Dr. I.I. Rabi May 13, 1966

Pupin Lecture - Physics Colloquium Columbia University

Dr. Rabi: We bring our own pointers here. (laughter)

Well, you may wonder why I am talking with such an extraordinary title. This has a history. For a number of reasons. In the first place, we scientists are really different from the civilians, in this sense: most people have a certain foothold in the present, but ~~we~~ really live in the past. The past is very real to them, and they take their referents to the past. The scientist has a certain slight foothold in the present, but he's always looking for the future. What will the future theories be? What will be the future of this or that experiment or this or that field of work?

As a result, there is a gulf between the two cultures. I have a feeling that it is more important now than it has been for physicists to have a greater feeling of history, a greater feeling of their position in history. I didn't feel this way when I was young, or when the Pupin Physical Laboratory was young, but times have changed, and there is no question that scientists, and particularly physicists, have an extraordinary role, a continuing role to play in the affairs of the world, a vital role -- sometimes one thinks of it as a fatal role. And in order to make contact with the rest of humanity (which I'm told is a billion) who live more in history than we do, I thought it would be somewhat worthwhile to say a few things about this particular element of history, science, which is represented by the Pupin Physics Laboratory.

While this was only one reason, of course it is the exalted reason. Another reason is quite personal. There are a number of things. This is the 40th anniversary of when Pupin was opened and made available. It's the 40th anniversary of when I sent off my doctor's dissertation. It's the 40th anniversary of my marriage. (laughter) It's the 50th of when I entered Cornell. So 1926 is a very fateful year for Rabi.

But more than that, as I will show you, it's a most extraordinary year for physics, which I will explain in detail. There is still one other point -- and you see, I need a great many excuses for saying what I did say, because physicists are generally pretty shy about talking about general subjects, and here in Pupin I never would have dreamt of talking about a general subject. The only reason I'm doing it is because I'm not a professor of physics, but a university professor. (laughter) So I have to generalize a bit. But the reason I'm really doing it is a scientific one. This dissertation which I sent away 40 years ago -(we were married the next day; the only way I could get it typed) (laughter)-- is a paper which was never presented in colloquium. I gave more colloquia when I was a graduate student than I've given since, because I always had graduate students to fob it off on. I gave a colloquium on the

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Stern-Gallic experiment. I remember it very vividly. And I gave a colloquium on reflection of X-rays and so on. But I never gave my dissertation in a colloquimm. So here you will also have a partial unfolding of my dissertation. (laughter)

And I can tell you right now, I was very pleased about this dissertation, because it was absolutely my own. It was one of the few ~~papers~~ cases I know in the history of graduate work in the United States: a student found his own problem. And this was a matter of desperation. The reason was that Professor Wills, to whom I went to suggest a dissertation topic, had a brilliant thought. Why not measure the magnetic susceptibility of alkili vapors? Now, those of you who are far enough in physics will think what this means to a graduate student -- the susceptibility of a gas, the susceptibility of a hot gas, of a corrosive gas. Well, it looked pretty tough. I thought about it for about two weeks, and I came back to him and told him I didn't think I was good enough to do this dissertation. He said very sorry, he's going to give it to somebody else. (He didn't find a sucker.) And there was no other suggestion.

Well, there was a colloquium by Bragg, where he measured the optical properties of crystals. And I said to myself: why not measure the magnetic properties? This I proceeded to do. I'll tell you about it when I come to it. But a little now of the general history, background, atmosphere of the university, and particularly physics.

Believe it or not, we have the most extraordinary history of physics at Columbia. It isn't very clear. I talked it over with Professor Haner. He was the first PhD in the physics department. There was a PhD. We're not clear whether it was the physics department or in the school of mines, but just the very title will show you the level of physics in the United States in the year 1897, in which there was a PhD dissertation of a Mr. Ihlsing, and the dissertation was: "The Velocity of Sound in Wool." (laughter)

This was not so very atypical as you might think, because one of the most respected members of the department once did a paper in the Physical Review on "The Magnetic Susceptibility of Marble." Another basic substance.

