THE EARLY DAYS OF QUANTUM MECHANICS

by

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Introduction by Professor Jan van Kranendonk

Could I please have your attention? Today's colloquium is the last one for this academic year, and it is clear that we are going to end with a very special occasion—a kind of BIG BANG!

Professor Rabi, it is not only a great honor for us, but also a great pleasure to have you with us today, and we are grateful that you have taken the trouble to travel to Toronto in this not yet so hospitable spring season.

As everybody knows, Professor Rabi has been associated with Columbia University for a very long time indeed, and he spent most of his scientific life there. However, he started studying chemistry at Cornell University, but then switched to Columbia to study physics, where he graduated in 1927. From 1927 until 1930 he was a postdoctoral fellow in Europe, at various places. It's about this period, I assume, that he will be telling us today.

He came back to the United States in 1930 and became assistant professor at Columbia University, and started his work then building up what became known as the Molecular Beam Magnetic Resonance Laboratory. His many years of work, in particular during the period from

* The discussion of Rabi's doctoral studies at Columbia has been repositioned in chronological order, the talk has been divided into subsections, a minimum of continuity has been added to the transcript, and some explanatory notes and references have been added by R. Fraser Code.

1930 to 1940 at Columbia, made Columbia one of the great centers of science in the United States. Many people, whose names we all know, passed through that laboratory—like Millman, Kellogg, Kusch, Zacharias, Ramsey, and so on. In 1944, he was awarded the Nobel Prize in Physics for his work on nuclear magnetic resonance.

During the war years he spent most of his time as associate director of the MIT Radiation Laboratory. He also set up and built up the Columbia Radiation Laboratory which made such a tremendous contribution to radar research during the war. After the war, he branched out into many other activities—like nuclear disarmament—and there is no doubt that Professor Rabi is now one of America's most distinguished scientists and statesmen.

However, today he will tell us not about these developments, but about the very early days of quantum mechanics, and it is a very great pleasure for me to ask Professor Rabi to address you.

Professor Rabi: I hope this albatross works! [Referring to the microphone for the tape recorder and public address system —*ed.*]

Thank you very much for this thumbnail sketch of early youth, misspent middle age—and now, here I am!

This is my first visit to Toronto. I've been to Ottawa. I've been to Montreal. How ever in the world I missed Toronto, I don't know. I wasn't repelled by the weather, because it's very much like New York weather, and I've gotten quite accustomed to it.

I have something in common with [Ernest] Rutherford—that distinguished Canadian physicist, 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 which broadened my education, but did not line my pocketbook, I went back to Cornell to study physical chemistry. But I'd taken all their courses in physical chemistry. 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 about 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.

Canada had more going for it since it had the great [John Cunningham] McLennan here at the University of Toronto, while we in the United States were quite backward.

There were a number of "peaks" around in the USA, like [Robert] Millikan, who was the first PhD at Columbia, and [Michael] Pupin, but it didn't do us very much good for modern physics.

I remember one time when I was a graduate student at Cornell, I was 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*).[1] That was all the exposure they had to the quantum theory. This was in 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, nor for that matter at Columbia. 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, consisting of names that became famous later. Some of you have studied from Mark Zemansky's *Thermodynamics* or his other textbooks, or Ralph Kronig's work on molecules, Francis Bitter's work on magnetism, and S. C. Wang's work on molecular structure and other things. I was a member of this group of graduate students, which 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, generally engineered by Wang.

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[2] first came out, we read it and worked through all the equations!

By the way, I was at a meeting in Europe a couple of years ago, in 1975, at which there must have been about twelve hundred people present, and I was one of the only two people there who had actually read Schrödinger's paper when it first came out! This, of course, tells you my age…

Then, just as an exercise, Ralph Kronig and I decided to write something—or do something with this new thing—Schrödinger's quantum theory. So we looked through (Max) Born's book *Atommechanik*[3] and found that the symmetrical top problem had not yet been done. So we sat down 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 which we didn't know how to solve.

And here's another lesson which 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 was a beautiful writer in German.

I understand that German is no longer required for graduate students here. Too bad, because, in reading through this 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.[4]

So that time was a very great time because we could be the first to do something. And we were the first to do this important molecular problem, just as graduate students! It was not for my dissertation, nor was it for his, but we did learn some quantum theory by doing it.

Remember that 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 A. P. Wills. Some of you may have seen his book on vector analysis and so on.[5]

Professor Wills suggested a very important problem to me, namely to measure the magnetic susceptibility of sodium vapor. This was clearly a very important question, and I knew enough theoretical physics to realize that it was.

