PAULI and NUCLEAR SPIN

Wolfgang Pauli and Paul Ehrenfest in deep conversation on railway ferry while traveling to a meeting in Copenhagen in April 1929.

By **S. A. Goudsmit**

THERE seems to be an increasing interest in the history of physics, especially of the physics of the roaring twenties when modern atomic structure and quantum mechanics were born. Writing about so recent a period, with most of the participants still alive, can lead to numerous unpleasant disagreements. It would be most valuable if the principal actors would each tell their own story, just as generals are now writing about their own world war. It must be understood that such stories relate only subjective experiences mixed with uncertain inferences. These accounts can be extremely interesting, but they should not be considered as an objective history of the new physics and should be written in the first person singular. An objective history of science is perhaps only possible in very broad outlines and in its relation to the rest of history. When we try to look at a recent event with a microscope, the resolving power may often be insufficient. We would like to see clearly the lines of thought followed by the participants, but even they themselves may not be aware of why and how they arrived at their important results, and they may tell us instead what are merely rationalizations after the fact. It is rather easy to construct a history which gives a very plausible account of how new ideas and experiments could or should have developed, but it seems impossible to know how they actually arose, particularly when we ask about details.

One such difficult detail is the assignment of proper "credit" to the various participants for their contributions. Scientists are very sensitive about this because recognition among their colleagues is the principal, and often the only, reward they get for their work. Thus, just as with the memoirs of generals, it may be unavoidable that individual recollections of physicists will cause ruffled feelings. This is especially so since, even in technical papers, authors often lapse into the habit of quoting primarily the greater names, while lesser contributors are left unmentioned even though their work may have been a link in the chain of developments. A historian once told me that even Whittaker's *History of the Theories of Aether and Electricity*,[1] which has the appearance of being an objective enumeration of facts, contains highly questionable implications, such as the giving of more credit to Lorentz and less to Einstein than each deserves for the theory of relativity. Such considerations are of no importance to the progress of physics, but they are significant to the people concerned who are still alive. How much simpler it would be if advances in physics were made anonymously, like ancient Egyptian achievements in art. In fact, most of the more significant data and theories of physics are in the public domain. They are regularly applied without mentioning the originators, as if they were truths revealed in Genesis. With the rapid development taking place now, a young physicist may find his original work used extensively by others without any reference to his articles after they are a year or two in the past.

A recent book, *Theoretical Physics in the Twentieth Century*,[2] which is primarily devoted to the work of the late Wolfgang Pauli, contains a series of articles which range in form all the way from interesting, subjective, first-person accounts to a useful enumeration of Pauli's publications in physics. It contains also an article written by an author who tries to "interpret" history and apparently attempts to read Pauli's thoughts from published papers and from some old and new correspondence. That such an analysis is

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neither objective nor useful is borne out, for example, by one impression given by that article—namely, that the concept of electron spin was, in 1925, one of the most stupid ideas in physics and that therefore all credit and praise should go to those who were then against accepting the hypothesis. Most unfortunately, we do not have Pauli's full account of those years in physics, nor do we have the accounts of several of his prominent contemporaries.

Another problem confronting the historian is that of evaluating the relative merits of the various contributions made by a scientist. Here too the general opinion may not always coincide with the scientist's own evaluation of his publications. Many physicists have a special liking for one of their papers, often one which has hardly been acknowledged by their colleagues. Pauli had such a paper and it is regrettable that in the above-mentioned book it merely rates a footnote in the bibliography. Since I too have such a paper I can sympathize with Pauli's annoyance at the lack of recognition for one of his favorite brainchildren. Moreover, I feel partly responsible for the obscurity of that interesting article and I therefore lapse into the first person singular to tell my side of the story.

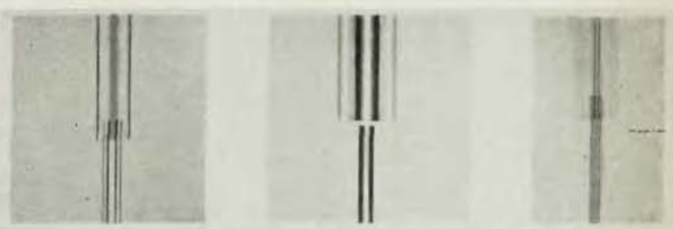
Friedrich Paschen (at left) as seen in 1927 photograph taken by Ral Sawyer in garden of the Physikalisch-Technische Reichsanstalt (t German Bureau of Standards). At right is Ernst Back standing besi the luxury express "Rheingold" which took us to Amsterdam. V traveled second class since it had no third- or fourth-class coach [Except for the picture of Paschen (above) and the snapshot of Gou smit and R. F. Bacher (p. 21), all photographs were taken by the autho