However, you will all be pleased to learn that the first honest-to-goodness authentic PhD in Columbia was the great R.A. Millikan, and he did his work with Pupin. Pupin was the great figure at Columbia at the turn of the century, and Millikan went to study with Pupin. He didn't mean to. He didn't mean to at all. He was a student at Oberlin, and there was a professor of classics there who was very fond of Pupin, and Pupin's ambition was to
physicist?
become a physical?education teacher. But he was very good in the classics, and the professor of classics was very fond of him, and wanted him back in Oberlin. And there was to be a vacancy in the physics department. Unknown to Pupin, he applied for a fellowship at Columbia -- I think it was a ? fellowship-- and Pupin first heard about it by reading it in the NYTimes, that he was awarded the fellowship.

Q: You mean Millikan.

Rabi: I'm sorry. Yes.

He went, and he came to work with Pupin. He was a good physicist, I suppose, but just the same, when I was chairman of the department, and wanted to frighten the timid students away, I'd say, "Columbia's pretty difficult. Pupin was our first PhD. Millikan? He got a fellowship, but it was not renewed."

Well, anyway, he was our first PhD, which I think was a very auspicious beginning.

The Physics Department was first housed in Fayerweather, some of you may know where it is, which is now the home of the History and other departments, and believe it or not, if you've ever gone into it, you wouldn't believe it, because it's actually built as a physics building. But in the grand old style of 1895, with ceilings as high as this and broad marble staircases, two great big entrances -- all in all, very posh -- but there was no room for research. Altogether they had two middle sized rooms and one small room and the in which to work there. Professor Haner, who now is here, and Mark Samansky, who's here, managed to find ~~mmm~~ space and work out their doctors' dissertations there. I'm not going to tell about Mark's dissertation. It was very fancy indeed.

Well, that building became antiquated very soon, as you can see, because it was an architectural triumph, and very beautiful. About 1924, I think this Eeno of Eno Salts died and to everybody's surprised, left a million to Columbia University for the needs of the physics building, and the other building cost $300,000 -- a venerable, beautiful structure, $300,000. Pupin, with all its equipment, I think cost about a million and a half at that time. Now, it wasn't all that cheap, because you have to divide by a factor of three or four, because you could get a perfectly good meal -- soup, main course, coffee, dessert and so on -- 35 cents at Ockie's? . So make your adjustments. Anyway, Pupin was built, and we moved over in 1926.

As I look around, we have three veterans here, people who did their dissertations in Fayerweather and came to Pupin. The place was empty. I was going to set up some experiments, and I had the whole of the fifth floor!

This is 1926. When I got back to my own case, ~~to/sum/up~~ - it's somewhat before. I would like to give you a little bit of the setting of how it was, then, and you can compare the situation to now. I was a graduate student at Cornell for a year, and the second year I applied for a fellowship and didn't quite get it. I'd met a certain lady who lived in New York. The following year I came to Columbia. It must have been about 1923. I'm never quite sure of these things.

Of course, there was a problem of keeping alive. Most of our graduate students had jobs here or there, somewhere around the city. Those who were fortunate, some had assistantships here. That's something I could never manage. I did get a job at City College as a tutor -- $800 a/year, 16 hours of teaching plus some extra work. In 1926, I was married. I had 15 hours in the daytime and 9 hours at night in classes, and I think as a total I got the sum of $1600. Now, I'm not trying to make any picture of some oppressed creature. I was delighted to have this, and somehow or other, in spite of that one found plenty of time. I had a certain device for getting rid of students in math class, but I won't tell others. (laughter)

Everybody had that. I don't know, there may have been one or two who had their own funds, or one or two that I know about who had some sort of fellowship which enabled them to work full time. So you see, for anybody to work in physics at that time, it was a self-selective process. You really had to want to. It wasn't an easy life of any sort, but it was a very rewarding life.

