feature article

Stories from the early days of quantum mechanics

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A colloquium delivered to the University of Toronto physics department on 5 April 1979 by the master of molecular beams offers a fresh look at an earlier era.

In 1979, **I. I. Rabi** (1898–1988) was the Emeritus University Professor of Physics at Columbia University in New York City. **Fraser Code** is an emeritus professor of physics at the University of Toronto in Canada.

I have something in common with Ernest Rutherford, that distinguished physicist and professor at Canada's McGill University, who deplored the fact that, although a physicist, he got a Nobel Prize in chemistry. My career is the opposite. I started at Cornell as a chemist, and got a degree of bachelor of chemistry, which has since been discontinued. So I'm an orphan like the DeSoto, one of those cars that are no longer manufactured.

Anyway, after some years in which I tried various things that broadened my education but did not line my pocketbook, I went back to Cornell to study physical chemistry. But I'd taken all those courses so I said to myself "I'll study physics, and put the two together."

You know, that is somewhat like the person who wanted to study Chinese philosophy, so he looked up Chinese in the encyclopedia, and then he looked up philosophy, and finally tried to combine them.

But for me, when I started studying physics, I realized that the part of chemistry I liked was called physics. So that was the beginning of my career, and I entered the subject of physics more seriously around 1922.

Learning quantum mechanics in America

The year of 1922 was very significant. In fact, that whole time from the early twenties onward was a period of great ferment in physics, enormous ferment, all over the world—by which one means Denmark, England, France, but not the United States.

I remember one time when I was a graduate student at Cornell, sitting in the library amongst the students, just before the time when Professor [Arnold] Sommerfeld was to come and visit. And you could see one professor after another sneak in and take a look at Sommerfeld's book *Atombau und Spektrallinien* (*Atomic Structure and Spectral Lines*, Friedr. Vieweg & Sohn, 1919). That was all the exposure they had to the quantum theory. That was 1922 in America. By contrast, in Europe, quantum theory had been extant for quite a number of years. But in America, it had not yet achieved full recognition as something suitable for graduate study at Cornell, or for that matter at Columbia [where Rabi completed his PhD]. I'm not even sure that quantum theory was working very well here at Toronto in 1922!

Anyway, the faculty in America wasn't very much concerned with quantum physics, except experimentally. But at Columbia, a number of graduate students formed a weekly discussion group that we called a "Sunday soviet," by which I mean that we met every Sunday near 11 o'clock in the morning, and went on right through a Chinese dinner.

We learned a great deal just by ourselves. I'd recommend this method of learning to all the graduate students in this audience: If any of the faculty are deficient in some subject that interests you, just form a little soviet and do it on your own. As a matter of fact, it worked so well that when the Austrian physicist Erwin Schrödinger's paper first came out,[1] we read it and worked through all the equations.

Then, just as an exercise, Ralph Kronig and I decided to do something with this new thing, Schrödinger's quantum theory. So we looked through [Max] Born's book[2] and found that the symmetrical top problem had not yet been done. So we sat down and, according to Schrödinger's prescription, formed the wave equation, separated the variables, got the angular momentum, as well as the various states, but then we ran into an equation that we didn't know how to solve.

And here's another lesson that I want you to hear from my own experience. Somehow or other after that Sunday soviet, I was sitting in the library reading the mathematical works of Carl Jacobi, who wrote beautifully in German. I understand that German is no longer required for graduate students here. Too bad, because in reading through that book, suddenly there appeared my equation—the one Kronig and I could not solve. It was the equation for the confluent hypergeometric series, which neither of us had ever heard of before. Using this reference, we were then able to solve the quantum mechanical problem of the symmetrical top molecule.[3]

But we did not have the faintest idea what the wavefunction ψ meant. It was a magical thing. What you got when you followed this prescription, as Schrödinger had done for the hydrogen atom, were the eigenvalues of the differential equation. These were the energy levels, which agreed with experiment. But we had no idea what the wavefunction was—what was this magic function ψ?

Of course, it became clear soon thereafter when Born[4] and others suggested that $|\psi|^2$, the absolute value of ψ squared, represented the probability density for finding that particular thing at that particular place. Suddenly the wave function ψ acquired a great meaning.