FOR a number of years, whenever I met Pauli, he would remark cryptically that he "could afford not to be quoted". It was only in the late thirties that I found out to what he referred. In the fall of 1926 I went to Tübingen, that Mecca of spectroscopists, on an International Education Board Fellowship. I had no prepared program and arrived there not knowing what to do and probably hoping to work very little. I had visited Tübingen before, the first time on a hot summer day in 1921. On that visit Friedrich Paschen himself showed me the fine structure of the ionized-helium line 4686A, the key to Sommerfeld's relativistic treatment of electron orbits. The experiment was set up in one of the laboratory rooms and Paschen tried to explain all details. I was successful, I hope, at hiding my complete ignorance.

Only after my return to Leiden did I learn that the spectral line I had seen was one of the wonders of the world. At that time I did not yet worry about one of its fine-structure components, which was visible against the predictions of the theory and which later became in my mind a strong argument for the electron-spin hypothesis. The great Paschen had treated me as a fellow physicist and not as a beginning student, which must have influenced my desire to continue in spectroscopy. (Do any of the present-day young physicists know who he was and how *important* his work was as a basis of modern physics?)

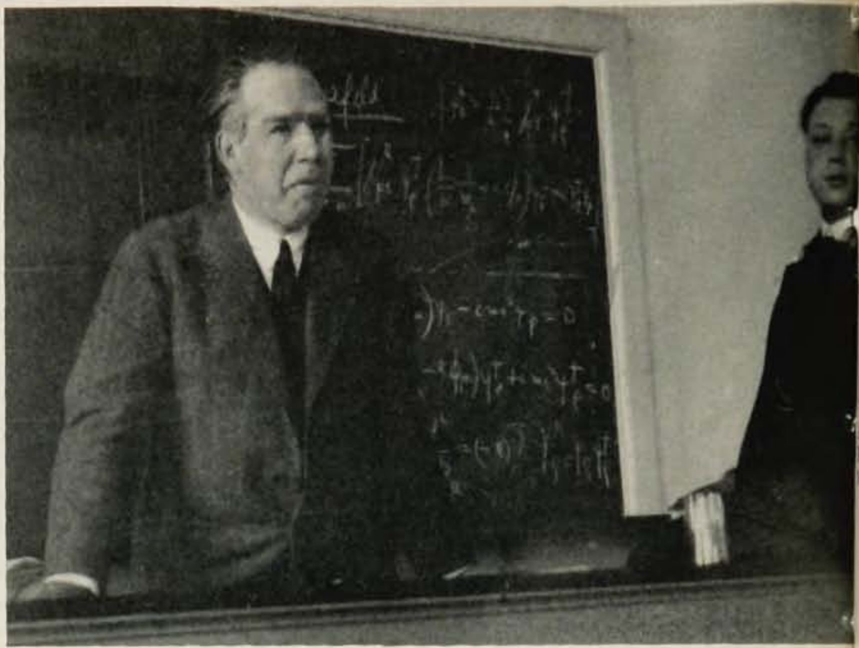
In the summer of 1923 I spent a few months trying to learn from Paschen the art of spectroscopic observation and from Ernst Back the techniques of the Zeeman effect. Thus I was well at home in that laboratory and not surprised, on returning in 1926, when Back immediately showed me some strange Zeeman-effect photographs which he had accumulated over a few years without being able to analyze them. These were the Zeeman patterns of lines in bismuth. Bismuth lines were known to have a hyperfine structure, not understood at the time. I had seen a short article by G. Joos[3] in which he pointed out that for one of the lines this structure looked like a miniature spectroscopic multiplet and he derived a fine structure for two energy levels. We found that a similar result had been obtained for two more lines by Ruark and Chenault.[4] Guided by this knowledge it was possible to obtain the hyperfine structure of many bismuth levels from material collected in Back's observations and earlier measurements by Nagaoka and Mishima.[5]

Our first paper on the subject appeared in the *Zeitschrift für Physik*[6] early in 1927. We devoted most of our attention in the article to a determination of the energy levels of bismuth, confirming earlier work of Thorsen[7] and showing that in this case the observed



Hyperfine structures (bottom) and Zeeman patterns (top) like these (bismuth 4122A, 4722A, and 3511A) had been predicted by Pauli in 1924. These figures were published in 1925 on a plate appended to the book by E. Back and A. Landé on the Zeeman effect. The structure was not explained in the text.

Professor Zeeman wanted to take my picture. He had no Leica.