Let me tell you just a few things about 1926, and the period before. I went to the library and looked up some dates. In late 1925, the   Physique -- by the way, that was the journal you read at that time, and if you go back and look at its issues in the 1920's, you'll see they are very very well-worn. In 1925 was Heisenberg's first paper (I looked up the   ) and   von Gauschmitt, the electron spin. Then followed almost immediately, by   with the matrix mechanics, and the theory of collisions, by Bonn. 1926, Vensler published the WKB method; Dirac ~~Durach~~, the fundamental equations of quantum mechanics; the hydrogen atom, 1926; the radiation theory, in 1926; the transformation theory was published in '27, sent in in 1926.

And finally, in 1928, was the theory of the electron. So in the space of about two years, you had Heisenberg's case -- one man constructing almost all of the bases of physics. Also in 1926, you had the basic papers of Schroedinger, from the first paper almost to the most important one at the end.

An interesting observation of what people can do when there is tremendous internal pressure for a time. Because if you take this period I've mentioned, either Schroedinger or of Dirac, received such tremendous momentum and start -- what's the next thing he will do? Well, people generally ask that. It reminds me of two stories. One, the daughter of our previous professor of astronomy, a little girl, was watching him, and he blew a smoke ring, and she said: "Daddy, that's an O. Now blow a T."(laughter

The other one is about the man who's sitting next to this lady at dinner, who is fascinated with stories of his adventures in Africa. He'd been on a safari, and a lion hunt. She asked, "How many lions did you kill?"

"Oh," he said, "I didn't kill any."

There was an embarrassed silence, and she asked in a small voice, "Isn't that rather few?" (laughter)

And he said, "Not for lions." (laughter)

So if you ask yourself: "What did Dirac do from then to now, aside from his capon poles?" you say, "Not for lions." It's the same thing with Schroedinger. James Franck ? once told me that there are three stages in man's life --
(Becoming, Being and Signifying). He suggested: "Please let the

fall by the wayside." But actually nowadays there's an enormous change. Signifying meant something at that time, and one simply had to look at James to see what he really signified. But now the mood has changed -- one reason for this lecture -- because "becoming" is the thing. "Being"? No, that's already slipping. "Signifying" is on a shelf in the back room. So that I suppose I have to say that both Schroedinger and Dirac had a long time signifying, but Heisenberg is still becoming. See? He's still becoming.

The physics courses which we had -- and Mark, you'll correct me -- the education of a physicist, well, Pupin gave ? and magnetism. I never attended his course myself because I heard he did a lot of reminiscing. I'm very sorry now I didn't go, because he knew Maxwell and it would have been very fine, but as you see, when I'm trying to correct a condition which is also my own. Professor Wills gave analytical mechanics and vector analysis. Vector analysis included dioptics ?diatics? . Professor Webb gave the partial differential equations of physics, and ran the EKA laboratory. Professor Pegram, who was the chairman of the department and Dean of the School of Engineering and a number of other things, taught thermodynamics and interbuious mechanics. And then there were courses in sound then. Professor Davis gave the course in sound. Then we had two extraordinary teachers, Professors Farrol and Settinghouse who monopolized the elementary ~~mmmm~~ courses. And this is about the size of it. There was once a course volunteered in atomic physics, where Professor Wills spent most of the term giving you a run-through of the theory of function of complex variables. Somehow or other the theory of function of complex variables had something to do with the structure of the atom. What it really was, of course, was evaluating integrals, which one had in the old quantum theory.

So we had a very good ground, in having had analytical mechanics, electricity and , in a fairly solid form, and the partial differential equations of physics. Now, this doesn't look like much as we expect a first year graduate student to have. There was no statistical ~~It was mostly physical~~ mechanics, for example, and of course quantum theory, the old quantum theory, was so new. When I was a graduate student at Cornell -- this is in 1925 -- Somerfell came to lecture. And I was sitting there in the little library. I saw one professor after another sneak in to look up his book, , to catch onto the thing. This was more or less the level, which was low indeed.

Actually the following year, 1927, when I went to Europe, I found that the Physical Review was received by Gottingen, but they waited until the end of the year to ~~collect~~ get all the twelve issues at once, to save postage. This was the level of regard which American physics had. I'll come back to that in a moment.

