Personal memories of Pauli

Looking back half a century, a disciple of Wolfgang Pauli recalls how Pauli's extreme honesty and directness expedited work on fundamental problems in quantum mechanics and made for unusual human relations.

Victor F. Weisskopf

We older disciples of the great Wolfgang Pauli remember with pleasure and nostalgia the prewar years when we had the privilege to work with him. It was one of the most interesting, stimulating and productive periods in physics. At the same time, however, Europe saw events that were among the most terrible and depraved actions of man against man. The coincidence in the history of humankind of the greatest achievements and the worst evils has always impressed and depressed me deeply. As Dickens said: "It was the best of times, it was the worst of times."

Let me begin my tale a little earlier. In 1932 I received a Rockefeller grant for one year, to study at places of my choice. I wanted to divide my time between Copenhagen and Cambridge, England, to learn from the two great men Niels Bohr and Paul A. M. Dirac. In Copenhagen, I not only profited greatly from Bohr's overwhelming personality—everyone who spent some time with him was deeply influenced by his way of thinking and living—but I also met my wife there, so the division of my Rockefeller time was somewhat biased toward Copenhagen.

My stay in Cambridge was also very important to me, but not so much because of Dirac. It was not easy to learn from him; he worked for himself and did not have much contact with other physicists or would-be physicists. It was in Cambridge that I met Rudolf Peierls, who also had a Rockefeller grant. He was a few years older than I, and I learned much from him. Two or three years are not much, but they make a big difference when one is young and, sometimes, at the end of one's life. He introduced me to relativistic field theory—how to make calculations with the Dirac equation, a skill that was referred to as "alpha gymnastics."

Peierls's stipend was $200 a month, whereas, to my slight annoyance, mine was only $150. I hasten to add that either amount represented at that time undreamed-of riches for an average European. The reason for the difference was that he was married, whereas I was only on the way to be. The Rockefeller Foundation asked us to send in reports on our achievements during the time of support. As proof of some of his activities, Peierls sent the foundation an announcement of the birth of his first child; of course, I was unable to match this. The officials in New York didn't appreciate Peierls's sense of humor, we heard.

How to deal with Pauli

At that time—it must have been May or June of 1933—came a letter from Pauli asking me to be his assistant in Zurich, replacing Hendrik Casimir, whom Paul Ehrenfest, shortly before his suicide, had called back to Leiden. What could be better for a young physicist than to work with Pauli? It was the fulfillment of a dream. Why did he take me and not other, more experienced people such as Hans Bethe? I found out later.

Now I had something else to learn from Peierls, who had been Pauli's assistant before. Of course, I asked him about his experiences. He said, "It is a great thing, but you must be prepared." Hence, Peierls taught me not only quantum electrodynamics, but also "how to deal with Pauli." He gave me much good advice; here is one example: "Be very careful when you give a talk at the Zurich colloquium. Pauli likes to interrupt a speaker when he thinks he is wrong or inconsistent. The best method to counteract this is: The day of your talk go to Pauli in the morning and tell him what you are planning to say. If he does not like it he will tell you in the strongest terms how silly it is, that it is all wrong or that it is trivial and known to every child, etc., etc. Then in the afternoon at the colloquium," continued Peierls, "you say exactly what you intended to say in the first place. You don't need to change anything, except if you really have been convinced by him. Pauli will sit in the first row, and when you come to the critical points, he will almost inaudibly

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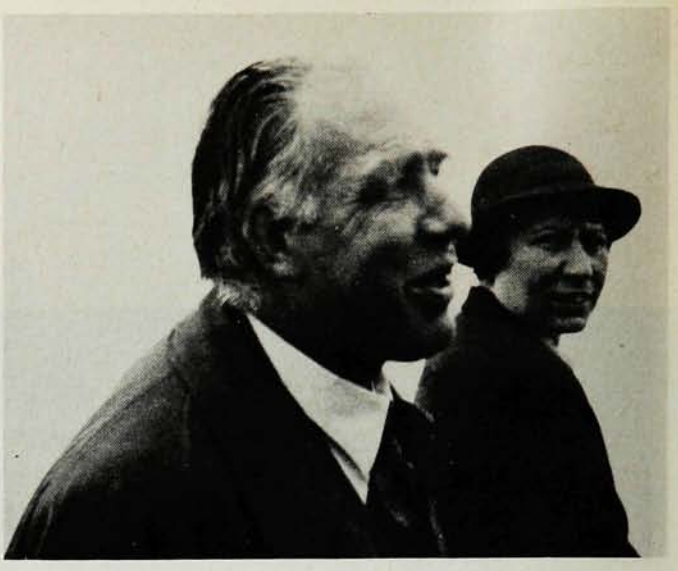
Arnold Sommerfeld (left) with Wolfgang Pauli at an October 1934 metals conference in Geneva. (CERN photograph, courtesy AIP Niels Bohr Library.)

mumble to himself: 'I've told him already, I've told him anyway!' So it won't be so bad at all."