Professor Bohr and Pauli simultaneously answer a question from the audience. Copenhagen, 1929.

hyperfine structure could be used in place of the Zeeman effect. We only hesitatingly stated that the hyperfine structure in bismuth might be caused by a nuclear angular momentum. However, in a footnote added in proof, which was written after we had succeeded in analyzing a couple of the complicated Zeeman-effect patterns, we claim that the bismuth hyperfine structure must be ascribed to a nuclear angular momentum of $9/2\ \hbar$.

A second paper,[8] which contained the Zeeman analysis and the confirmation of the nuclear angular momentum, was not finished until December 1927. I had left Tübingen in April and spent some time in Copenhagen, discussing these problems with many colleagues —especially (as I now see from a note in the article) with Professor Bohr and Linus Pauling. Next I paid a short visit to Göttingen and finally returned to Leiden before my departure for Michigan in September 1927. The paper appeared early in 1928.

WHAT has all this to do with Pauli? Two years earlier Pauli had published an article in *Die Naturwissenschaften*[9] in which he described and predicted exactly the kind of observations Back and I had analyzed. He stated clearly that such data would indicate whether the atomic nucleus possessed an angular momentum. An even more decisive statement was made by Pauli in the final paragraph of one of his famous *Handbuch* articles[10] in the 1925 edition, in which he too calls attention to the analyses by Joos and Ruark and Chenault. We had completely overlooked Pauli's most significant pioneering contribution to this new field which introduced nuclear physics into spectroscopy. None of the many colleagues in various places whom I had enthusiastically told about the Tübingen work had called his paper to my attention. A letter in *The Physical Review*[11] on some theoretical aspects of the problem written with Robert F. Bacher more than two years later does not mention Pauli either.

It was not until the middle of 1930 that I learned of Pauli's article. I do not remember how and where, but Linus Pauling and I give a reference to it in our book, *The Structure of Line Spectra*.[12] In the spring of 1929 I returned to Tübingen to complete the work on the analysis of the available Zeeman-effect patterns. Since the very narrow fine structures could not be reproduced successfully in print, Back and I traveled to Amsterdam where we were allowed to make recordings on the photometer in Professor Zeeman's laboratory. As was sometimes customary in those days, we

A 1929 Copenhagen discussion: N. Bohr, W. Pauli (rear view), L. Nordheim, E. Fues, L. Rosenfeld.

Bob Bacher (left) met me (right) at the pier in New York to get the latest news on hyperfine structure.

David Dennison (right) as he appeared not long after he postulated the spin of the proton.

asked Zeeman's permission to add his name as an author to one of the papers. The work was not finished until after my return to Michigan. The results appeared in two consecutive articles in the *Zeitschrift für Physik*, the first [13] by Zeeman, Back, and myself, with an appendix by Back and by John Wulff from MIT, who was then in Tübingen on a National Research Fellowship. The second paper [14] was by R. F. Bacher and me. In the first of those papers we finally mentioned Pauli's article, but did not stress its great importance.

Pauli took a mild revenge in his Nobel Prize lecture [15] in 1945. There he discussed his 1924 paper and stated that his interpretation of hyperfine structure was "definitely confirmed experimentally by investigations in which also Zeeman himself participated"—without a reference and omitting the names of the real participants! But Pauli also asserted that his 1924 article "influenced Goudsmit and Uhlenbeck in their claim of an electron spin". As we were not aware of his paper until five years later, this is obviously not so. Moreover, Pauli's article can definitely not be considered as introducing the concept of spin, which is the property of a *single* particle. He explicitly states that one may expect a nonvanishing total angular momentum to occur for complex nuclei; he did not predict a hyperfine structure for the hydrogen spectrum. This answers Ehrenfest's question,[16] when he wondered why Pauli had opposed the electron-spin hypothesis while a year earlier he himself had proposed the idea of nuclear spin. Pauli's own answer is unfortunately not recorded. The spin of the proton was, in fact, not discovered until 1927. It was arrived at from an entirely different approach by David M. Dennison [17] of Michigan in his brilliant explanation of the anomalous behavior of the specific heat of molecular hydrogen. The present generation of young physicists may be surprised to learn that the proton spin had to be discovered at all.

I have now set the record straight by giving one more proof of Pauli's deep insight and by adding an item to the list of his great achievements. I hope that in the future both Pauli and Dennison will be given proper recognition for their pioneering discoveries in nuclear spin. This is the end of my story [18] and I am even more convinced that this and similar microhistories of physics are in themselves of doubtful value, except perhaps as source material for a future picture of the history of physics, painted with broad strokes of the brush.

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