COLUMBIA UNIVERSITY

Michael Pupin (1858–1935) at Columbia University, probably in the late 1920s. Pupin and I. I. Rabi were part of a small group at Columbia that was trying to figure out quantum mechanics in 1926. (Courtesy of AIP Emilio Segrè Visual Archives.)

But it was so magical, that function ψ. You simply followed the formula, and out came real results. This was not a surprise. During the first period of its existence, quantum mechanics didn't predict anything that wasn't also predicted before by the old quantum mechanics plus that very magical abracadabra of the correspondence principle.

There were real artists at work on the correspondence principle. For example, they were able to deduce many things from the Kramers–Kronig formula, or from the Kramers–Heisenberg dispersion formula. The development of physical relationships from the correspondence principle was all done by artistry, by imagination, and from certain kinds of symmetry ideas. So the results that came out of quantum mechanics had to a large degree been previously anticipated from this correspondence principle.

But a very unfortunate thing happened to John Van Vleck, who wrote a remarkable book on the old quantum theory.[5] It was a wonderful book, a clear book, and he was a master. However, it was published and came out just at the time of the revolution in quantum mechanics. Unfortunately, it became obsolete almost on publication! The same was true with Wolfgang Pauli's first volume. When the revolution came, it all changed.

Now, it was the new quantum mechanics that was doing things and growing. Matrix mechanics, of course, was in many ways clearer, and in many ways more dense than Schrödinger's equation. But the matrix mechanics of Heisenberg used a different kind of mathematics.

Paul Dirac had been an engineer with a background in mechanics, rather than having been a physicist. So when he followed Heisenberg's first paper on matrix mechanics, he particularly noticed the commutation exchange relationships, and saw a certain parallel between Poisson brackets and the commutation exchange relationships. As a result, Dirac started his approach to matrix mechanics from that direction.

So that was a very great time because we could be the first to do something like the symmetrical top. And we were the first to do this important molecular problem, and just as graduate students! It was not for my dissertation, nor was it for Kronig's, but we did learn some quantum theory. While I was a graduate student at Columbia, there were no professors of theoretical physics. I was doing an experimental dissertation, and my supervisor was Professor Albert P. Wills.

In 1926, there was just our little group of serious thinkers, including Michael Pupin, sitting there trying to figure out Heisenberg's matrix mechanics. Schrödinger's formulation, of course, was our favorite. This was clear. It only required that you were familiar with differential equations, and it had a pictorial interpretation. In contrast, Heisenberg's approach involved matrices, which were not difficult but were messy. In addition, there was Heisenberg's use of abstract symbolism, which, of course, looked to us as the most mysterious of all.

And this shows how limited one can be if one is provincial. Because in the United States, as far as theoretical physics was concerned, we were provincial. Definitely provincial.

Visiting quantum physicists in Europe

So the time came when I had finished my dissertation.[6] But there were no jobs around in the US, so I got a small Barnard fellowship to go to Europe. It was $1500 a year for two years, not paying for transportation. And on this my wife and I went to Europe. Well of course, being an American, in many ways I was very naive. The first place I went was to Zürich, Switzerland, where I hoped to work with Professor Schrödinger.

Of course, I hadn't written a note beforehand to make arrangements to come. When I arrived in Zürich, I tried to find a pension [boarding house or small hotel] where I could stay. Afterwards, I went right down to the university, where there was a colloquium going on that afternoon. The man gave a fiery lecture, and I didn't understand a single word. I was very depressed, and I came out full of sorrow for what was going to happen to me. Here I had come all the way over to Europe from America, and now I felt very discouraged. So I looked around in the audience for somebody that I might know.

Well, I did find people in a very definite way. In 1927, the Russian revolution was about 10 years old. And Americans always wore white shirts, but with their collars attached. You could recognize an American anywhere that way. I looked around, and there at the colloquium was a man with a white shirt and collar attached.

He turned out to be Linus Pauling. I told him of my sorrow that I didn't understand what the lecturer was saying. He said "Don't worry, he was not talking German, he was talking Schwitzerdeutsch," which was the local German dialect. I was very pleased to hear that. Later, Linus invited me to where he was staying and gave me a drink. I don't suppose you realize what this meant: In 1927, Prohibition was on in America and drink was a rare thing, especially when you had no money. He also recommended a good pension for me to stay at.