And I began to think about the problems of making sodium vapor in a glass container, or a similar container of quartz. The problem was not easy, because making sodium vapor involved heating it up. The vapor would be corrosive, and the magnetic susceptibility of the vapor wouldn't be very big, so that measuring its value would be quite difficult.

So, after a few days I went back and told Professor Wills that "I'm sorry, but I don't think I'm skillful enough to do that measurement." He told me that he was very disappointed, and that he had no more problems for me.

But I can't tell you the great sigh of relief that came from within me. He might have had many more impossible problems like that!

Anyway, there I was, a graduate student at Columbia without a problem for my PhD thesis! But just at that time [William Lawrence] Bragg visited Columbia's physics department to give a talk. He spoke about the electrical susceptibility of a certain class of crystals called the "Tutton salts." These are a whole class of compounds that crystallize in the same general way—with the same structure—but with very significant individual differences.

The Tutton salts included a whole family of double sulphates, for example, iron ammonium double sulphate 6H_2O. Bragg had measured their electrical susceptibility, and so I thought I could do their magnetic susceptibility for my PhD. It sounded like a very good idea for me to see what differences there were between these two susceptibilities.

I told this to my supervisor, Professor Wills, and he agreed, being glad to get rid of me. And it was a wonderful subject for my PhD research, because I could start by growing crystals. All you had to do was pick up a dish, saturate the solution, put it in a box and let it stand. Then you could go to the opera while the crystals were growing. The project was very successful.

The crystals did grow. But how to measure their magnetic susceptibility was quite another matter. There was the standard method by Vogt, the great German crystallographer, on how to do this, but it was very difficult.

First you had to get a goniometer which was capable of both cutting and grinding. With this tool you could cut out sections of the crystal, and then put them in a magnetic field. Of course you also had to measure the magnetic field, calculate the various field gradients, and so on. Then you had to measure the force on the crystal section. From that you could calculate back and get a particular average of the principal magnetic susceptibilities. By repeating the measurements for different angles, you could then calculate the various principal axes of the ellipsoid of magnetic induction for the crystal. It looked terribly hard, so that I began reading other things very seriously.

Among the books I read was [James] Clerk Maxwell's treatise on electricity and magnetism. It came out about a century ago. From this, I got a much simpler idea about how to measure magnetic susceptibilities of crystals, which I actually used. It involved immersing the crystal in a saturated solution, and so on.

As a result, I could do the whole thing in a short time, and I could read theoretical physics as it came along. So that I had the best of two worlds: I learned some theoretical physics, and my thesis experiment, using the old method of Vogt, was so difficult that I got a great reputation as an experimenter. Actually, it didn't involve anything more than taking a piece of glass, pulling it out to make a spring, attaching it to the crystal with a piece of sealing wax, and so on. So there I was. I had to write up my thesis and I got my degree. But in between—and I'd recommend this to everyone—I took my time, didn't work too hard, and read about what was going on.

Visiting quantum physicists in Europe

So the time soon came when I had finished my dissertation.[6] But there were no jobs around in the USA, so I got a small fellowship to go to Europe [a Barnard fellowship —*ed.*]. I don't know how you would convert it into modern money, but 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 Zurich, 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 Zurich, I tried to find a *pension* 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 these people in a very definite way. In 1927 the Russian revolution was about ten years old, and Americans always wore a white shirt, but with their collars attached. You could recognize an American anywhere with a white shirt—a white shirt with its collar attached. 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 *Schweitzerdeutsch*, which was the local dialect of German. I was very pleased to hear that. Later, Linus invited me to where he stayed and he gave me a drink. I don't suppose you realize what this meant. 1927 was Prohibition in America. Drink was a rare thing, especially when you had no money. He also recommended a good *pension* to stay at.

Since then I've always had a tremendous loyalty to Linus Pauling. To this very day I take vitamin C upon his recommendation. I've no idea why, and when I'd tell some medical people about this, they would look down their noses and laugh at me. But I do this because, basically, I'm a loyal man.

Well, of course, 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 [Arnold] 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 on the theory of atomic spectra that he wrote with George Shortley, as well as Condon's other books. The other American was H. 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.

At that time, Unsöld was preparing to go to Caltech, where he had a fellowship. He was, in typical German fashion, very particular. He worried about what he should take, where he would live and what would he wear, what would he need, and so on. So Sommerfeld said to him "*Nehmen Sie das nicht so ernst. Das Leben in Amerika ist gar nicht so schwer. Da wird jeder junger Mann ein Assistant Professor.*" And let me translate that "Don't worry about this. Life in America is not at all that hard. There, every young man becomes an assistant professor."