I found out why[1] Pauli took me instead of Bethe when I came to Zurich to begin my duties in the fall of 1933. I knocked several times at the door of Pauli's office until I heard a faint "Come in." I saw Pauli at his desk at the far end of the room and he said, "Wait, wait, I have to finish this calculation" (*Erst muss ich fertig ixen*). So I waited several minutes. Then he said, "Who are you?" "I am Weisskopf, you asked me to be your assistant." "Yes," he said, "first I wanted to take Bethe, but he works on solid-state theory, which I don't like, although I started it." This, then, was the reason.

I made a contract with him. I said, "Of course, I am more than delighted to work for you but, please, that new stuff you are working on, the Klein–Kaluza approach to general relativity, that I am unable to understand. I don't want to deal with it, but I am ready to work on everything else." He accepted the conditions because he was already somewhat bored by it. (Today it seems to be the *dernier cri* of the most sophisticated particle theorists.) Pauli then gave me some problem to study—I have forgotten what it was—and after a week he came and asked me what I did. I showed him and he said, "I should have taken Bethe after all." I was well



Niels Bohr and Franca Pauli in 1936, at a conference at the Bohr Institute in Copenhagen. (Photograph by P. Ehrenfest Jr, AIP Niels Bohr Library, Weisskopf Collection.)

prepared by Peierls for events like this and I took it for what it was: a challenge to get a deeper understanding of physics.

The numerous Pauli anecdotes circulating among physicists give a distorted impression of Pauli's personality. He is seen as a mean character who wanted to hurt his weaker colleagues. Nothing is further from the truth. Pauli's occasional and highly publicized roughness was an expression of his dislike of half-truths and sloppy thinking, but it was never meant to be directed against any person. Pauli was an excessively honest man; he was of an almost childlike honesty. What he said were always his true thoughts, directly expressed. Nothing is more reassuring than to live and work with somebody who says everything that is on his mind—but you must get accustomed to it. Pauli did not want to hurt anybody, although he sometimes did, without intention. He disliked half-truths or ideas that were not thought through, and he did not tolerate talking around a half-baked idea. He was, as many people said, the conscience of physics. He wanted people to understand things thoroughly and express them correctly. He never tired of answering questions and explaining problems to anybody who came asking. He was not a good lecturer before an audience because he did not have the ability to judge how much the crowd could take in, and his listeners did not often dare to interrupt him with questions. Once a student did so and said, "You told us that conclusion is trivial, but I am unable to understand it." Then Pauli did what he did frequently when he had to think things over during a lecture: He left the room. After a few minutes he came back and said: "It is trivial!"

When you came to him and said, "Please explain this to me, I don't understand it," he would explain it with great patience and pleasure. We often said, "For Pauli every question is stupid, so don't hesitate to ask him whatever you want." He loved simple and illustrative explanations, but they had to be correct, and not misleading. Once his colleague in experimental physics, Paul Scherrer—an excellent lecturer and a lover of simple conclusions—came to him and said: "Look, Pauli, I would like to show you how I explained that effect in my course. You see, here the spin is up, and there it is down and then they interact, . . . isn't that simple?" Whereupon Pauli answered: "Simple it is, but it is also wrong!"

Pauli loved people and showed great loyalty to his students and collaborators. All of Pauli's disciples developed a deep personal attachment to him, not only because of the many insights he gave us, but because of his fundamentally endearing human qualities. It is true that sometimes he was a little hard to take, but all of us felt that he helped us to see our weaknesses. Ehrenfest expressed it well after J. Robert Oppenheimer came to him as a young postdoc in the late 1920s. Ehrenfest was unhappy because Oppenheimer always gave quickly an answer to any question, and Ehrenfest felt that the answer was not always correct but was unable to reply fast enough. So he wrote to Pauli: "I have here a remarkable and intelligent American but I cannot handle him. He is too clever for me. Couldn't you take him over and spank him morally and intellectually into shape?" (*Zurecht prügeln*). We all were spanked into shape by dear Pauli and we loved it.