Well, the timing of my trip to Europe was not very good. I had just arrived in Zürich to visit with Schrödinger, and then Schrödinger left almost the same day. He'd gotten a good job in Berlin. But I was traveling lightly, except for a very heavy suitcase. So I went down to Munich to visit Sommerfeld. I arrived there, and just as I did in all these places, I came in and said, "My name is Rabi. I've come here to work." I hadn't written anything beforehand.

So there it was—Sommerfeld's office in Munich! I was shown to a room where some of his students worked, and there were Hans Bethe and Rudolf Peierls, who were graduate students at that time, and Albrecht Unsöld, who later became a well-known astrophysicist—that is, a theoretical astronomer. There were also two Americans who became very notable later. One was Edward Condon. You know the book, *The Theory of Atomic Spectra*, that he wrote with George Shortley (Cambridge U. Press, 1935), as well as Condon's other books. The other American was Howard P. Robertson, who was very well known in circles that deal with relativity. So we were the three Americans in Sommerfeld's group, who gave each other strength because we were worried that our German was not of the best quality. Every once in a while, Peierls and Bethe would go out in the hall and laugh, and we did have the suspicion that they were laughing at us.

Anyway, in the Germany of 1927, the working conditions for graduate students were very interesting in a way when compared to now. Once, Sommerfeld showed me around his offices. In the basement was one place where there was a closet with a board across, and a naked incandescent bulb over it. Right there was where Bethe worked. So there was nothing very much in the way of conveniences. I think there were only three graduate students actually working with Sommerfeld. But you can see their character somehow by their selection. Two of those three were Peierls and Bethe. I don't remember the third one.

Sommerfeld was a man with enormous dignity, a wonderful person. I was invited on Friday afternoons to the Englischer Garten to have tea with the Geheimrat [an honorary German title conferred on outstanding scientists]. It was very dignified.

Sommerfeld had a very large office, and then there was the office of his assistant, a man named Becker, and finally the place for his students. All the journals were in Sommerfeld's office. So if you wanted to look up something, you made your way to the assistant, who would then knock on the door of the Geheimrat, and then you walked in. Under those circumstances, you didn't look things up very much.

I am telling you these stories to show another way of life, which existed at that time, and to contrast it in a way from the one we have now. Of course, I don't know how it is since I finished working [in 1967]. For example, I don't know whether you need clearance [the need to make prior arrangements] at all to go from one place to another to work. I don't know whether you could come in and say, as a fresh-corked postdoc could say, "My name is Rabi. I've come to work here." The answer would probably be, "Who said your name *isn't* Rabi?" Well, it was a wonderful way to live, in a place like Germany. And as an American, you weren't part of it. You never expected to get a job there, so you were free.

In the fall, I left Munich intending to go first to England and then to Copenhagen. In England, I discovered that six marks—equivalent to six shillings—which carried me through the day in Germany, wouldn't quite give me a room in London. I saw financial disaster staring me in the face. So I went to Copenhagen.

Copenhagen, of course, was the mecca for everybody at that time who was interested in theoretical physics. Everything good came out of Copenhagen in one way or another. And so my wife and I went off. When we arrived in Copenhagen, I checked my bag, and my wife and I took our map and walked over to the Institute for Theoretical Physics [renamed the Niels Bohr Institute in 1965]. I rang the bell and said my usual spiel: "My name is Rabi. I've come to work." So the Institute's secretary gave me a key. I asked her for a suggestion on where we might stay, and she gave us a good one. I brought my wife and my bag there, and then came back.

This was September—a month of complete holiday. There was nobody around except the secretary and me. But there was something about Copenhagen that was in its walls, somehow or other. You couldn't be idle there. You just had to sit there and work, and try to think great thoughts. I recommend that you try it. It can be very frustrating.

In the course of time, several people were to appear. There was one gentleman with an enormous stutter. He tried to tell me his name, and I tried to help. And I said "Klein, Klein," as I knew Oskar Klein was Bohr's assistant, but when he came up with his name, it was Pascual Jordan, who later on became a professor and lecturer. And how he ever did it I don't know, except that he did not have this stutter when he had enough beer in him, or when he spoke English.