Sommerfeld did have a very rosy picture of America. But here I was, a young American, and I had no job at all!

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. That was where Bethe worked. Right there. 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 the character of them somehow by their selection. Two of those three, and I don't remember who the third one was, were Rudolf Peierls and Hans Bethe.

Sommerfeld was a man with enormous dignity. A wonderful person. I was invited on Friday afternoons to the Englisher Garten to have tea with the *Geheimrat* [an honorary German title conferred on outstanding scientists, somewhat equivalent to the British title of privy counsellor]. 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 these circumstances, you didn't look things up very much!

Well, 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 now since I finished working.

For example, I don't know whether you need clearance [the need to make prior arrangements —*ed*.] 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, we spent a very delightful time there and learned a great deal from my two friends [Edward] Condon and [Howard] Robertson. It was a wonderful way to live, in a place like Germany, and as an American, because you weren't part of it. You never expected to get a job there, so you were free.

I remember once we went down to a local fair they were having. We walked down to the fair. We could smell it blocks away because there was this brass band whose members were inhaling beer and blowing it out through their trumpets. Now Condon and Robertson were two big burly fellows, and they started playing "Peas porridge hot, peas porridge cold."[†] And a little

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[†] A children's chanting and clapping game popular at that time in America, where two people chant, and clap in the specified way on the capitalized words.

Peas porridge hot,
[*Clap thighs, clap hands, then partners clap right hands.*]
Peas porridge cold,
[*Clap thighs, clap hands, then partners clap left hands.*]

circle formed around us. So we went around it with a hat and got a few pfennigs. This was an interesting part of our student life.

And of course the time in Munich wasn't entirely lost, because we stayed in a *pension* with three meals a day, and a room, a very nice room, for six marks, which was a dollar and a half at that time. So my $1500 fellowship wasn't so small after all.

I am just continuing along with my story in 1927. Quantum mechanics and its development will follow along as I go.

In the fall, I left Munich intending to go first to England, and then to Copenhagen. In England I discovered that the 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 down to Copenhagen.

Copenhagen, of course, was the mecca of 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 to Copenhagen, but we weren't too particular about the rough journey across the North Sea—in fact we were eager to get it over. When we arrived in Copenhagen, I checked my bag, and my wife and I took our map and walked over to the Institute of Physics [renamed the Niels Bohr Institute in 1965 —*ed*.]. I rang the bell and said my usual spiel: "My name is Rabi, I've come to work." So she [the institute's secretary] gave me a key to the institute. I asked

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Peas porridge in the pot,
[*Clap thighs, clap hands, then partners clap right hands.*]
Nine days old.
[*Partners touch left hands, clap own hands, then partners clap both of each other's hands.*]

Some like it hot,
[*repeat actions*]
Some like it cold.
Some like it in the pot,
Nine days old.

her for a suggestion of where we might stay, and she gave us a good suggestion. 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 institute's secretary and myself. 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 Jordan 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, Ralph Kronig, who had been there before me, and finally the great Professor came back from his vacation—Niels Bohr. He invited me to his office and he had the great kindness that, instead of asking me what I was doing, he started telling me about what he was doing, with great enthusiasm. There must be many people here [in the audience —*ed.*] who knew Niels Bohr. He was a man who couldn't talk to you sitting down: either you stood and he sat, or you sat and he stood. In this case I sat, and he walked back and forth, telling me what he was doing. Well, it wasn't a very successful operation because he had just bought a new pair of shoes, and they made a lot of static as he walked back and forth. I greatly enjoyed and will always remember this occasion, but I cannot tell you the content.

My arrival in Hamburg

And now I come to the beginning of the real story of my life—my direction—that is, the direction of my life. Bohr had had a very difficult summer, and his assistants thought he had been overworked, and that he should not have any people there except for Ralph 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 myself to go to work with [Wolfgang] Pauli in Hamburg. This seemed disappointing at first, to go away from the center to a place like 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 to that, 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 I to write a paper with him.[7]

But before we get into that part of my story, I'll make a few digressions. I'll first comment on the importance of reading about new ideas in physics, and then I'll also do a quick review of the history of quantum mechanics, including how it affected the physics side of my life prior to going abroad to Europe.

Digression A: More on the importance of reading

Remember that earlier in my talk, I advised the graduate students not to work too hard, and to keep reading about what's going on. The importance of reading current papers is illustrated by the following story.

When the Goudsmit–Uhlenbeck idea of the spin of the electron first came out,[8] it was a very shattering notion—a spinning electron! I knew about magnetism and could calculate from the free electron what its magnetic susceptibility should be. Of course, it—the actual susceptibility—should be very strong, a very high susceptibility indeed, because you have a free magnetic moment.