Now, on the other hand, don't get any idea that we, the graduate students --(never mind professors) -- that we the graduate students were ignorant. (laughter) When you're on your own, you can do great things, and I'm so glad that Mark Samansky is here, because during those years we had a private seminar, no member of the faculty, consisting of Mark Samansky, who is here, and of course we always had to have a brilliant Chinese and this was S.C. Wong ?, who was by far the brightest ? of all of us, and Ralph Brunich, who is now professor in Utrecht, and Francis Biddle of MIT and myself. We formed ourselves into a journal club, and when the first paper of Schroedinger came out, we read it. I was just then beginning to study the old quantum theory. And if you want to have feelings of ~~superiority~~ inferiority ?, I suggest to some of the graduate students here, pick up Bohl's and start reading the old quantum theory, and you'll find it's pretty tough. I was working my way through that when Schroedinger's paper came out. And I said, "This for me." I had Carlson's " ? physics" and this was there, and our seminar worked it up, and Kronig suggested that maybe, to show we understand it, we ought to do something. So we looked through the book to see what problems were not done, and we found the symmetrical topic had not been done. Schroedinger gave one a formula for setting up the wave equation. So you set up the wave equation, and we had the equation, and then we looked at it. We decided that it might be a good idea to separate the variables. So we separated the variables, got off the angle variables, fine, everything nice, those things. And then there was the other function, the other differential equation, which was something neither of us had ever seen, and we had no idea what to do with it.

Here's one of those fortunate coincidences that happen to one who is curious. In the midst of all this teaching, 25 hours a week, I found myself sitting in the library, Low Main Library, browsing, reading through Jacoby's works -- nice German, it was easy to read. I was reading along, and there was the equation! (laughter) Just, there was the equation. It was the differential equation for the confluent hypergeometric function. Of course, none of us had ever heard of it before, but there it was, and pretty soon we got to the formulas and set up the whole thing. We had the solution to the equation, which gave the energy levels, and in addition to that you could calculate the intensities and so on. It was all great. We published it in a very few pages in the Physical Review, and, to give you an idea of how the language hadn't hardened yet, the title of this is "The Symmetrical Top in the Undulatory Mechanics." (laughter) Snappy!

I might add parenthetically that the same thing was published a little later by a famous mathematician, together with a physicist, in Germany, in about 40 pages. Ours was very brief, cut down, because the Physical Review then was broke, and insisted on your cutting down the size of your paper.

However, this was not my dissertation. The dissertation, as I said before, was this experimental dissertation. About this time also, ~~the~~ as I said, the spinning electron was unveiled. Now, Ralph Brunig had been in Europe the year before, and was supposed

to have suggested the spinning electron. But when it actually appeared, he was very much against it, and he inspired me to be against it, and I found a valid objection. Because if the spinning electron actually had the moment of a
then the magnetic susceptibility of the alkiloid? , assuming a free electron, should be very big and paramagnetic, and actually it's diamagnetic.

I published this in Nature. After asking many questions, (in German)
because I said in that paper,
I dismissed Fermi's paper on Fermi's statistics. Otherwise perhaps it would have been I rather than Pauli that would have written the first paper on the electron theory of metals. However, this is how things go. (laughter)

Now, I've described the situation in physics, those papers that kept coming all this time, this little group of serious thinkers who met every Sunday more or less from 11 A.M. to 11 P.M., always finishing at the Chinese restaurant. My wife, I think, has heard more physics talked about than any woman I know except Professor Wu.

Now I want to tell you about my dissertation, and go on with it. As I said, this was my dissertation: I was going to measure the Principal ~~principle of~~ magnetic susceptibility of crystals. Now, the method for doing this was well known, and there were very important papers published on it. The method was in fact invented by the great German physicist .