Then, after a while, others showed up: great names in physics like Ivar Waller, Kronig (who had been there before me), and finally the great Professor Bohr came back from his vacation.

My arrival in Hamburg

And now I come to the beginning of the real story of my life, that is, the direction of my life. Bohr had had a very difficult summer, and his assistants thought that he had been overworked and that he should not have any people there except for Kronig, who had come earlier.

And here again a most fortunate thing happened. Without asking me, but making all the arrangements, they arranged for Yoshio Nishina and me to go to work with Pauli in Hamburg. This seemed disappointing at first, to go away from the center to a place like the University of Hamburg. But Hamburg actually was the greatest institution in the world for physics at that moment. Hamburg had Pauli; Walter Gordon [of the Klein–Gordon equation]; Wilhelm Lenz, who was in molecular theory, a brilliant man; and most of all, Otto Stern, in experiment. So there quite by accident, and partly against my will, I found myself in this very marvelous place. In addition, there was Ronald Fraser from Scotland, and John Taylor, who was an American. They had both done molecular beams before, and were working now with Stern. Pauli at that time, and this is toward the end of 1927, asked Nishina and me to write a paper with him.[7]

I became aware of the necessity for me to talk some English. This was a real physical necessity. The three of us English-speaking people there—Fraser and Taylor and I—formed a little group that I crowned "the three for we who were abroad." No matter what, you had to express yourself, and for me this was only possible in English. Shortly, I left Pauli's group. I had an idea about how to do an interesting experiment concerning the magnetic refraction of molecular beams and was invited by Otto Stern to do it in his laboratory at Hamburg.[8]

Remember, back at Columbia I said we were provincial. To show you the degree to which we were provincial—and by "we" I am talking about the United States, that land south of the Canadian border—in Germany they subscribed to the *Physical Review*, but waited until the end of the year to get their 12 issues at once, to save postage. It wasn't important enough to get each issue right away.

Yoshio Nishina and Rabi in 1948. The two men wrote a paper together as part of Wolfgang Pauli's group in Hamburg, Germany, in 1927. (Courtesy of AIP Emilio Segrè Visual Archives.)

We—and here I mean Condon, Robertson, and others among my friends—felt that this was very humiliating and vowed we would change it. I must say that we did, because 10 years later the *Physical Review* was the leading journal in the world. It didn't take long. We came back and distributed ourselves among our various universities and began teaching students.

Teaching was just like raising fish—there were a lot of eggs, which we began to fertilize. And so we had this time bomb of emerging physicists. In America, we had numerous colleges and universities, the students were there, and they needed teachers. And we came back from Germany with the magic of quantum theory. Indeed, by the time World War II came, physicists could man all of the American research laboratories. We were able to recruit hundreds or thousands of people, people with a very sophisticated educational background. So it [the conversion of American physics from the provincial to the international] could be done.

And this is what frightened me so about the Russians when the first Sputnik was launched. I thought they were on to this trick of raising fish. But you can't do it unless you have a free society. This was done freely by the people themselves and was done without government support. There was no government money for physics before the war. But I'm getting ahead of my story.

The magical role of experiment

And now I begin the experimental part of my talk. It is about those great days, and how people saw marvelous things and didn't understand them.

It is well known that Stern and [Walter] Gerlach did a famous experiment that was intended to demonstrate space quantization. They passed a beam of silver atoms through an inhomogeneous magnetic field. When silver was evaporated, the atoms were supposed to have magnetic moments, which could be deflected by external magnetic field gradients. Since the atomic beam of silver had a Maxwell distribution of velocities, the beam would be deflected and broadened by the field gradients. Some would be deflected one way depending on their orientation, some the other way, and some not at all, if their orientation was perpendicular to the magnetic field.

Stern and Gerlach had a brilliant concept, and with very poor equipment they did the experiment. (See the article "Stern and Gerlach: How a Bad Cigar Helped Reorient Atomic Physics," PHYSICS TODAY, December 2003, page 53.) And the experiment, as most of you have seen in elementary books, showed a split beam, plus and minus; some were deflected one way, some were deflected the other way. But what about the middle? What about the atoms that were perpendicular? [Rabi now refers to the old Bohr–Sommerfeld theory, in which ground-state silver had an erroneous orbital angular momentum ($L = 1$) and the electron's spin and g factor were yet to be discovered.] And the story at that time was that you assigned quantum numbers m_L that were equal to plus one, minus one, and zero. What about zero? There was no zero! Instead of that fact creating an enormous sensation, they just said, "Well, m_L equal to zero is missing," which was a great statement at that time, and nobody understood it.