But actually the measured magnetic susceptibility of free electrons in a metal is practically zero. And I went around very excited because there was something wrong. Either [George] Uhlenbeck and [Samuel] Goudsmit were wrong, or there was something wrong with the Boltzmann statistics on which this was calculated. The famous professor [Karl] Herzfeld came through Hamburg on a visit. I asked him if Boltzmann statistics could possibly be wrong. … "No! Out of the question!"

Well the point is, he had missed reading Fermi's paper, and I had missed reading Fermi's paper.[9] I must have been in the laboratory or something of the sort, or otherwise I would have had a great "*in*" on the problem of the electron theory of metals, which was later done by Pauli—on just this subject—this explanation of the negligible electronic magnetic susceptibility.[10]

But you can see in that great time, when things were coming along, how, if you were "along with the ideas," it was very easy to get close to really fundamental physics, and that's the excitement of this time, when quantum mechanics was young.

Digression B: Quantum mechanics in distant America

Before I went to Europe—as I told you earlier—Kronig and I did the problem of the quantum mechanical symmetrical top when we were graduate students. We got the Schrödinger equation, separated it and so on, and got the answer. And when all the work was done, we obtained a differential equation for the wave function ψ, but we did not have the faintest idea what it meant. It was a magic 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 wave function was—what was this magic function ψ?

Of course it became clear soon there after when Born[11] 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—and suddenly the wave function ψ acquired a great meaning.

But it was so magic, 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 which 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. There were other great equations of that sort—including the Kramers–Heisenberg dispersion formula. The development of physical relationships from the correspondence principle was all done by artistry, imagination, and it followed from certain kinds of symmetry ideas. So the results that came out from 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.[12] It was a wonderful book, a clear book, and he was a master at it. However, it was published and came out just at the time when the revolution in quantum mechanics came. Unfortunately, it became obsolete almost on publication! The same was true with Pauli's first volume, which had a lot in it. But when the revolution came, it all changed.

I think that Van Vleck was stranded by all this, and he didn't write any basic books after that. But later, he did write some wonderful books and papers on magnetism.

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.

Remember at Columbia in 1926, there was our little group of serious thinkers, including Professor 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! 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—"they" in Germany 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!

"We"—and here I mean Condon, Robertson, and others among my friends who were all from Hamburg—felt that this was very humiliating and vowed we would change it. I must say that we did, because ten 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.

I mean that it 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 needed teachers! And we came back from Germany with the magic of quantum theory. Indeed, by the time World War II came, we could man all of the American research laboratories. We were able to recruit hundreds or thousands of people, people with a very sophisticated background of education. So it [the conversion of American physics from the provincial to the international —*ed.*] 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. And my lecture time is almost over.

My own work in Hamburg

An earlier section of this talk concerned the time when I came to Hamburg, and Nishina and I published a paper there together with Pauli. Then, I became aware of the necessity for me to talk some English. This was a real physical necessity. At that time there were two other

English-speaking people who had also come to Hamburg: Ronald Fraser and John Taylor, working with Otto Stern. After they turned up, and I saw them frequently, we formed a little group together. I crowned it “The three for we who were abroad.” No matter what, you had to express yourself, and for me this was only possible in English. So I left Pauli’s group. I had an idea about how to do an interesting experiment and was invited by Otto Stern to do it in his laboratory at Hamburg.[13]

The magical properties of molecular beams

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 Otto Stern and Walther 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, they 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.

They had a brilliant concept, and with very poor equipment they did the experiment. 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 which were perpendicular? And the story at that time was that you assigned quantum numbers 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 said “Well, m equal to zero is missing!” which was a great statement at that time.

Since there was no logical theory available, you could play it by ear, and 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 $m = 0$ orbit should hit the nucleus. So they said “we can’t have it hitting the nucleus, so we say that the $m = 0$ 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 $m = 0$ 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 Otto 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 they got 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 off 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, pursuing the same idea, Stern and his collaborators measured the magnetic moment of the proton. This was done much against the 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. Max Born was then at the very height of his glory, with his probabilistic interpretation of the wave function 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! Max 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.[14]

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 believe it. At my stage in life, I had far too much at stake! On the other hand, you will hear such predictions again, and see them as your career develops. 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.

The essential role of experiment

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 lead 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.[15] [Hertz noticed that the spark produced by the detector ring was largest when the spark gap of the receiver was directly exposed to the spark produced by the transmitter. Subsequently, he showed that it was the ultraviolet portion of the transmitter spark's spectrum that was responsible for enhancing the brightness of the receiver's spark. —*ed.*] And so Hertz was the first to observe that there was a photoelectric effect, 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 be really 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.