I think I mentioned that. If you go to the library, you'll see a great big book there on crystallography, where he gave the method-- an excellent method. What you did was to have a cutting room grind , and you cut a section
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out of this crystal, ~~on/the~~ of a moom size and from the moom direction of the crystal. This you hang in a magnetic field, with a homogeneous portion of the magnet here, on a delicate ~~mm~~ tortion balance, and you measure the force on this crystal in tortion balance. You do this for three sections, and knowing the volume, you can get the susceptibility in those directions. Then you grind up the crystal, and get the average susceptibility. When you've found this, you have four equations, so you can determine the principal magnetic susceptibility and the angles. I didn't have the cutting and grinding nor did I feel like making one of these very delicate tortion suspensions. But there's one thing I can do easily. Since my undergraduate work was in chemistry, I could -- I had a box in my room, and I made some solution, and grew crystals. Believe me, that's a nice pleasant job, because you just put it in there and you go to the opera or read theoretical physics. This went on for some time, and I still wasn't getting my degree but I was getting plenty of crystals.

One day, in reading Maxwell -- and I really advise people to read the original papers -- reading Maxwell, it was something I already knew but it struck me, that if you have one substance immersed in another, the effective susceptibility is the difference between the two. So I got the idea of doing this problem by taking my crystal, suspending it inside a solution, saturated with respect to itself, and varying the susceptibility of the solution by adding or subtracting something. Now, you see, I was free of the other difficulties that you have when you have the thing by itself, which depends on its form and shape, and the great differences of susceptibility on the inside and the outside which gave you the force. So in this way, if I could find a solution which had the same susceptibility, then all I had to do was the measure the susceptibility of the solution, and that's very easy. I'll show you how it works:

This is the dedication of the laboratory. This is the notice that goes up. It's not a part of my experiment. You see, the date is 1927, but the laboratory had been in operation by that time, and from this platform no less a person than Nicholas Murray Butler gave the dedicating address, where he gave us the message that somehow or other the physicists weren't getting across to the public.

I knew that, and I didn't know quite why he was telling us that. I didn't see why we should. We were just as arrogant as the young high energy people are around here. (laughter) I realize now that he did have a point.

Now, the next line ~~Y (or XY?)~~ shows the apparatus. There was a magnet, in four pieces, and you see this circular thing that holds the solution, and the crystal is suspended.

Suspended in it. And of course, if there's paramagnetic, it would be sucked in between the poles. The next line. The next line shows the other face of this. You have your crystal, which is held by a glass thread with just a small piece of wax, the small piece is marked there and the vessel, and the reservoir of solution, and a microscope to see the thing is switched down the magnet, the thing moved, in or out, and there's a head which measured the angle. Believe it or not, this is all there was to this apparatus, except for one thing which I will show next.

Yes. After you had your solution, you would put it in a glass vessel, put it between the pole pieces of the homogeneous magnet here, and have actual balance, and we simply weighed this with -- weighed the force -- seal on and off ?? ~~along here~~, and then substituted ?? water, seal on and off, and you got the susceptibility of this with respect to water.

So this is all the apparatus which one had, and you can see there's nothing very learned about it. In fact, the fanciest thing I had was a switch, a switch when you take this magnet on and off.

The next line. The crystals which I did were so-called ? tutten salts, double salts of a paramide metacrystal, of a para and an ~~akk~~ alkili of ammonium, and there's a ? modicum of silicon . There's a whole series of these salts, where you substitute the paramagnetic ion, one for the other, and you substitute either ammonium or alkili for another. The important thing about these ~~x~~ salts, for my purpose, was, they crystallize in the same system and at almost the same angles. So this was an interesting thing to investigate, to see two things -- one, the magnitude of the susceptibility in the different directions, and the angle with respect to a given crystal axis, what relation that had to the other properties.

I discovered one wonderful thing, quite by accident. As you rotate the crystal, in the plane, the ellipsoid induction of course, the total ellipsoid induction is a -- no, ellipsoid.
(? or, north?)
So ~~fm~~ a section, then, is~~xx~~ no-ipsoid . So there'll be a direction of maximum ~~mmmm~~ susceptibility and a direction of minimum susceptibility. And I discovered that ~~it/was~~ the direction of minimum susceptibility was an unstable formula and that if you turned it to the right, the crystal would move sideways, in one direction -- to the left, in the other direction -- and you could get a very sharp indication of the direction of movement of susceptibility. So one of the things which had to be gotten out of the other calculation was measured here directly. ~~of/direction~~. In fact, in this way everything could be measured directly.