Since there was no logical theory available, you could play it by ear; it seemed obvious that the zero state was missing. And to support the argument, they appealed to the theory of the Stark effect, in which the $m_L = 0$ orbit should hit the nucleus. So they said, "We can't have it hitting the nucleus, so we can say that the $m_L = 0$ quantum number is missing—you just don't have it." Now you begin to see why this strange experimental result was so useful. You didn't have to resort to these odd forms of chicanery about why the $m_L = 0$ state was missing. The whole point of the experiment was that they had seen atomic silver to have spin equal to one-half, and its orientation was either one way or the other. So it was right there in front of them, and because they had been so accustomed to glib talk, they didn't recognize it.

At that time, Stern was also doing experiments to show the wave nature of matter. First, he was scattering hydrogen atoms with a ruled surface, and then he successfully used another type of lattice. He showed that the scattering was associated with the de Broglie wavelength—not only for atoms, but also for molecules.

Now a molecule is not an atom, at least if you go back to the unsophisticated days. Once you have a de Broglie wavelength for a molecule with only two atoms, then why shouldn't a grand piano have a de Broglie wavelength? Any collection of things should scatter in this way. In fact, these scattering experiments were really demonstrating the wave nature of matter. Not just electron scattering, or even atomic scattering, but also molecular scattering was consistent with the same de Broglie relationship.

Later on [in 1933], pursuing the same idea, Stern and his collaborators measured the magnetic moment of the proton. This was done against the strong advice of his friend Pauli, among other theorists. They all said, "We know the moment of the proton, because we know the difference in mass between the proton and the electron, and we know the magnetic moment of the electron." Stern went ahead and did the experiment anyway, and, of course, all of those theorists were wrong.

Will physics ever come to an end?

I'm coming to the end of my talk, and I just want to tell you one more small story. I could go on telling stories, as you see, for a long, long time. But this is one story that you should take to heart.

I went with my mentor, Otto Stern, to visit the great Max Born, who was then at the very height of his glory, with his probabilistic interpretation of the wavefunction and so on. At that meeting, he told us very seriously that in six months' time, physics as we knew it would be over.

That was quite a blow! Born had an impressive personality, and he said this with a certain amount of reason because it was 1928, and Dirac had just given us his miraculous theory of the electron.[9] Making no assumptions other than relativistic invariance, Dirac derived the correct spin and magnetic moment of the electron. Everything that one wanted to know about the electron came without any extra assumptions beyond relativistic invariance. So this was a terrific achievement, of course. And Born apparently felt that it wouldn't take more than six months for these very bright boys around him to derive the spin and moment of the proton from a similar theory, and then it would be all over. As he explained, there would be a lot to do, of course, but physics as we knew it—more or less groping blindly around in our optimistic way, that portion of physics—would be behind us.

Well, I found Born's prediction very hard to believe. In fact, I couldn't actually let myself believe it. At my stage in life, I had far too much at stake. On the other hand, you will hear and see such predictions again as your careers develop. Most probably this will be particularly true for the graduate students and young people in the audience, because at every past period of synthesis in physics, the future looked closed.

In Newtonian times, physics was a closed book. There were central gravitational forces, and equations describing what they could do. People tried to come up with solutions to these equations, but some types of problems led them to invent other forces. And of course, along came Maxwell's theory of electromagnetism—all very beautiful, set, done, and apparently closed. But occasionally Nature does something strange, such as the photoelectric effect, which appeared just at the peak, the very triumph, of the Maxwell theory. It was uncovered first by accident during Heinrich Hertz's experiments on the detection of electromagnetic waves,[10] but he missed its significance and was unable to explain it. And so I have come to think that physics is a never-ending quest.