There was one person to whom Pauli acted quite differently. When Arnold Sommerfeld, his former teacher, came to Zurich for a visit, it was all, "Yes, Herr Geheimrat, yes, this is most interesting, but perhaps I would prefer a slightly different formulation, may I formulate it this way. . . ." It was much fun for us victims of his aggression to see him well behaved, polite and subservient; a completely different Pauli.

Calculations and publications

There were not many regular duties for Pauli's assistants to perform. Pauli himself made up the homework problems for his course, and we assistants only had to grade them. Our main duty was to be ready for discussion of his work and of new developments. He took this very seriously and it was not easy to get permission to leave Zurich during the term. Once I asked him with great trepidation, "May I go to Copenhagen for a week?" "Why?," he asked impatiently. I answered, "I intend to marry and come back with my wife." To my great relief he replied, "I approve of that, I am going to get married also!" (*Ich heirate nämlich auch*).

Pauli's assistants had another pleasant duty. Pauli's wife asked him to change his eating habits because of his proverbial bulk. But Pauli loved sweets and cakes, and many afternoons he wanted to continue our discussions in a nearby *Konditorei* where one could get delicious pastries. One of my duties was to promise never to mention these secret outings to his wife.

Of course there were also more serious duties. A lively correspondence was taking place between Werner Heisenberg and Pauli about the problems of quantum electrodynamics. Some of these problems, such as the unavoidable infinities, were quite serious and were solved only much later. Many of them, however, could be straightened out at the time. Whenever a letter came from Heisenberg, Pauli discussed it with me and frequently asked me to draft an answer: "You write it, I will correct it and then we will send it to

Paul A. M. Dirac and Rudolf Peierls (right) with Pauli (center). (AIP Niels Bohr Library.)

him." A letter once came from Heisenberg and Pauli was terribly dissatisfied with its content: "Such silly statements; it is all stupid and wrong. You must tell this to him in your letter!" What could I do? Well, I started out explaining our disagreements as well as I could, and then I quoted Leporello in Don Giovanni: "My master wants to tell you, myself I would not dare to." Then I was able to repeat literally all of Pauli's curses.

It was the time of an unhappy episode in my career as a physicist. Pauli asked me to calculate the self-energy of the electron on the basis of the positron theory to see if this energy is less divergent in that theory. I found that it diverges equally badly and I published this result. A few weeks after the publication I received a letter from Wendell Furry, who worked with Oppenheimer at the time, informing me that I had made a simple mistake of a sign in my calculation. If it is done correctly, the divergence is only logarithmic. The positron theory improved things considerably, in contradiction to my paper. I was down and depressed to have made and published a silly mistake in such a fundamental problem! I went to Pauli and said that I wanted to give up physics, that I would never survive this blemish. Pauli tried to console me: He said, "Don't take it too seriously, many people published wrong papers; I never did!"

What followed shows how decent the relations between physicists were at that time. I asked Furry by letter to publish his result under his name, or, at least, to coauthor a paper correcting the mistake. But Furry was a gentleman. He answered, no, I should publish a correction in my name only and mention him as the person who drew my attention to the error. Since then, the logarithmic divergence of the self-energy of the electron goes with my name and not with Furry's. Yes, times have changed and so have the attitudes of physicists toward publication by others. I remember having shown Pauli a newly published paper on a subject of his interest. He said, "Yes, I thought of that too, but I am glad he worked it out, so that I don't need to do it myself."

Let me now say a few words about the origin of the paper that Pauli and I wrote[2] about quantum electrodynamics with Bose particles. In 1934, I was playing around with the so-called Klein–Gordon equation, which is the relativistic wave equation of particles with zero spin. I was struck by the fact that the wave intensity $|\psi|^2$ is not conserved in the presence of electromagnetic fields, whereas the expression for the charge density is different from $|\psi|^2$ and fulfills the charge conservation laws. I felt there might be something like a lack of conservation of particle number in that equation, and that this might lead to pair creation or annihilation. In spite of what I learned from Peierls in Cambridge I was not able to deal with this problem. It required what was called "second quantization," or the quantization of the wave field, something with which I was not very familiar at that time.