Next slide. ~~line~~. This is just put on to show that there is a considerable amount of mathematics connected with it. Here's the general expression for the faults, and then you take the comparable of the symmetries in the case of Maxwell's differential equations, and you get a system.

The next slide, this is only one page of the results, and you can see the relationship ,~~/particularly~~ the angle with respect to the axis of the magnetic quality, and the other with the optical one. You see in some cases they fit, in some cases they're very far apart. And you see the susceptibilities here, in the different directions. And the expression given. First, the OK way to write susceptibilities and to look for was called the "Weiss ? magnetron ." This is a name which I'm sure most graduate students haven't heard, but Pierre Weiss was a great physicist and worked in the field of magnetism for many years, and looked for units, and this unit was the Weiss magnetron. Then Bohr came along and gave a new unit, a more natural unit, and this was a very modern thing.

This list of susceptibilities -- and I might mention, there are some things of various physical interest. In one case you'll see that ~~with~~ as in the manganese salts, there is hardly any difference between the axes. In other cases there is a very large difference between the axes, as you can see, for example in this ? cobalt.

Here it is almost a sphere, and in these cases-- I'm not looking in the right place -- Oh yes. Yes, here it is. But we didn't know then was the natural explanation for this, because in the case of manganese the ion was in an S state, quite freely rotating but not very much influenced by the local environment, and in the other cases there were in a free ? state.

Now, this represented more results than had been in the entire literature before, and on the other page of the paper there was the susceptibilities. So the great thing about it was, I remember how surprised Mark Samansky was when he came in one afternoon and I did a crystal. So, after waiting for a year or two to think of the idea, I was able to do the dissertation in about six weeks. So sometimes it's worthwhile to take a little thought.

It had another result. There was a graduate student who left Columbia and became a professor at Bolton, and he finished his work, his course work, and wanted to do a dissertation. Bolton was not a rich college then, and the question was: what apparatus can you have? What do you have? Well, they had an electro-magnet. Really, this is all you need. So he carried this work forward. (off tape)

(side 2)

... at the temperature of dry ice, which you get from the local drugstore, and you get the difference in temperature, and find that you get three constants which come into the theory of susceptibilities.

On further history, I have two points to mention. Although it was published in the Physical Review, the first paper -- in the first issue, 1927 -- it was well abstracted, in the abstracts and in the journals, and so on, nobody ever saw it. And Van Vleck, when he wanted to compare experimental and theoretical work years later, found a reference to this work in an Indian journal. It happened there that

? picked this up, carried on the method,

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refined it, and made a career in moheeto chemistry, using these methods ~~x~~for measuring susceptibility.

I might say, the total expense for this was about $50, which was hard to get, to get some sesium ? and salts for carrying out these experiments.

Now I'll come back to the story, as the hour goes on. In those days, if you wanted to go very much further in physics, you had to go to Europe, which meant really mostly going to Germany. After I did my dissertation and got my degree, I got a small fellowship from Columbia, about $1500, and took off for Europe, where my wife joined me later. I was as informal then as I am now, only more so. I never wrote to anybody that I was coming. I got a ticket, and the first place I went to was Zurich, where ~~to~~ Schroedinger and were, and so I roomed in a pension and went over to the physics building, found that there was a colloquium, a seminar, going on, went in, sat down -- didn't understand a word!