In closing, there is one other mystical thought that occurs to me. Now, in a day when we need all this big equipment for physics experiments, such as those vast accelerators that we have, I began to think: Will God reveal himself only to rich people? Would it really be true that you had to have a very wealthy country with a large population in order to get some basic information about how the universe is made? At this point I am a mystic, and I don't believe that only the rich and powerful can achieve true understanding. And I suppose it is up to you to prove me right.

Thank you. And I love questions.

Discussion

Jan van Kranendonk: A very down-to-earth question, perhaps. When you worked with Otto Stern, from what funds were the experimental apparatus supplied? How was this research work funded?

Rabi: That's a very good question. There was something, I think, called "der Notgemeinschaft der Deutschen Wissenschaft." Somebody might properly translate this, but it's the Society of Need for German Science, which got some money for grants, but I don't know whether it came from rich people or from the government. But the greater part of researchers' money, at least in some cases, came naturally from America. Didn't we beat the Germans in 1918? And now we had to pay!

The Rockefeller Foundation, and other foundations, supported students—people like Felix Bloch and Edward Teller. Many other people applied for and got Rockefeller fellowships and grants. They had equipment in the laboratories at Hamburg that we certainly didn't have at Columbia—and it was funded by American money. And very wisely, the Rockefeller Foundation was interested in getting good research and the best science for its money. And that was to be found in Germany at that time. That's where they spent it.

My eyes boggled when I saw all the equipment they had in Hamburg that I couldn't get in America. There were special kinds of vacuum pumps and other things. They had pumps which would cost $200 or $300, which was an enormous sum then. But when I came home and started doing research, I had to get pumps for $8. So you can see how research in Germany was funded: There was an enormous respect in the United States for German science, and an enormous feeling of inferiority for American science.

I think, as [J. Robert] Oppenheimer once expressed it, "We went to Germany, so to speak, on our hands and knees." But it took only a very short time, in the post–World War II period, for the whole flow to be reversed. In 1926 you couldn't get anywhere with English in Germany, because they didn't know any. I remember how surprised one German was to hear another German speak English. And if you wanted your research to be recognized, you would publish either in German or in the British journal *Nature*.

And you can compare that with today; English has almost become a universal language. But I would like to warn you: From 1927, the year that I was talking about, to 1937 or the beginning of the 1940s was only about 10 years, and during that time there was a reversal. And some of you who are very proud of not knowing any other language but English have got to learn some foreign languages. One other point about that: I know at Columbia they have also abolished the language requirements for the PhD. This is an enormous mistake.

If you want to read the originals of many important physics papers from the earlier part of the 20th century and most of the previous century, you won't be able to read them in English. Most of these original papers have not been translated into English, and you don't get the flavor of the original papers from textbooks. So I would suggest you take that very seriously to heart and learn some other languages. I don't know which, it's your guess . . . maybe Dutch [said with a kind smile toward van Kranendonk, referring to his slight accent].

Question: Could you elaborate further on how it was that you could appear, apparently unannounced, to work at the institute that you spoke about, and they knew that you would be acceptable? Is that what you intended to say?

Rabi: I was intending to show another period of time, when the world was simpler, and despite the first great World War, it still had that simplicity. A scholar could roam around and be accepted where he went. I didn't mean to put this to the test. But being a romantic, and an American, it didn't seem to me necessary to prearrange things. I mean that this favorable reception didn't surprise me. I just thought it was normal.

Another view of things

One thing that I learned contains a tremendous amount of anthropology in just one sentence. One of Otto Stern's assistants was a man by the name of Fritz Knauer. One time I was telling Knauer that in my country you could travel from one place to another and you didn't have to register with the police—you just traveled freely. Knauer looked shocked at this, and he said to me, "You mean to say that you can live and die in America, and nobody cares?"

Now that may sound very funny to you, but it shows the other end of the telescope. Something that I thought was an awful imposition—registering with the police—was to him a great support. It takes quite a bit of training to live in a democratic country like America, it takes a lot of training indeed. Some people who came to America, such as Russian refugees, have been shocked to learn that they have to find a job by themselves.

It is only when I look back on that time, especially with modern terms in mind, that I am surprised that nobody asked who funded me. At Hamburg, I had an idea for an experiment and I was invited to do it, and so I did it. But nobody asked me, "Are you funded?" No one at all. They gave me the equipment, and space, and so on. I had a marvelous time doing it.

