LETTERS

Another Visit with Wolfgang Pauli

Thanks for the Wolfgang Pauli article by Karl von Meyenn and Engelbert Schucking in the February issue of PHYSICS TODAY (page 43). I read it with enormous pleasure. It is a masterpiece in six pages. A true portrait, with a wealth of good quotes that I had not seen before.

I was especially impressed by the section "Staying out of the potato race," with the story of Pauli's lost letters to Heisenberg. To this I would only add the following: I have a clear memory of Pauli saying to me, "If I had not wasted so much time trying to make sense of five-dimensional relativity (the Kaluza–Klein theory and similar attempts), I might have discovered quantum mechanics myself." I believe he said that when he was in Princeton in 1954.

FREEMAN DYSON
(dyson@ias.edu)
Institute for Advanced Study
Princeton, New Jersey

Karl von Meyenn and Engelbert Schucking provided many interesting facts and anecdotes about Wolfgang Pauli, but their article is marred by negative and inaccurate remarks about P. A. M. Dirac.

The authors make the astonishing claim that Pauli and Werner Heisenberg dismissed Dirac as a mere formalist. I have no knowledge of Heisenberg's views, but as a close associate of Victor Weisskopf for 48 years, I often heard that Pauli was deeply impressed, even intimidated, by Dirac's powers. And Weisskopf was Pauli's assistant at the time when they were focused on understanding Dirac's successful prediction of antiparticles.

I yield to no one in my admiration for Pauli, but it is unacceptable to denigrate Dirac while celebrating Pauli's enormous contributions. The authors say that, in creating the relativistic wave equation, "Dirac repeated Pauli's trick" of doubling the number of components of the wavefunction to incorporate spin. The Pauli two-component equation was an elegant restatement of what was already known about incorporating spin, as can be seen in the earlier calculation of the hydrogen fine structure by Heisenberg and Pascual Jordan. The discovery of the Dirac equation was one of the greatest achievements in the history of physics.

The authors' attribution of Enrico Fermi's golden rule to Pauli is also miscast; it was Dirac who developed time-dependent perturbation theory, including this formula, to calculate radiative transitions with his other great invention, the quantized radiation field. More than 20 years later, Fermi, in his Chicago lectures, called the formula a golden rule, and many physicists, with their habitual disregard for history, have ever since attributed it to Fermi.

KURT GOTTFRIED
(kg13@cornell.edu)
Cornell University
Ithaca, New York

The article on Wolfgang Pauli is very good, as one would expect from such a Pauli scholar as Karl von Meyenn. I have only minor comments.

The general relativity argument Niels Bohr used against Albert Einstein is from the 1930 Solvay conference, not from the one in 1927.

I don't think Pauli introduced vacuum degeneracy (the authors do not provide a reference). The concept appears elusively in Werner Heisenberg's unsuccessful attempt at a unified field theory of elementary particles (1953, 1958); Pauli collaborated on some stages of the theory. Vacuum degeneracy was not really understood until the advent of Goldstone's theorem in 1961.

P. A. M. Dirac's comment on explaining "much of physics and the whole of chemistry" does refer to quantum mechanics as a whole, not to Erwin Schrödinger's first paper on

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wave mechanics (27 January 1926).

The authors say Pauli "beat Schrödinger to the theory of the hydrogen atom." Although this statement is chronologically not incorrect (Pauli's paper was sent in 17 January 1926; Schrödinger's later that same month), it is misleading: Pauli obtained the H-atom spectrum by matrix mechanics, actually beating Heisenberg, who had never tried using his newborn matrix mechanics to solve for the hydrogen spectrum. And Pauli also beat Dirac, who solved the problem again (in two dimensions!) using matrix mechanics, five days later than Pauli. Schrödinger, of course, presented the wave-mechanical solution in a completely independent development.

The authors also say Pauli "discovered nuclear magnetism," but this is an overstatement. In 1924, Pauli published a paper suggesting that the hyperfine structure in sodium was due to the nuclear spin. People tend to consider Pauli to be the first who suggested that the nuclei should carry spin, but the first calculation of the hyperfine structure (for hydrogen) had to wait until Enrico Fermi's work (1930).