I went to Pauli for help. It was just after his separation from his first wife, and he was in a very bad mood. I tried to explain to him my difficulties and my tentative conclusions, but he was very impatient and repeated over and over how silly my remarks were. Finally I quoted to him a verse from Wagner's opera *Die Meistersinger* that said approximately, "Oh master why so much excitement and so little repose; I believe your judgment would be more mature if you would better listen." He looked up to me and asked, "What is that?" I said it is from *Meistersinger*, whereupon he replied, "Wagner, I don't like at all!" So ended the discussion.

The next day he was in a better mood and I repeated my story. He said: "That's interesting. Why didn't you tell it to me yesterday?" This began a wonderful time for me, when I learned from Pauli in detail how to deal with second quantization. We found that the quantum electrodynamics of spinless particles indeed leads to antiparticles, pair creation and annihilation—and all of it came out without the necessity of filling up negative-energy states with particles, as Dirac did to get these phenomena from his equation. Pauli never liked that trick and frequently referred to our paper as the anti-Dirac paper. Of course, later on it was shown that the trick of filling up negative-energy states was not necessary even with the Dirac equation.

Pauli asked me to calculate the pair creation and annihilation cross sections of spinless particles according to our theory. The calculation was not too different from the one for ordinary electrons and positrons that Bethe and his collaborators had carried out shortly before. I met Bethe at that time at a conference in Copenhagen and asked him to show me how to do the calculations. I wondered how long it would take to get to the final result and he told me, "It would take me a few days, it will take you a few weeks." It did. Moreover, I made a mistake of a factor of four. Again a proof that Pauli should have taken Bethe.

The exclusion principle. Our paper was little more than an interesting formal exercise, because at that time no particles with zero spin were known to exist. However, some years after I left Pauli, he used some of the ideas for his famous proof of the connection between spin and statistics: Particles with half-integer spin must fulfill the Pauli exclusion principle; those with integer spin

Eating. This photo from the 1934 Bohr Institute conference shows Pauli (center) with Gerhard Dieke (right) and an unidentified conferee who wanted to tell his ideas to Pauli. He got the reply, "First I must eat!" (Photograph by P. Ehrenfest Jr, AIP Niels Bohr Library, Weisskopf Collection.)

must have Bose statistics. This achievement shows Pauli in his greatness. He discovered the exclusion principle in 1925 from a careful analysis of atomic spectra and was not satisfied until, fifteen years later, he was able to show how it followed by necessity from quantum field theory.

One day in the course of my work as assistant I came across an interesting note that Pauli had made a couple of years before his discovery of the exclusion principle. It was one of my lighter duties to keep Pauli's collection of reprints in reasonable order. In doing so, I browsed through older papers and found a copy of Bohr's famous paper about the *Aufbauprinzip*, in which he explained the periodic system of elements as an effect of electronic shell structure. Everybody who teaches this wonderful triumph of quantum theory uses the Pauli principle to explain the way in which the electron shells are filled when going from one atom to the next one with an additional electron. However, when Bohr published his paper on the periodic system, the exclusion principle was not yet known! It was a testimony to Bohr's unfailing intuition that he nevertheless got the right results. Browsing through Pauli's copy of that paper, I looked at the page on which Bohr says, "Going from Neon to Sodium, we must expect that the eleventh electron goes into the third shell . . ." and my attention was caught by a remark that Pauli had written in the margin in big letters: "How do you know this? You only get it from the very spectra you want to explain!" Three heavy exclamation marks followed. (What I have just said differs slightly from my earlier account, but is the correct version. See PHYSICS TODAY, August 1970, page 17.) It took Pauli two more years to tell us why.

Moral integrity

Pauli was in Princeton during the Second World War. To stay in Switzerland would have been too dangerous because he was not yet a Swiss citizen; he carried an Austrian passport and was considered a German national after the Nazis took over Austria. The Swiss consented to give him citizenship only after his Nobel prize in 1946. There was some discussion in Los Alamos about whether or not to ask him to join the atomic bomb project. As far as I could find out, he was never asked. I am sure he would have refused to join for many reasons. He had a strong, fundamental aversion to any work connected with weapons, and he would not have felt at ease in a large team. He was a pure character who instinctively would have stayed away from such work. Moreover, nuclear physics never interested him very much, even though he discovered nuclear spin.