We showed the Germans something that we called the "Amerikanische Arbeitsmethode," the American way of working. Usually the laboratory was opened strictly at 7am and then closed at 7pm—it was all so very un-American. We would come at 10am, and then, around 11 o'clock, the wives would come and make toast, crumpets, and so on while we went on doing our physics experiment. And we finished in very good time. It really worked. Also we were very happy while doing it. We'd have requests from the top floor of the building, "Would you please sing more quietly?" So it wasn't a time when you gritted your teeth and did an experiment. It was a joy all the time. That's the only way to do physics, I think.

Van Kranendonk: Perhaps I can ask a different question. You said that you were associated with Pauli, and I know that Pauli had a big reputation for being quite vicious. How did you find him? How did you like him and interact with him? Did you understand how he was when he worked?

Rabi: I have seen him being extremely vicious, as you say. I think I got along with him very well, but it was a result of a mistake that I made. Right after I came to Hamburg, I told him about some calculations I was making on the hydrogen molecule. And we had a misunderstanding between the Roman letter p and the Greek letter π [the latter is pronounced "pea" in both German and Greek]. When Pauli said "pea," I though he meant the Roman letter p [momentum], but he meant the number π. And so I said, my German being pretty poor, "Aber das ist Unsinn!" (That's nonsense!)

Nobody ever said that to Pauli. He rolled around and he said "Um . . . ist das Unsinn?" Somehow I had gotten in the first blow! But, you know, I was so upset by the way he did talk to people, until I saw that he was completely democratic—he talked the same way to Bohr. This was just Pauli's character, it was just Pauli's own way.

There was something called the "Pauli effect," which states that wherever Pauli went, misfortune followed. Not for Pauli, but for others.

Pauli had visited the astronomical observatory in Hamburg. The astronomers talked to him and then forgot about what they were doing, so the telescope hit the dome. Pauli caused things of that sort to happen. Stern would never let him into the laboratory. They were good friends, and Pauli would knock on the door and would usually want to borrow some money, and they would make their transaction right at the door.

I saw one of the most remarkable examples of the Pauli effect at a Physical Society meeting in Leipzig. News had come from America about the invention of talking pictures, and this local professor, I forget his name, was going to give a demonstration of them. The equipment was all set up, and when the assistant threw the switch . . . bang! bang! bang! came out of the loudspeaker, and then smoke. Pauli was beside himself. He shouted out, "My effect!" And they brought up another projector, and the same thing happened. Then they had a third one set up in a balcony above, where I suppose they used to have music of some sort. They connected that projector, and it worked, which showed the relationship between distance and the Pauli effect.

But the real explanation was given by Paul Ehrenfest. You see, Pauli was born in 1900, the beginning of the 20th century, which was just an illustration of the fact that misfortunes could never come up singly. The 20th century has been a terrible century. In terms of Pauli, misfortunes never did come singly.

Derek York: Do you know anything more about why Sommerfeld never received the Nobel Prize? If so, is there any inside story on this?

Rabi: I haven't heard any inside story about it, and I don't think anybody would have raised any objection if he had been given the prize. But you must remember that the Nobel Prize is given by a committee of the Swedish Academy, and they have their own idiosyncrasies. You know, there was a book published some 25 years ago about the various Nobel awards. It discussed many things, for example, about why didn't Dmitri Mendeleyev get the Nobel Prize. It suggested some mistakes of the committee of the Swedish Academy. They were very human.

When the Nobel Prize was established, the choice of the awards was up to the Royal Swedish Academy, and they had very sincere doubts that they had the capacity to make such judgments. They felt they didn't have enough members that were *au courant* enough and mature enough to make good judgments. I must say that their early judgments were terrible. But they gave it to Albert Michelson, and they gave it to Pieter Zeeman. They really had a tremendous field to choose from, and I think that is what established the Nobel Prize with such prestige. In addition, the Nobel Prize is presented by the king and queen in royal fashion. All the Nobel recipients are able to live for a few days in the manner to which they would like to become accustomed.

Van Kranendonk: Well, perhaps on this note we should end, and may I then ask you to join me in thanking Professor Rabi for his visit, for his talk. And let's send him our best wishes.

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The online version of this article is linked to the complete and unedited transcript, which has considerably more material in it.