The article goes on to state, "In papers with Heisenberg and Pascual Jordan, Pauli introduced relativistic quantum field theory." This sentence is too short to appropriately establish the origins of quantum field theory. Quantum electrodynamics started with Dirac's 1927 paper on the quantum theory of radiation. In 1928, Jordan and Pauli established the fully relativistic commutation rules for the electric and magnetic fields in vacuum. Incidentally, Jordan is the unsung hero of the quantum theory of fields. But the modern covariant quantum theory of fields truly started with the two papers coauthored by Heisenberg and Pauli in 1929 and 1930.

The respective roles of Heisenberg and Pauli in the crucial years around and after 1925 are still a matter of discussion. I do not agree with the authors' opinion that "much of Heisenberg's work was inspired by Pauli's ideas." It is clear that Heisenberg sent many of his papers to Pauli prior to publication, in particular the 1925 matrix mechanics one and the uncertainty papers of 1927. But it is equally clear that most of Heisenberg's ideas were his own. When the draft earned Pauli's approval, Heisenberg happily sent it for publication. And when the two men got stuck, as in the naive application of quantum rules to the electromagnetic field, it was Heisenberg who eventually solved the riddle. The same is true with the anomalous Zeeman effect and the helium spectrum, monumental works done essentially by Heisenberg alone (although Jordan collaborated in the Zeeman paper).

I think the statement that Pauli and Heisenberg "were the phenomenologists par excellence" and "felt themselves to be the real physicists" is unfair and vague. True, both were conversant with the latest in theory and phenomenology, but so were others. Also, it is well known (and I have personal testimony) how much Heisenberg appreciated Dirac and Jordan, to mention just two; the same is true of Pauli.

LUIS J. BOYA
(luisjo@posta.unizar.es)
University of Zaragoza
Zaragoza, Spain

SCHUCKING AND VON MEYENN REPLY: Yes, indeed, it was at the Solvay Conference in 1930 that Bohr reminded Einstein of his elevator. How the wrong date crept into our manuscript baffles us still.

Kurt Gottfried is surprised that Pauli thought of Dirac as a formalist. Dirac was a formalist. He wrote in 1931: "The most powerful method of advance that can be suggested at present is to employ all the resources of pure mathematics in attempts to perfect and generalize the mathematical formalism that forms the existing basis of theoretical physics, and *after* each success in this direction, to try to interpret the new mathematical features in terms of physical entities."[1]

We disagree with Gottfried's characterization of the Pauli equation for the electron as merely an elegant restatement of a Heisenberg–Jordan paper. Paul Ehrenfest wrote on 25 March 1931 about the Pauli equation: "This work has become very important. It has opened the way to Dirac's ingenious work on the spin-electrons and probably also to Heisenberg's famous work about the helium spectrum."[2] Pauli introduced *spinors* into quantum mechanics and thus—in hindsight—made the step from two to four components not such a giant leap.

We agree with Gottfried that the golden rule[3] is due to Dirac. When Pauli derived this equation using Dirac's time-dependent perturbation theory, he analyzed its domain of validity using the uncertainty relation for time and energy that he had first pointed out in a letter to Heisenberg. Apparently, Pauli and Heisenberg[4] had first thought that Dirac had "gemogelt" (cheated) in its derivation.

We both cherish Dirac's beautiful physics, but his approach to theory was different from Pauli's or Heisenberg's. By praising Galileo, one does not demean Kepler.

On Dirac's comment about Schrödinger, we stand corrected. Luis Boya also may have a point on vacuum degeneracy. But Pauli's crucial role in the Heisenberg theory still needs clarification. There are 144 letters from the correspondence between Pauli and Heisenberg in 1957 and 1958, up to 30 pages in length, which still await analysis.

With regard to the early history of quantum field theory, we agree that much more could be said about it, and we refer Boya to Tian Yu Cao.[5] For the history of nuclear magnetism, we suggest that Boya might consult Hendrik Casimir.[6]

Boya remarks that Heisenberg "never tried using his newborn matrix mechanics to solve for the hydrogen spectrum." On 3 November 1925 Heisenberg wrote to Pauli: "After your first letter, I had also tried at once the calculation in three dimensions and was busy just this afternoon with the evaluation of the last equations and the elimination of the many possible mistakes in calculation when the postman entered and made my work superfluous."[7] The postman had brought Pauli's three-dimensional calculation for the H-atom. We believe that Boya underrates the role Pauli played in the dialogue with Heisenberg and that a careful reading of their correspondence supports our view. Freeman Dyson, with his unique insight into physics and the ways of physicists seems to agree with our assessment.

Finally, we thank Gottfried, Boya, and Dyson for their comments.

References

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