Well, I didn't know anybody, but afterwards there were some people standing around, and you could always tell an American then -- maybe now too -- he wears a white shirt with the collar attached. There were a couple of people like that, and I went up and introduced myself. One was Jay Stratton, who's just now retiring as president of MIT. When I told him my troubles, he said, "Don't worry, this wasn't German, this was Swissdeutsches. Nobody's expected to understand that." (laughter)

The other man was Linus Pawling. I introduced myself. He took me with him to the place where he was living. He was just finishing a year there. He fed me strong spirits, which was, for an American, very special -- we were in the throes of Prohibition, and this was almost my first real chance. (laughter)

Well, it turned out that as I arrived, Schroedinger was leaving, and I could just about say hello to him. So I went down to Somerfell, and meanwhile I'd gotten a suggestion for a pension to stay in Munich. Again, I arrived in Munich. Not a word. I came in, introduced myself to Professor Somerfell, and said, "I've come to work here."

Well and good. He gave me a room, where there were seated ? --Hans Weber was a graduate student at the time, and so was Piles, Easle. This was the room where they sat. I forgot to tell you, in the pension when I came down for breakfast there was the most German-looking man I'd seen so far, hair slicked way down and all that sort of thing, sitting there, reading the American Mercury. It turned out it was Ed Conant. With Robinson, we had very lovely times together.

But I had the problem, in a sense, of going in there and working, and it was not easy. To give you the impression -- it's not so easy to be a graduate student in Germany, because the journals were in the professor's office. Next to the professor's office was his assistant. Then there was the room where the students were. So if you wanted to look up something, you went into the next room and asked the assistant whether you could go in to the professor's room and take a look at the journal.

And downstairs there were some special places. You'd come
? room?
along and you'd see a dim, open bulge? and some graduate student writing on a dissertation.

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Some afternoons the heimarck would invite you to tea, and so on. But these assists which we had here in this country were unknown there.

Well, after some weeks of this, I decided to go to a meeting of the British Association in England, and then on to Copenhagen. My wife joined me there, and we came to Copenhagen. There again, I hadn't written a word. We arrived in Copenhagen. I bought a map, checked my bags, and my wife and I walked over to the Institute. I rang the bell. This was September. A secretary answered us. I said, "My name is Rabi. I've come to work here." She gave me a key to the building. (laughter) I asked about a pension, and she suggested a pension. I left my wife at the pension and came back and went to work. There was nobody else there. They were all on vacation.

After a while a man arrived who was very much at home. I assumed it was Bohr's assistant Klein. I went up and introduced myself. I won't give you his reply, which was in the most fantastic stutter. I tried to help him to say Klein,
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but this didn't help -- it turned out it was Yobar.

After a while the others came in. I was taken in to see Professor Bohr, and he was most kind. Immediately he started to tell me about what he was working on, and I felt very fine --

sat there. He always had somebody sit, and he'd walk up and down. Unfortunately he had just bought new shoes, and you know, between his general burr and this static, I didn't understand a word.

But it turned out that Bohr wasn't well, and that I could not stay. Very ~~waxxx~~ thoughtfully they arranged ~~than~~ for Mitschner and myself to go to Hamburg to work with Pauli. So we went to Hamburg to work with Pauli. Pauli had been apprised and was all set when we arrived there, arrived in Hamburg. And to show you how a person's life depends on sheer accident, present there ? were Fraser, who had written this book on ? and John Pale, the American, and an Englishman, a Scottsman.

Well, if you've ever had to be in a foreign country and speak a foreign language all the time, you realize that sometimes you have a real physical necessity to speak your own language. They were working with Otto Stern on light beams. The subject was not unfamiliar to me, because of a colloquium I'd had to give at Columbia -- and I really gave it. I remember, after the first hour, when I'd described every single screw of that apparatus, I turned to another document? doctoral and said, "Now we turn to the theory." Professor   was fast asleep. But I learned a lot out of it. Anyway, I knew the subject, and they were weak on theory, and I could help them out. And I had an idea for an experiment, suggested it through Professor Stern, who invited me to do the experiment.