When the war was over, Pauli was of great help to all of us. He was asked to give a series of lectures about the latest ideas in particle theory. He brought us back to fundamental physics. Shortly afterward he returned to Zurich. He wanted to keep in touch as closely as possible with American physics, and he asked me and others to write to him about the newest developments in theory and experiment. I remember a letter of mine reporting Chien-Shiung Wu's preparations to test the conservation of parity in weak interactions. Pauli wrote back that, in his opinion, this was a waste of time; he would bet any amount of money that parity is conserved in any process. When the letter arrived, I had just heard the news that parity was strongly violated. My better self won, and I did not send a telegram saying, "Bet for $1000 accepted," but reported to him the surprising result of Wu's experiment. Overseas telephone calls were not yet used for physics. Pauli was completely flabbergasted. He wrote back expressing his astonishment that "God is a weak left-

Hans Albrecht Bethe. Because of Pauli's lack of interest in solid-state theory, Weisskopf got an assistantship that Pauli had considered giving to the more-experienced Bethe, who had done work in that field. (AIP Niels Bohr Library.)

Hendrik Casimir (left) with Weisskopf, at the 1934 Bohr Institute conference in Copenhagen. (Photograph by P. Ehrenfest Jr, AIP Niels Bohr Library, Weisskopf Collection.)

hander" and added: "I am glad that I did not conclude our bet. I can afford to lose some of my reputation but not some of my capital."

Pauli mellowed much in his later years. This was mainly due to his second wife Franca, who was able to make his life bearable and even pleasant. This was not an easy task. Pauli had a very difficult character, was easily depressed and often felt thoroughly unhappy. Franca succeeded in creating a comfortable and protected home for him, in which he could feel at ease and pursue his many interests that reached far beyond physics.

He became very interested in various forms of mysticism, mainly through his connection with the Swiss psychologist Carl Gustav Jung. Later he developed a deep friendship with Gershon Scholem, the great scholar and world authority on Jewish mysticism, the Kabalah. (The Kabalah ascribes a number to each word of the Hebrew language, a number that has a deep symbolic significance. The number corresponding to the word Kabalah happens to be 137.) Pauli and Scholem saw each other frequently and exchanged their views in letters. With a few notable exceptions, Pauli rarely spoke about this side of his interests to his physicist friends. He did not speak much about it with me, except that he urged me to visit Scholem when I went to Jerusalem. It was a unique experience to meet a great man and to be introduced to ideas that are so alien to those of our science.

Pauli's interest in these different avenues of human experience was in many respects a natural expansion of his involvement in modern physics. He was a disciple of Bohr—perhaps Bohr's closest disciple. Bohr often applied his concept of complementarity to human concerns beyond natural science. The rational scientific approach is only one way of dealing with the world around us. There are other, seemingly contradictory approaches—as contradictory as the particle and wave pictures within physics—that deal with aspects of our thoughts and emotions. A given approach seems fragile and senseless when analyzed within the framework of another, but is forceful and convincing within its own frame. Pauli was very much attracted by this generalization of complementarity.

Pauli created a style of theoretical thinking and research that influenced physics all over the world. It is a style that emphasizes the essential roots and the symmetries of the laws of nature in their mathematical form without much talk or handwaving. His clean way of thinking and working appears to all of us as an ideal to be emulated. We often ask ourselves, "What would Pauli say to this?" We often come to the conclusion, "Pauli would not accept that."

However, Pauli set his example through more than the character of his work. He personified the striving for utmost clarity and purity in science and human relations. We owe it, in part, to Pauli that in the community of physicists there is still a certain amount of healthy simplicity, honesty and directness, in spite of all the politics, publicity and ambitions, attitudes that were so foreign to Pauli. He was not only a physicist, he was also a great personality, able to see deeper than others into scientific and human problems. The dark riddles of the human psyche were not unknown to him. He is an example to all of us of how to live a quiet and contemplative life of intellectual and moral integrity in these unruly times.

References

1. I have told the story that answers this question before; however, an author is allowed to republish his own results. See V. Weisskopf, *Physics in the XX Century*, MIT Press, Cambridge, Mass. (1972), p. 1.
2. W. Pauli, V. F. Weisskopf, Helv. Phys. Acta **7**, 709 (1934). □