Discussion

Van Kranendonk: Prof. Rabi, would you be willing to answer some questions?

Rabi: Oh yes, I love questions.

Van Kranendonk: I see…are there any questions? Any questions? 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 des 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 their money, 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 and got these fellowships from the Rockefeller Foundation. They had equipment in the laboratories at Hamburg that we certainly didn't have in 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 their 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 that I couldn't get in America. There were special kinds of vacuum pumps and other things. They had pumps which would cost two or three hundred dollars, which was an enormous sum then. But when I came home and started doing research, I had to get pumps for eight dollars. So you can see how research in Germany was funded: there was an enormous respect which existed in the United States for German science, and an enormous feeling of inferiority for American science.

I think, as once Oppenheimer 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, where English has almost become a universal language. But I would like to warn you, from the story I told you, that from 1927, the year that I was talking about, to 1937—the beginning of the forties—was only ten 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. This is not necessarily true forever, in that sense. One other point about that: I know at Columbia that they have also abolished the language requirements for the PhD. This is also an enormous mistake!

If you want to read the original of many important papers in physics 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 paper 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 towards Van Kranendonk, referring to his slight accent —*ed*.]!

Van Kranendonk: Any other questions?

Nathan Isgur: I don't know much about the biographies, but did you ever come across George Gamow in Europe?

Rabi: Oh yes! He was a good friend of mine.

Isgur: Was he really as much of a joker as it appears?

Rabi: He was, continually. And to some extent his appearance was a joke! He first came out from Russia wearing a strange suit. Somehow or other his trousers were "sort of big," especially in the rear. Maybe he had been on a diet, and he couldn't fill them anymore. They were droopy pants, in effect. But Gamow was really tremendous. He always thought about interesting things, his cartoons were excellent, and those popular books he wrote were very good. Bohr was a great admirer of his books, and said, "Everything about them was wonderful, except the physics." But Gamow was one of the great physicists of the 20th century.

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.

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 "Amerikansiche Arbeitsmethode," the American way of working. Usually the laboratory there was opened strictly at 7 am and then closed at 7 pm—it was all so very un-American. We would come at 10 am, 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.

Tony Key: Could you tell us of any later interactions that you had with Max Born?

Rabi: I didn't see Max Born after that except when attending a meeting. Then came the Nazi period, and he went to Scotland, and I didn't see him afterwards. I don't remember if I saw him after the war or not, when we went back. But Max was a very disappointed man because he didn't get the Nobel Prize at the time of Heisenberg, and so on. I've noticed that some of those people—refugees who had a deep sense of German culture—like Max Born, Otto Stern and some others—could never become acclimatized to an Anglo-Saxon culture, or to an American culture.

Key: Did Born finish his life in Scotland?

Rabi: No, he went back to Germany and he finished his life there. I think it had something to do with the size of his pension, and things like that. Of course, I think he felt at home in Germany. I can understand it, just as I described my own reaction to speaking German all the time. I did feel the necessity of speaking English from time to time. I can understand why he returned to Germany.

One other 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 which 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 find that they have to find a job by themselves.

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 which 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 "p" and "π" [the latter is pronounced "pea" in German as it is in Greek —*ed.*]. When Pauli said "pea," I thought 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 this to Pauli. He rolled around and he said "Um—ist das Unsinn?" Somehow I got 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 Niels Bohr! This was just Pauli's character— it was just Pauli's own way.

Paul Ehrenfest gave an explanation—not for the Pauli exclusion principle—but for 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 there in Hamburg. The astronomers talked to him and then forgot about what they were doing, so that the telescope hit the dome. Pauli caused things of that sort. Otto Stern would never let him into the laboratory. They were good friends, and he [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" once 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! A big 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."

As I mentioned earlier, the real explanation of the "Pauli effect" was given by 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.

Van Kranendonk: Any further questions? It is perhaps unfair to tire Dr. Rabi out so much, but then it is also such a marvelous occasion—for he is such an excellent lecturer—and willing to answer the questions that are asked.

Allan Griffin: During your wanderings around in Europe, during the period that you are discussing, did you ever meet any Canadians?

Rabi: To tell you the truth, it never occurred, but it was incidental. We—and many Canadians will agree with this—never distinguished between Canadians and Americans. For distributing fellowships and so on, this question never came up. But name a few from that period and I'll tell you.

Griffin: Professor Harry Welsh.

Rabi: Oh, he came much later. He is a young man.

Derek York: Do you know anything more about why Dr. 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] Mendeleev 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 were 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—a notable list—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 matter 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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