I didn't want to do the experiment. I came there to learn theory. But they told me it was a great honor. So there I was -- (laughter) -- a first-year PhD, no job, because I had to resign my City College job, they wouldn't give me leave, and a wife. I was in no position to refuse the honor. (laughter)

So we did the experiment, and we had a grand time. We did the experiment in a style they'd never seen in Germany before. As Dr. Stern said, he was going to introduce the "American ? because the normal way, the building was opened at 7 and closed at 7. We were not that sort of persons. We'd come at 11,but not at 7. And who wants to leave at 7 when you're just going good? Pale and I worked in the same room, and whatever I knew about ? techniques I learned from him, which he had learned from Illinois. We had a grand time. People would come down and ask us to stop because we were interfering with the lecture, we were singing so loud. (laughter)

Wives would come, late in the evening, and make toast and so on. It was just great. We finished the experiment in what they considered a record time, in a year, and left to go to Heisenberg, in ~~Heidelberg and~~ Leipzig. I wasn't worried about Heidenberg. He was really marvelous. There was a colloquium there, the Heisenberg seminar, and there again I just showed up. I arrived with my family; we lived there; it wasn't easy, because if you've ever tried to live in a temperature of 30 below, when the only heat you had was one of these ornamental stoves, the kind of stoves they had, where you could get the temperature above 50 only by hanging the thermometer on the stove (laughter) -- well, there was a wonderful spirit. Actually the seminar was devoted mostly to ping pong. It was interesting. They had a variety of nationalities. There was a Chinese named Cho, and he was the chairman, and he played ping pong like a Chinese sage -- calm, imperturbable, but he returned every ball. Ultimately the other fellow would make a mistake.

Another one played with this great Hungarian verve -- wham, wham, wham!

At the Institute, the theoretical colloquium of Leipzig was invited to Berlin, for a joint colloquium, and the Saxon government approved of this and paid the railroad fare and the hotel, and gave us each ten marks to have a good time. And we went. Among the people who went, there was only one German, Heisenberg. This was early 1929, well before the Nazis were of any importance and so on. I'm telling you this to show that there were very few Germans actually studying physics, even with the great Heisenberg. As to be expected, of the 12 or so that went, there were about 8 Americans, an Austrian, Bulgarian, Chinese, and so on, different nationalities, in this way.

And what we got over there --(this is a concluding remark) are most of the friends I have, of my generation, I met in Germany, who were there at the same time -- Oppenheimer, Austin and so on. What we got -- I discovered, when we came there, it's not that I knew less than the German students of my stage. I knew more. The education, as far as factual knowledge, was excellent. What we got in Germany -- it comes into the subject of this talk -- is the tradition of physics, by working closely with people who had made physics. I was fortunate enough in Hamburg for a whole year to have lunch every day with Stern, Pauli, Lenz, who were bachelors, and it was just a very great experience.

So what I would say I got in Europe, and the rest of us got in Europe, was not knowledge of physics, but the feeling for physics, the development of taste, the development of insight, the comparison of oneself with others. It's this difference between being, so to say, provincial, and metropolitan. And at that time it was hard to be metropolitan in this country, because we did not have the people who were making the important physics.

Now, I told you, American physics was a bit of a laughing stock in Europe in 1927. Our generation came back. And it was not realized that we had a time bomb operating, because contrary to what was going on in Germany, we had a lot of students. What they needed was leadership. And this developed in different centers in the United States, to a certain extent, with this group of us of that age group, who had determined in the first place to end this humiliation, and secondly, had something.

By 1937, this very journal was the leading journal in the world. So it doesn't take long to make the transformation, provided you have the people who were there, and we had the universities, and a certain amount of leadership for them.

So I have tried to give you a rather personal story. I could go on for another hour very easily. Some feeling for, in a certain sense, not the beginning of things -- the beginning of things in the United States was too horrifying for words. I could read you complaints by Henry ? which would describe what the situation really was. But when things were already under way, and then coming up in an exponential kind of way.

Is there a lesson in this for us? There is. Don't look down on any other countries, especially big ones. (laughter) Because I remember Lord Charwell said he did not believe that the Americans had the culture to do theoretical physics. You see, I'm faking you out. So this is in a certain sense a lesson in humility, and also a lesson in hope -- that physics, an intellectual thing, is a universal. It's a universal and a human thing, and when a culture is ripe for it and traditions are ripe for it, there's no doubt that any people can do physics and do it greatly. Thank you.

(applause)