynamics and *then used it for calculations in five different applications*. This experience had *thoroughly acquainted* Fermi with how photons were created/annihilated by the Bose–Einstein operators $a^\dagger$ and a. I guess this experience had enabled him, two months after the Solvay Congress, to think of the idea of creating an electron by the Fermi–Dirac operator $b^\dagger$. In a paper by F. Rasetti published after Fermi's death, there is the following passage which supports this guess of mine:[13]

> *Apparently he had some difficulty with the Dirac–Jordan–Klein method of the second quantization of fields*, *but eventually also mastered that technique and considered a beta-decay theory as a good exercise on the use of creation and destruction operators.*

IV

Let us now return to the question posted in Sec. I above: Why was there such a big difference between the evaluations of Fermi's β-decay theory by two generations of physicists? I believe there are two reasons:

(A) By my generation, field theory was a required course for graduate theory students. Noncommuting Dynamic Variables was *in the first chapter* of field theory. We therefore did not realize that it was in fact a revolutionary development first launched by Fermi in that 1934 paper.

Historically each introduction of new numbers or operators was a revolution. Before the 16th century, people knew only of real numbers and considered many quadratic equations as unsolvable. In 1545 G. Cardano[14] published "Ars Magna" in which he introduced a new symbol in $5 + \sqrt{-15}$. But he was evidently not bold enough to further investigate the meaning of this symbol. It was twenty some years later that R. Bombelli in "L'Algebra" introduced for the first time the numbers i and $-i$ that we use today.

Quantum mechanics was launched by Heisenberg in 1925. With Born and Jordan he then proposed to replace dynamic variables with noncommuting operators. And that was of course a very very great revolution.

The physics of 19th century was entirely built on real numbers. Therefore when Schrödinger in 1926 wrote down his first wave equation, everything was real. It was only after a big struggle that Schrödinger was later forced to accept complex wave functions.[15]

(B) The most important development in experimental physics in the 1930's was the discovery in 1938 of nuclear fission. The most important development in theory in the 1930's was Dirac's hole theory. Compared with these two developments β-decay theory did not have the same degree of impact. Twenty some years later, with longer perspective, my generation thus failed to appreciate Fermi's impact as much as his contemporaries did in 1934.

V

P. Jordan had made two first class contributions to theoretical physics: Quantum mechanics was launched in three papers, the one-man paper, two-man paper and three-man paper. Of these Jordan was a co-author of the last two. Field theory was launched in three papers. The first was by Dirac in 1927, and the next two respectively by Jordan & Klein and Jordan & Wigner. The Jordan and Wigner paper was especially important because it proposed the very new anticommutation relations

$$b_i b_j^\dagger + b_j^\dagger b_i = \delta_{ij}\,. \tag{3}$$

Fermi's β-decay theory of 1934 was based on this relationship.

After World War II Jordan's name seemed to have disappeared from physics. I never saw any paper by him, nor did I encounter him at any scientific conference. His name was only mentioned in connection with his having been a member of the Nazi Party.

Between 1949–1966 I was at the Institute for Advanced Study in Princeton. Wigner was at that time a professor at Princeton University. As a consequence we saw each other almost weekly. I remember at teatime, if somebody mentioned Eq. (3) and said it was first written down by Wigner and Jordan, Wigner would always immediately say: No, no, it was Jordan and Wigner. After a few exchanges like this, even though everybody knew Wigner was super polite, we all felt that that paper was mainly the contribution of Jordan.

Wigner received his Nobel Prize in 1963. One day after that he mentioned in a casual conversation that Jordan had asked him to propose Jordan for the Nobel Prize. I remember Wigner told this story with an expression of helplessness. Thus nobody asked him any further questions. Apparently[16] Wigner did later propose Jordan for the 1979 Nobel Prize just before Jordan died in 1980.

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